APPROACHES TO THE STUDY OF THE ANCIENT NEAR EAST

A VOLUME OF STUDIES OFFERED TO

IGNACE JAY GELB

ON THE OCCASION OF HIS SIXTIETH BIRTHDAY
OCTOBER 14, 1972

EDITED BY
GIORGIO BUCCELLATI

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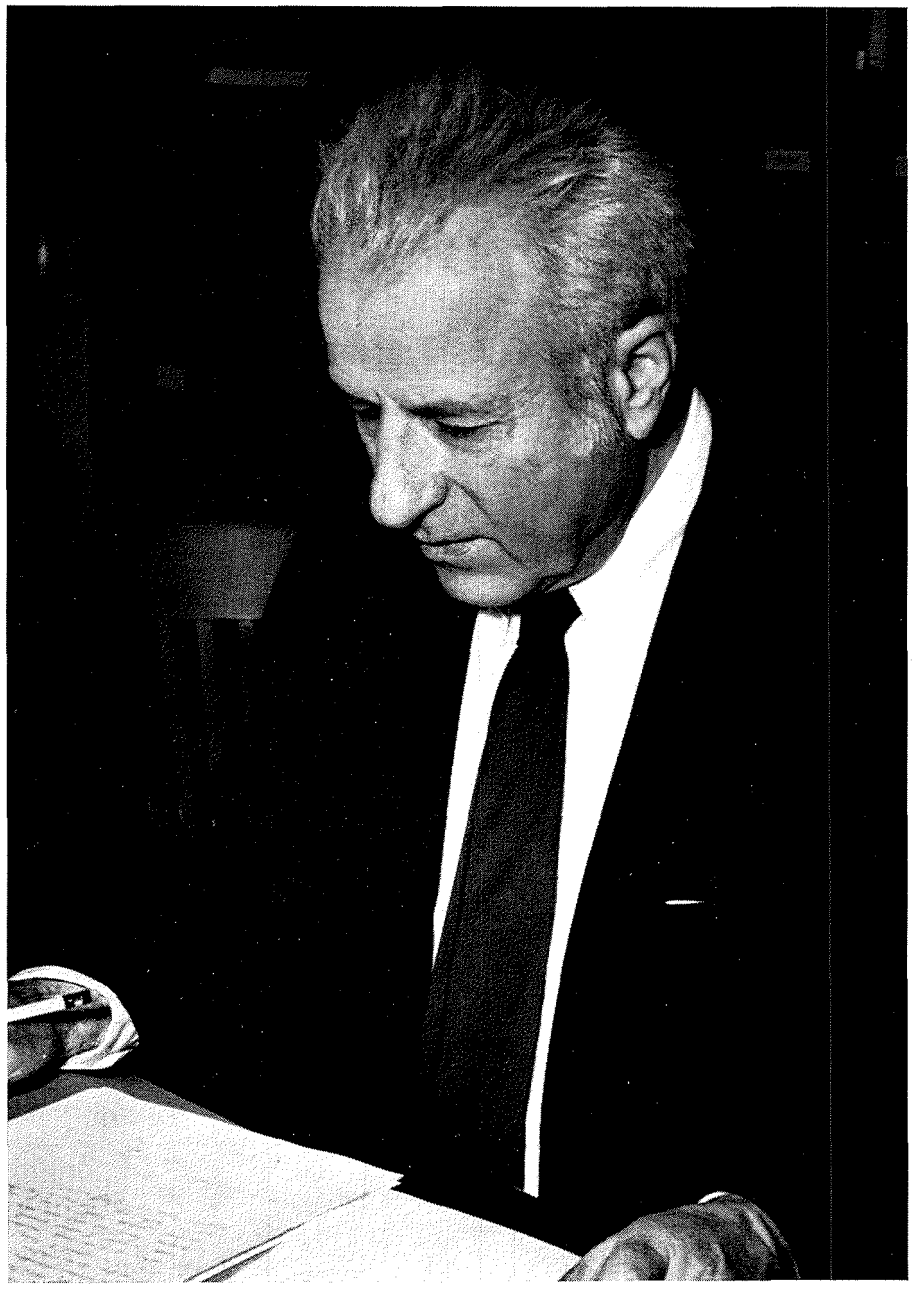

IGNACE JAY GELB

On the Occasion of His Sixtieth Birthday
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Methodological Concerns and the Progress of Ancient Near Eastern Studies

Giorgio BUCCELLATI – Los Angeles

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 unchartered 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 inderscored 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 progresss 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 Callender'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 prescinds 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 idiosyncraisies 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.

The Sumerian Writing System: Some Problems [1]

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1. Introduction

1.1. A considerable number of the most vexing problems of Sumerian grammar (e. g., the so-called amissible consonants, some verbal affixes, the root alternations, etc.) can be solved either by reducing them to graphemic problems or by interpreting correctly some peculiarity of the writing system. Sumerologists have too often taken the written version of a text *as transliterated by them* at its face value, disregarding scribal conventions, and adopting, in practice, if not in theory, the principle that "what is not in the written text is not in the utterance". The traditional position is best stated by Poebel: "Das sumerische Schriftsystem ... trägt einen durchaus phonetischen Charakter, indem jedem Zeichen oder einer bestimmten Zeichengruppe ein bestimmter ein- bis viersilbiger Lautwert zukommt" (*GSG* § 12). In other words, logograms are nothing but more or less lengthy syllabograms. Trying to improve Poebel's position, Krecher states that "für den Schreiber die Zeichen bzw. Zeichengruppe primär Phoneme fixieren, nicht Bedeutungsinhalte" (*ZA* 58 [1967] 18). While laudably attempting to dispel the notion (one which no serious Sumerologist defends at present anyway) that the signs of the script have a semantic content without any phonological connotation, Krecher's statement, if applied to the standard orthography, is misleading because it disregards the fact that the script is basically logographic, representing morpholexical [2] units which are subject to morphophonemic changes. Curiously enough, the pioneer Sumerologists sometimes had more exact ideas about the matter. Thus Lenormant affirms that "comme dans toutes les écritures en grande partie idéographiques [i. e., logographic], le scribe peut se borner à tracer l'idéogramme verbal ... laissant à l'intelligence du lecteur et à sa connaissance de la langue le soin de rétablir la modification du radical" (*Études sur quelques parties des syllabaires cunéiformes* [1877] 74 ff.), and consequently he interprets [mu–un–si–si–eš] as

[1] For some time I have been promising to publish a book on Sumerian writing. For practical reasons, however, I have decided to postpone its publication until the mss. of Ea (*MSL* 14) and Diri (*MSL* 15) are sufficiently advanced, so that the index of values in the book can be used as indices to these *MSL* volumes. In homage to I. J. Gelb, who has contributed so much to the study of writing, some of the ideas behind my book are given here in an abbreviated form. [ ] = graphemic transliteration; / / = approximation to a phonemic transcription; # = word boundary.

[2] I. e., morphemic units with a semantic content and as represented in the lexicon of the language. *Lexicon* should be understood in this article in the narrow sense with which it is used in the generative-transformational approach, and is to be considered, therefore, as an integral part of the grammar.

/ munsisigeš /. The traditional position would be acceptable only by assuming that Sumerian completely lacks morphophonemics of the normal sort. While this may be true of a few languages, e. g., Vietnamese, it is hardly applicable to Sumerian, where (in composition) [sikil] becomes / skil / in [ki + sikil], [lugud] becomes / lgud / in [sa+lugud], and [ka+guru$_7$] was pronounced / kugre /; (in inflection) certain types of reduplication of a root such as [gir$_5$] have the allomorphs /gir/ ~ /gig/ ~ /gr/ (note that the last one can not even be represented in isolated form in a syllabic script). A more detailed study of verbal root alternations marû-ḫamṭu, for instance, undoubtedly would show that, even excluding the cases of suppletive paradigms, the phonological changes involved often go beyond the simple addition of [–e]. For a further discussion of the function of logograms see 2.3, section 2, and 3.21.

1.2. In the study of an extinct language, the description and analysis of its writing system(s) forms an *integral* part of its grammar. The chapter on phonology must of necessity be preceded by a chapter on the writing system(s) used in the corpus of texts since in such cases phonology is accessible only through a system of graphic correspondences with other known scripts (see 4.1). The chapter on graphemics should provide, in addition to an inventory of symbols, the rules which establish the correspondences between the graphic symbols and the elements of the spoken utterances. No matter how interesting, all other considerations (which clutter most studies on the Sumerian script) about the origin, history, shape, etc., of the symbols used, have no place in the grammar. They are irrelevant to the message-conveying function of the signs, and fall within the realm of anthropology or archaeology. The "graphemic" terminology has been introduced precisely to make this distinction clear.

2. Theoretical Considerations

2.1. Because no graphemic theory encompassing all types of writing has been proposed so far (most studies deal exclusively with alphabetic scripts) [3], a brief preliminary outline of the basis for analyzing Sumerian script is required here. The written counterpart of a spoken utterance can be considered as a coded version of it. The process of encoding-decoding involved in writing down and reading a text can best be stated in terms of the now classic model of a generalized communication system in the information theory [4]. Although in the field of general linguistics this theory has seemingly failed to fulfill its expectations, it appears to be the most powerful model available in the case of

[3] For a brief but substantial exposition of graphemic terminology and theory in a context of historical grammar, see H. M. Hoenigswald, *Language Change and Linguistic Reconstruction* (Chicago 1960) 4 ff. Up to this point, the generative field has produced nothing viable on writing; for a somewhat ineffectual attempt, see R. D. King, *Historical Linguistics and Generative Grammar* (Englewood Cliffs, N. J. 1969) 203 ff. Examples of applied graphemics are, among others: S. Allén, *Grafematisk analys som grundval för textedering* (Göteborg 1965), and I. T. Piirainen, *Graphematische Untersuchungen zum Frühneuhochdeutschen* (Berlin 1968). Piirainen's statistical definition of grapheme is difficult to accept.

[4] For an elementary introduction, see G. Raisbeck, *Information Theory* (Cambridge, Mass. 1964). The linguist's point of view is presented by H. A. Gleason, *An Introduction to Descriptive Linguistics* (revised ed.: New York 1965) 373 ff.

writing systems. Writing is obviously a typical situation to which the information theory applies since (1) there is a change in the media in which the symbols are formulated (acoustic signals in the speech, graphic marks in the writing), and (2) there is a difference in the coding capabilities (number of symbols which can be handled) of the media. While the spoken language will have a number of phonological segments which will usually fall within the twenty-fifty range, there is no such limitation in the case of writing. Several consequences of the application of the information theory to writing (some of which were applied intuitively by the ancient scribes) are given informally here:

a) Each symbol which can be used to compose a message has associated with it a certain degree of probability of its appearing in the message: thus in written French the probability of a letter being [e] is about 15.8%, that of its being an [r] only 6.4%.

b) If several of the symbols preceding the one whose probability is being investigated are taken into consideration, higher degrees of probability can be reached. Due to the internal structure of the language, this probability can reach 100% in certain contexts; in other words, certain elements are completely *predictable* and therefore *redundant*. Syntactic concord, for instance, always involves redundancy. It has been estimated that in a natural language redundancy at the phonological level is about 50%. That is, if the communication conditions were always optimum, only about one-half of the elements in operation would be needed. The redundant elements, however, are not superfluous in real life situations where imperfect articulation, noise, and other interferences may result in the loss of some elements of the message. Redundancy in such cases helps in reconstructing the original message and may have, furthermore, an aesthetic value.

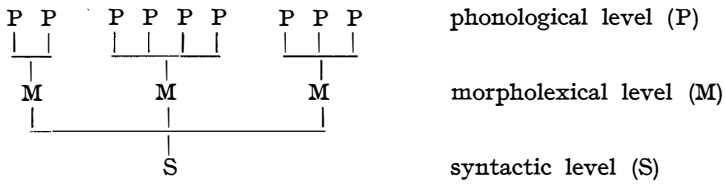
c) Redundancy becomes truly superfluous in the case of a written text which can be repeatedly reinspected if necessary. Many characteristics of certain scripts, including Sumerian, can be explained by their built-in capacity for abbreviation due to the presence of redundant elements of the spoken language no longer needed in a written text[5].

d) The size of the message (assuming a more or less uniform size of symbols) is inversely proportional to the number of symbols available for its formulation. This is the reason for the survival of syllabic and logographic scripts which, despite putting a heavier burden on the memory of the encoder-decoder, give shorter messages than an alphabetical system.

e) If the symbols vary in length or complication, there is an advantage in assigning the shorter or less complicated ones to the statistically more frequent items, and *vice versa*.

2.2. Some characteristics of the writing systems are common to all coding and transmitting processes, but others can be traced back to the complex structure of the spoken message itself. For the purpose of this analysis, the spoken message may be considered as a multilayered structure with several "encoding" levels in a hierarchic relationship:

[5] Sumerian in its earlier stages goes farther than any other known script in its omission of elements predictable only to the well-informed native reader; see Biggs and Civil, *RA* 60 (1966) 1 ff.



The related elements can be mutually substituted, but with important restrictions: a subset of elements at levels P or M can be replaced by the corresponding elements at levels M or S respectively, but substitution in the opposite direction (from bottom to top) is not always possible because the lexical component of M-level elements, taken in isolation, and all S-level elements can be encoded in different languages, i. e., they can be translated. Encoding identifies unambiguously the language of the message only when level P is at least partially involved. A message encoded exclusively at level M can be read in all languages which happen to have the same syntactic order. A message encoded at level S could be read in any language. The feasibility of these different types of encoding is limited by the number of encodable items at each level: 20 to 50 at level P, 2×10^3 to 20×10^3 at level M, infinite at level S. Encoding at level P will produce the longest messages but also the easiest ones to encode and decode. At level M the messages will be shortened by a factor equal to the average number of sounds per morpholexical unit, but the increase in the number of symbols needed will be enormous. Encoding at level S is not practical except for marginal, mostly paralinguistic, cases. Encoding is possible, due to the bonds between levels, in two levels simultaneously.

2.3. The main types of graphic representation are the following:

1) Encoding at P-level:

P1 one grapheme corresponds to one phoneme
P2 one phoneme has no corresponding grapheme
P3 one grapheme does not correspond to any phoneme
P4 one grapheme can represent different phonemes
P5 one phoneme can be represented by different graphemes
P6 a phonemic cluster is represented by a single grapheme
P7 a phonemic cluster is represented by different graphemes
P8 a cluster of graphemes (or a discontinuous grapheme) represents one phoneme
P9 a cluster of graphemes represents different phonemes

For mixed types see 3.22.

P1 with a one-to-one correspondence between phonological and graphic elements is the ideal case never found in practice in "natural" writing systems. The deviations from this ideal norm are due to the historical observation that most languages have been reduced to writing by borrowing a foreign script, to an inertia in the spelling habits (historical writings), and last but not least to a tendency to simplify by omitting sets of phonological elements (e. g., tone or stress) which are more or less predictable for the native reader [6]. Syllabic

[6] The controversy about whether the Semitic scripts which do not represent vowels are alphabetic or syllabic has no place in the theory proposed here, since "alphabetic" and "syllabic" do not exhaust all the possibilities. The omission of vowels is simply a case of P2 encoding.

writing, a particular case of P6, deserves special consideration because of its use in Sumerian and Akkadian. Its obvious advantage is that, without unduly increasing the number of graphemes, it cuts the length of the written message to almost one-half as compared to a P1 system. Among its disadvantages are an inability to represent certain consonantal clusters, and a tendency in some systems to cut down the number of symbols by omitting consonants in determined positions. This tendency, rooted in part in native concepts of the syllable different from the popular one in Western linguistics, can be illustrated by systems as far apart as the well-known case of Minoan, and the syllabic script used up to the XVIth century A.D. for Tagalog in which [ba–ta] can represent /báta/, /bantá/, /batày/, /bátac/, etc. [7]. The fact that the symbols represent syllabic shapes does not guarantee that the syllabic boundaries of the spoken word will coincide with those of its graphic segmentation.

2) Encoding at M-level (logographic writing):

M1 there is a one-to-one correspondence between morphemes and graphemes
M2 a morpheme is not represented in writing
M3 a grapheme does not correlate with any morpheme
M4 one grapheme represents different morphemes
M5 one morpheme may be represented by different graphemes
M6 a group of morphemes is represented by a single grapheme
M7 different groups of morphemes are represented by a single grapheme
M8 a cluster of graphemes represents one morpheme
M9 a cluster of graphemes represents different morphemes

A purely logographic writing connotes only indirectly a determined phonetic shape, and it is not possible to tell which allomorph is used in a particular environment. For further discussion of the M-types, see 3.21.

3) MP-encoding (*simultaneous* encoding at phonological and morpholexical levels):

MP1 a set of allomorphs is represented by a grapheme which represents phonemically only one of the members of the set (morphophonemic spellings)

MP2 if a system admits spellings of the type P2-P7, homonyms may be represented by different sets of P-graphemes

MP3 in a predominantly M-type system additional P-graphemes (phonetic complements) give wholly or in part the phonological shape of the morpheme. Examples of MP1 are found in almost all languages represented by P-systems: the plural morpheme for French nouns is /s/ ~ /z/ ~ /∅/, all represented by [s], German [Wald] represents

[7] The phonological status of a consonant in initial position and one in final (syllable or word) position is quite different: E. Pulgram, *Syllable, Word, Nexus, Cursus* (The Hague 1970) 72 f. The omission of the last consonant in these syllabic scripts is more apparent than real due to overrigid modern transliteration systems. The values transliterated [CV] should in fact be transliterated as [$C_1V \pm C_2$], where C_2 may correspond to several consonants whose representation is subject to given environmental constraints.

both /valt/ and /vald–/, etc. MP2 is useful to differentiate homonyms as in English [bear] and [bare], both /be(ɛ)r/. French [sept], [cet(te)], [Sète], all /sɛt/. MP3 is typically exemplified by Egyptian hieroglyphics.

3. The Sumerian Writing System

3.1. Although the cuneiform signs used to write Sumerian and Akkadian are essentially the same for both languages, the way they are used is quite different, at least as far as the standard orthography of Sumerian is concerned. Akkadian has a typical P6-system of syllabograms with some M-graphemes used as abbreviations for common, often repeated terms. For instance, a letter of Anam to Sînmuballiṭ (XIXth century B.C.) uses 95.7% syllabograms, 3.5% logograms, and 0.7% classifiers or determinatives (a M3-type grapheme with a purely lexical function described below 3.21); in a sample from Sargon's annals (early VIIIth century B.C.) the ratios are 85.6%, 6.7%, and 7.6%, respectively. These figures must be compared with the normal ratios in the standard orthography of Sumerian as represented in samples from the following texts:

Ratio between types of graphemes

	Urukagina Cones B-C	Gudea Cyl B	Šulgi hymn B	Enmerkar-Aratta	An–ta–è–a–ra	Laḫar-Ašnan
Logograms	60.3%	56.0%	53.0%	47.2%	48.5%	42.8%
Syllabograms	36.4%	40.6%	44.5%	49.5%	47.2%	54.3%
Classifiers	3.1%	3.3%	2.4%	3.2%	4.1%	2.9%
-CV Syllabograms [8]	15.1%	15.3%	20.8%	14.2%	16.8%	16.4%

Akkadian administrative and legal texts have, not surprisingly, a larger proportion of logograms (30%, for instance, is a typical figure for a Seleucid document). Omina, medical manuals, rituals, and similar technical, repetitious texts with probable esoteric tendencies, have a much higher number of logograms: 84% in a typical tablet of šumma ālu. In any case, a high ratio of M-graphemes is a secondary development in Akkadian.

3.2. Sumerian must be considered basically as a M-system with P-graphemes reserved for bound morphemes, loan words, and a few roots (most of them non-native). In detail:

3.21. M1 writings are relatively infrequent due to reasons of economy. In order to keep down the number of signs, most of them function as M4 (the improperly designated polyphony [9] of signs). In the archaic texts, isolated cases of M2 are found but they soon disappear (for some particulars see Biggs and Civil, *RA* 60 [1966] 1 ff.). The classifiers mentioned above (3.1) are M-

[8] Percent of the total of syllabograms.
[9] *Polyphony* ought to be reserved for P4, P7, and P9 writings.

graphemes which alone function as M1 or M4, but may accompany a logogram to indicate lexical classes. Occasional M5 encodings may be found when the same lexical item is written differently by different scribal schools, but a more typical, yet not too frequent, function is to distinguish semantic disjuncts in lexical entries: for instance /ḡarza/ "regulations" may be spelled PA + DINGIR when it refers to "divine regulations" and PA + LUGAL when denoting "royal regulations". M8 is used in the so-called Diri-writings or in compound signs: several graphemes are combined to represent a single morpheme, thus avoiding the need for a specific grapheme and helping to keep the graphemic inventory within reasonable limits.

3.22. The P-graphemes are of the type P6 or P7 and have a syllabic shape: V, CV, VC, and less frequently CVC. Polysyllabic segments are found only in exceptional cases of rebus writing such as [nammu–ul] instead of [na–am–mu-ul–(lu)], or irregular spellings like [gaba–kar-re] for the standard [ga–ba–kar–re]. The types P6 and P7 may be considered as including several subtypes since each phonological segment of the syllabic unit can, at least in theory, be omitted, underdifferentiated, or overdifferentiated. Its representation, therefore, can be mapped in correspondences analogous to types P1-P4. Thus the sign [ÁG] is in fact /V_fḡ/, where V_f represents any front vowel, and can be considered as a case of P6 (41). If the possibility that [–Ca≠] and [–Ce≠] may represent in some particular instances nothing more than /-C≠/ turns out to be acceptable, it would be an example of P6 (13) or 6 (43), and so on.

3.23. The MP1 or morphophonemic spellings are of considerable grammatical importance. For examples see Biggs and Civil, *RA* 60 (1966) 14 ff. MP3 writings are used in several ways: they can give the full phonological shape of the logogram as in [ú. NAGA.ga.mušen] = /uga/, [ḡeš–túg. PI] = /ḡeštug/, etc., or only part of it as in [túg–ŠE.KIN], optionally written [túg–gur_8.ŠE.KIN], representing /tu(g)gur/, or in [šub.ub] = /šub/. A particular and rather unusual case is the explicit indication of an allomorph as in [ba–ta–ra–zal], an exceptional variant of [ba–ra–zal] and [ba–ta–zal], all representing /bad^r azal/, where [–ra–] gives the pronunciation /–d^{r}a/ of the infix /–ta–/ in intervocalic position.

4. Determination of the Phonetic Shapes

4.1. The phonetic shapes of a written extinct language L_1 are accessible only through a system of graphic correspondences with texts of another language L_2, for which the rules governing the equivalences between the graphic symbols and the phonological segments are known. It is possible that such information will not be immediately available for L_2, so that L_2 in turn is accessible only through L_3, and so on. Sumerian is accessible only through Akkadian, whose graphic-phonological equations are in turn reconstructed from the correspondences with other members of the Semitic linguistic family, most of which are accessible only through similar reconstruction processes based on later stages of the same or related languages. Needless to say, if the reconstruction requires several steps, the uncertainties accumulate and the degree of probability of a given solution will be lowered accordingly. Two important considerations in reconstructing the phonology of an extinct language are: (1) are L_1 and L_2 related? are there any other relatives? and (2) which one is primary and which one is secondary from the point of view of the scribe and/or speaker? For all practical purposes Sumerian has to be considered as an isolated language.

The question of whether Sumerian or Akkadian ought to be considered the primary language in the sources used to reconstruct the phonology of Sumerian is a difficult one and is partly discussed in 5.2.

4.2. Considering L_1 as secondary and L_2 as primary, the scribes or speakers of L_2 will tend to replace a sound of L_1 with the sound of L_2 which is nearest, in some sense, to the sound of L_1. The results sometimes can be startling, as when English /s/ is replaced by /h/ in Maori and by /k/ in Hawaiian. There are three basic types of distortion [10]:

1) underdifferentiation: if L_1 distinguishes $/n_1/$ from $/n_2/$, but L_2 has only one sound which can be considered similar to those, the speaker of L_2 will tend to confuse the two sounds and the scribe will normally lack the means to differentiate then. For this reason the vocalic system of Sumerian will never be satisfactorily recovered.

2) overdifferentiation: phonemic distinctions which are significant in L_2 but not in L_1 are imposed on L_1 by the scribes/speakers of L_2. If, for instance, L_2 has three classes of stops and L_1 only two, L_1 texts written by L_2 natives will, under certain circumstances, give the impression that L_1 has three classes of stops.

3) reinterpretation of distinctive contrasts: a speaker of L_2 distinguishes the phonemes of L_1 according to the features which are relevant in his own L_2 system, but may be redundant or concomitant in that of L_1. E. g., if, in L_2, the relevant contrast is between voiced/voiceless, but is between lax/tense in L_1, the speaker of L_2 will apply the voice distinction to separate the two classes of stops. This type of distortion is most likely to be the cause when a scribal school systematically replaces the (apparently) voiced stops of another language by voiceless ones, and *vice versa*.

4.3. As a rule, the phonological reconstruction of L_1 cannot be more precise nor more detailed than the knowledge of L_2 allows. In an initial approximation, using L_2 to decipher L_1, one can assign to L_1 a number of phonological segments only equal to or smaller than the number of phonemes of L_2. Qualitatively, these will appear to be identical to the phonemes of L_1, so that Falkenstein's surprise at the observation that Sumerian and Akkadian phonologically "in einer *unerwartet* [emphasis added] engen Weise zusammengehen" (Genava 8 [1960] 303) is unwarranted, and there is no need to seek an explanation for this presumed similarity on the influence of a substratum.

5. Sumerian Phonological Sources

5.1. The native sources which make it possible to define the phonetic shapes of the cuneiform signs used in Sumerian cannot be discussed in detail here (see, for instance, Krecher, *AOAT* 1 158 f.), but a few remarks are needed. One type of source ordinarily disregarded is the comparison of the divergent spellings provided by the same Akkadian translation. The use of this source is justified by the logographic nature of the script and is easily available in the lexical sections of the *CAD* and, to a lesser extent, of *AHw*. Cf. [du_9], [tur], [še–er], [šír] = enēšu, or [(si)–si–ig], [síg], [$šeg_5$], [$šeg_{10}$], etc. = šaqummatu, and

[10] Cf. U. Weinreich, *Languages in Contact* (The Hague 1968) 18 f.

so on. The importance of loan words and of sign names tends to be overrated. As far as loan words are concerned, the likely existence of phonemic subsystems reserved for words of foreign origin, as well as the adaptations across linguistic borders due to the distortions discussed in 4.2, are consistently neglected in Assyriology. Rhymes, puns, plays on words can occasionally provide useful information. The presence of /arganum/ and /argibil/ among words which are expected to include the syllable /al/ in the "Song of the Hoe", is an indication of an underdifferentiation of /r/ and /l/ in given environments. An extremely important distinction occurs between sources which give the phonetic shapes context-free (such as syllabaries) and those which provide a context (such as syllabic texts and certain glosses).

5.2. The syllabaries form a closed system in which the phonetic shapes of the signs are defined in terms of a relatively small set of basic syllabograms. In this system, the basic syllabograms can be defined only tautologically; some copies of Proto-Ea, in fact, leave the pronunciation column of such signs blank. To break the circle, a series of phonetic equations between the basic syllabograms and a foreign known system is required; without such outside sources, the syllabary would be undecipherable. For the Assyriologist, the obvious solution is to assign to the basic syllabograms the phonetic value they have in Akkadian. It is necessary to ask, however, whether this Akkadian value was exactly the same as that used by the native scribes. In other words, which is the primary and which the secondary language (cf. 4.1) in the syllabaries? The oldest syllabaries preserved (Proto-Ea and Proto-Diri) leave no doubt that the phonetic value of the signs was determined by an oral tradition: the pronunciation was learned not from the written tablet but from the teacher's mouth. Otherwise it would be difficult to explain why many copies, even when provided with an Akkadian translation, lack the pronunciation column [11]. Furthermore, the existence of variant values not explicitly given in the syllabaries (cf. Civil, *JCS* 20 [1966] 120 [8]) also must be explained by oral tradition. In this type of oral instruction, were the basic syllabograms assigned the very same phonetic value they had when used for Akkadian, or were they assigned a traditional Sumerian pronunciation, similar, to be sure, to the Akkadian one, but with phonologically significant differences? At first glance, it appears somewhat difficult to believe that an essentially extinct language, surviving only in cultural and religious usages, a language whose morphology was being strongly influenced by Akkadian and whose lexicon was increasingly less well understood, could preserve phonetic peculiarities foreign to Akkadian. Nevertheless, there are traces of articulations not found in Akkadian such as the already mentioned intervocalic articulation of [d] as /d^r/, or the like, attested at very late dates: syllabic gú-ru-su-un for gud-sún, Ξισουθρος for zi–u_4–sud–rá, etc. The different rendering of the Sumerian and Akkadian stops in the Greek transliterations seems to require a similar explanation. Despite the linguistic difficulties, the importance of oral transmission in traditional societies with a highly restricted degree of literacy can hardly be overestimated. In short, the existence of an independent phonological tradition for Sumerian has an appreciable degree of probability. Obviously such a tradition is not recoverable

[11] Out of the thirty-one tablets of Proto-Diri from Nippur, only ten include the pronunciation column.

as a whole, and only in particularly favorable instances and by indirect means can the Sumerologist venture beyond the method of assigning Akkadian values to the basic syllabograms.

5.3. To determine the phonetic value of the basic syllabograms the following steps must be followed:

1) The signs are provisionally assigned values similar to the ones they have in Akkadian. These Akkadian values must be the ones used by the scribes of the same school and period.

2) The distortions referred to in 4.2 must be detected, if possible. They will appear either as a surplus of similar values, as for instance the several [u] signs of Proto-Ea, or as variants (diachronic or synchronic). A surplus of signs must be investigated to see whether it results from an overdifferentiation from the Akkadian, or is a truly Sumerian phenomenon. Some of the diachronic variants, due to the nature of the sources, will turn out to be mere scribal errors. An example of genuinely Sumerian synchronic variation is [m] in Proto-Ea which is split in [m] and [g] (= /ḡ/) in canonical Ea. But the split of [Z] in [s] and [z] is an Akkadian scribal practice, as shown by the frequent use of [sà], [sí], and [sú] in Nippur OB Akkadian texts.

3) The requirements needed to postulate a particular non-Akkadian sound are described in more detail in the discussion of logograms in 6.2. Such sounds can be properly described only as a system of variants, and their possibility is acceptable only if there is a series of similar cases. An example would be the alternation /g/ ~ /b/, amply attested and with clearly preferential environments (before /–ur–/). These alternations will have several plausible phonetic solutions: /g͡b/, /g^{w}/, etc. A successful example of the detection of a sound alien to Akkadian is the old discovery of /ḡ/, for which several phonetic solutions are likewise possible: /ŋ/, /n͡m/, etc.

4) Even when one of these sounds has been reasonably postulated, it is not always possible to ascertain all of its occurrences: [g] and [b] are certainly in relevant contrast in [su$_8$–g] versus [su$_8$–b], by semantic criteria.

5) It must be remembered, finally, that the scribal habits of the same school and period will almost never be completely consistent.

6. Determination of the Phonetic Shapes of the Logograms

6.1. The only viable definition of the Sumerian logogram implies that it represents a lexical entry with all its possible phonetic shapes (allomorphs). Which of these shapes is usually entered in the syllabaries? The syllabaries give the Akkadian pronunciation of some very common logograms such as [A.ŠÀ] and [NA$_4$]; Ea gives consecutively their two most common allomorphs, /eqlu/ and /eqel/ and /abnu/ and /aban/, respectively. In other cases the syllabaries give only one allomorph (e. g., Proto-Ea has only /kalab/ for [UR]), and not necessarily the one which the modern linguist would consider the basic or underlying form. There is a tendency in canonical Ea to give the two forms CV and CVC for the same lexical entry, but Proto-Ea in most cases has only one form (mostly CV). Up to this point no attempt has been made to reconstruct the rules which govern the choice of a given allomorph in the lexical lists. It is quite possible that when some of the rules have been rediscovered (a complete recovery is most unlikely), it will become evident that they are based on reasons other than grammatical ones. It is probable, for instance, that the

choice of /zi/ as the phonetic shape of [ZI] is not simply due to the existence of the allomorph /zi/ of /zig/ and /zid/, but rather to the fact that the two entries (including, naturally, /zi–∅/) could be represented more economically by a single form [zi]. Thus the interpretation of the syllabaries requires a considerable degree of native linguistic competence which is not accessible to the modern Sumerologist. It is clear that, for practical purposes, to say nothing of the requirements of a generative formulation, the lexicon must list the words in a form that includes the final consonant. It is easy to write quite general rules for deleting final consonants, but it would be absurd to write rules for adding various consonants to the end of CV roots.

6.2. The main objective must be not merely a simple assignation of values to signs, but a reconstruction of the phonology of the lexical entry. It would be not only impractical but also misleading to give the full status of phonetic value to every single variant provided by the sources. The discovery procedures should be as follows:

1) Collection of all values which fulfill two conditions: a) similar or at least compatible phonetic shapes, and b) identical meaning.

2) Preliminary clean-up to eliminate obvious scribal errors and semantic misinterpretations.

3) Interpretation of the scribal data according to the scribal habits of the different schools and periods. For instance, /iki/ (Boghazköy) should not be given as a value of [IGI], since the habits prevalent among Hittite scribes requires it to be reinterpreted as /igi/ (Sturtevant's law).

4) Morphophonemic classification according to morphological class, allomorphs, and syntactic context: all these must be identical to make the values comparable.

5) Test for consistency: are all values which have been interpreted and classified identical or do several variants remain?

6) Final determination of the phonetic shape: if all the values are identical, the resulting phonetic shape must be accepted as the best available one in the present state of information; if there are variants, an explanation for them must be sought. Variants can be explained either as dialectal, historical, or, if they appear to fit the same diachronic and synchronic slot, as phonological. The latter case may be due to the presence of a Sumerian sound foreign to Akkadian, or to attempts to render an exotic non-Sumerian sound. A phonological solution must be acceptable from the point of view of phonological universals, and must be based on a series of similar cases (see 5.3, point 3).

6.3. A few examples illustrating the application of these principles are given here:

1) For the number 10 the syllabaries give the forms [a_6], [u], [ḫà], and [ḫù]. Is a restoration */ḫo/, or the like, warranted? A close examination of the syllabaries shows that the forms with /a/ come from sources that give the numerals followed by a suffix /–a/, so that /a/ comes from /u/+/a/, and the only forms to be considered are /u/ and /ḫu/. Since there are not sufficient examples of /ḫ/ ~ /∅/ in initial position to support any general statement, the two forms must be considered free variants, and [ḫù], being the more explicit form, must be given as the main lexical entry, with a cross reference under [u].

2) [tur] and [di_4–(1)] seem sufficiently close phonetically and semantically to warrant an investigation. The existence of constructions such as lú di_4–di_4–lá tur–tur–ra–ke_4–ne "the smaller (or smallest) of the little ones" shows that the two forms are not semantically interchangeable. The only valid conclusion is that the two forms be regarded as possible evidence that the process, well known in other languages, of expressing the diminutive by alternations in the root consonants (see, e. g., J. Nichols, "Diminutive Consonant Symbolism in Western North America", *Language* 47 [1971] 826 ff.) could exist in Sumerian. Naturally, other parallel formations of this kind are needed to postulate with certainty the existence of this process in Sumerian.

3) [dib] and [rib] have the same meaning "to surpass" (Akk. *šūtuqu*). Are both graphic variants of the same word? In view of the very frequent alternation /d/ ~ /r/, it is quite likely. Note, however, that [rib] does not seem to be used in finite verbal forms, so that the criterion of morphophonemic identity is not fulfilled.

4) Can the forms [ri–gi_4–bil–lú/lu], [ar–$gi_{(4)}$–bil–lu], and [ar–$gibil_2$] be combined? The word is certainly the same, but the forms are too separated in time and space, and a historical and/or dialectal explanation is to be sought.

5) Consider the following equations:

bé–e KU = *zû*, bé–ed KU = *tezû*, and še–e KU = *zû*, še–ed KU = *tezû* (Ea I 150ff.). /b/ and /š/ are not normally considered as similar sounds, but, because of semantic identity, the question of "one or two lexical entries?" has to be asked. In the first place, the distinction between noun and verb as given in the syllabaries is probably incorrect. There are a number of abbreviating conventions in the lexical lists whereby, according to one of these conventions, instead of

$value_1$ SIGN = $meaning_1$, $meaning_2$
$value_2$ SIGN = $meaning_1$, $meaning_2$

the scribe, in canonical Ea, simply writes:

$value_1$ SIGN = $meaning_1$
$value_2$ SIGN = $meaning_2$

The textual variants in the case under discussion (see *MSL* 14, ad. loc.) seem to confirm the presence of an abbreviation. The passage can be used only as evidence for the alternation of short and long forms of the root. Can the two forms be combined by assuming /b/ ~ /š/ (hypothetically one could think of a fricative labial)? So far, I believe, the only other similar alternation is in kaaš–bar/ šar[12], and this is not enough to establish a phonological pattern. The two forms must therefore be considered as two separate lexical entries, unless more examples of /b/ ~ /š/ come to light.

7. Problems of Transliteration

7.1. The preceding paragraphs lead to conclusions regarding transliteration that are far from encouraging, and show how difficult the Sumerologist's task can be:

[12] Not explainable as /b/ → /š/ in environment /š_ /, because the form is found in a finite verbal form of the compound verb. See G. Gragg, *TCS* 3 178, and cf. J. Klein, *JCS* 23 (1971) 118.

1) There is no such thing as a *Sumerian Syllabary* which can list all the values of the signs. Instead, what is needed is a relatively simple *Syllabary* and a lengthy and rather complex *Lexicon.*

2) A definitive list of phonetic values *for the logograms* will be possible only when the reconstruction of the lexicon is sufficiently advanced.

3) Contrary to common expectations and to the resolutions of Assyriological meetings and committees which have dealt with these matters, it would be impractical, and perhaps impossible, to establish a transliteration system equally valid for Sumerian and Akkadian.

This does not mean, of course, that values need to be given different subindices in the different languages, or anything as confusing as that, but it does mean that the Assyriologist must recognize that he is transliterating two writing systems which function in quite different ways.

7.2. The distinction between transliteration and transcription, which was first applied to the cuneiform field by I. J. Gelb, is a useful one and should be carefully maintained. This distinction is not sharply defined, however, because several subtypes and gradations are possible, so that one can speak of a "narrow" or a "broad" transliteration or transcription. *Transliteration,* the sign by sign rendering of one writing system into another, admits several levels; a text, for instance, could be adequately transliterated by merely replacing each sign by the number assigned to it in some standard list of signs. Such a transliteration conceivably could be useful for computer work, but is of very limited usefulness for anything else. The system traditionally used by Sumerologists, a good one and one that must be preserved, works as follows: each sign is assigned one or several values represented by Latin letters according to the data of the native sources; the selection of values (in the case of logograms) is made in such a way that the value is the one that corresponds to the meaning intended in a given passage. This process involves more than simple substitution, because it includes a semantic interpretation of the text, and thus it is more than a transliteration in the usual sense.

The basic rule for transliteration is that it must be kept as much as possible *on the same level,* in a narrow sense. Consequently, non-graphemic distinctions must be systematically disregarded. When some paleographic information is considered important, reference should be made to the number given to the sign in some standard paleographic list, even when the graphic variants are provided by the native syllabaries. Thus there is no need to make a distinction among all the [karadin] signs as in Thureau-Dangin *HS* (although the writing with [NIGIN] should appear, of course, with a special subindex), since the sign has still other graphic forms in the fairly infrequent occurrences of this lexical item in context. To keep the transliteration on the same level also requires making a decision about the choice of allomorphs. Morphophonemic inconsistencies must be eliminated. A transliteration [lugal–ni] has to be preferred to [lugala–ni]. Note that in this case the solution is even misleading: it is not that /lugal/ has an allomorph /lugala/ before the suffix [–ni], but the suffix is really /–ani/, so that it would be preferable in any case to attribute to [NI] a value [ani]. Similarly, even when a particular type of reduplication is attested in more phonological detail than usual, it has to be sacrificed for the sake of level uniformity. Thus one has to transliterate [gir_5–gir_5–(r)e], not [–gigre]. As long as the various types of reduplication and their distribution are not

well established, the use of narrowly defined reduplicated forms may well lead to an incorrect transliteration in a particular context. Furthermore, this information is not available for all roots, so that the use of the gigre-type of transliteration infringes again upon the rule of uniformity. Similar considerations apply to nominal compounds. It appears that the best rule for transliterating logograms is to give them in the form accepted as basic in the lexical entry. Since the lexicon is supposed to indicate, whenever possible, which morphophonemic rules apply to each lexical entry, the choice of the particular allomorph applicable to a specific context is already implied by the main form of the lexical entry.

7.3. *Ceteris paribus*, the *simplest* transliteration should be preferred. When a gloss gives a reading identical to that of the individual signs, the signs should be written separately, thus avoiding the proliferation of values. For instance, one must transliterate [a–gàr], not [agar]. The unwarranted inclusion of a determinative in the logogram should be avoided: no special value must be given to [GIŠ.SAR] or [GIŠ.BAD] as opposed to [SAR] and [BAD] with the same meanings; these readings come from a misinterpretation of some entries from the Diri series. The use of hyphens implies a morphemic analysis; the practice of hyphenating entire nominal phrases should, in my opinion, be avoided. The argument that postpositions affect the full phrase is insufficient (no one would think of writing [RemusRomulusque] as a unit) and, if it were applied consistently, it would then be necessary to hyphenate entire subordinate clauses, with a consequent loss of clarity and readability. The use of the hyphen must be reserved for indicating the different graphic parts of a morpheme, the affixes joined immediately to it, and the nominal compounds. In this way it is possible to make a distinction between [ki sikil] (noun + adjective) and [ki–sikil] (compound noun). Incidentally, the extra spacing between letters customarily used in printing Sumerian texts not only fails to add anything of scientific value to the transliteration, but also, as anyone who has used Kramer's edition of the "Lamentation over the Destruction of Ur" can testify, impairs readability, is uneconomical, and of dubious aesthetic value.

7.4. There are many problems which still need discussion: which non-Akkadian phonemes are well established enough to be accepted in the list of values? Hardly anyone would oppose the inclusion of /ḡ/, but what about [b] ~ [g] and similar alternations? Someday, some Sumerologists with good phonological sense will no doubt propose to transliterate [kalak] rather then [kalag], pointing to the probable existence of a rule that adds a feature + voice to final stops before suffixes with initial vowel (/kalaga/). Or will they prefer a devoicing rule? Many other questions are still unanswered [13]. A universally accepted system of transliteration, completely uniform in the smallest details, is an unlikely prospect, and perhaps not even desirable. But the Sumerologist has the right to ask his colleagues to use a system that is unambiguous, clear, economical, and above all, consistent.

[13] If and when the question of the so-called amissible consonants is satisfactorily answered (a much more difficult task than is ordinarily assumed), a means must be found to differentiate roots which are apparently homonymous but which differ in being subject or not to a certain rule. This rule may very well take a form other than consonant deletion, such as a change in the vocalic nucleus of the syllable.

New Cases of Morphophonemic Spellings

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The concept of morphographemics has been introduced in Assyriology by I. J. Gelb. Long before the publication of his article, "A Note on Morphographemics", *Mélanges Marcel Cohen* (The Hague, 1970), pp. 73-77. I was privileged to discuss with him this concept, and it no doubt influenced my thinking when I was working on my *Linguistic Analysis of Akkadian* (The Hague, 1966; in the following cited as *LAA*). In that book, one of my concerns was to separate morphophonemic alternations from other sound changes (historical or dialectal). Moreover, I divided the morphophonemic alternations (normally assimilations) into those which occur at the boundary of a nominal or verbal form and an affix and those which occur in the interior of a nominal or verbal form. Historical or dialectal sound changes (e.g. OB *št* > MB *lt*) may be reflected in the spelling (e.g., MB ⟨*il–ta–nu*⟩) or they may not (e.g. ⟨*iš–ta–nu*⟩), representing phonological |*iltānu*| [1]. A spelling which does not reflect the change may be interpreted as traditional spelling (for other possible interpretations see Gelb, loc. cit., p. 75f).

The description of alternations in *LAA* Chapter 6, on the other hand, is valid for all periods and dialects of Akkadian — on the basis of the now available evidence. For instance, a dental (D) or a sibilant (Z) followed by the palatal spirant š always goes to a long (geminate) sibilant ({D, Z} + š → ss), an assimilation phenomenon that is most commonly documented when a third person suffix, which has an initial š, is affixed to a noun or verb form ending in a dental or a sibilant. The various ways in which the cuneiform writing system renders this assimilation are enumerated by Gelb, loc. cit., p. 74. These various possibilities represent two alternatives: (1) spellings which always use the base,

[1] The change *št* > *lt* was documented for the post-OB period only, until new texts revealed that the change was attested as early as the OB period, and the new dictionaries now have the oldest form listed as *ištānu* and not, as the former dictionaries had, *iltānu*. Once this *št* > *lt* change is established for the OB period on the evidence of *ištānu* > *iltānu*, we may look for it in other words too containing the sequence *–lt–*. An example is the verb form *al–ta–ba–ak–ku*, found in a recently published OB prayer (Goetze, *JCS* 22 [1968] 25, obv. 8); this form cannot be derived from *labākum* (a possibility considered but rejected by Goetze because of its vocalism), nor does it have "appurtenance to *labūm* (= *lawûm*) ... considered and preferred" by Goetze; we may take it simply as derived from *šapākum*, i.e., *aštapak*+*ku*(*m*) which appears, with the *št* > *lt* change, as *altapak*+*ku*(*m*) and yields the satisfactory sense "I am pouring out for you". The spelling with the ⟨*ba*⟩ sign instead of the expected ⟨*pa*⟩ sign is a spelling feature which poses no problem.

or unassimilated, morphophonemic form whatever word or morpheme follows it, and (2) spellings which use the surface form, the phonemic realization of the base in the given environment, before the initial š of the following morpheme. The spelling which always uses the base or morphophonemic form is the morphographemic or morphophonemic spelling; the alternation is predictable from the initial phoneme of the following morpheme (the suffix), which may be spelled, as it usually is, in its surface form (i.e., with initial sibilant) or, more rarely, in its underlying form (i.e., with initial š).

So far, most of the examples cited for morphographemic or morphophonemic spelling were of this type, namely spellings which do not indicate the expected boundary alternation before a suffix (usually before the mentioned š-initial suffix). To the examples commonly cited for this spelling practice I would like to add a new type. I propose to include in the consideration of boundary alternations not only single words composed of a base and bound affixes, but also word groups forming constructions which behave like single words. The first element of such groups behaves as a bound morpheme, as indicated by the variation between phonemic and morphophonemic spelling. Such a group is a word in the construct state followed by a genitive, a construction comparable to a compound [2].

While most groups of this type are written morphophonemically, that is, the final consonant of the first word in the group remains unchanged in the spelling, there exist spellings which indicate that assimilations that occur at morpheme boundaries in single words in fact took place at word boundary too. Morphophonemic alternations before free words reveal additional assimilation phenomena (i.e., other than dentals and sibilants), such as those involving m, n, and k (see *LAA* 6.1.1, 6.1.2, 6.1.5, for assimilations involving m, n, and k at morpheme boundary, and 6.1.1.1 for an example at word group boundary).

The assimilations which occur in word groups, that is, at word boundary, and those which occur at morpheme boundary, are alike. The word boundary actually behaves in the word groups under discussion as a morpheme boundary, because the first element of the word group is, in fact, a bound morpheme: it never occurs alone, but is followed, both in the case of the construct state and in the case of the preposition, by a suffix or by a genitive. As our collection of examples grows, we begin to realize that the spelling of such word groups without indication of word-boundary assimilation is nothing but morphophonemic spelling. There was, at least in some Akkadian dialects, such close juncture between a word in the construct state and the following genitive that assimilative spellings do turn up, despite the scribes' preference for writing the morphophonemic form. I would like to illustrate this with the following examples:

1. Old Babylonian (also cited previously in *LAA*):

ḫi-ṣí-im-ma-tim = *ḫiṣib* + *mātim* (*LAA* 6.1.1.1)
ḫi-mi-iṣ-ṣe-tim = *ḫimiṭ* + *ṣētim* (*LAA* 6.1.4.2)
ša-pí-in-na-ri-im = *šāpir* + *nārim* (*LAA* 6.2.1.4)

[2] Such a group behaves like a single word also on the morphological level, because its plural is formed by attaching the plural suffix to its end (in this case, to the genitive); for examples see *LAA*, p. 133.

2. NA:

Ma–za–mu–a = *māt* + *Zamua*, geographic name; for references see S. Parpola, *Neo-Assyrian Toponyms*, (Neukirchen, 1970), pp. 381f.

qa–ṣa–bi–ti, *qa–ṣa–bi–it–te* = *qāt* + *ṣabitti*, variant of *qāt ṣibitti*[3], for references see *CAD* Ṣ, p. 156f., *s.v. ṣibittu* mng. 4 and *AHw* 910b s.v. *qātum* 8 and 906a, *s.v. qaṣṣabitti*.

These examples illustrate the assimilation t + Z = Z:, i.e., |t| assimilates to a following sibilant, documented for morpheme boundary in *LAA* 6.1.4.2, see also the example cited above sub (1). In conformity with NA spelling practice, the long (geminate) consonant resulting from the assimilation is written as short or single.

Another word group where it may be expected that assimilation took place is that composed of a preposition and the following noun. It is known that when the short form of the prepositions *ina* and *ana*, namely *in* and *an*, precedes a word beginning with a consonant, the preposition-final n assimilates to the following consonant. For this assimilation in OA see K. Hecker, *Grammatik der Kültepe-Texte* (Rome, 1968), § 36 a; note, also in other periods, the compound prepositions and conjunctions *aṣṣēr*, *iṣṣēr*, *aššumi*, *akkī*, the adverbs *assurri*, *issurri*, *ammīni*, *immati*, and the like. No examples have been found so far for other prepositions, e.g., *kūm* and NA *bīt*, which are written morphophonemically.

In an article published elsewhere, I suggest that no new information is gained by adjusting the transliteration of cuneiform signs to reflect the phonological surface form that is required by the environment (the context). The new values thereby introduced would only reflect assimilation or dissimilation phenomena that are entirely predictable from the phonological environment and that, for the most part, remain unexpressed in the writing system. The paucity of examples for assimilative, phonemic spellings at word boundary attests to this morphophonemic spelling practice. Nevertheless, however small the number of examples cited, the fact that they do occur indicates that word groups also participate in boundary assimilation, and may be considered compounds whose first elements (constructs followed by a noun in the genitive) are comparable to the constructs that are followed by a bound suffix. If the ancient scribe did not consider it necessary to indicate in the spelling the alternants of such words, it need not be necessary for us to do so. We may transcribe the sign or sign groups ⟨*ḫi–ša–ad*⟩, ⟨*ḫi–ša–as*⟩, ⟨*ḫi–ša*⟩ or ⟨*GÚ*⟩ before, e.g., the suffix |*šu*| spelled with its alternant ⟨*su*⟩ phonemically as |*ḫišāssu*| and morphophonemically as |*ḫišādšu*| and, partly morphophonemically, partly phonemically, as |*ḫišādsu*|. The latter type of transcription, more precisely the one where the final consonant of the morpheme preceding the boundary is spelled morphophonemically, and the initial consonant of the morpheme following the boundary is spelled phonemically, is used, e.g., in the citations in *AHw* and in most instances in the cuneiform writing system, which diverges from the morphophonemic principle only in the periods, geographical areas, and text categories which had no strong orthographic tradition.

[3] Recognized by Deller, *Or* 33 (1964) 93. Note also, in NB, the spelling ŠU[II] (= *qāt*) *sa–bit–ti* (*TCL* 13 142:12), which may indicate word-boundary assimilation, the second element of the compound — just as the third person suffix — being written with its surface realization with initial |s|.

Since it has been, up to this point, the exception that proved the rule, i.e., the exceptional phonemic spelling of the morpheme final proved the existence of morphophonemic spelling, I will give a few more examples of the same kind. The examples illustrate explicitly partial or total assimilation at morpheme boundaries (i.e., phonemic spelling). With few exceptions, these examples come from letters or administrative texts, text categories which do not rely heavily on the morphophonemic tradition of orthography.

	morphophonemic spelling	phonemic spelling
n + m	**nu–ut–tu–un–ma*	*nu–ut–tum–ma* *MDP* 24 369:8
	niš–kun–ma	*niš–ku–um–ma* *AMT* 84,4 ii 9
b + m	*ṭa–ab–ma*	*ṭa–am–ma* *BE* 14 42:6
l + š	*šu–me–el–šu*	*šu–mi–šu* (for *šu–mi–iš–šu*) *Or* 32 383:7
m + š	*šu–lam–šu*	*šu–la–an–šú* *ABL* 109 r. 12
	a–ra–am–šu–nu–ti	*a–ra–an–šú–nu–ti* Borger *Esarh.* 3 ii 40
	ṭè–em–šu	*ṭè–en–šu* passim

These examples show that a nasal is always realized as homorganic with a following consonant, whether the nasal is originally part of the morpheme or whether it arose from dissimilation of a long (geminate) voiced consonant, a phenomenon long known and most often documented by the spelling ⟨*a–nam–din*⟩ for |*anandin*|, dissimilated from |*anaddin*|. Since a syllabic value ⟨*nan*⟩ has sometimes been positited for ⟨*nam*⟩ in ⟨*a–nam–din*⟩ and similar forms, I would like to adduce some further examples which show that the spelling of the nasal preceding a consonant is immaterial, and that any nasal appears as n (phonetic [n]) before dental stops and spirants (d, t, ṭ, z, s, ṣ, š) and as n (phonetic [ŋ]) before velar stops and spirants (g, k, q, ḫ).

(*a*) before sibilants: *i–nam–ši* (phonemic |*inanši*|, from |*inašši*|) *TCL* 9 84:14, 147:10

tu–ša–am–šá–šú (phonemic |*tušanšāšu*|, from |*tušaššāšu*|)

ú–nam–zi–im (phonemic |*unanzim*|, from |*unazzim*|) *CT* 40 11:69

(*b*) before velars: *ú–šam–kír* (phonetic [*ušaŋkir*], phonemic |*ušankir*|) Streck *Asb.* 64 vii 102, etc.

nam–ḫu–za–at (phonetic [*naŋḫuzat*], phonemic |*nanḫuzat*|) *LKA* 142:27

These hypercorrect spellings show that the final nasal of the sign ⟨*nam*⟩ is automatically to be read as a homorganic nasal. The establishment of a separate value ⟨*nan*⟩ would be justified only if the correct reading would not be predictable from the phonemic environment, e.g., if the sign ⟨*nam*⟩ would have to be read |*nan*| before a vowel or before a labial.

On Regional Cuneiform Handwritings in Third Millennium Mesopotamia

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Assigning even an approximate date to undated inscriptions is a notoriously difficult problem, and very substantial errors can be made even when dealing with tablets from periods from which there are large numbers of dated texts [1]. Usually one assigns a date to an inscription on the basis of such criteria as size and shape of tablet, physical arrangement of writing, repertory of signs employed, prosopography (in the case of documents), grammar and lexicon, and handwriting. The judgment is usually quite subjective and takes into account whichever criteria seem relevant in the circumstances.

Since dating is so often based on such overall subjective impressions, it is difficult, but important, to formulate criteria which can be used by others in making more objective judgments. Few such specific formulations have been made for any period of cuneiform writing. One may refer to J. Finkelstein, *RA* 63 (1969) 21, n. 2, for some comments in regard to Old Babylonian "documentary hands" for northern Babylonia.

It is probably more difficult to date cuneiform inscriptions of the third millennium B.C. than those of any other period, partly because of the paucity of dated documents and the substantial chronological uncertainties which remain. Nevertheless, with the increase in material in the past few years it seems useful at this point to discuss the problem of determining the date and provenance of texts from the time of the Fara tablets to the Dynasty of Akkad.

The general evolution of individual cuneiform signs over the period of approximately three millennia in which cuneiform was used can be traced, although acquaintance with this development is not sufficient for the needs of cuneiformists who wish to determine the date of a text.

The third millennium was a time of rapid evolution from pictographs to cuneiform signs, and parts of the material are well documented. The characteristics of the script in Levels IV and III at Uruk are represented in Falkenstein's *Archaische Texte aus Uruk* (Leipzig, 1936); the Jemdet Nasr script in Langdon's *Pictographic Inscriptions from Jemdet Nasr*, *OECT* 7, (London, 1928) [2]; the Ur archaic script in E. Burrows' *Ur Archaic Texts* (London, 1935); and the Fara script in Deimel's *Liste der archaischen Keilschriftzeichen* (Leipzig, 1922)[3]. It

[1] J. Finkelstein, "The Hammurapi Law Tablet *BE* XXXI 22", *RA* 63 (1969) 21, n. 1, cites several instances where errors of as much as a thousand years were made.

[2] It must be said that the volume does not meet the needs of today's cuneiformists in some respects.

[3] See *JCS* 20 (1966) 75, n. 20 for a comment on the reliability of the sign forms in the list.

should be stressed, however, that in each of these instances the evidence applies specifically to the site involved and that one cannot assume uncritically that it applies either to the Mesopotamian area as a whole or to any other site.

In general, the problems of distinguishing pre-Sargonic texts from Old Akkadian texts appear to be solved. The most widely known distinguishing mark is the direction of the vertical wedge in the sign ŠU and in signs such as DA and ID [4]. Gelb [5] has been able to refine the time distinction by saying that the change in the direction in which this particular component was written occurred during the reign of Sargon, i.e., the older style was still in use in Sargon's time. It must be stressed, however, that the validity of his observation is limited to the specific group of texts he was concerned with, since the "later" forms are already in use in the time of Lugalzagesi (see *BIN* 8 No. 86 and other Umma texts in that volume, also *CT* 50 Nos. 47 and 48) [6]. The identification of texts of the late Old Akkadian period appears to constitute no problem and need not detain us here.

The groups of earlier texts mentioned above cover more or less the first third of the third millennium, though not in a demonstrably close continuum. In view of the evidence that different styles of writing existed simultaneously, it is clear that no straight line of development can be assumed. Still, confidence in the details of evolution of cuneiform writing would be enhanced if it were possible to have a more or less continuous attestation (preferably from a single site) from the earliest times to the time of the Fara tablets.

In my opinion, the greatest problem in cuneiform palaeography is our lack of knowledge about the particular handwritings of the various scribal centers. In spite of an occasional dissent [7] it has long been customary to assume — albeit tacitly — that there is a uniformity in characteristics of the script throughout Mesopotamia [8].

A proper palaeographic study would include description of several technical points such as the angle of wedges (here I mean not the angle between different wedges in a sign, but rather how the impression was made), the evidence for kind and shape of stylus, perhaps even evidence for how it was held, and, of course, it would also require detailed analysis and description of each text in terms of such characteristics as size of writing, inclination of signs, distance between lines, relation of signs to rulings (e.g., do they "hang" from the ruling above them), use of ligatures, use of optional elements in signs, etc. Another feature that might prove useful in delimiting regional and temporal practices is the sequence in which the various wedges in a sign were written (only possible to see, of course, by using the original tablet or a cast) [9].

[4] See Pohl, *TuM* 5, p. 7.

[5] *OAIC*, p. 170, n. 4 and *MAD* 5, p. XXI.

[6] M. Civil has pointed out to me that the archaic ŠU sign survived in Ur III times when combined with another sign as in NAGA × ŠU. See Civil, *RA* 54 (1960) 69, n. 2 and *RA* 55 (1961) 93.

[7] E.g., Ferris Stephens in his introduction to *BIN* 8, p. 8.

[8] E.g., D. O. Edzard, "Enmebaragesi von Kiš", *ZA* 53 (1959) 9-26; A. Goetze, "Early Kings of Kish", *JCS* 15 (1961) 105-111; W. Nagel, "Frühdynastische Epochen in Mesopotamien", *Vorderasiatische Archäologie* (Festschrift Moortgat) (Berlin, 1964), pp. 178-209 and Table 1.

[9] The sequence can be observed easily in such signs as LA by noting which wedges have their "heads" obliterated by a wedge made later. In the case of LA, it appears (in Abū Ṣalābīkh at least) that the top row of verticals was made

In my work on tablets from Tell Abū Ṣalābīkh and from Fara over the past few years I have been struck by the variety of handwritings and the range of sign forms found in texts from Tell Abū Ṣalābīkh which are in all probability contemporary, and at the same time the differences in handwriting between Abū Ṣalābīkh and Fara, a distance of only a few miles [10].

It appears to me that the distinction made in Greek palaeography between "book hand" and "documentary hand" may be valid for third millennium Mesopotamia (its validity for later periods — say Old Babylonian and Neo-Babylonian — is less debatable), though for our purposes it is preferable to call it a "literary hand". [11] In general, it appears that the Fara "literary hand" is a more careful handwriting than the "documentary hand" from the same site [12]. The "documentary hand" is not entirely uniform. It appears that contracts [13] are usually written with greater care than more ephemeral documents. It is difficult on the basis of published photographs to be sure of the variations in the Fara "documentary hand" since it is possible that the tablets illustrated in Fara 3 were selected not because they were typical, but because of the comparative elegance of the script. Here a careful description would also require comment on the apparent speed of the writing. It seems to me that the distinction between the "literary hand" and "documentary hand" is less marked in Tell Abū Ṣalābīkh, though the examples of "documentary hands" are still too few to provide adequate material for comparison.

Even though there is a more or less standard "literary hand" in Abū Ṣalābīkh, there are a number of texts written in handwritings quite distinct from it [14]. Nevertheless, there is also a considerable homogeneity, and it seems better to remark here on comparisons with the Fara script than on local variations within the Abū Ṣalābīkh texts.

A general distinction between Fara and Abū Ṣalābīkh handwritings is that signs in Fara often have the vertical wedges slanting to the left and that signs slant upwards far more often than in Abū Ṣalābīkh. There are a number of clear distinctions between sign forms from the two sites as well. A striking example is that in Fara GI and ZI are usually distinguished only by a *gunû* in ZI. The few occurrences in the Nippur "Fara-period" texts agree with the Abū Ṣalābīkh distinction. In Abū Ṣalābīkh GI and ZI are distinguished by the orientation of the wedges around the "stem" as well as by the *gunû*.

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first. Their "heads" are normally obliterated by the horizontal stroke at the top. The small horizontals in the right part, however, are normally not obliterated by the left vertical, and therefore were probably made after the vertical. A similar sequence is employed in the sign KÉŠ. In fact, it seems to be a general characteristic of signs with a rectangular shape on the left that the verticals are made first. Whether or not this specific observation is valid for other sites and other periods in the third millennium cannot be investigated here.

[10] I assume that the tablets from Fara and Abū Ṣalābīkh are approximately contemporary, though their precise contemporaneity is, of course, unprovable.

[11] In this instance "literary" should be understood to include all traditional texts, including word lists and sign lists.

[12] It must be admitted that it is not certainly known that the two groups of texts are contemporary.

[13] For the most recent publications, see M. Lambert, "Quatre nouveaux contrats de l'époque de Shuruppak", in *In Memoriam Eckhard Unger* (Baden-Baden, 1971), pp. 27-49.

[14] Nearly all the divergent scripts are illustrated by photographs in *OIP* 99.

Among many signs which might be singled out for comment, I may mention the sign LUL. In Abū Ṣalābīkh it has a full "head" inside of which are crossed wedges in the form a of a "lazy X" (the term is borrowed from descriptions of cattle brands), whereas in Fara the usual LUL sign is LAK 242 rather than 243. The ways of writing rare signs are even more divergent, though in most instances consistent within each of the two bodies of texts.

A particularly striking feature of the Abū Ṣalābīkh script is the occasional use of a rounded impression for the final wedge in such signs as TA and ŠA which normally have a vertical wedge where the oblique strokes join. The same feature occurs in Fara (at least in the "literary hand"). It is perhaps "reasonable" to assume that the rounded impression is more archaic than the vertical wedge (it is not attested in the Ur archaic texts, however); the fact is that it is used by skilled scribes from the same time, and even within the same tablet (e.g., *OIP* 99 no. 136 column iii).

If such variations occur in contemporary texts from known sources, we should be reluctant to ascribe chronological significance to individual archaic looking features when they occur in a text of unknown provenance, particularly since a "literary hand" is likely to be more conservative than a "documentary hand".

Obviously, not all signs are of equal worth for palaeographic purposes. For example, the number of wedges in some signs in certain positions is entirely arbitrary (e.g., the number of horizontals in ŠÈ or KI), even though a specific minimum may be required (in the case of ŠÈ and KI in order to distinguish them from NÁM or KU and DI respectively).

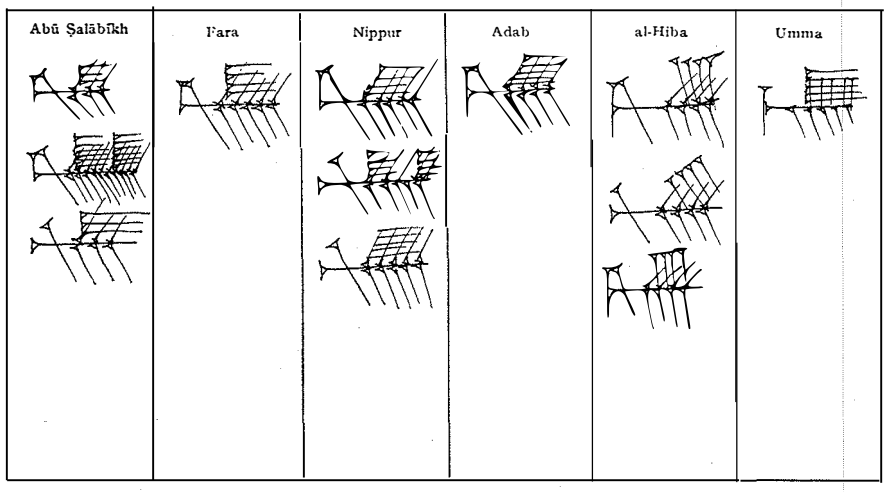
For many signs the potential for variation is limited (e.g., MAŠ, BAR, DÙ, NI, ME) or else the shape of the sign does not permit much variation in angles or orientation (e.g., LA, DAB).

As a sample, I have chosen several signs which seem to have potential for distinguishing provenance at least (if not date), quite apart from any general impression one may have from an original tablet or a copy. At the same time, relatively common signs were chosen so that occurrences could be found easily. I am aware that choosing a limited number of signs resembles the "test-letter" theory in Greek palaeography now generally abandoned[15], but it seems to me that it may be useful at this stage in cuneiform palaeography. I have chosen for the most part texts to which I have had direct access: texts from Tell Abū Ṣalābīkh (cited here by publication number in *OIP* 99 (in press)), Fara (citations from texts from Fara 2 of which I have photographs), Nippur (Enlil-Ninhursag cylinder[16] and other pre-Sargonic tablets available to me in the original or casts), al-Hiba[17], and Umma (citations are from published sources of assumed Umma provenance).

[15] See E. G. Turner, *Greek Manuscripts of the Ancient World* (Princeton, 1971), p. 23.

[16] With the permission of Dr. Åke Sjöberg, curator of the tablet collection at the University Museum, Philadelphia, I have recently made a new copy of this text, first published by G. Barton, *MBI* 1 no. 1. I have cited it according to correct column number with column numbers of the published edition in parentheses.

[17] As far as I can judge, the script of al-Hiba is indistinguishable from that of Tello for the same period and consequently what is valid for al-Hiba appears to be valid for the entire Lagash area.



ZI (Fig. 1)

Abū Ṣalābīkh: (a) *OIP* 99 passim, (b) *OIP* 99 298 ii, (c) *OIP* 99 124 v: *Fara* 2 No. 12; Nippur: *MBI* 1 ii ("xi"), 4 N-T 55 ("documentary hand") (c) 7 N-T 6 ("documentary hand"); Adab: *OIP* 14 55; al-Hiba: (a) 2 H-T 21, (b) 2 H-T 12, (c) 2 H-T 25; Umma: CT 50 48 i and vi.

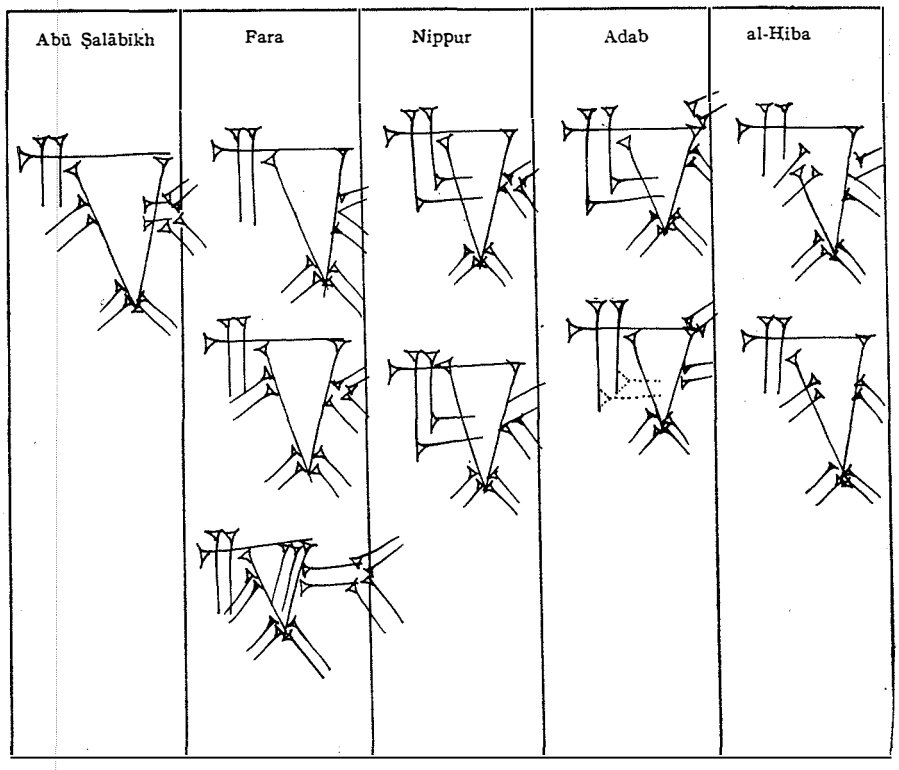
The most noteworthy feature of Fig. 1 is the form of ZI in the Lagash area. By and large, the other examples cited are quite similar. It can readily be seen that the vertical at the beginning of several examples is not obligatory (note that two al-Hiba examples, one from an inscription of Enannatum I and the other from his son LUM-ma-TUR, differ in this respect. In Abū Ṣalābīkh occurrence of the vertical is much more common than its omission. A double *gunû* in a Nippur example is paralleled in a number of occurrences in Abū Ṣalābīkh [18].

NAM is a sign with a great deal of potential for variations. It can be seen from Fig. 2 that the form of the sign in Nippur and Adab is very similar and that it has a feature not shared by texts from the other sites covered (this form of NAM is used at both sites in both the "literary hand" and the "documentary hand"). At the same time it bears a resemblance to the NAM on the pre-Sargonic text illustrated by S. N. Kramer on the dust jacket of *From the Tablets of Sumer* (Indian Hills, Colorado, 1956) and as Fig. 6a, with the exception that it resembles the Abū Ṣalābīkh form in having two horizontal wedges on the right from which the oblique strokes come [19].

UD is another sign where there is an important distinction between Fara and Abū Ṣalābīkh. In Fara texts (at least in the "literary hand") UD is formed

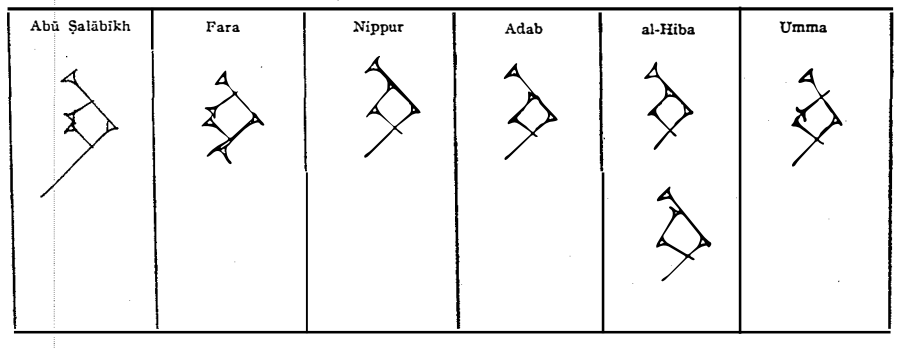
[18] This is truly an archaic feature (See *JCS* 20 [1966] 78 n. 38) but is not evidence that these individual tablets are earlier than others.

[19] Other features, such as UD, the long horizontal in DINGIR, the form of MU (in which the final cross strokes nearly join the tails of the first set of cross strokes) suggest that the tablet is from Adab or Nippur.



NAM (Fig. 2)

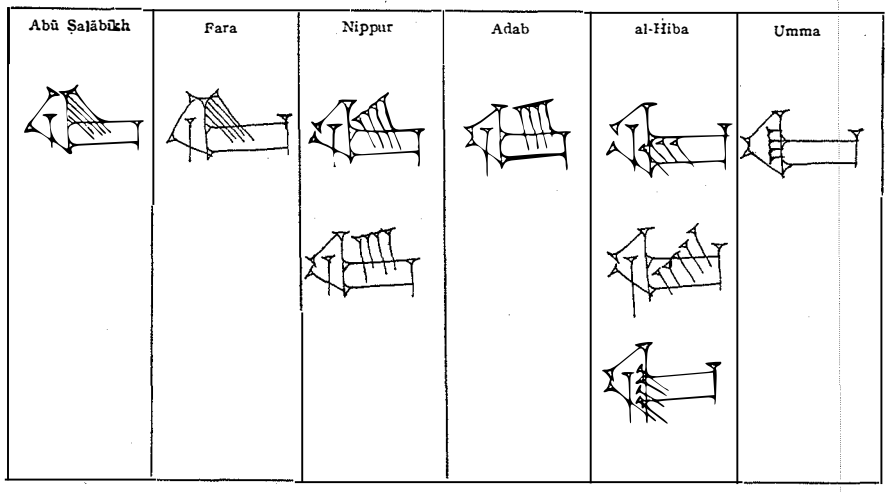
Abū Ṣalābīkh: *OIP* 99 passim; Fara: (a) *Fara* 2 40 vi, (b) *Fara* 2 58 rev.; TSŠ 46; Nippur: (a) *MBI* 1 i ("x"), (b) *MBI* 1 xvii ("vi"); Adab: (a) *OIP* 14 53, (b) *OIP* 14 56; al-Hiba: (a) 2 H-T 12, (b) 2 H-T 22.



UD (Fig. 3)

Abū Ṣalābīkh: *OIP* 99 passim; Fara: passim; Nippur: *MBI* 1 vi ("xv"); Adab: *OIP* 14 51 iii ("documentary hand"); al-Hiba: (a) 2 H-T 21, (b) 2 H-T 12; Umma: *BIN* 8 86 iii ("documentary hand").

with five wedges, one of which is superimposed on another, but with the "head" at the bottom. Nippur, Adab, and the Lagash area share the form of UD in which the top one of the smaller strokes has the "head" uppermost. In Umma, on the other hand, the "heads" are together.



KA (Fig. 4)

Abū Ṣalābīkh: *OIP* 99 passim; Fara: *Fara* 2 20 rev.; Nippur: (a) *MBI* 1 i ("x"), (b) *MBI* 1 xii ("i"); Adab: *OIP* 14 56; al-Hiba: (a) 2 H-T 21, (b) 2 H-T 18, (c) 2 H-T 6; Umma: *BIN* 8 86 v ("documentary hand").

The differences between KA in the various sites is less striking than the other signs listed here. Normally in both Abū Ṣalābīkh and Fara the "beard" is formed with a heavy stroke at the top with a well defined "head" and then a number of fine strokes, usually virtually without "heads". The top wedge in the "head" (i.e., the part of the sign KA which represents a head) of the later examples has its "head" at a different point. In Nippur and Adab the "beard" is often formed of nearly vertical wedges. In the Lagash area the "beard" often begins inside the "head" part of the sign. These characteristics naturally apply to all compound signs formed on the basis of KA. Again, as with other signs, there is a distinction between KA in Umma and in the Lagash area. Note occurrences of the Umma KA in the text published by E. Sollberger, "La frontière de Šara", *Or* 28 (1959) 336-50.

Examination of figures 1-4 will show at once that there are marked differences in writing and that this is not merely a matter of date (as between Abū Ṣalābīkh-Fara and the later sources), but equally a matter of geographical location. Moreover, it is clear that the various peculiarities pointed out here do not necessarily fall into groups and that sites which share some particular feature do not necessarily share some other feature. Consequently it would be erroneous to suggest that one of the styles derives from any other.

It seems particularly clear that there is a distinctive handwriting for the Lagash area (Tello and al-Hiba) at least from the time of Enannatum to Urukagina, for which the most striking features are the forms of the sign ZI (see

Fig. 1) and SAR [20]. Neither form is known to me outside the Lagash area. Likewise, Nippur and Adab share a number of features. It should be noted in particular that Umma and the Lagash area have distinctive handwriting styles.

The result of this brief examination of signs from several sites suggests that a great deal of caution is needed in proposing relative dates for texts from the pre-Sargonic period, even more so when the provenance is not known, and when a "literary hand" (which may have a long life) is involved. Even in the case of the Enlil-Ninhursag cylinder from Nippur, I am reluctant to suggest a relative date more specific than "late pre-Sargonic". It is apparent that at no point within the time range covered by this paper was there a single style of handwriting.

I am even less optimistic about the possibility of making significant chronological distinctions based on "development" of signs when the material is stone. I here call into question explicitly such efforts as that of Nagel (see n. 8) and recommend that such palaeographic arguments be disregarded. A number of variables, such as size and shape of object, hardness of stone, type of engraving tools used, and obviously the level of training and skill of the stonecutter, are significant for the end result. It may be useful in this context to point to the elegant, careful inscriptions on stone from Enannatum I (see *Artibus Asiae* 32 [1970] Fig. 13 [following p. 250]) and the awkward, inelegant script on the stone vases of Lugalzagesi (see *BE* 1 no. 87). It would probably be "reasonable" to date the Lugalzagesi vase earlier on the basis of the script. In this instance our historical knowledge prevents such an error. It is my belief that pre-Sargonic votive inscriptions scratched on stone (as opposed to a few carefully engraved examples) were more than likely made by an inexpert, probably illiterate, stonecutter and that their worth for dating purposes is virtually nil.

Perhaps if more texts with known provenance were available from the century or so after the Fara tablets, a development for individual scribal centers and the particular styles of writing could be demonstrated, but at the moment it does not appear that one can isolate palaeographically significant features of cuneiform writing which are valid for all of Mesopotamia. Nevertheless, I have tried to show that there are specific regional characteristic handwritings which can be defined for several locations.

[20] See Y. Rosengarten, *Répertoire commenté des signes présargoniques sumériens de Lagaš* (Paris, 1967), No. 446.

Grammatical Models in Egyptology

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Since our ability to interpret written evidence is the mainstay of our study of antiquity, the way we go about interpreting is invariably of concern to scholars. Although the scholar brings all the knowledge he possesses of whatever sort to the analysis of a given text, applying it according to his sense of the probable and the improbable in human affairs, still his interpretation must conform to, or at least confront, those rules of grammar generally accepted as valid for the language with which he is working. These rules of grammar, that constrain and guide his interpretation, embody, in turn, the strengths and weaknesses of the presuppositions upon which they are based. Therefore, I would like to dedicate this essay concerning grammatical presuppositions to Dr. Ignace J. Gelb, who throughout his career has shown a lively interest in the problems of grammatical models and the presuppositions that underlie them.

In this essay I would like to analyze the directions taken by grammatical work in Egyptology, their causology, and implications. During the approximately one hundred and fifty years of Egyptology, four main grammatical models have shaped the study of the grammars of the various stages of Egyptian and Coptic. None of them has totally displaced the others, but all continue to be represented to some degree, since the theoretical positions of Egyptologists show the normal variety that one should expect in any discipline. None of these models originated within Egyptology; all are importations from the general grammar of the period in which they were borrowed. Within Egyptology there has been little observable inclination to scrutinize these models; more than most disciplines Egyptology has been empirical rather than theoretical [1]. Symptomatic of this is the rarity of theoretical discussions in the literature. Therefore, a critique of the four main grammatical models is not out of place at this point in the history of Egyptology, particularly since generative grammar is now beginning to make an impact on grammatical work in the discipline. The four grammatical models will be examined in the order of their historical appearance, i. e., traditional grammar, Neo-grammarian analysis, structuralism, and generative grammar [2].

[1] Egyptology seems generally to be understood as an area study, an application of as many disciplines as possible to the study of every aspect of the culture of ancient Egypt. As such it has no research model uniquely its own, and so in pursuing the obligation to continually test general theory with empirical evidence from ancient Egypt, the Egyptologist must make special efforts to follow developments in outside disciplines, since it is precisely there that his general theory is to be found.

[2] The scope of this essay does not allow more than an extremely cursory glance at the history of linguistics, with all the simplifications and distortions

I. Traditional Grammar

Of all the models under consideration, traditional grammar has played the most decisive role by far. At the beginning of Egyptological work it was the only known model, and it has exercised influence in proportion to its age and familiarity. Its influence is partially due to its temporal priority, partially to its being practically universally known, and partially to the fact that no acceptable alternative to it existed for the first century of the discipline's life; even Neo-grammarian analysis, beginning in the 1880's actually represents more a variant of traditional grammar than an autonomous model in its own right.

As it is well known, traditional grammar originated in Athens in the fifth century B.C. as a response to the practical need of training would-be orators in the art of disputation, an art of great importance in a city in which a political leader's success depended more on the persuading of large numbers of people than it did in more traditional societies. It is, therefore, no accident that the study of rhetoric preceded and provided the impetus for the study of grammar. The first to address themselves to the problem of what constituted an effective speech were the Sophists, to whom we owe most of the rhetorical terminology still used today [3]. The role that sound plays in rhetoric prompted Gorgias to investigate the nature of sounds and syllables. The need for balancing variety and repetition in the use of words led to early discussions on the nature of synonymity, in which the name of Prodicus figures large. The work of Plato and Aristotle essentially represented a continuation of the concern for effective and telling descriptions.

Aristotle defined his logico-grammatical categories as entities consisting of sounds to which meanings were assigned in a particular (social) context. These sounds were specific for individual cultures but represented the manifestation of ideas, or as Aristotle puts it, impressions on the soul [4], which were the same for all men [5]. Therefore, from the beginning, grammar was conceived

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that inevitably implies. The few general ideas that I have chosen to discuss have been determined, by and large, by whether they can be observed within Egyptological writing or not, as I have limited my research to published works and have not attempted to pursue biographical evidence, although this dimension is important, and should eventually be worked on. For the sake of smoothness of exposition I have presented all quotations in English, with the originals to be found in the footnotes.

[3] For a more detailed discussion see F. J. Dineen, *An Introduction to General Linguistics* (New York 1967), ch. 4.

[4] A rather good discussion of the social *realia* is to be found in H. P. Cooke's introduction to Aristotle's "The Categories on Interpretation" (Loeb Classical Library no. 325).

[5] Aristotle (On Interpretation) considered that sounds acquired meaning κατὰ συνθήκην. By this it is now understood that he meant "by (linguistic) context". Boethius, however, translated this term into Latin as *ad placitum* "by convention" and it was this understanding of the term that influenced the development of medieval grammar. Supporting this interpretation was the well known nomos-physis controversy concerning the way sounds acquired meaning, whereby it was assumed Aristotle was aligning himself with the contention that sounds acquire meaning by convention (nomos) rather than by their phonetic nature (physis). Cf. J. Pinborg, *Die Entwicklung der Sprachtheorie im Mittelalter* (Beiträge zur Geschichte der Philosophie und Theologie des Mittelalters, 42 v. 2, [Munich 1967]), 36, note 60.

of as specific in form and universal in substance. The specific form that universal ideas take in particular languages was a result of historical processes by which sounds suitable by their nature to represent specific ideas formed roots (etyma) which in time suffered modification in various ways, the study of which (etymologia) was a legitimate concern of grammarians.

Since Aristotle had formulated the notion of language as a specific manifestation of universal ideas, he and his successors were naturally more interested in examining sentences that expressed universal propositions than in those that expressed judgments about specific subjects in specific times and places. He, therefore, posited the declarative sentence as the basic sentence type, since this is the usual form in which universal propositions are couched, and considered other sentence types, such as questions and commands, as deviations from the basic type. The precedence of the declarative sentence over all other sentence types has been a basic feature not only of traditional grammar but also of more recent grammatical models.

Aristotle distinguished the categories of nouns and verbs by their ability to express time. His successors were quick to point out the various features that characterized the form of the various parts of speech and by around 100 B.C. Dionysius Thrax could define a noun as follows[6]:

> The noun is a part of a sentence having case inflections, signifying a person or a thing, and it is general or particular, for example, "stone", "education", "man", "horse", "Socrates". It has five simultaneous features (grammatical accidents): gender, type, form, number, and case.

The various categories so defined were also suitable for the description of Latin because of its structural similarity to Greek. As a result these categories were applied to Latin by various grammarians of whom the most important for the subsequent development of grammar was Priscian.

The Middle Ages continued the development of classical grammar within the context of a changed social situation with needs different from those of classical antiquity. Specific factors combined to produce a major shift in emphasis in grammatical research toward a "semanticization" of classical grammar. Whereas in the definitions of Dionysius Thrax and Priscian, semantico-logical and formal criteria had assumed more or less equal importance, by the fourteenth century formal criteria retained only a vestigial importance, completely overshadowed by their semantico-logical counterparts.

Pinborg has pointed out two main developments operated to produce this semanticization of grammar[7]. On the one hand, medieval grammarians found Priscian clearly inadequate to explain all the uses of Biblical and medieval Latin. As a result scholars renewed basic grammatical research and published their findings in the form of commentaries on Priscian.

On the other hand, the rediscovery of certain of Boethius's lost writings on logic led to a renewed interest in logic during the "Boethius renascence" of the ninth century. Further the rediscovery of some of Aristotle's lost writings on logic, notably the *Analytica Posteriora*, available to scholars by the twelfth century, strengthened interest in logic even more.

[6] Cf. Dineen, *An Introduction*, 99.
[7] A long and detailed discussion of the *modistae* is to be found in Pinborg, *Entwicklung*, 21 ff.

Intellectual conditions of the period strongly favored the rapid growth of the influence of logic on all disciplines. The closed system of interrelated studies, the *septem artes*, over which grammar reigned supreme, became more and more discredited with the explosion of knowledge in new disciplines not included in it, namely theology, medicine, and law. Logic had much to contribute to the theoretical foundations of these new disciplines, whereas grammar, more narrowly concerned with language arts, had little to offer. The applicability of logic to the development of the theoretical components of these new disciplines strengthened the traditional view that logic provided the basis for all inquiry with the corollary that every branch of learning must ultimately be demonstrated to be not autonomous but simply the working out of logical processes in some domain of experience. In short, demonstrating the way that logical processes were manifested in particular disciplines became a *sine qua non* for establishing their validity.

What this semanticization meant in practice may be illustrated by how the dominant grammatical school of the Middle Ages, the *modistae*, handled the problem of the same root occuring in various forms of speech. The root *love* in the following expressions would have been interpreted as *an action in the mode of a substantive*, adjective, verb, or adverb as the case might be:

(1) the love of money ...
(2) God loves sinners.
(3) ... loving kindness of God ...
(4) he lovingly described the situation ...

The crucial concept of *mode* which led to such an analysis was the result of a development that extended the meaning of the concept of *consignificare* of which the verbal noun is *modus significandi*. The Latin verb *consignificare* translates two grammatical terms of Aristotle, *prossēmainein* "(for a word) to have meaning with reference to (an object)" and *syssēmainein* "(for a word) to have meaning within the context of (a word or phrase)". In the early Latin grammarians the nuance of *consignificare* that derived from *prossēmainein* was widened to refer to situations where a word was considered to have meaning with reference not only to some external object but also to another word. In this way *consignificare* acquired the third meaning of "to mean the same as". Thus in one indissoluble bond the primarily logical term *prossēmainein* was linked with the primarily grammatical term *syssēmainein*. The way was thereby opened for the development of purely semantic (i. e., logico-semantic) explanations of grammatical phenomena; the thirteenth-century Peter Kilwardby capsulized this relation as follows [8]:

> The parts of speech are not differentiated according to how things differ, but according to how the modes of signifying differ. All things may be signified, however, in the same mode, e. g., by the mode of custom or similarly the things referred to by all predicates may be signified by a noun, such as quantity, and quality, and in such a way in regard to other things as well. And for this reason there are not ten parts of speech, as there are ten predicates of things.

[8] "Non distinguuntur partes orationis secundum distinctionem rerum sed secundum distinctionem modorum significandi. Possunt autem omnes res eodem modo significari, scilicet per modum habitus; ideo res omnium predicamentorum possunt per nomen significari, ut quantitas, et qualitas et sic de aliis. Et hac de ratione, non sunt decem partes orationis, sicut sunt x predicamenta rerum". (Pinborg, *Entwicklung*, 48).

The semanticization of the definitions of grammatical categories is perhaps the major legacy of the *modistae* [9]; their elaborate attempts to relate the various parts of speech that might be derived from a single root did not survive the fall of scholasticism. It is, therefore, the *modistae* who are ultimately responsible for the typical definitions of categories that one finds in Orientalist grammars. There is no trace of the formal criteria of classical antiquity and the terms are purely semantic:

> Verbal sentences are those in which the predicate is a verb form having the sense of a simple finite verb in English or Latin (' loves', 'loved', 'amat', 'amavit',) [10].
>
> ... we may define the grammatical predicate as the element in a sentence (or even in a subordinate clause, § 182) which either by position or form would normally express the meaning of the logical predicate [11].
>
> The use of a simple preposition as adverb is still present in the word for "there", "therin", that also still survives in Coptic as ⲙⲙⲁⲩ [12].
>
> Of the compound prepositions one uses as an adverb ḫr–ḥ*r*t "earlier" ... [13].

One consequence of the role played by logic and semantics in traditional grammar was that the categories of grammar defined in reference to extralinguistic objects were considered to embody a hierarchy of relationships parallel to those of human society. The order of presentation in traditional grammar normally commences with the noun, continues with a discussion of the verb and then proceeds to the remaining parts of speech ending with interjections and conjunctions. The medieval grammarian Peter Helias gives us what is apparently the reason for this order of presentation, which with minor deviations is still followed in most grammars of Egyptian [14]:

> The noun is the first and most noble part of speech, but the pronoun takes its case from the noun and therefore should have its case in the most noble part. But the most noble part of a word, since the meaning is most to be discerned from the ending, is the last part. For the mind generally understands nothing when we hear language until we get to the end of the word ...

[9] Pinborg, *Entwicklung*, p. 90 lists the stages of semantification as follows:
(1) Grammar is conceived of as a universal science, which preserves its independence vis-à-vis logic.
(2) The concept "mode of signifying" is defined more precisely and is applied to the entire grammar as a key concept. On the one hand, the definition renders the concept univalent, and on the other hand, it grounds it in the basic science of the period, in ontology or metaphysics.
(3) The definitions of the word classes become "modalized".
(4) Syntax becomes formalized, i. e., the syntactic description comes to be constructed on the basis of the concept of the "mode of signifying" and not upon that of the "significatum".

[10] A. Gardiner, *Egyptian Grammar* (London 1927) § 27.

[11] Gardiner, *Grammar*, § 128.

[12] A. Erman, *Neuägyptische Grammatik* (Leipzig 1880), § 592: "Die Verwendung einer einfachen Praeposition als Adverb liegt noch in dem Wort für "dort", "darin" vor, das auch kopt. noch als ⲙⲙⲁⲩ weiterlebt".

[13] Erman, *Neuäg. Grammatik*, § 593 "Von zusammengesetzten Praepositionen braucht man als Adverbien *ḫr–ḥ*r*t* 'früher' ...".

[14] This is quoted in Dineen, *An Introduction*, 131.

An indirect consequence of the understanding of language as a manifestation of logical relationships was the tendency of traditional grammar in the period following the Renaissance to be prescriptive. Since language, in particular Latin, the language of learning and international affairs, mirrored the natural and immutable order of creation, it was important that it be kept free from impurity and corruption. The vulgar tongues, it followed, were to be refined and rationalized to correspond as closely as possible to the universal language of the mind. This understanding of language as permanent and immutable could be maintained only as long as Latin, the language upon which this view was based, continued to dominate Europe, for if consideration of language was limited to Latin, this view was, in and of itself, reasonable.

As is well known, general developments in the Renaissance forced a reevaluation of the role of Latin. In Italy a combination of factors produced the beginnings of an intellectual revolution that would find its inspiration in the learning of Classical antiquity. Racked by constant German, French, and Spanish intervention, a certain spirit of local patriotism favored a resuscitation of the Roman past, and this at least as early as the Roman commune of 1140, in which certain republican institutions were temporarily revived. The constant contact with the Middle East following the First Crusade acquainted the trading cities of Italy with new ideas and customs, particularly with the Greeks and Arabs, each of whom possessed a classical language held in no less high esteem than Latin in the West. The stage was set for the appearance in 1305 of Dante's Latin treatise *De Vulgari Eloquentia*[15] that made the radical claim that the common language was superior to Latin, citing the new humanistic reasons of (1) the use of spoken language being an attribute only of humans; (2) its being natural rather than artificial as was Latin; and (3) its universal use for all human affairs. This claim was then backed up and given concrete form in *The Divine Comedy*, written in Tuscan, which had a revolutionary effect on raising public esteem for the vernacular languages of Europe in general. To be sure this was not felt immediately.

Various factors eventually contributed to a decline of Latin and a concomitant view of language that took other languages into consideration. The spread of literacy created vernacular markets of a sufficient size to make writing in particular vernacular languages worthwhile. The more literature that accumulated in a vernacular language, the more pride its speakers took in it. The invention of printing greatly accelerated the process of cultivating the vernacular reading public. The increasing prominence of vernacular languages was coupled with an increased interest in Greek and Hebrew, both of which rivaled Latin in prestige, because of their sacred character. As a result of the discovery of the New World and the circumnavigation of Africa, new languages began to claim attention, languages with structures radically different from those known heretofore. The categories of traditional grammar often fit these languages only in a very imperfect way.

Resolving the contradiction between traditional grammatical theory and this new linguistic information was hindered by the imposition of Latin grammar on the rising new vernaculars. This imposition, motivated by a desire to legitimize the new vernaculars by codifying and regularizing them, gave a

[15] Cf. H. Arens, *Sprachwissenschaft: Der Gang ihrer Entwicklung von der Antike bis zur Gegenwart* (Munich 1955), 40-45 and the literature cited on p. 526.

powerful normative thrust to the study of grammar; because of the extension of grammatical instruction to ever widening circles of pupils, too much was at stake for any radical reformulation of grammatical models.

II. Neo-Grammarian [16] Analysis

In the course of the nineteenth century traditional grammar came to be applied to the analysis of a great number of languages with structures vastly different from those of Latin and Greek, at the same time that research into Sanskrit with its admirable native grammatical literature was driving scholars to the conclusion that not only were Latin and Greek rather late examples of a larger Indo-European family of languages, but that the grammatical tradition based on them exhibited severe inadequacies when applied to other more exotic languages. Thus the claim of traditional grammar in its late form that logic must be normative for the usages of language began to ring increasingly hollow. In Germany younger grammarians, beginning about 1870, started to formulate a substantial revision of traditional grammar. When their work was eventually accepted, it could be said that this new Neo-grammarian analysis had once and for all shattered the claims of traditional grammar to be normative.

This revision must be understood in the light of the intellectual history of Germany in the last quarter of the nineteenth century. Two main currents of intellectual thought may be singled out for their influence on the new grammatical revolution: Darwinism and Pre-Freudian psychology. The intellectual significance of Darwin's explanation of the gradual evolution of plants and animals forced scholars to a re-examination along genetic lines human institutions as well. A great deal of evidence seemed to lay at hand that human language developed in ways very much like those of a living organism, developing new grammatical and phonetic features and shedding outworn forms, sprouting offshoots and dying like some great social plant.

If the development of language was not as easily explicable as the development of the species it was because it was a human activity as well, reflecting the foibles and eccentricities of the human mind. Fortunately laboratory psychology was beginning to show that the paths of the mind could be charted, if not with total success, then with a degree of rough approximation. Language, then, was a manifestation of psychic processes that was evolving in an organic way, subject to analogic change. Language was no longer the exclusive concern of logic; it was now the province of history and psychology.

Thus, from 1870 on, the Neo-Grammarians began to mount a strong attack on the prescriptive claims of late traditional grammar, which they rightly viewed as an impediment to research:

> There has been no error producing more unhappiness in things linguistic than the former view: *Logic*, the operation of rigorous logic,

[16] Historically speaking this term is misleading. The German original *Junggrammatiker* was first coined to refer to the *younger* generation of grammarians who were the main advocates of the new methods. However, the element *Jung-* was early interpreted in its other possible sense of "recent" and taken to refer to the grammatical model itself rather than its advocates. The English translation preserves the sense of this secondary interpretation.

> must be the norm for judging whether this or that is appropriate or correct in the speech of a writer [17].

Language was to be properly understood as a dynamic process, subject to Hegelian laws of development:

> We wish here, in order to speak as Hegel did, to take the substance back to the process, and, with the only justified basis of a genetic view, penetrate all individual phenomena through all the stages of their development. Syntactic objects are often a product of dark forces or factors and grow out of their interactions [18].

These "dark", one is tempted to say "Wagnerian", "forces" resided in the human psyche in constant uneasy movement:

> By a psychological viewing of a syntactic phenomenon we understand an analysis of it with regard to its origin in the speaking human being. Undertaking this analysis should lead to the recognition of what the psychic movements were through which that linguistic form was brought into being or what psychic movements incorporated themselves in it [19].

An understanding of the operations of psychology could be expected to explain, among other phenomena, various examples of changes in language that could be interpreted as the working of analogy, based on mental associations of sounds and meanings. Phonetic changes and elisions were to be understood as manifestations of the mind's need to maintain semantic or logical distinctions or alternatively the desire to reduce redundant signals [20].

As far as the effect of Neo-Grammarian analysis on the grammar of Egyptian is concerned, the appearance of the definitive theoretical work, Herman Paul's *Prinzipien der Sprachgeschichte* preceded the equally revolutionary

[17] H. Ziemer, *Junggrammatische Streifzüge im Gebiete der Syntax* (Colburg 1883), 41: "Keinen mehr Unheil stiftenden Irrtum hat es in sprachlichen Dingen gegeben als die frühere Auffassung; die Logik, das Einhalten strenger Logik müsse die Norm für die Beurteilung sein, ob dies oder jenes in der Sprache eines Schriftstellers angemessen oder richtig sei".

[18] Ziemer, *Streifzüge*, 34: "Wir wollen hier, um mit Hegel zu reden, die Substanz auf den Process zurückführen und mit dem allein berechtigten Grundsatz einer genetischen Betrachtungsweise einzelne syntaktische Erscheinungen durch alle Stadien ihrer Entwickelung durchdringen. Syntaktische Objekte sind oft ein Produkt dunkler Kräfte oder Faktoren und aus deren Wechselwirkung entsprossen".

[19] Ziemer, *Streifzüge*, 31: "Unter psychologischer Betrachtung einer syntaktischen Erscheinung verstehen wir eine Analyse derselben mit Rücksicht auf ihrer Entstehung im sprechenden Menschen. Die Vornahme dieser Analyse soll zu der Erkenntnis führen, welches die psychischen Bewegungen waren, durch die jene Sprachform ins Leben gerufen wurde oder welche psychischen Bewegungen sich in ihr verleiblichten".

[20] An example of the need to establish distinctions may be illustrated by the phenomenon of "semantic lengthening". In English, for example, in long sentences conveying a heavy information load, the longer expression *in order to* is more likely to be used than the shorter and more easily overlooked, but synonymous, *to*. In a similar way, in literary Arabic, in Egypt at least, the longer باعتباره *bi-i'tibārihi* (*malikan*) "as (king) / in the capacity of (king)" lit. in considering him (king), has replaced the shorter and more classical *ka-* "as, like". The longer expressions are easier to perceive and less likely to be overlooked or misheard. Another factor influencing the Arabic phenomenon, is, to be sure, the complete absence of *ka-* in the spoken language.

grammar of Adolf Erman, *Ägyptische Grammatik*. The acceptance of the Neo-Grammarian claim that neither logic nor Latin were the arbiters of possible grammars provided the precondition for the formal analysis of the alien structures of Egyptian without the stigma of corruption and primitivity that prescriptive traditional grammar would have had to impose. It is Neo-Grammarian analysis that must be thanked for the relative tolerance that grammars of Egyptian following Erman show for idiomatic constructions, as well as the careful distinction between spoken and written language that is characteristic of the Egyptological grammatical tradition.

The desire to view language as an historical phenomenon shows up in various ways in grammars of Egyptian. Almost all grammars provide brief historical sketches at the beginning of the work. These sketches acquire more significance, once it is realized that historical explanations of forms and constructions are not considered simply supplementary information but rather as necessary and sufficient explanations of usage. A scrutiny of the order of priorities in paragraph 117 of Gardiner's Egyptian grammar may illustrate this attitude. The paragraph consists of two main sections and an observation printed in small type. The subject of the paragraph is the use of the particle *iw*. In the first section the way in which it is used to introduce main clauses is illustrated and in the second section its use in subordinate clauses. The point is made that its use in subordinate clauses is mostly conditioned by the presence of a pronoun subject, although the formulation of hard and fast rules is not attempted. Now the observation in small type might be expected to deal with problems yet unsolved by the preceding analysis, and several come immediately to mind. Can Gardiner's distributional explanation be made a grammatical rule by explaining away the counter examples? What is the difference between the use of *iw* and that of other particles such as *mḫ* or *isṯ* whose meanings seem to overlap in certain environments? However, Gardiner is not interested in any of these questions. What disturbs him is the following:

> A certain contradiction may seem to be involved in the use of *iw* to introduce (1) detached independent sentences and (2) clauses subordinate in meaning, even though the latter use is confined, or nearly confined, to examples where a suffix-pronoun is the subject. The difficulty disappears if we assume that what we take to be a clause of circumstance was originally felt as *parenthetic* i. e., as an independent remark thrown into the midst of, and interrupting a sequence of main sentences. The use of parentheses to express temporal and circumstantial qualifications is frequent in all languages ...

One now realizes that "subordinate" and "independent" are logical rather than grammatical terms, that the historical origin of forms is more important than an exhaustive explanation of their uses, and that psychological factors ("... was felt as ...") are an integral part of grammatical analysis.

Occasionally there are teleological overtones in such historicistic statements. The following note from Erman's *Neuägyptische Grammatik*, paragraph 691, is appended to a short description of word order. It presupposes an optimal situation in which syntactic relations are marked by particles or case endings:

> Among the principal parts of the sentence in Late-Egyptian as well, the subject and object do without any external marking; one can recognize them only by their place in the sentence. The language has retained the preposition for the dative relation. One might have

> expected that it might have developed a clearer expression for the object relation in a preposition, as Coptic in fact did with its ⲛ- and ϵ- ...

The cumulative effect of such diachronic observations to the exclusion of synchronic ones has been to circumscribe the development of an autonomous Late Egyptian grammar. Unintentionally this historicism lends itself to a "classical age" interpretation of the Egyptian language, an interpretation with parallels in other areas of Egyptology as well. According to this view Late Egyptian is an intermediate stage between "classical" Middle Egyptian and Coptic, with the covert belief that it is a corrupt form of the former and a primitive form of the latter. This view is reinforced by the pedagogical practice (in and of itself logical) of introducing beginning students to Middle Egyptian first and also by the absence of a distinctive system of transcription for Late Egyptian. Admittedly how this situation is to be remedied remains unclear.

The other aspect of Neo-Grammarian analysis that stands out is the emphasis on a close psychological examination of the speech act. How this works out in practice may be seen in one of the most unfortunate paragraphs in Gardiner's Grammar, where he lays the groundwork for all subsequent discussions of coordination and subordination in Egyptian:

> § 211 *Difficulties in connection with virtual adverb clauses in Egyptian.* — This topic was touched upon as far back as Lesson II (§ 30), where it was learnt that the verbal sentence *wbn rˁ m pt* might, in certain contexts, correspond to English 'when (*or* if) the sun rises in the sky' or 'that the sun may (*or* might) rise in the sky', and that the non-verbal sentence *rˁ m pt* might correspond to English 'when the snu is (*or* was) in the sky' or 'the sun being in the sky', etc. Such virtual adverb clauses play an important part in Egyptian, and our task in the next few sections will be to illustrate the range of English meanings covered by them. By way of preface, we must caution the student that there is here a serious risk of imputing to the Egyptian writers distinctions which are, in fact, due only to the analysis of our English translations. It must be remembered that in *form* the virtual adverb clauses are complete sentences and that what they *say* is simply (e. g.) 'sun rises in sky' and 'sun in sky'. But we must take care not to run into the opposite error of maintaining that, because the Egyptians used one and the same form of words for (e. g.) 'the sun rises in the sky' and 'when the sun rises in the sky', therefore they did not *feel* that the first was a statement and the second a clause of time. Such a contention would be absurd; broadly speaking, the Egyptian must have known as well as we do the difference between an assertion and a temporal qualification, often, he was content with leaving the matter to the discrimination of the listener, where we should be at pains to convey our precise intentions.

Within the context of Gardiner's description of Egyptian this amounts to nothing less than the renunciation of any attempt to deal with the phenomena of co-ordination and subordination in any formal way [21]. The linguistic presupposition that psychological operations, capricious and inexplicable, take up the slack between formal explanation and actual linguistic reality, has, needless to say, been shown by Polotsky in recent years, to be unnecessary [22].

[21] The Neo-Grammarian position on this issue was that, with the passage of time, co-ordination would eventually give way to subordination.

[22] Much of the support for the need of a psychological explanation in which the value of verb forms depends on extralinguistic context derives from the supposedly polyvalent character of the *sḏm·f* form, which apparently could

It is possible to formally differentiate independent, coordinate, and subordinate constituents in far too many areas of grammar which were formerly abandoned to psychology.

Although many of the preceding observations about the applications of Neo-Grammarian models of language analysis have been negative in tone, the effect of the Neo-Grammarian revolution was in its day beneficial, both in general theory and in application. It shattered the claims of normative grammar. It presupposed that languages possessed a certain individuality. It provided for a chronologically differentiated analysis of texts. It required that the distinction between spoken and written language be kept in mind. This found its application in Egyptology with a consistency and familiarity that precludes any need for illustration. The Neo-Grammarian model exhibited all these virtues without which a successful attack, despite its shortcomings, on the grammatical problems of Egyptian, Akkadian, and Sumerian with their alien structures could hardly have succeeded.

III. Structuralism

The work of Neo-Grammarian scholars had been mainly concerned with phonology [23]. The enormous work involved in relating the various Indo-European languages to each other had resulted in the postulating of sound correspondences that increasingly were formulated as laws admitting of no exception. The existence of immutable laws in one area of grammar had a revolutionary effect on the general notion of what grammatical description was capable of being, and produced a certain amount of pressure for extending a similar scientific exactness to the study of syntax, which had undergone little of the revolutionary development known in phonology and morphology. The thrust of grammatical research in the twentieth century was to be towards developing first an adequate model for syntactic explanation, whose rules could be formulated with a degree of exactitude comparable with that of phonology, and secondly a general model of grammar as a whole. This program was only to be achieved by eventually abandoning the view that the study of language should be the study of the history of psychological decisions, and by adopting the view that language should be studied as communication.

Viewing language as communication lies in the long tradition of explaining language in reference to some other human activity. Traditional grammar conceived its task to be to explain the way thought was manifested in speech. Neo-Grammarian analysis attempted to relate the forms of language genetically, to a history of psychological decisions. Structuralism was to examine language in relation to the ability of people to communicate with one another. The concern of traditional grammar had been to understand how language

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mean *inter alia* "when he hears", "so that he hears", "he will hear". Polotsky has shown in his *Études de Syntaxe Copte* (Cairo 1947) and most recently in his "Egyptian Tenses", *Bulletin of the Israel Academy of Science* 2 (1964) 1-25 that this apparently polyvalent form actually conceals a number of subforms, each with clear and distinct meanings. The disappearance of polyvalence in this major area of grammar makes the use of extralinguistic context increasingly suspect.

[23] For the Neo-Grammarian neglect of syntax in general, see Ziemer, *Streifzüge*, 15, with special reference to Paul's *Prinzipien der Sprachgeschichte*.

expressed the universal logic of the mind. That of structuralism was to understand how ideas were *conveyed*. Behind the concern of structuralist work for identifying meaningful formal distinctions in language lay the question of the nature of human production and perception of language; of the encoding and decoding of what was seen to be a code of communications.

The means by which communicational phenomena were to be formalized with the desired exactitude of laws admitting no exception was provided by Durkheim's concept of the social fact [24]. A social fact was not the observation of something with material form, such as a mountain, nor was it an observation of an historical event. Nevertheless, a social fact had its epistemological coordinates in the form of the constraint it was demonstrably seen to exercise over those members of society subject to it. A social fact was "any kind of action, whether of a set nature or not, capable of exercising *external constraint* over the individual". Since the symbols of language, whose symbolic nature had been perceived to some degree from the beginnings of Western grammar, exercise constraint by causing changes in behavior, it now became an object of research as a social fact, as a behavior modifying signal system rather than as substance independent of the act of communication. The whole new dimension of language perception, how a hearer decodes a series of language signals, now had to be taken into account. The question could no longer be posed as one of determining what patterns, regularities and irregularities existed in a language, as a reflection of universal ways of thinking but also it was necessary to ask which of these conveyed a message to the hearer, with the possibility, most notably presented by Whorp, that patterns of thought were as particularistic as the language that mirrored them.

Part of the definition of a social fact is that it operates in reference to a set of conventions, customs, and values held by the social group in question in structured form. Since society itself is structure, its social facts are structured as well. The structure of language as a social fact is that of a system of relationships and it is the system, rather than anything intrinsic, that gives the elements of language value as signals that condition behavior:

> Briefly, this means that each language is regarded as a *system of relations* (more precisely, a set of interrelated systems), the elements of which — sounds, words, etc. — have no validity independently of the relations of equivalence and contrast which hold between them [25].

Thus, for example, in Spanish there are two voiced dentals, d and ð , which are allophones, the first occuring following a consonant and in sentence initial position, the second only in intervocalic position. Structurally speaking they are simply different forms of the same signal. The analogous [26] sounds in

[24] Cf. Dineen, *An Introduction*, 192 ff.

[25] J. Lyons, *Introduction to Theoretical Linguistics* (Cambridge 1968), 50. Thus in English the morphological category *adjective* may be defined by the endings *–er* and *–est* (i. e., happy / happier / happiest), a definition which would exclude *antagonistic* and similar words that cannot have these endings added to them. On the other hand, the partially overlapping category in syntax, *adjectival*, could be defined by a syntactic frame, such as *the* ——— *thing*, in which any word that could fit in the slot would be considered an adjectival, e. g., happy, antagonistic, stone etc.

[26] The English /d/ is alveolar and not dental, as is the Spanish /d/. English / ð / is also made with the teeth and tongue in closer contact than is the case with its Spanish counterpart.

English are structurally distinct, however; *die* / day / and *thy* / ð ay / have different meanings, and condition behavior differently. In Spanish and English, what is different is the system of phonetic relations, the code or rules of the game.

Viewing language as a system of relations requires defining the categories of language in terms of relations they enter into [27]. The structurally significant sounds of a language can be physically defined as the relation that the tongue, lips, vocal chords, and other organs of articulation have to each other when the sound is produced. Such are the phonological categories structurally defined. At a more abstract level of analysis, the various ways that these structurally significant sounds relate in combination serves to define morphological categories (e. g., words) while the ways in which these larger categories combine with each other serve to define the categories of syntax.

A chronological view of Egyptological work shows that the effects of structuralism have been surprisingly slight, although not entirely absent. Much of this can be explained by considering that there is an inevitable time gap in the application of new theory to specific languages. De Saussure's posthumous *Cours de Linguistique Générale* was first published fifty-seven years ago in 1915. The grammars of Erman [28] (*Neuägyptische Grammatik*, 1880; *Ägyptische Grammatik*, 1894) all antedate de Saussure as does Sethe's contemporary *Verbum* (1899-1902). Sethe's influential *Nominalsatz* [29] (1916) could also hardly be expected to have reflected the new structuralist view. All of these works understandably enough follow the Neo-Grammarian model of language analysis with its rigid laws of sound change (difficult to formulate because of the nature of the Egyptian consonantal writing system) and a syntax almost identical with that of traditional grammar.

In the years following the publication of de Saussure's work the implications of it were not immediately obvious. Neither was it clear how this new concept of grammar would affect the actual working out of the grammar of a specific language. Meanwhile in Europe work was going on in a number of sociolinguistic areas; most important was the investigation of the nature of meaning, where de Saussure's notion of language as form rather than substance led to the development of the notion that what was significant in the meaning of words was the limits of their semantic fields, the shape of these fields, what they included and how they related to the semantic fields of words with related meanings. The relation of semantic fields to communication and thought is well expressed by Arens:

> It is clear that the essence and value of everything linguistic resides in signification itself, however the "what" of signifying is once again moot. The lexical world stands between consciousness and external reality in the form of a representative intermediate world created by

[27] The purely relational basis of structuralism has its most detailed development in the work of L. Hjelmslev, *Prolegomena to a Theory of Language*, tr. F. J. Whitfield, (Madison, Wisconsin 1961).

[28] A. Erman, *Neuägyptische Grammatik* (Leipzig 1880); A. Erman, *Aegyptische Grammatik mit Schrifttafel, Literatur, Lesestücken und Wörterverzeichnis.* Porta Linguarum Orientalium, pars 15, (Berlin 1894); Kurt Sethe, *Das aegyptische Verbum im Altaegyptischen, Neuaegyptischen und Koptischen*, (Leipzig 1899-1902).

[29] Cf. K. Sethe, *Der Nominalsatz im Ägyptischen und Koptischen* (Abh. der kgl. Sächsischen Ges. der Wiss. 33, fasc. 3, [Leipzig 1916]).

> man, which he on the one hand uses as a mediating instrument of rendering himself intelligible, and which on the other hand no longer permits him to penetrate through it to reality. He has this only as far as his lexical world reaches, for all form is, in reality, limitation .... "The" meaning of a word is, therefore, is not graspable in an exact way, but its semantic functions are to be rendered perceptible only in various semantic sequences or contexts [30].

Such a view of the nature of meaning, allowing each word its own specific semantic field, by its nature precludes the existence of genuine synonymy. This concept of semantic field represented an advance in the general direction of formulating semantics in structural terms, a goal that was to be realized only by generative (transformational) semantics in the later years of the 1960's and an achievement which would eventually provide at least provisional solutions for some of the subtler problems involved in working out an adequate structural model for syntax itself.

These advances in semantics must be considered more as background to the Egyptological work of the late twenties and thirties than as directly influencing it. What is striking about the work of Gunn [31] and Gardiner (*Egyptian Grammar*, 1927) is the enormous increase in attention paid to the meaning of forms. Erman's grammar had given often very insightful definitions of the meaning of forms but had devoted no long discussions to the subject, presumably believing that the student would acquire the finer nuances of meaning through his subsequent reading of texts. Gardiner and Gunn continue the Neo-Grammarian meticulous attention to phonetic form, combined with discursive essays on the meaning of forms.

In works of this period, the increased interest in psychological and social interaction reinforces the tendency of Neo-Grammarian analysis to explain the use of grammatical forms in terms of social and situational context.

> The difference between *ḥr* + infinitive, on the one hand, the old perfective, on the other, may best be summed up by saying that the former is dynamic, active, and expressive of *action*, while the latter is static, passive, and expressive of *condition* [32].

Gardiner is generally credited with being the founder and major representative of situationalist grammar. In his *Theory of Speech and Language* [33] he places great emphasis on evaluating carefully the situation in which any interpretatively critical utterance is produced. However, he seems not to have been followed in this particular interest by other Egyptologists.

[30] Arens, *Sprachwissenschaft*, 424: "Es ist klar, dass Wesen und Wert alles Sprachlichen eben im Bedeuten liegt, jedoch das Was des Bedeutens ist schon wieder strittig. Die Wortwelt steht zwischen dem Bewusstsein und der äusseren Wirklichkeit als eine vom Menschen geschaffene stellvertretende Zwischenwelt, die er einerseits als ein vermittelndes Instrument der Verständigung braucht, die ihm aber anderseits nicht mehr gestattet, durch sie hindurch zur Wirklichkeit zu stossen. Diese hat er nur so weit, wie seine Wortwelt reicht, denn alles Formen ist ja ein Begrenzen ... 'Die' Bedeutung eines Wortes ist also nicht exakt erfassbar, sondern seine Bedeutungsfunktionen sind nur in verschiedenen Bedeutungsgefügen oder Kontexten wahrnehmbar zu machen".

[31] G. B. Gunn, *Studies in Egyptian Syntax* (Paris 1924).

[32] Cf. Gardiner, *Grammar*, § 320.

[33] A. Gardiner, *The Theory of Speech and Language*, (Oxford, 1932).

German Egyptological work has continued more in the tradition of Erman than that of Gardiner and Gunn, although their discoveries are dutifully incorporated. The grammars of Edel [34] and Westendorf [35] have a strictly traditional order of topics and far fewer discursive essays than Gardiner's work. They make little attempt to depart from Neo-Grammarian principles in either terminology or the formulating of questions and explanations. In view of the immense erudition displayed in these works it is particularly regrettable that practically none of the theoretical advances made in the last half century are reflected in them. Edel's work, in particular, since it constitutes the only detailed grammar of Old Egyptian, will tend to perpetuate the continually widening gap between general grammatical theory and the continued cultivation of Neo-Grammarian notions of language in Egyptology.

Against the continued dominance of Neo-Grammarian theory in Egyptology, one can point to a certain amount of work using more modern grammatical models. Among the Egyptologists using structuralist models one may name H.-J. Polotsky, J. Vergote, F. Daumas, W. Schenkel, S. I. Groll, and C. Hodge. There is explicit evidence that Vergote and Daumas were influenced by Lucien Tesnière's [36] work. Polotsky shows a progression from a traditional model through various stages incorporating structuralist notions to a point [37] where his work is almost completely structural. His student S. I. Groll has carried the classificatory tendencies found in much of Polotsky's work to an unfortunate extreme in this writer's opinion, although it contains many interesting observations [38]. Of the other scholars mentioned above, W. Schenkel's work is somewhat eclectic, quoting Paul's *Prinzipien der Sprachgeschichte* on the one hand and using Bloomfieldian notions and procedures (e. g., immediate constituents) on the other hand. The American scholar C. Hodge is to be mentioned mainly for recasting certain basic notions, derived primarily from Gardiner, in structural terms, in an article whose value is more that of presenting familiar notions in a new medium than in providing new structural insights. The most consistently structuralist original work in Egyptology, is in my opinion, that by F. Daumas, rather explicitly titled: "Application de la Syntaxe Structurale: La proposition relative égyptienne étudiée a la lumière de la syntaxe structurale" [39], in which he shows that Egyptian distinguishes formally between restrictive and non-restrictive relative clauses.

The work that influenced both Vergote and Daumas, that of Tesnière, stands on the borderline between structuralism and generative (transformational) grammar. It contains elaborate sub-categorizations of the parts of speech as well as a certain number of operations similar to the transformations of generative grammar. For example, adverbs are first sub-categorized into adverbs of localizations and adverbs of relation. Adverbs of localization are further subcategorized into adverb of place and of time. Adverb of place, of time, and the

[34] E. Edel, *Altägyptische Grammatik* (*Analecta Orientalia* 34, 39, [Rome 1955, 1964]).

[35] W. Westendorf, *Grammatik der Medizinischen Texte* (Grundriss der Medizin der Ägypter, 8 [Berlin 1962]).

[36] L. Tesnière, *Éléments de Syntaxe Structurale* (Paris 1953).

[37] H.-J. Polotsky, "The Coptic Conjugation System", *Or* 29 (1960) 392-422.

[38] S. I. Groll, *Non-Verbal Sentence Patterns in Late Egyptian* (London 1967).

[39] *Orbis* II (1962) 21-32.

earlier derived category of adverbs of relation are all further sub-categorized according to their respective Latin interrogative, e. g., *ubi* "where (at)", *quo* "whither, to where", *unde* "whence", "from where", and *qua* "how". This yields twelve distinct adverbial categories. Adjectives have a separate classification that produces eleven sub-classes. Following Jespersen [40], he provides for a process he terms *translations* whereby one part of speech may be derivationally related to others, e. g., *soft* ←→ *softness* ←→ *soften* ←→ *softly*.

One of the few critiques of general linguistic theory published by an Egyptologist is that of Vergote's review of Tesnière's work. Vergote criticizes certain of Tesnière's definitions as unsuitable on pedagogical grounds [41], reasoning that they vary too greatly from traditional usage. In some cases the term chosen is deemed misleading, as when Tesnière uses *substantive* to refer to an abstract noun such as "goodness", implying the presence of substance [42]. In regard to categories of relation, Vergote points out that the referents of an adjective like *Cornelian* in *a Cornelian heroine* may equivocally be the *writer* Corneille if the heroine is a character in one of his plays, or alternatively the *character* itself if the heroine is not a character of the play. Vergote then proposes a new set of categories, defined by essentially non-structural criteria [43]:

> Given that our definition of the semantemes lies firstly in the meaning and secondarily in the function, the infinitive and the participle should be treated as verbs.

Daumas's article on the relative clause discusses the various forms of Egyptian relative clauses as manifestations of a unitary phenomenon. He views the relative clause as a sentence that has been converted into an adjective by means of certain grammatical devices, which, following Tesnière, he terms "translatives", i. e., the relative pronoun *nty*, the ending *-w* of the relative form, and the zero marker of "virtual" relative clauses [44]. He points out, in passing, that Egyptian distinguishes restrictive and non-restrictive relative clauses, but the bulk of his discussion is simply a restatement of previously known facts about Egyptian relative clauses within a structuralist framework with transformational overtones.

[40] O. Jespersen, *The Philosophy of Grammar* (London 1929).

[41] I expect that, in general theory, pedagogical considerations have weighed far heavier among Egyptologists than is generally acknowledged.

[42] Cf. *Orbis* 9 (1960) 485.

[43] *Orbis* 9 (1960) 491: "Étant donné que notre définition des sémantèmes repose en premier lieu sur la signification et secondairement sur la fonction, l'infinitif et le participe doivent être traités comme des verbes".

[44] The term *"virtual" relative clause* (i. e., by virtue of meaning rather than form) refers to unmarked relative clauses which follow indefinite antecedents in Egyptian and various other Afro-Asiatic (Hamito-Semitic) languages, e. g., *msḏr di·f mw ḥwꜣ* "an ear which gives off a putrid liquid" (lit. an ear such that [circumstantial *sḏm·f* ???] it gives off.) or Arabic: ẓurūfu lā taghīru "unchanging circumstances' ". The Arabic relative clauses have indicative verb and possibly originally arose from a sort of coordination (i. e., lit: circumstances and they do not change) or conceivably from apposition (circumstances, unchanging things) at some earlier unattested stage of the language. The Egyptian evidence is far from conclusive but tends to favor understanding the following *sḏm·f* forms as circumstantial *sḏm·f's*, mainly on the basis of the forms that replace them (*iw·f ḥr sḏm*) which is clearly a circumstantial construction (*Umstandsatz*), in Late Egyptian and Coptic.

Polotsky's article on the Coptic conjugation system[45] is an insightful classification of tense affixes and is important for the systematic use of syntactic criteria it shows. It is the first article that uses the de Saussurean notion of a paradigm, that is, not simply a table of forms, but rather an exhaustive set of complementary distinctions. As a result of working this notion out, he exludes the Coptic I Future (ϥⲡⲁⲥⲱⲧⲙ̄) from the paradigm as a basic tense, showing that it is simply the present tense of a complex construction found in other tenses as well. For the classification of the Coptic tense affixes he uses the syntactic criteria of the form in which the direct object is introduced, as well as the class membership of the constituents of the construction, whether verbal (an infinitive), adverbial or a stative verbal form (the qualitative). The analysis reflects an awareness of Bloomfieldian immediate constituent analysis.

Thus the impact of structuralism has been limited in Egyptological work. In addition to the factors of time lag which were pointed out above, the reason for this slightness of impact may have a geographic basis as well as a chronological one. The great bulk of grammatical work in Egyptology has been done in Europe and the main thrust of European structuralist work has been phonological. One need only cite the work of the Prague school under Trubetzkoy and the London school under Firth to provide examples. Because of the nature of the Egyptian written evidence, the relevance of structuralist work was not yet clear for Egyptologists as they went about wrestling with the mainly syntactic problems of Egyptian and Coptic. In the United States, however, where Egyptologists, in general, were far less interested in grammatical problems, the structuralist advances of Bloomfield and his successors in the realm of morphology and syntax generally went unnoticed.

IV. Transformationalism

In the years following the publication of Bloomfield's *Language*, a benchmark in the development of structuralism, the examination of the categories of language encouraged ever greater exactness in definition as well as the increasing adoption of elaborate symbol systems to facilitate reference to complex constructions[46]. Towards the end of the war years, the effects of the cybernetic revolution began to be felt among mathematicians, logicians, and linguists. A greater and greater need began to be felt to formalize language in a way that it could be used in computer work. Following a tradition going back to Descartes that language is to be explained mechanistically, a tradition reinforced by de Saussure and general structuralism, by 1951, when Zellig Harris published his *Methods in Structural Linguistics*[47] the amount of algebraic symbolism and more importantly the rigor with which formulated rules were applied and taken to their logical conclusion characterized a grammatical model far closer to mathematical and logical models than had yet been known. Even as linguistic analysis of language was coming to look more and more like the analyses of symbolic logic, Quine and Reichenbach were pursuing their studies of language

[45] Polotsky, *Or* 29 (1960) 400 ff.

[46] L. Bloomfield, *Language* (New York 1933); the editions following this first edition were the most seminal.

[47] Z. S. Harris, *Methods in Structural Linguistics* (Chicago 1951).

in this latter discipline. Reichenbach's *Elements of Symbolic Logic* [48] provides rules for the conversion of one type of logical statement into a different although equivalent form. From mathematics Reichenbach borrows the term *transformation* to represent the process whereby one argument may be converted into another equivalent formulation, allowing one to consider (1), (2) and (3) to be considered logically equivalent and formally relatable.

(1) George VI was crowned at Westminster Abbey.

(2) The crowning of George VI took place at Westminster Abbey.

(3) Where George VI was crowned was at Westminster Abbey.

This concept of *transformation* was to radically transform the study of grammar.

By 1952 Zellig Harris and Noam Chomsky, then a graduate student in linguistics with an earlier education in mathematics, were both working on a method for representing the structure of larger units than the sentence [49]. The normal grammatical representation of sentences, e. g., subject-predicate represented as NP–VP would have allowed only a meaningless column of identical chains of symbols as a representation of any discourse. However by applying the symbolico-logical notion of transformation equivalences could be found in sentences of diverse form, such as the above sentences (1) – (3). Harris was thereby able to analyze discourse as a chain of equivalences combined in various ways, with and without negation.

More importantly than as a tool for discourse analysis, the concept of *transformation* allowed, for the first time, a formal explanation of relations, such as positive and negative, active and passive, that had been recognized intuitively, but which had never been able to be formalized, since the very beginnings of grammatical analysis. Following Harris's precept that the consequences of a formalized rule must be pursued to its logical end no matter what awkwardness might ensue, Chomsky devoted his dissertation to pursuing certain traditional structuralist assumptions to their logical conclusions, showed that it was impossible to account for a number of linguistic observations without positing a new level of analysis *P*, more abstract than the surface form of language. The new grammar would be based on specific assumptions about what a grammar should do — *generate*, i.e., give the rules for forming all possible grammatical sentences in the language, and fail to generate all ungrammatical ones. The form of such a grammar was to be conceived of as a machine with three parts or components; a *base component* containing rules for generating the most basic forms of sentences, a *transformational component* containing rules for converting the sentences of the base component into more complex forms, e. g., change active to passive, and a *lexical component* that would contain rules for converting the abstract symbols (NP, VP, prep. etc.) used in representing the sentences of the base and transformational components into real words of the language.

The work of Chomsky has produced rich results. Transformational analysis has been applied in great detail to almost all areas of English syntax and an enormous amount of new information about how English functions has come to light as a result [50]. In addition, a very large amount of work has been done

[48] H. Reichenbach, *Elements of Symbolic Logic* (New York 1947).

[49] Z. S. Harris, "Discourse Analysis", *Language* 28 (1952) 18-23 and "Discourse Analysis: a Sample Text", *Language* 28 (1952) 474-494.

[50] A good bibliography can be found in R. P. Stockwell, P. Schachter and B. Hall Partee, *Integration of Transformational Theories on English Syntax* (duplicated: Los Angeles 1969).

on other languages of the world. For some languages the only grammatical work that has been done has been done with the transformational model. Year by year facts about how natural languages function have been piling up: new facts requiring modifications of the original model. As a result of disputes about the role of the lexical component and whether the base component exists at all, there are now two separate schools, one of the orthodox followers of Chomsky, and a school of generative semantics, represented by a number of younger scholars.

The existence and continued flourishing growth of transformationalism poses a problem for scholars in other disciplines unacquainted with the method. The form of the grammar is radically different, the terminology is often difficult and not immediately comprehensible and the discussions often difficult if not impossible to follow. The enormous gap in communication, the result of the great rapidity of advance in both structuralism and transformationalism threatens to build an unbreachable wall around other disciplines, of which Orientalism, and specifically Egyptology, is our main concern here. An isolation that divorces the study of Egyptian from the study of language in general is obviously an untenable intellectual position, utterly opposed to everything science has stood for since the Renaissance. The Egyptologist faces the option of either working himself into the new grammatical method, painful as that is, or depriving himself of the insights to be gained from other languages by other scholars working with transformational methods, gradually allowing the study of Egyptian grammar to sink to the status of an arcane craft, starved by the absence of contact with the larger study of language.

Since this writer is *parti pris* and feels that the application of transformationalism to Egyptian and Coptic is a matter of the highest importance, the remainder of this essay is devoted to a critique of four grammatical issues in Egyptian that it is hoped will illustrate the possibilities inherent in the application of transformational methods to the study of Egyptian and Coptic. The four issues are: (1) the relation of active and passive sentences; (2) prepositional usage; (3) locative, manner, and time expressions; and (4) the Late Egyptian Conjunctive.

1. *The Active-Passive Relationship*

In discussing a number of topics, including the syntax of infinitives and passive participles in Egyptian, Gardiner utilizes the notion of *semantic subject* and *semantic object*[51] to achieve a certain generality in speaking of formally distinct phenomena. Thus, in the following sentences and phrases, the constituents marked with a subscript S are all semantic subjects, while those marked with a subscript O are semantic objects, irrespective of the *grammatical* relationship that might obtain (WH = relative clause marker, oblique):

(3) *iw in(i)·n ḫ3styw*$_S$ *mfkȝt*$_O$
brought foreigners turquoise
"The foreigners have brought turquoise".

(4) *iw in(i)· –w mfkȝt*$_O$ *in ḫ3styw*$_S$
brought-Passive turquoise by foreigners
"The turquoise has been brought by the foreigners".

[51] These terms are defined by Gardiner, *Grammar*, §§ 297, 300.

(5) *mfkȝto in(i)· (w)t·n ḫȝstywS*
turquoise brought-WH foreigners
"The turquoise which the foreigners brought..."
(6) *mfkȝto in(i)· (w)t in ḫȝstywS*
turquoise brought-WH by foreigners
"The turquoise which was brought by the foreigners..."

Now the notions *semantic subject* and *semantic object* correspond to acceptable intuitions concerning how language works. The disadvantage of employing such terms lies not in their reflection of legitimate perceptions but rather in the sporadic, *ad hoc* and non-formal way in which they are used. One finds that they are only used when the semantic subject and the semantic object differ from the grammatical subject and grammatical object. They do not refer to formally defined relationships but are used with the expectation that their referents will be immediately identifiable to all.

A transformational formulation of the active-passive relationship has the advantage of being both completely formal and simpler. No *ad-hoc* semantic categories are necessary. Instead, either one can derive (4)-(6) from (3) according to the Chomskyan "interpretive" model, or one can derive all four examples from an abstract case frame such as (7) where the relational cases *Agent* and *Object* have the same referents, although not the same theoretical implications, as Gardiner's *semantic subject* and *semantic object* [52].

(7) *ini* (Agent) (Object)

Using either base form, (3) or (7), the remaining constructions could be derived by transformational rules. Since rules are required to form the constructions anyway, no matter what grammatical model is used, such rules not only remove the need for the notions *semantic subject* and *object* since these are equivalent to the grammatical subject and object of the base form, but also these rules add no further complexity to the grammar (the mechanics of this process may be seen below in Appendix One). As such they represent an advance in both economy and concreteness in rule formulation.

2. *Prepositional Usage*

The various grammars and lexicons of Egyptian generally provide information about what if any prepositions are used after particular verbs. Thus a random scanning of the dictionary will yield *sḏm n* "listen to, obey", *iri m*

[52] The case frame model proposed by Ch. J. Filmore, "The Case for Case", in *Universals in Linguistic Theory*, E. Bach and R. T. Harms, eds., (New York 1968) 1-90 provides that, rather than deriving the surface forms of sentences from underlying declarative sentences in the active voice, the surface forms of sentences be derived from entries in the lexicon. In the lexicon each verb will be provided with a "case" frame that will indicate which abstract relations a verb may have with other elements in the sentence; thus *open* would have the case frame: ——— Agent Object Instrument. Subsequent transformations would choose one of the cases as subject, e. g., (Agent) John opened the window with a hammer; (Object) The window opened / The window was opened (by John) (with a hammer); (Instrument) The hammer opened the window. Such a formulation goes a long way toward removing the traditional bias in favor of active sentences over passive ones for example, that tends to skew both grammatical descriptions and the perceptions of researchers.

"amount to", *mdw m* "speak against". The use of these prepositions is considered immutable and part of a bound construction.

Traditional grammar, however, generally ignores the idiomatic use of prepositions to introduce the objects of nouns with verbal meaning. Thus, although the dictionaries record that the verbal expressions (8) and (9) require the specific prepositions shown, neither dictionaries nor grammars provide the information that (10) and (11) require the same prepositional regimen:

(8) Egyptian: *nḫt r* "be stronger *than*, conquer" [53]
(9) Coptic: *ϩisvō əntən* "receive instruction *from*"
(10) Egyptian: *nḫtw r* "victory against" [54]
(11) Coptic: *refϩisvō əntən* "a pupil of" (historically: one who receives instruction from the hand of)

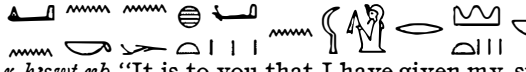
Traditional grammar can, of course, record the prepositional usage of (10) and (11) in the lexicon, but it has no synchronic way of formally explaining why the choice of prepositions should be the same for both the verbal and nominal expressions. Clearly the problem is one of derivation; no problem, in effect, is presented in the case of infinitives and participles, treated as derived parts of the verb and understood to require the same prepositional regimen for their objects that their source does. Traditional grammar fails to consider the derived nouns of (10) and (11) in the same way as participles and infinitives because they are not predictable derivations in the sense that they are presumed to exist for all verbs in the language.

Generally in the morphological sections of traditional grammars of Egyptian words such as *nḫtw* "victory", *sḫɜw* "remembrance" and *mtrw* "witness(ing)" are said to be derived from their respective verbal roots, but here "derived" is understood historically rather than syntactically. Only a notion of syntactic derivation would adequately explain the syntactic phenomenon of the idiomaticity of the regimen of such nouns.

Information about prepositional regimen is, of course, not all the information one needs to know in order to use a noun properly, and the eventual form of the lexical rule will have to reflect all relevant information. In addition to the usual information about gender and number, one needs to know the selectional restrictions for the noun, e. g., what class of verbs or adjectives it can be used with, to avoid ungrammatical (outside of poetic language) constructions such as "a perspicacious stone" or "the dictionary elapsed". Traditional grammar normally expects "meaning", in the sense of intuition about the nature of the world, to prevent such combinations, but it is hardly necessary to point out that "meaning" is a truly treacherous guide in a foreign language, particularly an ancient one.

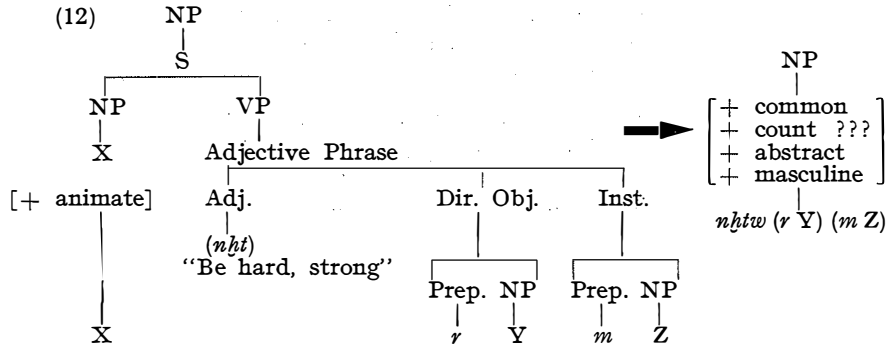
[53] See Erman-Grapow, *Wörterbuch der ägyptischen Sprache* (Berlin 1926-50), Belegstellen, II, 314 exx. 13 and 14.

[54] For example, Erman-Grapow, *Wörterbuch*, Belegstellen II, 317 ex. 7:



dì·n· (i)n·k nḫtw n(y) ḫpš·ì r ḫɜswt nb "It is to you that I have given my sword's victory against every foreign land".

How, concretely speaking, would a transformational entry look for an abstract noun such as *nḫtw* "victory, triumph"? I would suggest that it would look something like the following:



The information that figure (12) conveys is the following: (1) The abstract noun *nḫtw* is generated (indicated by the arrow) from a complex construction that is itself a noun phrase (marked by NP at the topmost node). (2) This complex construction is itself a sentence (marked S) with the meaning "Someone (X) is hard (or strong) in relation to someone (Y) by means of something (Z)". The brackets mark off *features* of the participating noun phrases. (3) The NP variable X must be [+ animate], i. e., a person, since unlike the related *nḫtt* our abstract *nḫtw* never seems to mean "hardness" of things. (4) The feature [+ common] to the right of the arrow indicates among other things that *nḫtw* cannot be used as a vocative, nor in apposition with nouns referring to persons. (5) The feature [+ count] indicates that it can be used with numbers and with words like *ꜥšꜣ* "numerous". (6) The feature [+ abstract] indicates that it cannot be used as the subject of verbs marked [+ animate], such as "run", "think", "scream" etc., nor can it be modified by adjectives appropriate to animate beings. Furthermore, apparently it may be referred to by the "neuter pronoun" *st* "it", although the use of this pronoun requires further investigation. (7) Lastly the feature [+ masculine] means that it takes masculine agreement forms, e. g., uses *pn* rather than *tn* for "this" etc.

This formulation of the rule, incomplete and subject to modification as it is, does provide a great deal of information within a formalized frame. The use of feature analysis also represents an advance in that it provides the analyst new analytical concepts and forces him to consider more distinctions than he would have otherwise. Increased power of observation is certainly something that every scholar hopes a new analytical model will give him.

3. *Locative, Manner, and Time Expressions*

Closely related to the problem of providing for the derivation of certain types of nouns with prepositional regimens is the problem of classification and interpretation of prepositions. This problem may be seen in an acute form in paragraph 162 of Gardiner's grammar where twelve uses of the preposition

m are listed. Does the preposition *m* really have this many meanings? If this is so, one would expect a substantial amount of ambiguity where none actually occurs. The traditional answer is that ambiguity is removed by the context, but the concept of context turns out to have little explanatory power.

In this list of uses, section 9 is entitled "idiomatically with verbs" and deals with the situation discussed in the previous section. The immediately preceding section 8 deals with the notion of separation, but on closer inspection turns out to include verbs that really should belong in section 9, such as *pri m* "leave from", *ii m* "return from" *šs*^{c} *m* "begin from" and *šwi m* "free of". As we have argued above, in these cases the preposition has no meaning in and of itself, but is a required particle to introduce the direct object of certain verbs and must be simply specified in the lexicon. Thus there exist at least two formally distinguished "uses" of the preposition *m*: (1) idiomatic, lexically specified marker of the underlying direct object and (2) all other uses. At this point our attention turns to usage 7 "of instrument" where there are cited: *m ḫps·i* "by my strong arm"; *m sḫrw·i* "by my counsels" and in a separate sentence *mḥ m* "fill with" and *ʿpr m* "equip with", the latter two surely belonging to category 9. The first two examples of "instrument" require a preceding verb, but, unlike the lexically specified use of *m* to introduce the direct object, with other verbs using other prepositions, when the instrument used is specified, the preposition *m* is always used. Noun phrases used as instruments will also have certain features, e. g., [–human] [– time]. Prepositional phrases of instrument, since they require a preceding verb, also cannot occur as adverbial predicate, e.g., the ungrammatical: **iw·s m sḫrw·i nfrw* "It is by my good counsels".

The remaining uses lose their ambiguity according to the nature of features they bear. Time words, for instance, must be marked [+ time] for other reasons, e. g., to allow them to be preposed, or to be used adverbially without any preposition at all. Adverbial expression of states, e. g., *m ršwt* "in joy" *m snb* "in health" have to be marked [+ abstract] (to prevent their use as subjects of verbs such as "run", "scream") although they otherwise behave as locatives. The genuine locative uses, in "a place", "house", "boat", "heart", "mouth" etc. involve noun phrases that have the features [+ interior]; similarly words like *tȝ* "earth" *ȝt* "back" will be marked [-interior] and will require the preposition *ḫr* "upon" to express the locative. When the preposition *m* is used with noun phrases marked [– interior], it then forms a phrase that expresses what some previous noun phrase consists of, or is classified as, e. g., *m ḏbt* (a building) of brick, *m ḥȝty-*^{c} "(do something) as a prince". Gardiner's categories (10) followed by infinitives, (11) followed by the *sḏm·f* and (12) followed by the *sḏmt·f* form will all be marked [+ time] and handled similarly to other time expressions.

What I have attempted to show in this unfortunately rather involved discussion is that context can be given a form, and that it possesses in effect an internal structure or set of structures relevant to other parts of grammar, and that the tools provided by transformational grammar allow one to penetrate this *terra incognita*, find in it familiar elements and integrate it with the rest of the grammar.

4. *The Late Egyptian, Demotic, and Coptic Conjunctive*

Egyptologists operating with a traditional grammatical model have experienced difficulty in analyzing a tense, termed the conjunctive, which appears

in Late Egyptian texts and continues through to Coptic. For our purposes it is interesting to observe how the model of traditional grammar contributed to their difficulties, predisposing them to look for solutions along certain lines and to fail to consider solutions along other lines.

There are two presuppositions that interact to present difficulties. The first is that the tenses of verbs are basically temporal, expressing actions or events occurring within specific stretches of a time continuum, conceived of as linear. This entails the belief that languages may be expected to have means of distinguishing actions occurring in the past, the present, and the future, at the very least, and that this distinction will be the critical criterion of the forms in question. It is envisaged that other distinctions may occur, such as *durative/non-durative*, referring to actions that occur over a significant period of time, or many times, versus those that occur only once and relatively instantaneously. If a tense under observation seems to fit such criteria, then it is considered non-problematic and natural. This is to say, such tenses are the counterparts of well-known tenses in European languages.

For those tenses whose usage eludes a unitary time-based definition, the second assumption is that such tenses still basically refer to time-based activities, but occur under certain idiosyncratic syntactic conditions which cause the irregularity of the tense and which must be listed one by one. Such syntactic conditions are considered to be individually discrete and are not expected to necessarily show any common features. Such tenses are viewed as the product of an erratic historical development (Neo-Grammarian) and as eccentric instances of the specific "genius" of a language.

The conjunctive "tense" is an example of the latter sort of apparently idiosyncratic tenses, which may be illustrated by the underlined portions of (13)-(17):

(13) I will beat you *and I will take away* the cattle *and they will belong* to me (Horus and Seth, 7,7).

(14) Hurry *and come by here ... and write* me as well *and ask* about the merchants ... (P. Bologna 5, 5-6).

(15) his taking the chisel *and breaking* it ... (P. Salt, 219).

(16) We used to take my chisels of copper in our hands *and carry off* the mummy masks that had gold on them ... (Peet, The Great Tomb Robberies, II, plate 6).

(17) What then *should I do?* (Mt 27,22).

The first serious formulation of the way this verb form is used is that of Stern:

> Whereas the participle indicates the circumstance which accompanies the action of the main clause, without being dependent upon it, the conjunctive expresses an action which constitutes the object or the consequence of the action of the main verb[55].

Erman, defines the use of the conjunctive as follows:

> A verb that is in the conjunctive, is linked with a verb preceding it, and receives its grammatical meaning. It is, therefore, according

[55] L. Stern, *Koptische Grammatik* (Leipzig, 1880), § 440: "Während das participium den umstand bezeichnet, der die handlung des hauptsatzes begleitet, ohne von derselben abhängig zu sein, drückt der conjunctiv eine handlung aus, welche den gegenstand oder die folge der thätigkeit des hauptverbs bildet".

> to what it follows, to be conceived of as a future, as an imperative, as an infinitive, etc. We regularly translate it by a verb attached with 'and'. Still one must observe that it is often not a matter of simple coordination, but that the conjunctive expresses a particular relation of the second verb to the first one; it, therefore, corresponds approximately to our 'and so', 'and then' [56].

Subsequent scholars have referred to the conjunctive in terms that indicate they consider the verb form to be a complex of usages, with a core of "basic" uses; so Wente (Italics mine):

> The conjunctive *commonly* continues the nuance of a preceding verb form ... Because the conjunctive is *basically* a continuative form, its use to continue past habitual action is not out of keeping with its basic function or its generally accepted etymology from *ḥn' ntf sḏm* [57].

This view of the conjunctive as a shifting and loosely used form is clearest expressed by Lichtheim:

> In conclusion it may perhaps be said that in its Late-Egyptian phase *mtw·f sdm* was a complex tense whose usages had not yet crystallized [58].

From the general tone of the grammars, the conjunctive is clearly an awkward problem for the traditional grammatical model with its assumption that tenses inherently express specific stretches of time, since the conjunctive apparently continues all tenses except the punctiliar past, as Wente has shown. The model of traditional grammar has great difficulty accomodating such a state of affairs, and the anguish felt by Egyptologists working within that system can be read from between the lines of Lichtheim's concluding words above.

Such a tense, however, can be easily handled in a natural way within the generative model of tense, particularly in its more recent formulations. A transformational account of tense is derived from a consideration of sentences such as (18) and (19).

(18) John will go on Thursday.

(19) John's going will take place on Thursday.

In view of the synonymy of these two sentences, the predicate "will take place" of (19) corresponds to the future tense of (18). Because of this correspondence of tense and predicate, and for other reasons, notably the behavior of

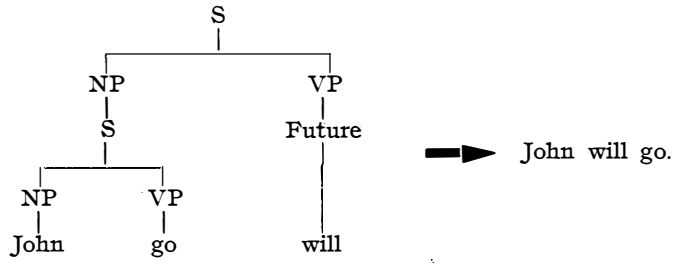
[56] Erman, *Neuäg. Grammatik* § 577: "Ein Verbum, das im Konjunktiv steht, schliesst sich an ein ihm vorhergehendes Verbum an, und erhält dessen grammatische Bedeutung. Es ist also je nachdem als Futurum, als Imperativ, als Infinitiv, u.s.w. aufzufassen. Wir übersetzen es in der Regel durch ein mit 'und' angeknüpftes Verbum. Indessen beachte man, dass es sich vielfach nicht um eine einfache Koordination handelt, sondern dass der Konjunktiv ein besonderes Verhältnis des zweiten Verbums zum ersten ausdrückt; er entspricht also etwa unserm 'und da', 'und dann', 'und so' (vgl. die Beispiele in den folg. Paragraphen)".

[57] E. Wente, "The Late Egyptian Conjunctive as a Past Continuative", *JNES* 21 (1962) 307, 311.

[58] M. Lichtheim, "Notes on the Late Egyptian Conjunctive", *Studies in Egyptology and Linguistics in Honour of H.-J. Polotsky* (Jerusalem, 1964), p. 8.

adverbs, it is concluded that tense should be considered a type of predication with a (tenseless) sentence as subject, such as (20)

(20)



The consequence of this view is that it dethrones tense from its special place in grammar. Since the predicate "tense" ("future" in our example) is a member of the larger relation *predicate*, and has a sentence as a subject, one should expect that any suitable predicate of a sentential subject might actually be realized as a specific verb form. This is, in fact, what one actually finds when one examines natural language. The Coptic tense *šafsōtem* means "he hears as a matter of course / by the nature of things", and contrasts with a "present" *fsōtem* "he usually hears, has been known to hear".

A second consequence of viewing tense as predication allows a natural explanation of the conjunctive. If tense is a predication, then it can be conjoined like other predicates. Thus (21) is essentially the same structurally as (22):

(21) John will come and give me the money.

(22) John's coming and giving me the money will take place.

Such a view explains why the "tense" need only be expressed once, and why the conjunctive can follow such a large number of tenses. The reason is that the conjunctive has no tense of its own just as "giving" in (22) has no tense of its own; it is a neutral form sharing its tense with other conjoined verb forms, and marked only once for the entire chain. Even those cases where it is unmarked, the so-called independent uses, fit perfectly into other grammatical patterns; (24) is a logical and perfectly understood addition by a second person to (23):

(23) John's coming and giving me the money will take place then.

(24) And his giving you excuses for being late too.

V. Summary

Examining the course of development from the beginnings of classical grammar to transformationalism, one perceives that it has moved in the path of a spiral. When Western grammar began in fifth-century Greece, it took the form of studying and elucidating the way the universal language of the mind manifested itself in speech, spoken and written. During the course of antiquity various implications of this view were developed and elaborated. The Middle Ages paid disproportionate attention, in modern eyes, to the logical side of language study. With the Neo-Grammarians language acquired an additional dimension, that of history, as well as providing the first intimations that not only principles but also performance must be taken into account, since the uni-

versal language of the mind was filtered through the psychic structure of individual speaker's minds. In structuralism one reaches the antipodes of Classical Antiquity's conception of grammar: the inner universal form of language is fictive, or at best, irretrievable. With the development of generative grammar the increasing abstraction of rules designed to generate a multiplicity of related forms began to approximate more and more the universal language of the mind as this was now conceived by logicians of the twentieth century. The universal language of the mind, if it did not exist, would have to be created. Thus twentieth-century grammar would seem to have gone back to its beginnings in fifth-century Athens.

Fifth-century grammar posited its universal language of the mind as given; generative grammar derives it from the application of formulated rules carried to their logical conclusions. Fifth-century grammar was based on one language: Greek; modern grammar must take all the languages of the world eventually into account.

Orientalists have always been keenly aware that the languages of antiquity represent the main source of our knowledge about ancient cultures. Where no written records are available, we know little indeed, in spite of the valiant efforts of archeologists. The grammarian is responsible for the decipherment and correct understanding of the relevant languages, and thus the state of linguistic research is, in a certain sense, the touchstone of the discipline as a whole. To maintain grammatical methodology in sound scientific health, it must be open to advances in general grammar. The difficult idiom of generative work, I am convinced, must be mastered [59], unless Orientalism is willing to deprive itself of the increasing number of insights about how language works that are pouring in from linguists working within a generative model. The alternative is an increasingly sterile methodological *cul de sac.*

[59] For a scholar who wishes to work his way into transformationalism, it is difficult to say exactly where he should start, but the following is my suggestion. The simplest and clearest work is R. Jacobs and P. Rosenbaum, *Grammar* I/II, (Boston 1967). These are two short volumes, bound as pamphlets, that are intended for beginning students and present the basic concepts of generative grammar in an intelligent and readable way. My feeling is that it would then be best to gain some perspective in the various models within generative grammar. A good compilation is E. Bach and R. T. Harms, *Universals in Linguistic Theory,* (New York 1965) and for the important notion of performative, J. L. Austin, *How to Do Things with Words* (New York 1965) and J. R. Ross, "On Declarative Sentences", in R. A. Jacobs and P. S. Rosenbaum, *Readings in English Transformational Grammar,* (Waltham, Mass. 1970), 222-272. For individual treatments, the above mentioned volume edited by Jacobs and Rosenbaum may be consulted, as well as the following, which also represent collections of articles originally circulated in duplicated form only: D. A. Reibel and S. A. Schane, *Modern Studies in English: Readings in Transformational Grammar,* (Englewood Cliffs 1969) and J. A. Fodor and J. J. Katz, *Readings in the Philosophy of Language,* (Englewood Cliffs 1964). In the course of his reading one may wish to consult the index (in lieu of a glossary) in R. A. Jacobs and P. S. Rosenbaum, *English Transformational Grammar,* (Waltham, Mass. 1968), 290-294. Finally the following very important integrative work should not be forgotten: R. P. Stockwell, P. Schachter and B. Hall Partee, *Integration of Transformational Theories on English Syntax* (duplicated: Los Angeles 1969). This last two volume work is now difficult to obtain but it should appear shortly in print.

VI. Appendix One: Generative Mechanics

It may be illuminating to give a schematized derivation of sentences (3) - (6) repeated here for convenience as (1)-(4): (Abbreviations: WH = relative clause marker; PAST-PASS. = a portmanteau marker of the past tense passive voice, FACT = a particle that indicates the action of the verb or the beginning of the action is considered a fact and not contingent or hypothetical):

(1) *iw in(i)·n ḫ3styw mfkȝt*
FACT bring-PAST foreigners turquoise
"The foreigners have brought turquoise".

(2) *iw in(i)· w mfkȝt in ḫ3styw*
FACT bring-PAST-PASS. turquoise by foreigners
"Gold has been brought by the foreigners".

(3) *mfkȝt in(i)·(w)t·n ḫ3styw*
turqoise bring-WH-PAST foreigners
"The turqoise which the foreigners have brought".

(4) *mfkȝt in(i)·(w)t in ḫ3styw*
turquoise bring-REL by foreigners
"The turquoise which was brought by the foreigners...".

These sentences will be generated by the application of the rules of the grammar. The "interpretive" model of Chomsky requires that an abstract syntactic structure first be derived by applying in order the rules of the base component to yield a kernel sentence. This kernel sentence structure will then have applied to it the rules of the transformational component of the grammar, which will convert the structure of an active sentence to a passive one, or to a relative clause in the cases under consideration. The resulting structures will then be eligible to receive lexical items and morphemes, inserted in the lexical component of the grammar. A final stage will consist of sound changes in the phonological component.

According to this model the first rule of the base component may be expressed as the following tree structures (NP = noun phrase; VP = verb phrase; TNS = tense).

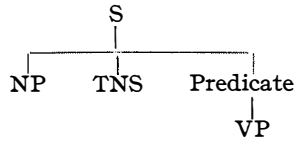
(5) S → NP TNS Predicate

Since, however, a predicate in Egyptian may be verbal, nominal or adjectival, a further sub-categorization rule is necessary:

(6) Predicate → $\begin{Bmatrix} \text{VP} \\ \text{NP} \\ \text{ADJ} \end{Bmatrix}$

Since our structures have verbal predicates, applying (6) to (5) will yield a tree structure (7):

(7)

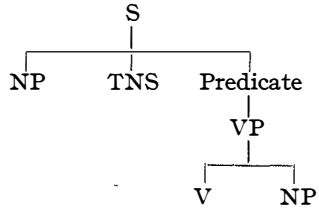


Our structures further require that the VP be transitive, so there must be rules (8) and (9) to yield us a tree (10):

(8) $VP \rightarrow \begin{Bmatrix} VP_{intr.} \\ VP_{tr} \end{Bmatrix}$

(9) $VP_{tr} \rightarrow V\ NP$

(10)

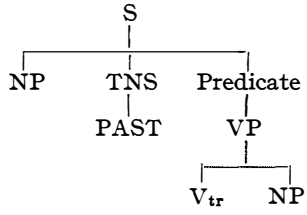


Now the tense node must be sub-categorized by a rule of the type (11):

(11) $TNS \rightarrow \begin{Bmatrix} \text{Present} \\ \text{Past} \\ \text{Future etc.} \end{Bmatrix}$

Choosing the past tense will yield us a tree structure (12):

(12)



Structure (12) represents the kernel sentence for all the sentences we wish to derive at this point. In order to convert this structure into those suitable for passive sentences and relative clauses, we need to apply rules of the transformational component:

(13) Passivization: (with optional agent phrase)
$NP_1\ PAST\ V_{tr}\ NP_2 \rightarrow iw\ V\ w\ NP_2\ (in\ NP_1)$

(14) Relativization:
$NP_1\ PAST\ V_{tr}\ NP_2 \rightarrow NP_2\ V_{tr} \begin{Bmatrix} n \\ in \end{Bmatrix} NP_1$

The structures resulting from (13) and (14) together with the kernel structure (12) are now eligible for lexical insertion. Rhe rules of the lexical component will have the form of the following:

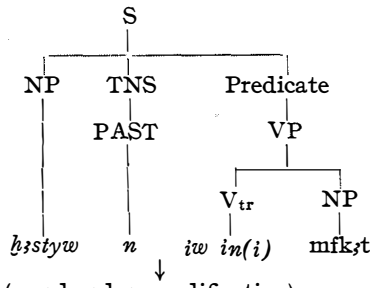
(15) $NP \rightarrow \begin{Bmatrix} mfkȝt \\ ḥȝstyw \text{ etc.} \end{Bmatrix}$

(16) $V_{tr} \rightarrow \{in(i) \text{ etc.}\}$

(17) $PAST \rightarrow iw\ \ldots n$

When these are inserted we then have the terminal strings for (1)-(4);

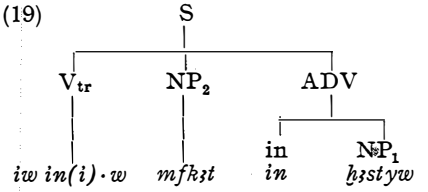
(18)



(word order modification)

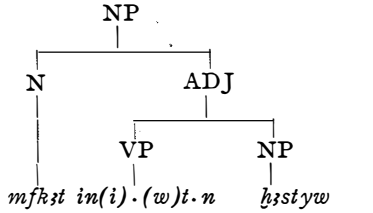
iw in(i)·n ḥ3styw mfk3t "the foreigners have brought the turquoise"

(19)



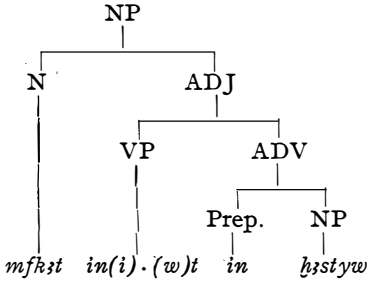
"The turquoise has been brought by the foreigners".

(20)



"The turquoise which the foreigners have brought".

(21)



"The turquoise which has been brought by the foreigners".

VII. Appendix Two: The Generative Semantics Model

This model, represented in Filmore's work (see note 45) would turn the interpretive approach upside-down and begin with the lexicon rather than end with it. In the lexical component of the grammar each verb would be provided with a case frame, such as (22):

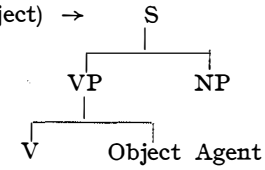
(22) *ini* (Agent) (Object)

The agent and object cases do not represent morphological cases but rather syntactic relationships between *ini* and other nouns in the sentence.

The resulting frame is now eligible for rules of the transformational component (25)-(28):

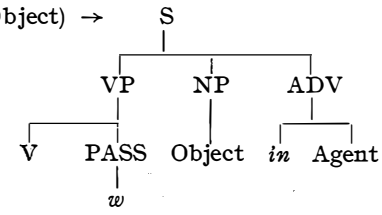
(25) Agent Subjectivalization:

—— (Agent) (Object) →



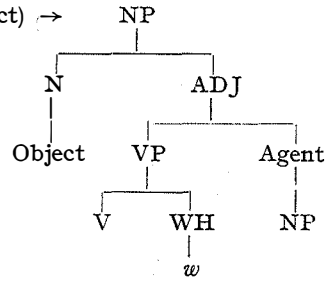
(26) Object Subjectivalization:

—— (Agent) (Object) →



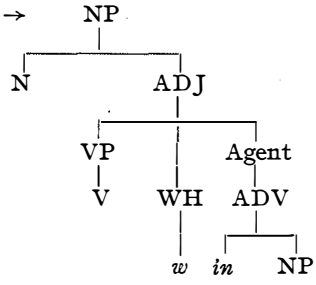
(27) Relative Clause Formation:

—— (Agent) (Object) →



(28) Relative Clause Formation:

—— (Agent) (Object) →



Further recursion to the lexicon will allow one to insert *in(i)* for the node V, *ḥ3styw* for Agent, and *mfḳ3t* for Object. Special transformational rules, which need not be gone into here, will be required to insure agreement of nouns and relative forms, to spell out irregular tense formations, and many other matters such as selectional restrictions.

Linguistics, Method, and Extinct Languages: The Case of Sumerian

Gene GRAGG – Chicago

At present researchers in areas which until recently had gotten along quite well, it seemed, in happy innocence of linguistics are increasingly confronted with articles of the genre "Linguistics and ..." or "Linguistic Approach to" their own field – a partial list would include philosophy, education, psychology, literary criticism, and, naturally, the various language disciplines. In attempting to do something of the sort for Sumerian, I am not proposing a complete treatise on the method and materials for a Sumerian Grammar, for which in any case modern linguistic theory plays only one role among many [1]. Nor can I give a complete exposition and justification of transformational linguistics itself. What I intend here is simply: (a) to note that there is, even in Ancient Near Eastern circles, an awareness of and curiosity about recent developments in linguistics; and (b) to give some background for and raise some questions of principle involved in the claim that Ancient Near Eastern language studies could profit from these developments. Note that I cannot even propose to justify this claim, for there is only one real justification, namely a body of work which accepts the claim and achieves results superior to what would have been possible without it. Finally, it is especially appropriate that an article which raises methodological questions should appear in a tribute to Professor Gelb, who has never ceased to impress on students and colleagues the importance of asking "What do you really know?" and "How do you know it?"

The specific language envisaged here is Sumerian, but I believe the principles involved are of sufficient generality to be of interest to researchers in other areas (hence I beg the indulgence of Sumerologists for the elementary, and for their purposes over-explicit character of some of the explanations of points of Sumerian grammar). Sumerian is an especially appropriate context for such an enterprise in as far as it is one of the Ancient Near Eastern languages where, apart from the texts themselves, there is no concrete line of evidence for grammatical statements such as is available when the language belongs to a more-or-

[1] If apology be needed, may the author excuse himself on the grounds that he has treated some of the other components elsewhere, and, having delivered himself of his programmatic visions, will proceed with the project for which the present article is a kind of methodological prolegomenon: within the context of the Sumerian Dictionary Project, a complete (or at least representative) grammatical coverage of the language of the Sumerian literary texts. For the moment, taking a cue from questions, apprehensions, and misapprehensions of students and colleagues, I am trying to give some kind of rationale for something between wholesale rejection and blind endorsement of the relevance of recent developments in linguistics for the study of Ancient Near Eastern languages.

less well-known language family, or has been preserved by an unbroken line of individuals who have known the language up to the time it became the object of systematic study. That is, in these languages one is more directly thrown on explicit or implicit linguistic theory for the concepts in terms of which the language is to be grammatically interpreted. In the present paper, after a brief sketch of the theoretical background (1), I will discuss the notion of method in its relation to linguistics (2.1) and to Sumerian (2.2), and finally project some of the properties and consequences of a transformational model of Sumerian (3) with a few examples (4).

1. Whose Linguistics?

Two questions immediately arise in our present context: "Do we need *any* linguistics?" and, in view of the apparent welter of claims and counterclaims in linguistics, "If so, whose?" It should be remarked first that the actual proliferation, though very real, is less radical than appearances and rhetoric might indicate, and that there is a mainstream of linguistics marked by common origins, common presuppositions, and converging developments [2]. In the second place it is not the case that cuneiform studies have never been contaminated by presuppositions concerning language and linguistic theory. On a trivial level, any grammar which describes a language in terms of "noun", "verb", "adjective", etc. is already availing itself of a theory of language, even if a very old, general, and widely accepted one. But more to the point, it should be kept in mind that cuneiform studies came to maturity at the end of the last century in the aftermath of that great "shaking of the foundations" commonly referred to by the nickname of one identifiable subgroup, as "the Neo-grammarian" movement [3]. By the beginning of this century many of the once revolutionary

[2] Not that there is a smooth continuum of development — rather a dialectic of action and reaction in the course of which one generation often revives what a previous generation regarded as uninteresting and a generation before that crucial. Thus Chomsky's claim that he is continuing insights developed by the grammarians of Port Royal, but left fallow by the positivist linguistics of most of the 19th and the first half of the 20th centuries. Moreover, with speeded up communications, the relevant time period is often less than a generation! The structuralist phoneme, which was regarded as the keystone and immutable center of linguistics before 1960, had been officially pronounced dead by 1964 (N. Chomsky, *Current Issues in Linguistic Theory* [The Hague]), but was beginning to show new signs of life, in a more modest reincarnation, by 1970 (S. Schane, "The Phoneme Revisited", *Language* 47 [1971] 503-21).

It should be kept in mind that, perhaps because of the comparatively well-delimited nature of its subject matter (grammatical behavior) and the relatively small number of people professionally engaged in it, transitions in linguistic theory tend to be abrupt, and polemics black-white. Cf. the almost Jacobin fervor of some transformational Streitschriften of the 1960's which echo something of the " Ça ira " of the Junggrammatiker in the 1870's — hang the representatives of the old order, a new era begins!

[3] Some ten years afterward H. Paul wrote: "Trotz dieser Stellung, welche die Sprachwissenschaft schon seit ihrer Begründung einnahm, gehörte noch viel dazu ihre Methode allmählich bis zu demjeniger Grade der Vollkommenheit auszubilden dessen sie fähig ist. Besonders seit dem Ende der siebenziger Jahre suchte sich eine Richtung Bahn zu brechen, die auf eine tiefgreifende Umgestaltung der Methode hindrängte. Bei dem Streite, der sich darüber entspann, trat deutlich zu Tage, wie gross noch bei vielen Sprachforschern die Unklarheit

theses of Brugmann *et al.* had been debated, qualified, reformulated, gathered up in compendia such as that of H. Paul, and had become the common property of the philological tradition. By this time the mainstream of cuneiform studies could already look back on an earlier period of relative naïveté, when pioneer cuneiformists held beliefs about the organization of language, the relationship between languages, the ways in which languages change, etc. which had been since revealed to be erroneous. Cuneiform studies *had* already committed itself, at least negatively, to a theory of language, and had profited by the results.

Since that time however linguistics has gone through two additional revolutions, both of which have left cuneiform studies relatively untouched. The first was not so much a revolution, as an organic development of the neo-grammarian movement. It arose out of reflection on, and refinement of neo-grammarian theses, and gave rise to two parallel developments. In Europe the teaching and posthumous writings of the most brilliant of the "neo-grammarians" of the 1870's, F. de Saussure [4], influenced, directly or indirectly, centers of "structuralist" linguistics in Geneva [5], Paris [6], Copenhagen [7], (less directly) London [8], and — especially important because of its role as a bridge between the first and second "revolution" — Prague [9]. A dominant concern of these centers

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über die Elemente ihrer Wissenschaft war". (*Prinzipien der Sprachgeschichte* [Halle, 1880], pp. 5 f.).

An exact chronicle of linguistic development in this crucial period (as opposed to catalogue-like compilations such as H. Pedersen's *Linguistic Science in the 19th Century* [Eng. trans., Harvard, 1931]) has yet to be written, and probably never will be. Consequently it is hard to be precise about the use of the term "movement" in this context. In any case by "neo-grammarian" (in quotation marks) I do not mean an organized, self-conscious movement (although for a while something like that existed around Leipzig) but rather a loose fraternity of intellectual fellow travelers.

[4] This is not to deny that there are other sources for structuralist thought and other types of relation to the neogrammarians. Cf. for example, E. Stankiewicz (ed.), *A Baudoin de Courtenay Anthology: The Beginnings of Structural Linguistics* (Indiana Univ., 1972). De Saussure's lectures were edited by his students, C. Bally and A. Sechehaye, and published posthumously as *Cours de linguistique générale* (Paris, 1915). On the extent to which this represents de Saussure's thought, cf. R. Godel, *Les sources manuscrites du Cours de linguistique générale* (Geneva, 1957).

[5] Represented in the first place by the students of de Saussure, mentioned above, and by their student, H. Frei. Cf. R. Godel, *A Geneva School Reader in Linguistics* (Indiana Univ., 1969). Cuneiformists will be interested to note that Frei was a director of E. Sollberger's thesis, *Le système verbal dans les inscriptions "royales" présargoniques de Lagas* (Geneva, 1952).

[6] Outstanding names are A. Meillet, E. Benveniste M. Cohen, A. Martinet. Cf. a convenient overview from a Romance linguistics point of view in I. Jordan and J. Orr, *An Introduction to Romance Linguistics* (Berkeley, 1970²) pp. 279-382 and Epilogue.

[7] L. Hjelmsljev and the "glossematic" approach associated with his name. Cf. his own, *Prolegomena to a Theory of Language* (rev. Eng. trans., Univ. of Wisconsin, 1961). Also, B. Siertsema, *A Study of Glossematics* (The Hague, 1965²).

[8] Note that the London school, roughly J. R. Firth and his students in the 30's, 40's, and 50's, relied also on an independent English tradition stemming from such pioneers as the phonetician H. Sweet. Cf. J. R. Firth, *Papers in Linguistics 1934-1951* (Oxford, 1957).

[9] J. Vachek, *A Prague School Reader in Linguistics* (Indiana Univ., 1964); *idem, The Linguistic School of Prague: An Introduction to its Theory and Practice* (Indiana Univ. 1966).

was synchronic structure (system or network of relations between linguistic elements), for the most part of the better known languages. Similarly in America systematization of neo-grammarian principles and their application to the description of "exotic" (for the most part American Indian) languages led to the growth of a descriptivist tradition whose best known early representatives were E. Sapir and L. Bloomfield [10]. The American school shared the structuralist concerns of the European, but its distinctive preoccupation was the elaboration of linguistic methodology, of descriptive techniques for hitherto unstudied languages — and occasionally the revision in the light of these techniques of traditionally accepted analyses in languages already described.

The second revolution can be dated from 1957, when Noam Chomsky published a small book called *Syntactic Structures*. While sharing the structuralist conviction that language is to be treated as an integrated synchronic system, he made three capital points [11]:

> 1) The goal of linguistics, as of any science, is not primarily the elaboration of methodologies for describing data, but of a theory to explain them.
>
> 2) Language is rule-governed behavior, and the proper object of linguistic study is the set of rules which govern the production of sentences, rather than static structures defined by the relations between linguistic elements. A grammar of a language is then a set of rules; linguistic theory is the theory of the possible form, content, and relations of these rules.
>
> 3) The most important (or interesting, or graspable) type of rule is that by which more complex sentence types (sentences with subordinate clauses, conjoined sentences, marked sentence types) are derived from one or more logically simple propositions corresponding roughly to simple unmarked declarative sentences [12]. These rules, which use

[10] Their most well-known works are both entitled *Language*, Sapir's appearing in 1921, Bloomfield's in 1933. A convenient collection of American structuralist articles up to 1957 can be found in M. Joos (ed.), *Readings in Linguistics* (New York, 1958). The question of the extent of de Saussure's influence on American descriptivism is problematic. He was certainly read and appreciated. However it seems that many of the most typical traits of American linguistics were already established before de Saussure was available on this side of the Atlantic.

[11] The broader aspects of Chomsky's radical critique are not treated here (cf. J. Lyons, *Noam Chomsky* [New York, 1970]; N. Chomsky, *Language and Mind* [New York, 1968]). Note that much of it is not concerned with specific questions of linguistics but with the philosophical and psychological presuppositions which, in Chomsky's interpretation, have governed language research (and research into man and society generally) since the mid-19th century. In any case Chomsky has not annihilated 150 years of linguistics, nor has he claimed to, even if he has shown basic deficiencies in 150 years of the study of man. Note in passing that Chomsky's reading of intellectual history has not gone uncontested in linguistic publications; cf. H. Aarsleff, "The History of Linguistics and Professor Chomsky", *Language* 46 (1970) 570-585.

[12] Consider, for example, the sentences:

(1) To understand Chomsky is difficult.
(2) It is difficult to understand Chomsky.
(3) Chomsky is difficult to understand.

The sentences are semantically very close, even if not synonymous. (1) is a proposition about a feat of understanding, while (3) is a proposition about

combinations of simple operations such as deletion, adjunction, sub-

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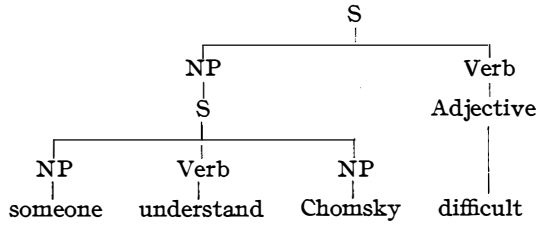
Chomsky. But all involve, among other things, an underlying syntactic structure corresponding to:

(4)



(S = Sentence; NP = Noun Phrase)

in which the proposition, "Someone understand Chomsky", functions as subject of the simple copulative sentence, "NP (is) difficult". From (4) is derived: (1) by insertion of the subordinate clause marker "(for) ... to", deletion of the indefinite subject of the infinitive (compare "For me to understand Chomsky is difficult"), insertion of "be", and concord with a 3 sing. subject; (2) by extraposition of the subordinate clause in (1) to the end of the sentence and substitution of a "dummy subject", "it"; (3) by raising the object of the subordinate clause of (2) into subject position of the main clause (a transformation, sometimes called Tough Movement, which is possible in English with a class of abstract predicate adjectives such as "tough, hard, difficult, easy etc.").

Each of these rules (Subordinate Marker Insertion, Indefinite Subject Deletion, BE-Insertion, Extraposition, Tough Movement, Concord) consists of two parts: one which describes the syntatic structure to which it can or must apply, one which gives the transformed syntactic structure. The derivation of a sentence is thus a series of syntactic structures the first of which represents the "basic form" (in some sense, cf. footnote 13) of the proposition, and the last its actually enunciated syntactic form. Transformational rules define possible sequences of syntactic structures in a derivation. The point is that all these rules (a) can be formulated quite generally in terms of well defined syntactic and semantic properties of the underlying sentence, (b) are widely used in other, unrelated structures in English, and (c) correspond to processes which are known to occur in a wide range of the world's languages. The rather strong claim being made by Chomsky is that the speaker/hearer understands 1-3 in terms of 4 and that the process of producing/understanding 1-3 is informed by these principles rather than by simple association of patterns with a unit-meaning. These principles are sometimes equated by him to the "innere Sprachform" of von Humboldt or the "faculté linguistique" of de Saussure. More psychologically agnostic interpretations are sometimes adopted, but there is general agreement that what is to be projected as the grammar of a language is an ordered system of rules which, for each of the indefinitely many sentences in the language, can account for the way in which its underlying structure is generated and then transformed into the concrete occurring syntactic structure.

The above sketch is schematic in the extreme, and for further details I must refer the reader to Chomsky himself (his last technical statement of his position, *Aspects of the Theory of Syntax* (M.I.T., 1965), is basic, but unfortunately out of date; a less formal exposition is given in the work cited in footnote 11), introductory manuals (these have a very short life-span: one of the more complete is still N. Ruwet, *Introduction à la grammaire générative* [Paris, 1968]; for transformational formalism with an abundance of illustration cf. M. Burt, *From Deep to Surface Structure: An Introduction to Transformational Syntax* [New York, 1971]), and the many collections of articles showing recent developments (for earlier and background articles, J. Katz and J. Fodor [eds.] *Structure of Language* [Englewood Cliffs, N. J., 1964]; for contemporary trends, especially R. Jacobs and P. Rosenbaum, *Readings in English Transformational Grammar* [Waltham, 1970], E. Bach and R. Harms, *Universals in Linguistic*

stitution, movement, are called "transformational rules", hence the name "transformational linguistics" [13].

Transformational linguistics has undergone profound changes (and splits) in the fifteen years since 1957, and there is every likelihood that the next decade or so will see further changes at least as profound, in all probability culminating in an orientation different enough to be called by a new name. But in the meantime, whatever the status of individual points claimed by Chomsky (the Passive Transformation in English, used as an elementary illustration of a transformational rule in 1957, is still considered an unsolved problem in 1972) [14], he has provided a whole generation of linguists with a handle on syntax [15]. In this

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Theory [New York, 1968], M. Bierwisch and K. Heidolph, *Progress in Linguistics*: *A Collection of Papers* [The Hague, 1970]; D. Steinberg and L. Jakobovits, *Semantics*: *An Interdisciplinary Reader* [Cambridge, 1971]; a convenient source for "news from the front" is *Papers from the Regional Meetings of the Chicago Linguistic Society*, published annually at the University of Chicago since 1968).

[13] The rules governing the formation of the logically simple propositions are sometimes called "generative" rules, hence the name "generative grammar". These rules, and hence the term, are much less prominent in more recent literature.

It should be kept in mind that the exposition given above is somewhat updated over 1957, and considerably streamlined — almost dangerously so. Note especially that there is at present a heated controversy over the status of the "logically simple propositions". One tendency, called "generative semantics" (cf. G. Lakoff, "Generative Semantics", in Steinberg and Jakobovits, pp. 232-96), holds that there is no autonomous syntactic level distinct from the semantic, and that the underlying structure contains in principle a complete logical — the "natural logic" of ordinary speech, as opposed to the special logics of various types of scientific discourse — representation of the sentence to be explicated. This tendency would claim, for example, that an apparently simple declarative sentence such as "Floyd broke the glass" is actually complex and contains more than a half-dozen simple propositions (cf. J. Ross, "On Declarative Sentences", in Jacobs and Rosenbaum, pp. 222-72). The other tendency, currently held by Chomsky, would take sentences like the one quoted as syntactically simple, and would claim that there exists an autonomous syntactic level of representation (called "deep structure"), distinct from the semantic.

[14] Cf. R. Lakoff, "Passive Resistance", *Papers from the Seventh Regional Meeting of the Chicago Linguistic Society* (Chicago, 1971), 149-62.

[15] This is not to give the impression that phonology, much more intensively developed than syntax by structural linguists, has not undergone profound changes in the context of transformational grammar. It is here that the influence of the Prague school has been especially important, arising in part from the fact that Chomsky's close collaborator for the past fifteen years has been Morris Halle, a student of Roman Jakobson, one of the founders of the Prague school. Cf. Chomsky and Halle's major work, *Sound Pattern of English* (New York, 1968). A branch of linguistics which *has* become something of a stepchild in transformational linguistics is morphology. It is sometimes claimed as a part of phonology, sometimes as a kind of bridge component between syntax and phonology. At times traditionally morphological generalizations are made either in the context of syntax or phonology, but for the most part the area is ignored. There are some attempts to treat pieces of complex inflectional morphology (cf. characteristically S. Schane, *French Phonology and Morphology* [M.I.T., 1968]), but they fail to pose the question in principle of the synchronic organization of inflectional paradigms and simply extend the kind of analysis used for simple phonological alternation. The results are, in part, synchronic recapitulations of established results of historical phonology (as

way he has inspired a concerted research effort in syntax (and phonology) of unprecedented scope. There are few younger linguists in America who are not occupied in one way or another with issues and concepts arising out of transformational linguistics; and indications from across the Atlantic are that the effort is gradually becoming international [16].

It is in this fluid context then that we are discussing linguistics, hopefully not in an attitude of dogmatic adherence to a school among others, but of willingness to learn from the past as well as the present, from the mistakes and exaggerations as well as the achievements of linguistics. It would of course be an illusion for an Ancient Near Eastern language specialist to think of himself as floating in an Olympian, atemporal, universal viewpoint from which, by virtue of a special insight not granted to linguists themselves, he can discern the true from the false in all positions while committing himself to none. The viewpoint has to be the best he can achieve at a given time and place, relying on his own insight and critical judgment, but also on that of the better (and nearer!) of his contemporaries. In what follows then the viewpoint is that of transformational linguistics and its precedents as perceived by an Ancient Near Eastern scholar in a Midwestern American University in the 1970's.

2. How to Write a Grammar

Method has been something of a magic word in the modern era. In a broad perspective "scientific method" in basic to our whole approach to the systematic understanding of man and the physical universe. More concretely the term conjures up such paradigms of a body of techniques as "statistical method" with independent, internal criteria for significance, and applicability to wide ranges of data. And in almost every field refinements and specifications of "scientific method" have led to special methodologies or projects for methodologies (cf. the recurrent quest for "the method" of literary criticism). For our own purposes two concrete questions arise: "In what sense is there a linguistic method?" "What is the position of a language such as Sumerian vis-à-vis this 'method'?"

2.1 *Linguistics and Method.* Linguistics is a child of its time and, not surprisingly, "method" in one sense or another, has been a recurrent theme in linguistic writings of the past hundred years [17]. "Method" as used in linguistics ranges between two semantic poles. "Method" in the sense of expressions like "field methods" is intimately bound up with the concrete conditions under which data are gathered. Field methods range from rules-of-thumb for good, orderly working habits to more or less ingenious techniques which have been found useful in eliciting or controlling phonological or syntactic information

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one would expect, given the approach), but in part simply extrapolations from the phonology of the given language and from phonological theory generally in a manner which is ingenious, but often, in its own way, just as arbitrary as the most fanciful creations of the structuralists.

[16] Recall Ruwet's book, mentioned in footnote 12. Notice also the frequency of transformational articles in journals and series such as *Journal of Linguistics* (England) *Studia Grammatica* (D.D.R.), *Voprosy Jazykoznanie* (U.S.S.R.), *Langages* (*France*), etc.

[17] Cf. the Paul citation in footnote 3.

from (often bewildered, recalcitrant, or bored) native speakers [18]. While some of these techniques might be suggestive for individuals working with dead languages, "method" in this sense is for the most part either not relevant, or exactly parallel to long acknowledged canons of philological method. More to the point is "method" as used in expressions like "the comparative method" [19].

The comparative method stood as the archetypal linguistic method, and probably influenced directly or indirectly subsequent attempts to codify linguistic method, attempts which reached their peak in America in the 40's and 50's. What is important to realize about the comparative method however is that it is not itself an empirical discovery, reached by trial and error experimentation, but rather a series of practical deductions from a few substantive hypotheses about language and language change, which by the 1870's had become part and parcel of the intellectual baggage of historical linguists (that sound change is regular and does not admit wholesale, arbitrary exceptions; that the effects of sound change can be masked by borrowing and analogy; implicit, but most important, that language is an organized whole, etc.). The point is that method here consists of practical, common sense (at least common sense, given a general adherence to scientific method) conclusions from an empirical theory, and is valid or interesting only in proportion to the theory's intrinsic value.

This point has not always been fully appreciated in linguistics. During the period when "method" was very much in the foreground of linguistic discussion, there was a great deal of interest in the formulation of a theory, a set of presuppositions or postulates which would ground a method for linguistics as a whole [20]. It is now generally conceded that what had been arrived at as "methods in linguistics" were distillations of a theory, of sets of presuppositions which were too general and impoverished, not specifically linguistic enough, to bear the descriptive burden which was thrust upon them. At present therefore there is a widely observed moratorium on the use of the term "method", pending more solid results in a substantive theory of language.

It remains none the less true that transformational grammar defines in

[18] Cf. W. Samarin, *Field Linguistics: A Guide to Linguistic Fieldwork* (New York, 1967).

[19] Here a successive application of (a) a series of procedures leads unfailingly to (b) a conditionally guaranteed result, as in:

a) 1) line up cognates in languages A and B;
2) discount effects of analogy and borrowing;
3) identify corresponding segments;
4) observe complementary and contrastive distributions;
5) label.

b) The final inventory represents the phonological segments of proto-A/B to the extent they are accessible to the comparative method. (For "structuralist" codification cf. A. Meillet, *La méthode comparative en linguistique historiques* [Oslo, 1925]; H. Hoenigswald, *Language Change and Linguistic Reconstruction* [Chicago, 1960]).

[20] What was envisaged was the determination of a sequence of operations, for the most part operations of segmentation and classification, such that only results which *in principle* (the fact that in practice one took intuitive shortcuts was always allowed for) could be arrived at by this sequence of operations counted as valid, "scientific", or linguistically significant. Cf. Z. Harris, *Methods in Structural Linguistics* (Chicago, 1951). That such preoccupations were not limited to America can be seen for example from K. Togeby, *Structure immanente de la langue française* (Copenhagen, 1951).

a certain sense a way of doing linguistics, and to this extent a "method". It defines, tentatively to be sure, a way in which language is organized, and a set of categories and processes in terms of which this organization can be described. We must now ask to what extent transformational grammar defines a method for the grammatical investigation of an extinct language such as Sumerian.

2.2 *Some Limiting Conditions*. From what has been said, if transformational grammar makes some true statements about language and is thus relevant to the grammatical investigation of Sumerian, it should be clear that it does not provide a set of "how-to-do-it" techniques for the construction of the grammar of Sumerian, or of any other language. Its relevance, and the limit of its relevance, I will try to sketch out in five points.

1) It should be obvious that in this domain, the most beautiful method or well-constructed theory will yield interesting results only in proportion to the insight and intelligence that inform the data presentation. There is no magic methodological wand which will transform misunderstood, garbled, or trivial data into a genuine contribution to knowledge[21]. The most general constraint on a grammar of Sumerian is that it will be no better than our understanding (with all this implies of unprogrammable, intuitive grasp) of the only data we have, the texts. The point of departure is not a text to be understood, but text with interpretation (hence edited and translated, the first step in a grammar of the Sumerian literary texts).

2) This being said, it should be kept in mind that a theory and its accompanying heuristics provide not only a way of answering questions, but also, and even more so, of asking questions. Often enough a new theory does not so much provide miraculous answers to old unsolvable problems, as a new set of questions, or a better formulation of old questions, answers to which can be found in the old data. The birth of a new theory is followed by a shakedown period when the formal apparatus is used to recast and paraphrase old truths. Then research branches out into the area in which the theory best answers questions (cf. the decline of what little morphology appeared in early transformational grammars). In some cases a new theory will show that an old quest is invalid or uninteresting (cf. efforts to find operational definitions of grammatical categories). In this context therefore what is to be expected as a result of linguistic research in individual languages such as Sumerian, is not so much that all the old question marks will be cleared up (though there should be a certain amount of that), as that new aspects of the texts will be accounted for, those aspects, namely, which are more syntactical in character. In the process it is to be expected or hoped that data which could be only partially elucidated from a point of view which was heavily morphological (recall how comparatively little difference there is between the first volume of Falkenstein's *Grammatik der Sprache Gudeas von Lagaš*, subtitled "Formlehre", and the second, subtitled "Syntax") acquire a new explanatory perspective. Here one could think of sections of Sumerian grammars devoted to the "non-finite forms of the verb" (where what is really at stake is the syntax of subordinate clauses), to certain modal and subordinating prefixes of the verb, etc.

3) However one has to take into account a radical difference in point of view between a theoretical linguist and a specialist in a particular language.

[21] Compare the expressive acronymic used in computer programming: GIGO — Garbage In, Garbage Out.

(In this section we will indulge in the fiction that they are two separate persons, although in reality one person frequently wears both hats.) From the point of view of the former, the center of attention is that aspect or those aspects of the theory which happen to be arousing a certain amount of interest at a given time; and elaboration of the theory progresses according to a dialectic of its own. A specialist in a given language on the other hand (especially a language like Sumerian, where one of the main tasks is to characterize the principal mechanisms lying behind the language as a whole) is in some sense responsible for all the grammatical phenomena of a language. To the extent that the paths of the linguist and the language specialist cross, they mutually benefit, otherwise the language specialist has no choice but to proceed into theoretically unexplored territory. For example no one today would claim to know completely how to formulate the passive transformation (or rather, how to represent the underlying structure of passive sentences as opposed to the corresponding active version; whether all passive sentences differ from the corresponding active in the same way, etc.). This fact however does not excuse the language specialist from saying on the one hand what is known about the syntax of the passive (assuming there is one) in his language in the appropriate context. On the other hand this situation does not justify him in "faking" an ad hoc passive transformation where he has nothing essentially new to add to the state of the question. What all this amounts to is the claim that, to the extent a language specialist, for example a Sumerologist, needs to account for the data of a language as a whole, he cannot yet hope to cast the whole of his grammar in the ideal form projected for grammars by transformational theory, for the necessary groundwork has not been done. The price of doing this is to end up with something which bears only a superficial resemblance to what a transformational grammar should be. There are already too many works bearing the title "Transformational Syntax of X" (where X is usually an "exotic" language) which are not a real contribution to or innovation in syntactic theory, and where genuine insights into substantive syntactic processes of the language in question are buried in a forest of ad hoc, private symbols and formalisms, accesible without decipherment only to the author. These grammars work, to be sure, in the sense of grinding out the correct constructions when the crank is turned, but they fail to relate the processes in question to what is known or hypothesized about language generally. Ancient Near Eastern languages will hopefully not contribute to this genre. In short, especially considering the kind of data such grammars work with, care should be taken not to gloss over unexplored areas with superficial extrapolations.

4) The restriction just noted is reinforced when one considers the actual rate of obsolescence of many theses and terms in linguistics generally, and in transformational linguistics in particular, where mimeograph, xerox, and oral communication have to a certain extent replaced journal articles and books as the locus of scholarly give and take. Here again one can chronicle cases of grammatical works which enthusiastically updated themselves with notions such as "kernel sentences", "generalized transformations", (and now perhaps?) "deep structure", only to come out looking as quaint as a treatise on phlogistons when published in the wake of a major or minor shift of course in the transformational mainstream. The lesson to be drawn from this is not to presuppose the relevance of a term without regard for the data, not to use terminology needlessly, to follow terminological innovation at a safe distance. In other words, to relativize the terms against their content, to grasp the function of

a given term both in terms of a general theory of language and the particular goal of a specific grammar. There is a sense in which the notion of "kernel sentences" (a class of sentence–types from which other sentence–types are derived), prominent in Chomsky's early work, was valid, and a grammar which genuinely grasped what the function of kernel sentences was, made this explicit, (and avoided overuse of the term) could stand as a valuable linguistic contribution even after a better term or more adequate concept had replaced an earlier attempt.

5) A final motivation for caution is the contrast between the type of data relevant for transformational grammar and the type of data typically available in languages like Sumerian. For arguments in transformational grammar frequently rely on subtle, intuition-based (and often hotly contested) judgments of synonymity, paraphrase, etc. In languages like Sumerian in many cases either the relevant sentences are not attested, or if attested their exact interpretation is not known. This means that a complete transformational grammar, which can only be projected at present for any language, in Sumerian is in principle beyond our grasp forever. We are not dealing with a living language, but only the skeleton of a language, which we try to flesh out to the best of our ability by extrapolation and analogy, but always with a great measure of caution and reserve. This also means that apart from explicit surface phenomena, or broad typological considerations, languages like Sumerian are not going to be much help in establishing or supporting details of linguistic theory.

One should not exaggerate, however. We do understand some things. If not we would never dare translate a text. If our grammar can never be any better than our understanding of the texts, it should at least be nearly as good [22]. Moreover the theory itself can provide confirmatory evidence, to the extent that it is shown that certain properties hold true of grammars generally or that certain options or configurations follow more or less fixed patterns in a large number of languages. Finally we should keep in mind that the Sumerian (or whatever) specialist is not *in principle* in a situation which is qualitatively different from the situation of an English (French, Japanese) specialist. Neither has any direct access to the primary data. Neither can look into the minds of his informant (who may be himself) to see the mental data which constitutes language behavior (let us call them intuitions). The modern language researcher of course has much easier indirect access (he can elicit any possible sentence in the language, and he can be directly informed that what he thinks might be a good sentence is in fact a terrible one). But for both some intuitions are harder to trap than others, and some seem almost permanently elusive (recall, again, the passive). The night and day difference of course is that the researcher in an extinct language runs into the elusive intuitions much earlier in the game.

3. Towards a Model of Sumerian

What has just been suggested in relatively negative terms can be approached more positively, I believe, in terms of the notion of "model" in the

[22] Cf. R. Lakoff, *Abstract Syntax and Latin Complementation* (M.I.T., 1969), a contribution to a general theory of subordinate clauses based on a study of the Latin subjunctive; on the possibility of using a dead language for an investigation of this kind cf. review by G. Greene in *Language* 46 (1970).

sense of an intellectual construct which reproduces or accounts for the intelligible relations between the components of a phenomenon, and correctly predicts behavior not directly programmed into it. (This as opposed to "model" in the sense of a facsimile, a possibly rescaled, but more or less exact picture.) Note that transformational grammar is already a model for a language in the first sense; for it has never been claimed that there is a one-to-one correspondence between whatever mental and neurophysiological goings on are involved in the production and understanding of sentences, and the rules and derivational steps in terms of which a transformational grammar accounts for them. Moreover not only is a transformational grammar a model in this sense, it is also at present and for the foreseeable future an avowedly incomplete, open-ended, and revisable model; it should be pictured as circling in on the intelligibility of language behavior, rather than having it firmly in its grasp. In seeking to make use of transformational linguistics in the elucidation of Sumerian, then, we are presented with a model which already has its own built in "more/less". In formulating his findings the Sumerologist will be presenting a model which has gaps, ragged edges, and question marks — just like any transformational grammar — only more so.

Research naturally proceeds by bits and pieces, but if we were to project an ideal picture of the whole, what would it look like? From what we have said, the "model" we have been talking about obviously cannot stand alone as the total grammar. For in addition to the data which is explained by the model, there is data which is not explained, or only partially explained, or only questionably explained — and in fact a model of the type we are dealing with will probably have to coexist with concrete (and even major) details it does not account for in practically every sentence. The transformational model then can be presented only in the context of an orderly grammatical-philological presentation of all the available data. To put it the other way around, I am suggesting a philological-grammatical exposition oriented towards and informed by a linguistically responsible model — the model continually being revised in the light of the data (and developments in theory), and itself suggesting new questions and potential data. The model (perhaps periodically explicitated) forms then a kind of permanent core for the philology, a reconstruction, to the extent possible, of something of the "innere Sprachform" which shaped the once-living language. Around this core and related to it, to the extent possible, is the preserved data of the language, held together by the appropriate philological scaffolding.

A completely explanatory model is obviously a limiting concept, which can only be approximated with increasing adequacy as progress is made in linguistic theory and Sumerian grammar. The motivation for including such an enterprise in Sumerian grammatical investigation is to have an explicit expression, and hence control, of our understanding of Sumerian. Not all of the data can be explained by such a model, but this is one of the valuable results of attempting to construct it. For such data, far from being forced onto a Procrustean bed, should raise questions which either point out the way for fruitful investigation, or show the limits of possible knowledge by making explicit what would be crucial evidence for a statement, and then observing that this evidence is not contained in the texts we happen to possess, or, in the extreme case, not likely to be contained in any text we will ever possess.

As for the scaffolding, philological method is the subject for a different article, but in the present context I would like to point out that the nature

of the data available to the linguistic investigation of Sumerian makes it necessary to take into account two kinds of evidence, usually not considered in a transformationally orientated context. One is the use of statistical evidence for semantic and syntactic properties. This is important for areas in which an element or process is syntactically compatible with all or most other elements, so that one cannot simply determine its occurrence in terms of a given surface context, but where its semantic properties make it *more likely* to occur in one context than in another. Observation of contexts in which it predominates can sometimes give a valuable (and sometimes the only) clue to these properties. An example of this in Sumerian are the verbal prefixes, where tabulation of co-occurrences can show certain claims to be unlikely (e. g., that *im-ma-* functions as a locative prefix) and suggest some potentially illuminating correlations (e. g., correlations between dimensional prefixes and semantic and syntactic classes of verbs; correlations between classes of conjugation prefixes and the dative, mentioned below; statistical predominance of *ba-* prefixes in *-a-ba* clauses, etc.).

Another type of argument arises in the situation where enough is known about a group of constructions to grasp some of the core semantic and syntactic characteristics of its members, and their partial similarities and differences, but where for none of these constructions is one in a position to make a detailed hypothesis about the underlying form or the derivation involved. Even if one is convinced that the real organization of this group is in terms of underlying structures and rules, the closest one can come to presenting their form is in terms of the static relationships (opposition, intersection, inclusion, etc.) obtaining between the individual surface structures. In other words, one ends up with a type of exposition (and diagram) which resembles that used in structuralist works. (Cf. the author in "A Class of 'When' Clauses in Sumerian", to appear in *JNES* 32 [1973].)

4. For Example

What I have just said is extremely programmatic, and even a tentative "model" is far from being ready. But as an example of the implications of such a project, I will conclude with a sketch intended to illustrate the problems and perspectives involved in the first few steps of the construction of such a model. What will be at issue are some of the gross syntactic properties of the following sentences [23]:

1) a. *lugal* + *e* (*uru* + *a*) *é* *mu* + *n* + *dù*
king-agent city-in house pref-built

b. *é* *ba* + *dù*
The house was built.

c. *lugal* *é* *dù*
the king who built the house

[23] These examples have been lexically simplified for expository purposes, but are exactly parallel to very commonly attested Sumerian sentences. For example; all but the last type can be easily found in Gudea: (a) Cyl. A 8, 12; (b) Stat. B 4, 10; (c) Cyl. A 13, 10; (d) Cyl. A 2, 28; (e) Cyl. 4, 20; (f) Destruction of Agade 1 ff.

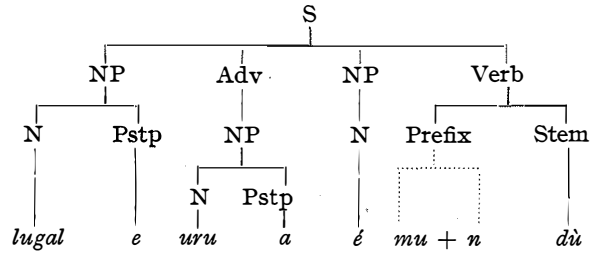
d. *é lugal + e dù + a*
the house which the king built

e. *dingir + e lugal + ra é dù + (e)d + a mu + na + n + dug*$_4$
The god (to) the king the house to-build said.

f. *lugal + e é mu + n + dù + a + ta*
After the king built the house

4.1 *Basic Word-Order Type*. A possible syntactic analysis of (1) would be to consider a-f as unanalyzed syntactic wholes — to be labeled and glossed as irreducible units. Obviously this would be wrong. Obviously there are common principles involved in the formation of all sentence and clause types given here. In particular all involve a basic sentence structure of a type most fully exemplified by (a). Disregarding questions if deeper semantic representation, we can represent this structure as:

(2)



(Where NP is Noun Phrase, Pstp Postposition, and the dotted lines indicate that there is no attempt made to explicitate here the structure of the verb-prefix system) [24]. In (1a) the Adv constituent, *uru + a*, does not have to be present (the parentheses indicate optional occurrence); otherwise (2), with different terminal lexical items and with no Adv or other kinds of Adv can provide a certain representation of any transitive sentence in the language. In other words we are making the traditional generalizations that a transitive sentence in Sumerian has the shape: NP (Adv) NP Verb, that Adv consists of a NP, that Verb consists of Prefix plus Stem, that NP consists of N plus Pstp (e, if NP is subject of a transitive verb, another Pstp if NP is one of the various Adv, zero if NP is the object of a verb) [25].

These are quite obvious generalizations (even though the formalism of the tree-diagram in (2) helps make the structure more explicit). But already at this level it has proved possible to make some interesting theoretical and

[24] It is probable that categories of tense, mood, focus, relation to speech situation, etc. encoded in the verb prefix are to be construed in the underlying representation as predicated of the sentence as a whole, and hence as a series of higher or co-ordinate sentences. Cf. footnote 13.

[25] Cast in terms of a generative system, syntactic structures such as (2) would be specified by the following set of rules:

(1) S → NP (Adv) NP Verb
(2) Adv → NP
(3) NP → N (Pstp)
(4) Verb → Prefix Stem

typological generalizations. We are stating that the basic transitive sentence type in Sumerian has the order S(ubject) O(bject) V(erb). Now it has been discovered that basic syntactic properties do not occur in arbitrary combinations, but that the presence of one implies the presence of some others [26].

a) Note first that of the six logical possibilities of ordering S, O, and V, only three seem to be basic in natural languages: SOV, SVO, VSO (i.e., the three where the subject precedes the object).

b) For reasons which have still to be clarified, SOV languages, such as Sumerian, are almost without exception postpositional [27].

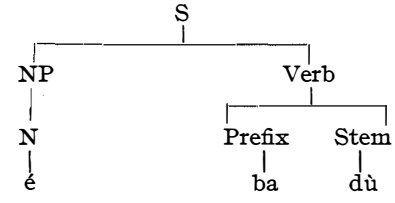
c) Interrogative words in these languages are almost never put first in interrogative word questions (whereas in VSO languages this is invariably the case) [28].

d) Conjunction reduction and "gapping" (sentences of the type: "John ordered meat and Bill fish") are almost always to the right in VSO and SVO languages (as in the sentences just cited), whereas they operate to the left in SOV languages ("John meat and Bill fish ordered") [29].

Other such "universals" are mentioned in the works cited in the last four foot notes. In any case it should be clear that already the first elementary step has profound typological implications, is not always as obvious as might at first appear, and predicts data for Sumerian which might already go beyond what is attested in our texts (cf. especially [d]).

4.2 *Ergative-Passive.* Next consider (1b). The structure of this sentence is:

(3)



I. e., identical to structure (2) without an agentive NP (the Adv optionally present in (2) could also occur here or in any of the following sentences). In other words, as opposed to the genuine passive construction (cf. most Indo-European and Semitic languages), where the object of a transitive verb is moved into subject position of a passive verb, a "passive" in Sumerian is characterized simply

[26] Cf. J. Greenberg, "Some Universals of Grammar with Particular Reference to the Order of Meaningful Elements", in J. Greenberg (ed.) *Universals of Language* (M.I.T., 1963).

[27] In Greenberg's sample, out of some 150 languages and language families, only five were SOV and prepositional. Of these five, three (Persian, Akkadian, Amharic) are demonstrably VSO or SVO languages (hence prepositional) which became SOV because of contact with SOV languages; a similar explanation might be possible for the other two. It has been argued recently by E. Bach, "Is Amharic a VSO Language?" *Journal of Ethiopian Studies* 8 (1970) 9-20, that there might be syntactic grounds for saying that the underlying word-order of Amharic is VSO, even though all actually produced sentences have SOV word order.

[28] Cf. E. Bach, "Questions", *Linguistic Inquiry* 2 (1971) 153-66.

[29] J. Ross, "Gapping and the Order of Constituents", in Bierwisch and Heidolph.

by the absence of a definite or explicit agent, and the logical object of the verb has the same shape (i. e., zero postposition) as in the corresponding active sentence. If we combine this fact together with the following two considerations:

1) Intransitive sentences have the same shape as (3), i. e., with subject corresponding to object of transitive sentences, and without an agentive.

2) First and second person object-agreement affixes on the transitive verb are identical to first and second person subject-agreement affixes on the intransitive-"passive" verb (the third person probably also fits this pattern, but the details are less straight-forward), we can conclude that the general formula for Sumerian sentences (transitive, "passive", and intransitive) is: (Agentive) NP (Adv) Verb (with optional Agentive, as opposed to non-ergative NP (Adv) (NP) Verb, with optional object); and we thus make yet another major typological claim about Sumerian, namely that it is, in one sense of the word, an "ergative language" [30]. In passing we note that Sumerian needs thus no passive transformation (a transitive verb without an agentive must be translated as passive into languages having that construction), and we are thus relieved of a crux other linguists have to live with.

4.3 *Focus.* Observe now that the verb prefix in (1b) has been changed to *ba-*, as opposed to (1a). This is perhaps statistically the most frequent prefix in this type of construction, but it could also be *mu-*, or even *mu-n* or *ba-n/b* [31]. And in any case *ba-* also occurs with explicit transitive sentences and is in no sense a "passive" prefix. This is certainly not the place to go into the still obscure and entangled area of the verb prefixes. But the hypothetical prefix alternation (not an automatic, obligatory alternation, to be sure) in (1a) versus (1b) illustrates a class of syntactic processes which are of capital importance in many areas of Sumerian, and which we will group together under the term "focusing processes". By this we mean that there is a class of underlying representations which indicate, in one way or another, that certain syntactic or semantic components of a basic proposition are to be foregrounded, and that there are a series of transformations which accomplish this. Much more work needs to be done in both theory and data before the underlying representations and transformations can be specified. Suffice it here to indicate some of the phenomena to be taken care of under this heading.

Ba- belongs to a class of prefixes, called conjugation prefixes after Benno Landsberger, which is obligatory with all finite verbs, and whose other members are *i-* and *mu-*. Without giving evidence for it, let me suggest that what is at stake in this class is focus or non-focus on the speech situation and its sphere, with *mu-* positively marked for focus, *ba-* negatively, and *i-* neutral. (Note that *mu-* is obligatory with a 1st person dative, overwhelmingly frequent with a 2nd, and very frequent with a 3rd; *i-* is infrequent with 2nd, frequent enough with 3rd; *ba-* is impossible with 2nd and infrequent with 3rd.) This focusing

[30] C. Fillmore, "The Case for Case", in Bach and Harms, pp. 51-60, points out that there is no simple ergative/non-ergative dichotomy, and that in natural languages various combinations of markings exist for the categories active/passive, active/stative, subject/object, and transitive/intransitive. In terms of generative rules, we would replace rule (1) in footnote 24 by a new rule (1') S → (Agent) NP (Adv) Verb.

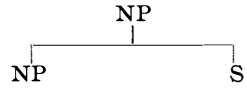
[31] The *-n-* and *-b-* are generally considered to be pronominal elements which refer to the subject/object of a transitive verb. On their occurrence in "passive" sentences, cf. the author in *JAOS* 92 (1972) 10 f.

works out in a number of subtle ways (cf. *ba-ug*$_6$ "he died", more frequent than *mu-ug*$_6$); and there are certainly nuances of usage involved in our texts that are forever beyond our grasp. In the case of the "passive" however it seems fairly clear that the distant, "impersonal", connotation implied by the absence of an explicit agent is optionally but frequently reinforced by the use of *ba-*, which focuses on the "over-there-ness" of the situation. Similarly the presence of subject/object markers in these constructions (there are cases of *ba-n-dù*, which in itself could mean "he built it", where the context supplies no agent to which a definite pronominal subject could refer) is perhaps not an automatic agreement but simply a focusing on the fact that, even if the agent is not identified, *someone* performed the action, and *something* was its object or result.

Another class of prefixes, the "dimensional infixes", (so-called because they follow the conjugation prefixes and are nearly homophonous with the postpositions) is also to be explained in terms of focus, rather than simple automatic agreement. In this case what is focused on are the syntactic and semantic properties of the sentence or the individual verb with which the prefixes occur [32]. The non-automatic character of this focus is shown by the fact that these infixes are optional in some cases, undergo a certain amount of neutralization in imperatives, and cannot appear at all in non-finite forms of the verb. Finally the use of the enclitic copula for the foregrounding of various sentence constituents would also come under the general heading of focusing.

4.4 *Embedded Sentences.* Finally in (1c-f) we see a few elementary examples of a very important type of construction, involving transformations which integrate an embedded (subordinated) proposition into a higher (matrix) proposition. Investigation of these structures and processes constitutes the core of much of present-day transformational investigation. Here we will simply sketch out some structures and indicate informally how they are transformed. We will presume that there are two types of structure involved (conceivably there is only one). In one a NP consists of a NP head and a modifying S, which we can represent as:

(4)



This is exemplified in (1c, d). In the other structure, exemplified in (1e, f), the S is head and sole constituent of the NP [33]:

(5)

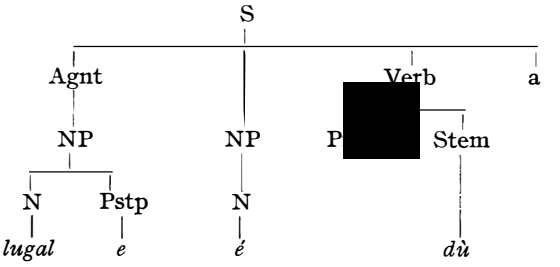
[32] Cf. G. Gragg, *Sumerian Dimensional Infixes* (Münster, 1972).

[33] This implies an additional optional rule in the system of footnotes 24 and 29, to be inserted between (3) and (4):

(3^a) $\text{NP} \rightarrow \left\{ \begin{matrix} \text{NP} \\ \text{S} \end{matrix} \quad \text{S} \right\}$.

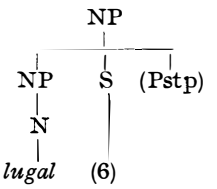
All of the clauses (c-f) contain the embedded S, followed by a nominalizing particle (complementizer) -a:

(6)

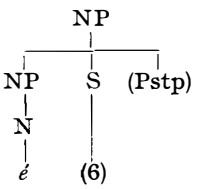


The structures into which (6) is embedded are represented as (7c-f ■ 1c-f):

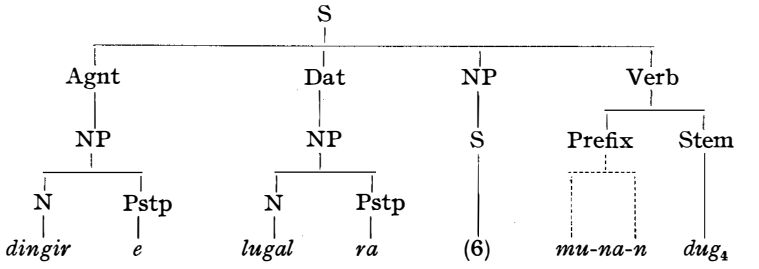
(7) c.



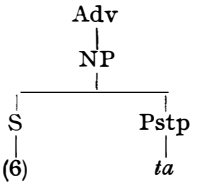
d.



e.



f.



The NP of c, d, and f of course are themselves part of a larger sentence, and in c and d can be followed by a postposition indicating the relation of the NP to the proposition as a whole.

(1c, d) are derived from (7c, d) in two steps: (1) by deletion of the noun in the subordinate clouse identical with the head noun (*lugal* in c, *é* in d) — this leaves the well-formed phrases *lugal é mu-n-dù-a* and *é lugal-e mu-n-dù-a*; (2) optional deletion of the verbal prefix (and the *-a* in c). In morphologically

oriented grammars the difference between *dù* and *dù-a* is usually characterized as the difference between an active participle (as in 1c) and an intransitive or passive (as in *kingia uru-šè du-a* "the messenger who goes to the city", or *é uru-a dù-a* "the house built in the city"), while the construction of (1d) is taken as a regular exception to the generalization. In a syntactic context it becomes evident that -a is a sentential complementizer which occurs with all subordinate clauses, whether active or passive, but that if the head noun of a NP is identical to the agentive NP of the subordinate clause, and the prefix is deleted, the complementizer is deleted along with the prefix.

In (1e = 7e) the embedded sentence is the object of the verb. A very general type of rule, responsible for many non-finite verb clauses in a large number of languages (Equi NP Deletion) deletes the occurrence of lugal in (6) under identity with lugal in the main clause. A complementizer transformation deletes the Pref, and inserts the -(*e*)*d*- complementizer demanded in sentential complements of verbs of commanding. Finally in (1f = 7f) the embedded sentence, with no deletion or reduction, takes the place of an adverbial complement to a higher sentence.

In sumary what a transformational model of a Sumerian grammar will attempt to do is, first, to specify the form of underlying proposition types, and show how semantically and syntactically complex propositions are built up by combinations of embedded simple propositions, using structures like (4) and (5). This underlying form represents the meaning of the proposition, to the extent it can be determined. It undergoes rules in such a way that the final transferred version is uniquely determined by explicit properties of the underlying form and the set of rules which operate on that form. An important set of rules will show how the embedded propositions are integrated into the matrix structure by combinations of transformational processes such as Complementizer Placement, Reduction, Equi-NP Deletion, Relativization — the form and conditions of applicability of these rules to be determined in as much detail as the data permit. Other transformational processes will be responsible for other spects of the concrete shape of Sumerian sentences — cf. the examples given above of a series of "focus" transformations. Finally the relations among these rules are to be formulated, e. g. the extent to which they form an integrated system such that the output of one rule is the input to another [34], and typological conclusions are to be drawn from the underlying forms and the rule types.

[34] For example, if one substitutes "linguists" for "Chomsky" in the sentences (1)-(3) of footnote 12, one can readily see that Concord has to follow Tough Movement. For while one gets the third person singular form of the verb "be" in (1') "To understand linguists *is* difficult" and (2') "It *is* difficult to understand linguists", where Tough Movement does not apply, if one applies Concord to (4) before Tough Movement, when either "it" or "S" is subject of the verb, one will obtain the incorrect "Linguists *is* difficult to understand", instead of the desired, "Linguists *are* difficult to understand".

Statistical Methods in the Study of Ancient Near Eastern Languages

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Some years ago the scholar to whom these pages are dedicated, proposed to H. Paper and R. Lees to apply to the classification of Semitic languages the quantitative method for determining linguistic relationship (Gelb, 1955: 96). In fact among the linguistic questions which can hardly find answer from a purely qualitative point of view, the problem of genealogical classification stands out. Nevertheless, the point is not whether statistical methods are to be used in linguistics, but what they mean in terms of linguistics whenever they are applied. For this reason, it has seemed useful, on this occasion, to discuss the validity of some of them, rather than offer a complete review. To this purpose, we shall consider the care that is to be taken when quantitative data are used to descriptive ends, and then we shall discuss three methods of classification concerning morphological typology, lexical changes and linguistic relationship [1].

1. The utility of grounding grammatical or stylistical evaluations on a wide series of data is evident, and such researches were made within assyriological studies already in the last century [2]. I shall mention those by Bradner (1890, 1892) on sentence and word order, by Kent (1891) on the last vowel of a noun in the construct state, by Lindl (1896) on the vocalism of strong verbs in the present and the preterite of the simple stem. In our century, the stress laid by the school of Prague on the functional load, gave rise to the quantitative researches by Cantineau (1946, 1950, 1951) on phonemic systems of some Semitic languages and dialects [3] and, in the morphological field, to such researches as that on Geʿez verb by M. Cohen *et al.* (1950) [4], on verbal formations in the Qur'ān by Elchouemi (1954 *a, b, c*), on internal plurals, again in the Qur'ān, by Alsamirrai (1954) [5]. A recent example of applying a statistical method to descriptive purpose, has been offered by Andersen (1970), in a volume on the Hebrew verbless clause in the Pentateuch. The

[1] The help offered by computers to the analysis of linguistic materials and the problems pertaining to programming will not be discussed in this article; for an example see Weil (1964 *a*; 1964 *b*).

[2] The interest for statistical researches in Oriental studies can be dated back to Whitney (1874), quoted by Petráček (1956 *a:* 622).

[3] For a review of phonemic studies on Semitic languages, see Petráček (1956 *b*); the same scholar applied statistical data to the historical problem of *ġ* (1953; 1955).

[4] See now also Mantel-Niećko (1964; 1969).

[5] Further bibliography on similar problems in Petráček (1956 *a*).

main part of this book is a concordance of verbless clauses, whose different types are successively interpreted [6].

In all those cases in which statistical research is limited to the compilation of frequency distributions of linguistic forms, neither researchers nor readers need a specific statistical preparation. Nevertheless, in order that the frequency distributions compiled may have a linguistic validity, it is necessary to have taken into account criteria valid to identify the examined linguistic forms, and to fix the limits of the considered sample. As to the first problem, it will suffice to mention, for instance, that the linguistic definition of the "word" is very controversial and some scholars even doubt that one is possible [7]. As for the second problem, I would mention that it is quite necessary that the sample be homogeneous both chronologically and qualitatively (or, when it is a matter of diachronical statistics, that each compared sample be homogeneous). The influence of different "styles" upon the frequency of the examined phenomena has to be taken into account, and some linguists have judged statistical linguistics in a very severe way for the practice of neglecting "functional languages" (Coseriu, 1967: 36, note 1). As an example of the mistaken evaluations to which one could be lead if one disregards these criteria, I may mention the conclusions of Schott (1925: 35 ff.) pertinent to his analysis of morpheme *-iš* in Akkadian texts. Placing the attestations in the historical inscriptions of the Assyrian kings from the ninth to the seventh century B.C. on the same plane as the ones of hymnic-epic poetry, he came to the wrong conclusion that *Enūma eliš* had to be dated to the first millennium B.C. Starting from the same data, von Soden (1932-1933: II, 104 ff., 125 ff.), who correctly separated the different use of *-iš* in Babylonian epic and in Assyrian historical inscriptions, showed that *Enūma eliš* dates back to the second millennium B.C. [8].

When these linguistic criteria have been taken into account, if the frequency distribution has been compiled on a well defined universe, and if the

[6] As an example of an analytical exposition of data, although not tabulated and not statistically interpreted, see Dietrich (1969) on the 53 different conjunctions found in a corpus of about 1100 Neo-Babylonian letters. Leslau (1969) emphasizes the importance of the relative frequency as determinant of phonetic change, though he is compelled to rely on intuition, no statistical data being available for the Ethiopian languages. Hodge (1970) studies the survival of Egyptian morphemes, from Old Egyptian to recent Coptic, by means of a list of 70 items.

[7] Among Semitists, see Harris (1941: 147, 159 ff.; 1951: 325 ff., 352 ff.), Greenberg (1954: 192), Fronzaroli (1966: 220 ff.), Gelb (1969: 133 f.).

[8] In ancient Near Eastern studies the stylo-statistics which has been used elsewhere, especially in order to determine the chronological order of texts and to solve problems of disputed authorship (Herdan, 1956), has not aroused a great interest. As an example of the first type of application, I shall just mention the paper of the Egyptologist Bennet (1941), who provided criteria for dating memorial or funerary documents of the Middle Kingdom; recently Del Francia (1970) has proposed a quantitative method, based on the use of kinship terms, from the Eleventh to the Seventeenth Dynasty. As an example of statistical application to problems of disputed authorship, the paper by Radday (1970) on the unity of the Book of Isaiah, may be mentioned. Finally, as an example of a larger application to a given literary genre, failing works on the ancient Near Eastern languages, the book by Beyerl (1971) on the style of the modern Arabic short story, should be mentioned.

researcher does not wish to extend its validity beyond this universe, the gathered data may be directly interpreted on the linguistic level. On the other hand if the examined sample has to be regarded as representative of a larger universe, appropriate methodologies in evaluating the data are to be used. Not all the differences found in the data have the same value; weighing their importance involves resorting to some significance test. This in turn leads to the question of just how large the sample has to be in order to achieve valid results.

Let us consider, for example, Table 1, in which some of Bradner's data (1892: 12) are gathered. Examining the two columns it could be observed that the frequency of the declarative sentences increases greatly from the first column to the second; Bradner (1892: 7) speaks of "greater [preponderance] than in Tiglath-Pileser". In terms of percentages, it means that the declarative sentences pass from 79 % to 86 % of the total.

Table 1 (Observed frequencies)

	Tiglath-Pileser I	Ashurnasirpal II - Annals	Total
Declarative Sentences	322	760	1082
Relative Sentences	86	119	205
Total	408	879	1287

Nevertheless, the mere examination of the percentages does not suffice to ascertain whether the observed difference could be accounted for by chance fluctuations or not. This can be verified by means of the following procedure. Let us formulate the hypothesis of independence and calculate the expected frequencies, namely the figures we would expect if the differences pointed out in the inscriptions of the two kings were due only to the different total numbers of attestations of declarative sentences and relative sentences. We get these values by multiplying the border totals and dividing by the general total. The following Table 2 exhibits the expected frequencies. We can now calculate a quantity called Chi-square, which in our case is equal to the

Table 2 (Expected frequencies)

	Tiglath-Pileser I	Ashurnasirpal II Annals
Declarative Sentences	343	739
Relative Sentences	65	140

sum of the values got by calculating the difference between the observed frequencies and the expected frequencies, squared and then divided by the expected frequencies:

$$\chi^2 = \Sigma \quad (f_{ij}-f'_{ij})^2/f_{ij} \qquad (1)$$

The result is $\chi^2 = 11.82$, which, for one degree of freedom (as in our Table), exceeds what is required at the 0.01 level of probability (namely 6.63) [9]. It means that the probability that the observed frequencies are to be accounted for by chance, is below 0.01; our data equal what may be obtained less than 1 time in 100. Thus we may consider to be widely ascertained that, from a statistical point of view, an association exists between a frequent use of declarative sentences and Ashurnasirpal's Annals.

The statistical procedure which has been applied above, is correct on the condition that, in the statistical universe which the sample refers to (namely the complete corpus of the inscriptions of the two kings, whether they have reached us, or not), the ratio between the two kinds of sentences is the same as in the examined sample. Only in this case the values of the hypothesis of independence we have calculated on the ground of the border totals of Table 1, have a linguistic meaning. Assyriologists can judge whether this is likely in the examined case. From a more general point of view this means that the sample must be a valid one, i. e., a random sample (Chrétien, 1945: 486). Moreover, as to the interpretation of the results of the Chi-square test, it must be observed that, while a very small probability allows us to reject the assumption of independence, a not small probability does not prove the hypothesis to be correct. It only indicates that, according to the test, the hypothesis is not necessarily to be disproved (Yule and Kendall, 1958: 470).

In the field of Semitistics, the notion of probability has been used by Greenberg (1950) in his well-known research on consonantal incompatibilities in the verbal root. He tabulated the occurrences of each phoneme as first, second and third radical consonant in 3775 Arabic verbal roots, and then he compared the data referring to a given position with those expected on the ground of the coefficient of probability CR/n (where C and R equal the number of presences of the same phoneme or two different phonemes in a given position, and n equals the total number of the roots). At the end, he calculated the standard error for each coefficient and the standard deviation for each frequency. This allows us to evaluate the observed data. For example, by comparing the observed 20 cases of Arab roots whose first and third consonants are equal to each other, with the coefficient of probability (in this case about 154, namely a standard deviation of -10.8) [10], attestations turn out to be fewer than the expected frequency thus proving that Arabic, as well as Semitic languages in general, avoids the pattern with identical consonants in first and third position.

[9] See a table of significance point of Chi-square, e. g., in Yule and Kendall (1958: Appendix Table 3).

[10] A standard deviation of 1.96 is equivalent to a probability of 0.05 and a standard deviation of 2.57 is equivalent to a probability of 0.01. Thus for a standard deviation of 10.8, the probability that observed values are to be due to chance fluctuations is infinitesimal.

At first sight, Greenberg's procedure may seem similar to the one applied to a sample of phonemes or words of which the coefficient of probability and the standard deviation are calculated. But the two problems are different and some discussion will have to be devoted to the meaning these measures may have in the case in question. When we ask the question whether a sample of the distribution of the phonemes in a given language is valid, we start from the assumption that phonemes in a corpus are distributed according to the normal distribution, and their frequency is consequently proportional to the length of the sample. In this case the procedure is used to measure the validity of the sample compared with the corpus and to establish whether the differences observed between each phoneme approximate the differences in the corpus or whether they may have arisen by fluctuations due to chance (e. g., see Reed, 1949). Similar hypotheses constitute the basis of the researches on the distributions of words (e. g., Guiraud, 1960: 36 ff.). The problem treated by Greenberg is different. He calculates the coefficient of probability in comparison with the internal distribution of the phonemes in the three positions possible in a triconsonantal root. In this connection Chrétien's objection (1966: 528 f.) seems valid: radical morphemes are not built by extracting phonemes at random; they are the result of a precedent historical situation and of a phonetic development, of the entry of loanwords into the language and of onomatopoeic formations; each of them is formed once and for all and constitutes a non-repeatable event. It is not even, in this case, a problem of adjusting the statistical sample to the universe, since the 3775 roots constitute in practice a complete corpus. Greenberg's procedure seems to be a proof, *ab absurdo*, that attestations do not correspond to a random distribution but reflect restrictive rules. The coefficient of association proposed by Chrétien (1965 *a*: 258 ff.) works in the same way; he uses the following formula to measure the deviation of the observed frequencies from those required by the hypothesis of a random distribution:

$$K = \frac{a - a_o}{a_o} \qquad (2)$$

where a_o is the coefficient of probability. Applying, for example, this formula to the 20 observed cases of Arab roots whose third and first consonants are identical, we should have $K = (20 - 154) / 154 = -0.87$. This indicates a strong negative association, as a proof that Arabic avoids this pattern [11].

2. In the last few years special interest has been aroused by typological classification on quantitative basis, especially after the publication of a paper by Greenberg (1960) which develops and completes on a quantitative basis criteria suggested by E. Sapir. Greenberg proposes a classification of languages according to ten important features, mostly morphological, such as synthesis, agglutination, composition, suffixation, etc. But since it is evident that contrasting features, as e. g., agglutination and fusion, may coexist in the same language, some devices can be conveniently provided, in order to

[11] The notion of probability is used by Greenberg (1963: 2 f.) as a methodological criterion also in mass comparisons, and on this he bases a classification of African languages (for the Hamito-Semitic question, see ibid.: 42 ff.). On involved mathematical and linguistic problems, see now Bender (1969).

calculate the relative importance of each feature. To this end he suggests a series of indexes, calculated from texts chosen as samples—for instance the index of agglutination, namely the ratio of agglutinative constructions to morph junctures (A/J). In this way, comparisons among different languages can be grounded on observations and not on mere impressions. Among the indexes, I shall mention, referring to languages of ancient Near East, those calculated for Hittite by Cowgill (1963) and for Old Babylonian, Biblical Hebrew and Classical Arabic by Fronzaroli (1966). Table 3 shows some of them (synthesis [M/W], agglutination [A/J], compounding [R/W], derivation [D/W], inflection [I/W], prefixing [P/W], infixing [Inf/W], suffixing [S/W] for three Semitic languages and for Hittite, to which Classical Sanskrit and polysynthetic Eskimo (calculated by Greenberg, 1960: 193) have been added, as examples of specific types for a typological comparison [12].

Table 3

	M/W	A/J	R/W	D/W	I/W	P/W	Inf/W	S/W
Sk.	2.59	0.09	1.13	0.62	0.84	0.16	—	1.18
Hitt.	1.95	0.42	1.00	0.24	0.71	0.01	0.00	0.94
Ba.	2.99	0.01	1.00	0.59	1.40	0.24	0.70	0.98
He.	2.47	0.00	1.01	0.41	1.05	0.31	0.48	0.57
Ar.	3.26	0.02	1.03	0.55	1.68	0.31	0.49	1.33
Esk.	3.72	0.03	1.00	1.25	1.75	0.00	—	2.72

I shall not discuss here the linguistic problems presented by the analysis of the sample that precedes the reckoning. It is obvious, however, that samples of different languages must have been analyzed with homogeneous criteria for results to be comparable. For instance, Cowgill (1963: 96), unlike Greenberg, regards enclitics as independent words. But problems arise also from the statistical side, in spite of the simplicity of required reckonings. Greenberg (1960: 193 f.) calculated his indexes on samples of 100 words each, leaving to possible developments of the method the task of ascertaining the statistical validity of a sample of such an extent and the eventual influence of individual style, or of literary genre, on the indexes. Householder (1960: 197) questioned the validity of such short samples; Cowgill (1963: 113, note 7) pointed out that the indexes calculated by Contrera for Latin and Spanish authors seem to show that, while the figures derived from 100 words long samples for the indexes of synthesis (M/W) are probably fairly reliable, for the syntactic indexes they may not be. Pierce (1966) reckoned for Turkish 14

[12] The meanings of abbreviations in the index labels are: M = morphemes, W = words, A = agglutinative morph junctures, J = morph junctures, R = roots, D = derivational morphemes, I = inflectional morphemes, P = prefixes, Inf = infixes, S = suffixes.

samples of 100 words each, which represent four styles and six different authors, and afterwards, from the same sources, 10 samples of 200 words each, chosen so that there were not overlaps. Of these samples he studied the index of synthesis (M/W), the compounding index (R/W) and the suffixial index (S/W), reaching the following results: in a group of small samples, drawn from different styles, indexes are significantly different in comparison with those of a large sample drawn from a single source; on the contrary, the indexes calculated on 200 word long samples do not differ significantly from those calculated for a series of 100 word long samples. These results are obtained by means of significance tests. Thus it seems evident that further tests on samples of various length and character are necessary, for the different languages, before judging what the optimal dimension is in order to apply this method of typological classification on a vast scale. At the present state of research, it seems that the indexes calculated on 100 word long samples can be considered as indicative, especially for the morphological indexes[13].

3. Among the attempts to apply statistical methods to diacronical problems, we must mention the so-called "glottochronology". Starting from the observation that the greater the differences between languages of the same family are, the longer we must suppose the time required for such differences to develop, Swadesh (1950, 1952) thought of a diagnostic list of meanings, chosen among the simplest, so that they could not be lacking in any language. This list is the pattern by means of which words are drawn up for each language to be studied. Subsequently, it is possible to proceed in two ways. We can compare the lists of two related languages, e. g., of two Semitic languages, to calculate the time past since they began to differentiate (case of application). On the other hand, it is possible to compare the lists of two different stages of development of the same language — the dates of which are known — e. g., of Qoranic Arabic and of the modern Meccan dialect, to calculate the loss of words in a given unity of time (control case). The formula indicating the relation between time (t), the items common to two stages of the same languages (c) and retention rate (r) is

$$t = \log c / \log r \tag{3}$$

In the control cases, knowing c and t, we can obtain the value of r, i.e., determine the retention rate on the basis of the time and number of the common items. In the cases of application, knowing the number of items common to two languages and assuming the retention rate to be constant, we can determine time. In that case, the formula becomes

$$t = \log C / 2 \log r \tag{4}$$

where $C = c^2$. The mathematical patterns of glottochronology have been carefully studied by Lees (1953).

Glottochronology found a rapid diffusion; but there was also considerable criticism regarding several philological and statistical aspects of the method.

[13] A particular development of the method was offered by Krupa and Altmann (1966). They observed that only A/J, W/R, P/M (in the place of Greenberg's P/W) are not correlated, neither among themselves nor with other indexes. They are to be considered as basic indexes, whose value is not predictable by any other.

About the arguments advanced by the two sides and the relevant bibliography, Hymes (1960: *a*, *b*) can be consulted; here we shall just emphasize some difficulties. The difficulty of obtaining a list of meanings which can define a basic vocabulary, good for every language, is shown by Hoijer (1956) with copious arguments, and Swadesh himself (1955) became aware of it, since he cut down the number of words from 200 to 100. Hoijer, in particular, showed that each one of the chosen words can be subject to cultural influences and gave some examples for Navaho and other languages of the Athapaskan family. As far as we are concerned, it can be remarked that even the application to Semitic languages presents some difficulties, as D. Cohen (1961) pointed out. In comparing the lexicon of Semitic languages, it is impossible, for instance, to use the five names of colours (red, green, yellow, white, black) of the diagnostic list, since they are etymologically independent in the different languages, and, moreover, it seems uncertain that they may be named in all languages. The problem leads to that of statistical validity. If we cut down the sample too much, in order to obtain a list subject as little as possible to cultural influence its statistical validity will also be reduced.

The retention rate constitutes another critical question. Its degree of constancy cannot be deduced *a priori*, on the basis of mathematical or statistical proofs, but only by means of a series of tests (control cases). And there is no doubt that some scholars have produced results sensibly different from those obtained by the supporters of the method (according to Swadesh [1955] about 0.86 per 1000 years). An ample discussion on the different aspects of the problem has been offered by the book of Fodor (1965)[14]. But a still more radical objection was raised by Chrétien (1962), who, assuming the postulates of glottochronology to be valid, examined again its mathematical implications and, in particular, the validity of functions (3) and (4). According to Chrétien, the two functions do not agree with the hypotheses and consequently the results obtained in glottochronological studies are illusive. Even if basic hypotheses of glottochronology are to be considered correct, it is impossible to obtain valid results until the functions used now are replaced by valid functions. Van der Merwe's attempt (1966), which gave rise to a large discussion in *Current Anthropology*, shows how difficult it is to bring the complex phenomena within the frame of a mathematical treatment.

Coming back to Near Eastern languages, I shall mention two researches on Arabic and some of its modern dialects (Samarrai, 1959; Satterthwaite, 1960), some papers on the classification of South Semitic dialects (D. Cohen, 1961, 1964; Bender, 1966 *a*, *b*; 1968 *a*, *b*; 1970), and an attempt by Rabin (1970) at classifying the six main Semitic languages[15]. Rabin, at least in the paper delivered at the London Colloquium, does not seem to know the close criticism of Chrétien, and works out a real glottochronological research, setting forth reservations only on the philological matters used and on the difficulties of dating them. Bender, on the contrary, turned his attention since his first paper (1966 *a*: 6, note 3) to the mathematical difficulties of the

[14] Subsequently see Dyen *et al.* (1967) and Sankoff (1970) who take into account the possibility of using synonyms for a single meaning of the test list.

[15] Petráček (1968: 473 f.) compares the results of Bender (1966 *b*) and D. Cohen (1964) with Fronzaroli's coefficients *T* (1961 *a*, *b*).

method proposed by Swadesh. He came to the conclusion that results, for a period of time shorter than 3000 years, are not strongly touched by this criticism and may be regarded as valid. Subsequently (1968 *b*: 9, 11) he admitted that the situation is worse than he thought before, and suggested considering cognate percentages to be only relative figures. Also D. Cohen's papers (1961, 1964) seem to be an attempt to reevaluate this method in a lexicostatistic rather than in a glottochronological way. He tried to determine the basic vocabulary of Semitic languages, first through a merely statistic choice, then through a linguistic check aimed at distinguishing the words which date back to Common Semitic, finally through a chronological criterion which sets apart the most stable ones, namely the most constantly attested, among the common words. The statistical study of vocabulary for historical aims, besides improving our knowledge of the lexicon in its diachronical aspects, could be used to prove relationship between languages or groups of languages. But to this end it is advisable not to disregard other aspects of the language (Cowan, 1959; Teeter, 1963).

4. Among the attempts to classify through statistical methods languages originally related among themselves I shall mention at least the studies by Kroeber and Chrétien (1937, 1939), Ross (1950), Ellegård (1959) [16]. In all these works a sample of linguistic features, whose presence or absence in each language is recorded, is interpreted by means of given procedures which provide relationship indexes between languages. The traditional way of interpreting data, used by Kroeber and Chrétien (1937, 1939) and by Chrétien (1956), is to calculate the interdependence for each couple of languages through the correlation coefficient

$$r = \frac{ad - bc}{\sqrt{(a+b)\,(c+d)\,(a+c)\,(b+d)}} \tag{5}$$

where a indicates the number of isoglosses present in both languages, b the number of those present in the first language only, c the number of those present only in the second, d the number of isoglosses absent in both [17]. According to Ellegård (1959), since d could distort the value of the coefficient with its heavy influence, it would be better to modify the formula by making d infinitely large and calculate the relations between languages according to the coefficient

$$r_n = \frac{a}{\sqrt{(a+b)\,(a+c)}} \tag{6}$$

[16] A different problem is that of determining relationships in a group of languages, where the genetic unity is not attested by sufficient historical evidence. In this connection, see Cowan (1962), Chrétien (1966), Bender (1969); cf. also Dyen (1969). In Near Eastern studies this problem ought to be borne in mind in the Hamito-Semitic question.

[17] Chrétien (1945: 482 f.) thinks that in ethnological or linguistic investigations the coefficient r should not be used if the total number of features is less than 500; the rule may be followed not too strictly, if the coefficients are used to determine large groupings (Chrétien, 1956: 96, note 22). In any case, the test of any method is and must be empirical. As to the ethnological meaning to be given to the terms "independence", "association", "dissociation", see again Chrétien (1945: 483 ff.).

In fact, *a* and *d* help in making the correlation positive, while *b* and *c* help in weakening it. If the number of isoglosses absent in both languages is very large, the coefficient will appear increased. But before we accept Ellegård's solution[18], we must wonder whether it is correct to ignore the value of *d*. As Herdan (1962: 86 f.) points out — as well as Chrétien (1965 *b*: 351 f.) in his review — the interdependence we measure is not grounded only on the isoglosses common to both languages, but also on those, belonging to a larger whole, which do not appear in either language. This wider frame of reference within which the comparison takes place, is considered infinite in formula (6); on the contrary, when we use formula (5), it is restricted within the family to which the two languages belong, represented by the whole of the isoglosses of the sample. As linguists, we wish to obtain the measure of the interdependence of two languages within a given family, and the sample, with all the reserves on its validity, is the only way of taking into account this relationship. To this end, formula (5) seems the most appropriate.

In a research on Semitic languages (Fronzaroli, 1961) a slightly more complex method was tried. Starting from a phonological and morphological sample of 217 isoglosses, the extent of the isoglosses present in each language was considered, in order to have a judgement on their "connectivity" (cf. Herdan, 1960: 104 ff.). Table 3 shows the extent of the isoglosses present in each language (Old Babylonian, Ugaritic, Biblical Hebrew, Syriac, Classical Arabic, Ge'ez): the vertical columns marked by numbers from 1 to 5 indicate the number of isoglosses belonging to one language (1), shared with any sample language whatsoever (2), and so on (isoglosses which one language shares with all the others are not included in the sample). From these data, as a parameter with which the values of each language can be compared, a chance model was calculated, i. e. a model (obtained by means of the calculus of probability) which indicates what the distribution of isoglosses would be if the six languages were equally connected (Fronzaroli, 1961: 365 ff.). The figures

Table 4

	1	2	3	4	5	Total
Ba	33	11	18	15	8	85
Ug	3	18	17	17	18	73
He	9	22	22	20	16	89
Sy	12	14	16	21	17	80
Ar	12	28	14	20	16	90
Ge	14	21	12	11	15	73

of the model can thus be compared with those of each language, and it is possible to obtain, for each of them, the coefficient of contingency

$$C = \sqrt{\frac{\chi^2}{N + \chi^2}} \qquad (7)$$

[18] The formula had beed used before by Driver and Kroeber (1932: 219).

then reduced according to the formula by Tschuprow (T)[19]. Table 5 shows the values of T for the different languages and for the chance model[20].

Table 5

	T
Ug	0.187
He	0.139
Sy	0.128
Chance model	0.000
Ge	— 0.148
Ar	— 0.149
Ba	— 0.438

The data of the sample were considered also from the point of view of interdependence. In this case too, a chance model was calculated and subsequently the figures of the model were compared with those of each pair of languages by means of the coefficient of contingency C (Fronzaroli, 1961: 370 ff.). Table 6 shows the coefficient C and the level of probability for each pair of languages, to which the coefficient r has been added for comparison.

Table 6

	P below than	C	r
HeSy	0.01	0.353	0.411
UgAr	0.01	0.294	0.332
UgHe	0.01	0.277	0.324
UgSy	0.05	0.199	0.247
ArGe	0.05	0.195	0.212
SyAr	0.70	0.083	0.055
SyGe	0.70	— 0.100	— 0.018
BaUg	0.70	— 0.109	— 0.090
HeAr	0.50	— 0.117	— 0.017
UgGe	0.50	— 0.128	— 0.075
BaHe	0.30	— 0.138	— 0.074
BaGe	0.20	— 0.157	— 0.092
HeGe	0.10	— 0.177	— 0.098
BaSy	0.05	— 0.204	— 0.163
BaAr	0.01	— 0.314	— 0.273

[19] For an evaluation of this method, see Herdan (1964: 112 ff.).
[20] As to the negative sign added to some coefficients, see Fronzaroli (1961: 369).

We have already seen how the Chi-square test is to be understood. The same problems arise as far as the classification of languages is concerned. In the column of probabilities we read that only BaSy and BaAr, among the negative coefficients, keep below the probability of 0.05; other negative coefficients keep far from that level and offer values, which may be considered as fluctuations of a same distribution. On the contrary, all the positive coefficients (except SyAr) are statistically significant, always at the usual level of 0.05. If the sample of isoglosses cannot be regarded as exhaustive with respect to linguistic matters, but must be judged as representative of them, only a part of the coefficients could be utilized in order to classify these languages. Only if the researcher, from a linguistic point of view, may consider the sample complete, at least in the present state of our knowledge in a given field, the tests of significance could be disregarded and all coefficients could be taken into account.

Other questions remain to be solved owing to the difficulty of bringing an evaluation of each isogloss into the method. Linguists not very favorable to the application of statistical methods, have frequently observed that one isogloss can be much more weighty than many others all together, in order to establish a dialectal differentiation. This is stated in particular with regard to innovatory isoglosses, that can be peculiar to a special relationship, while a shared preservation of ancient features does not prove anything in itself. This problem was rightly raised by Maniet (1964), but the mathematical solution he suggested shows how inconvenient it is to apply statistical methods without being aware of what they involve. The coefficient of correlation calculated by formula (5) takes into account four given quantities, that cannot be changed at will. Attributing to d the task of indicating common innovations means altering the formula so that its employment becomes quite devoid of meaning. As for ascribing different values to innovations according to their importance, it is not easy to see how it would be possible to agree on a criterion of evaluation. Even the proposal of Polomé (cf. Maniet, 1964: 411), of taking into account the functional load of features, is not free from criticism, since it is not certain that a frequent feature is always more significant than the less frequent ones, in order to determine relationship. Nevertheless, when from a linguistic point of view this could be considered satisfactory, no difficulty would arise from the statistical side. It would be sufficient to use the correlation coefficient of Bravais or the Product-Moment Formula [21].

A further development was tried by Herdan, who used Factor Analysis, according to C. Spearman's method, for the determination of relationship of Indo-European languages (1962: 87 ff.) and of Semitic languages (1964: 125 ff.). This method, originally formulated for psychological investigations, supposes that correlations between tests (in our case, languages) are due to one general factor (g), present in all tests besides specific factors. Herdan interpreted the general factor as the genealogical nearness of relationship and calculated it from the coefficients r_n. Though this choice is theoretically justified (Herdan, 1962: 87; 1964: 124), it was probably made because Spearman's method

[21] See Yule and Kendall (1958: 218 ff.) and, for linguistic applications, Herdan (1956: 347 ff.).

requires that only positive coefficients are used. Chrétien (1965 *b*: 352 f., referring to Herdan, 1962) criticized this choice, showing that it is not possible to leave the values of *d* out of consideration. But the choice was all the more inadequate in the case of Semitic languages, as the sample of Fronzaroli (1961), which Herdan used, does not consist of independent isoglosses, and thus the values of *g* so calculated are distorted from the beginning [22]. This does not mean however that the proposal of applying factor analysis should not be examined.

In the above mentioned research (Fronzaroli, 1961) non-independent isoglosses were chosen, since they had to represent the wealth of trends present in the Semitic area, rather than the organic unity of a linguistic stage as the supposed Common Semitic condition [23]. From a linguistic point of view, it would be desirable to have at one's disposal a method capable of extracting from the correlations not only the portion due to the Common Semitic heritage but also other components of the observed situation, e.g., the speed of typological evolution and the weight of the contacts arisen from the geographical position in the historical age. Multiple factor analysis, carried out on the coefficients *r*, ought to be able to meet the case [24].

[22] In fact Ellegård's observations (1959) about the coefficient *r* refer to independent isoglosses; cf. Fronzaroli (1961: 375).

[23] But, if the same value is given to isoglosses which presumedly represent the original condition and to isoglosses which represent a regional development, the stress on innovations required from a linguistic point of view would be wanting. In fact the chance model is not a mathematical equivalent of the linguistic notion of Common Semitic, but only a theoretical pattern, to which observed data can be compared. Thus the high value of interdependence, measured by the coefficient of contingency *C* for the pair UgAr (0.294), cannot be immediately interpreted as due to dialectical affinity (Fronzaroli, 1961: 379).

[24] As an introduction to factor analysis, see e.g., Kendall (1957), Cattell (1965), Harmann (1967): for an application to ethnographic data, see Driver and Schuessler (1957). Chrétien's criticism (1965 *b*: 353 ff.) does not seem definitive.

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Verbal Aspect in Semitic

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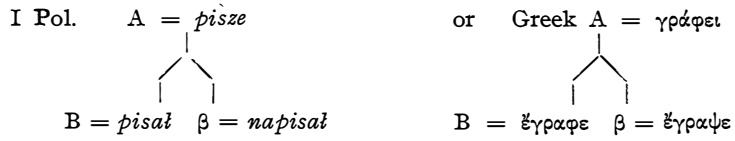
Analysis of verbal systems like those of English, French or German, on the one hand, of Slavic languages like Polish or Russian on the other, has induced scholars, among them A. Meillet, to contrast tense languages with aspect languages. At first blush there is an analogy between e. g., Fr. 1. *il écrit* – 2. *il écrivait* – 3. *il a écrit* and Pol. 1.*pisze* – 2.*pisał* – 3.*napisał*, and actually there is a one-to-one relation between Fr. and Pol. as regards 1.-2. The real difference, however, becomes clear if opposing 1. and 3. we built the corresponding infinitives or imperatives. It is evident that the distinction between *écrire* and *avoir écrit* (*simultaneity*: *anteriority*) is of another nature than that between *pisać* and *napisać* (*imperfectivity*: *perfectivity*, or *linearity*: *punctuality*), whereas the difference between Pol. *pisz*! and *napisz*! has no direct counterpart in Fr. The category of *anteriority* or time-reference, as attested in Fr., does not characterize the verbal action in itself though one may be allowed to speak of a *relative* feature of the action which is either simultaneous with or prior to another. In Slavic perfectivity and imperfectivity are characteristic traits of the action itself.

Still more important is the fact that the relation *pisze*: *pisał* like that of *il écrit*: *il écrivait* is an opposition of mere tense (simultaneity with the *moment of speaking*: simultaneity with a *moment of the past*). This means that whereas the existence of tense does not entail that of aspect, the latter presupposes the existence of tense.

The real difference between the two types of languages mentioned boils down to *time-reference* (German *Zeitbezug*): *aspect*, tense being a common feature.

As regards the personal verb, genuine or "classical" aspect — as attested in Ancient Greek or in Slavic — is proper only to the preterite and to the future. The grammatical present *includes* the moment of speaking and may be arbitrarily extended both to the left (= into the physical past) and to the right (= into the physical future) being thus by its nature always linear, i. e., imperfective. The existence of the category of aspect presupposes a verbal system of at least three members, cf.:



If at all attested for the future (e. g., Pol. *będzie pisał*: *napisze*) aspect in the future tense is to be considered as secondary with regard to its existence in the preterite. Distinction in the future presupposes the existence of aspect in the preterite though not *vice versa*.

What is the status of *tense* and *aspect* in languages with a *two-member* verbal system like that of Semitic? Is it permitted to posit them for a language like classical Arabic? It is of course not the possibility of *expressing* certain meanings and shades — they may be expressed in any language — but the existence of verbal *categories* which interests us here. Although the predicative use of the participle *qātil* and the use of *ḥāl*-sentences plays a major role in Ar., we are justified in limiting ourselves to the personal verb represented (in the indicative) only by two forms: *ʾaqtulu, qatala* (with their morphophonological and morphological variants *ʾaqtilu, ʾaqtalu, qatila, qatula*). Since *ʾaqtalu* may be used in a secondary (i. e. text-conditioned) function as a form denoting *simultaneity with a past moment* (i. e., as a kind of the European imperfect), e. g., *ǰalasū n-nāsu ʾašrabūna l-ḫamra* "people sat (and) were drinking wine", one could posit a triangle

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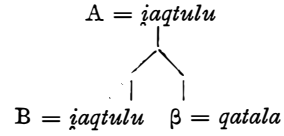


where the relation B:β would be functionally identical with Pol. *pisał* : *napisał*, Gr. ἔγραφε : ἔγραψε. Drawing such a parallel would be, however, a methodical mistake, since in B the meaning of *ʾaqtulu* is context-conditioned, i. e., secondary, not primary like in A. The opposition would be that of a system-conditioned β versus a context-conditioned B.

There is still more to it. A binary system like Ar. *ʾaqtulu*: *qatala* excludes not only the category of aspect, but also the category of tense. Whereas A: B is not rendered by different forms, the fundamental relation A:β is neither one of aspect nor one of tense. Its correct definition is *simultaneity* (or *non-anteriority*) versus *anteriority*. This is what may be called the *overall meaning* or *value* (French *valeur*, German *Gesamtbedeutung* [1]) of A/β. From the point of view of the system it is the only pertinent definition. The different individual meanings of both *ʾaqtulu* and *qatala* are the result of their use in different contexts (linguistic situations). One of them is the chief or primary meaning (German *Hauptbedeutung* [1]), all the rest are subsidiary or secondary meanings (German *Nebenbedeutungen* [1]).

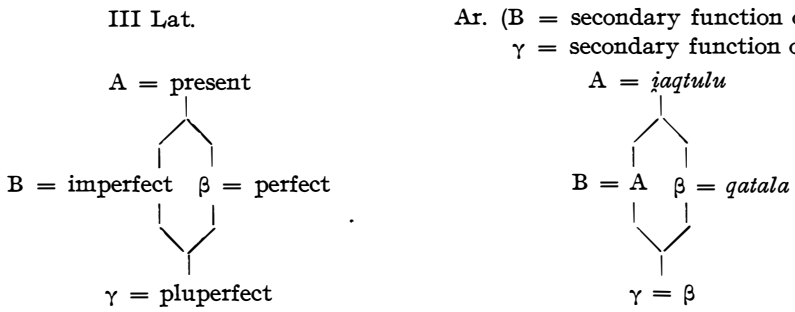
The primary meaning of *ʾaqtulu* is action simultaneous with the moment of speaking; the primary meaning of *qatala* is action prior to the moment of speaking. Teaching the student that *ʾaqtalu* is a present, and *qatala* a preterite may therefore serve as a first introduction to the verbal system of Ar.

Context-conditioned, i. e., secondary, functions of *ʾaqtulu* and *qatala* are due to their referring to a past or a future moment. They may correspond, as regards their semantic function, to the Lat. imperfect or future (*ʾaqtulu*), to the Lat. pluperfect or second future (*qatala*).

The binary system *ʾaqtulu* : *qatala* is thus functionally related to the Lat. opposition *infectum* : *perfectum*, and not to the Slavic opposition as represented by Pol. *pisać* : *napisać*, *zabijać* : *zabić*, etc. Ar. *ʾaqtulu* : *qatala* is comparable

[1] Cf. R. Jakobson, *Travaux du Cercle Linguistique de Prague* VI (1936), p. 244.

to Lat. *amāt / amābat / amābit* (infectum): *amāvit / amāverat / amāverit* (perfectum), but whereas the difference of tense belongs in Lat. to the *system*, it is in Ar. a matter *of context*:



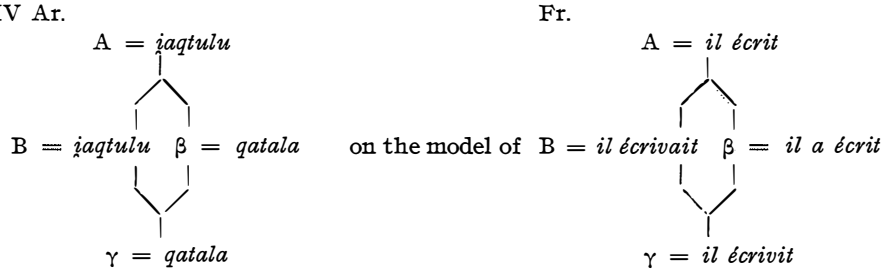
It is the lack of the category of grammatical tense which has induced scholars to speak of verbal aspect in Semitic, especially in Ar. Owing to imperfect functional analysis the term *aspect* seems to have adopted a kind of negative meaning ("if not tense, then aspect"). Now the term has a precise meaning in Slavic linguistics where it has been created (Russian *vid*) and always used as a technical one. It could be easily adopted in descriptions of the Greek verb. Both in Slavic and in Greek aspect is defined as a *semantic* feature characterizing the verbal action: *imperfective / perfective* or *linear / punctual*. This being the case it is not the desinences but the verbal *stem* (theme) which is the exponent of aspect. Cf. the differences *pisał* : *napisał* or *zabijał* : *zabił* as in Pol., or γραφε/ο–: γραψα– as in Greek. In a certain measure the relation between the two stems is a *derivative* one. Thus in Pol. the perfective *napisać* is derived from the imperfective *pisać* by means of prefixation (*na–*), and *vice versa* the imperfective *zabijać* stems from the perfective *zabić* via addition of the suffix *–aj–* (occasionally accompanied by a morphonological change of the root vowel). In Germanic the exponent of verbal aspect was originally the prefix *ga–* forming perfective stems, cf. Greek ἀπέθνῃσκεν (Lk 8,42): ἀπέθανεν (Lk 8,53) rendered by Gothic *swalt* : *gaswalt*. This quasi (or partially) derivative relation between imperfective and perfective accounts for the fact that the perfective has an imperfective counterpart not only in the personal forms, but also in the infinitive and in the participles. Cf. Gr. ἔλειπε: ἔλιπε, λείπῃ: λίπῃ, λείποι: λίποι, λεῖπε: λίπε, λείπων: λιπών, λείπειν: λιπεῖν. The present of the indicative is the point of neutralization of this opposition.

There is no trace in Ar. of such oppositions. There is only one form for the subjunctive (*ʾaqtula*), the "jussive" (*ʾaqtul*), the imperative (*uqtul*), the active or passive participle (*qātil, maqtūl*). The choice of the maṣdar (*qatl* etc.), if dependent on semantic factors at all, is influenced only by the diathesis of the verb. The only non-modal opposition of personal verb forms is *ʾaqtulu* : *qatala* equal to *simultaneity* (or non-anteriority): *anteriority*, tense being context-conditioned.

Since we are interested in aspect, the next question will be whether there are textual constellations apt to create shades of meaning identical with or related to verbal aspect. In Lat. or in the Romance languages simultaneity with a moment of the past and anteriority with relation to the moment of speaking are sharply distinguished, e. g., Fr. *il écrivait*, It. *scriveva* (imperfect):

il a écrit, ha scritto (perfect) [2]. Although in both cases the action denoted is a past one, the point of reference is a moment of the past in the former, the moment of speaking in the latter case. From the point of view of the *system* there is no direct opposition between these forms, *il écrivait* : *il écrit* (opposition of tense) and *il a écrit* : *il écrit* (opposition of time-reference) being the only relevant ones. The opposition *il écrivait*: *il a écrit* is indirect and *complex*, its terms differing by *two* features: *simultaneous* with a *moment of the past* versus *prior* to the *moment of speaking*. This opposition becomes a simple one if the perfect is replaced by what is called in classical French *passé défini* (*il écrivit*) or in Italian *passato remoto* (*scrisse*). In the opposition *il écrivait* : *il écrivit* (*scriveva* : *scrisse*) both terms denote simultaneity with a past moment, the semantic difference between them consisting in *aspect* (linearity or imperfectivity versus punctuality or perfectivity). There is, however, no further development of this germ of aspect in Romance, the chief obstacle being the fact that it is not represented by a special stem allowing the formation of the imperative, infinitive, or participle. In languages like South German, disposing only of two forms, viz. *er schrieb, er hat geschrieben*, the latter form combines the functions of both *il a écrit* and *il écrivit*. This means that even the trace of aspect we discover in Romance, is in Germanic a secondary, context-conditioned function (opposition between *er schrieb* and the secondary, narrative, function of *er hat geschrieben*).

These examples show how far we are from attributing aspect to Semitic, especially to Arabic. In order to look for it it would be necessary to transform the opposition *ʾaqtulu* (simultaneity): *qatala* (anteriority) into a quadrangle



An opposition of aspect comparable to *il écrivait* : *il écrivit* could be therefore found only between B and γ, *both* of them representing *secondary* functions in Ar. (B secondary function of A; γ secondary function of β).

The two forms *ʾaqtulu* and *qatala* necessarily cover all meanings and shades expressed by verbal systems with a greater number of terms (e. g., Latin six, French eight in the indicative). Both in quadrangle III, where the subsidiary axis B/γ refers to a moment of the past, and in quadrangle IV, where it refers to aspect, French disposes of special terms whereas in Ar. B/γ is identical to A/β [3].

[2] The terms *imperfect, perfect*, relating to tense, have nothing to do with *imperfective, perfective* denoting aspect, though some confusion may be due to their etymological relationship. Cf. also German *perfektisch*, term of time-reference, as against *perfektivisch* (aspect).

[3] In Pol. and Russian we have β = γ owing to the merger of the old aorist (γ) with the old perfect (β) in favour of the latter. But in spite of this fact,

Secondary or context-conditioned semantic functions can be established only by contrastive studies. Thus e. g., a time-reference necessitating the use of a pluperfect or a second future cannot be established by translating Ar. into Pol. or Russian. On the other hand, a context-conditioned shade of aspect (diagram IV, B/γ) would necessarily escape a German translator. Secondary semantic functions do not belong to the system of language, they are not system-conditioned, but context-conditioned (*systembedingt* : *feldbedingt*, cf. Bühler, *Sprachtheorie* [1934], pp. 183 ff.). The context responsible for secondary semantic functions is the same which makes us translate an Ar. verbal form by a Fr. pluperfect or second future, by a Pol. imperfective preterite etc.: *it is the context of the translation.*

In defining the binary system of the Ar. verb we are therefore obliged to contest the existence of the category of aspect (like in Slavic) or of detailed time-reference (like in Romance). There is only one of general time-reference (*simultaneity*: *anteriority*), tense being as a rule context-conditioned.

In binary verbal systems *aspect* can be only a *context-conditioned tertiary* function of the verb. The opposition between *ʾaqtulu referring to a moment of the past* (secondary function of *ʾaqtulu*) and *qatala* is interpreted as *imperfective* versus *perfective* action if an additional condition is fulfilled, viz., if *qatala* is used as a *narrative* tense, denoting an action simultaneous with a definite moment of the past (primary function of *qatala*: action prior to the moment of speaking). This tertiary relation *ʾaqtulu* : *qatala* (*imperfective* past: *perfective* past) may be carried over into the sphere of the future since *ʾaqtulu* may also have the secondary function of referring to a moment of the future. Hence *ʾaqtulu* = imperfective future, *qatala* = perfective future.

In this way both forms, *ʾaqtulu* and *qatala*, may adopt secondary and tertiary functions, thus

	secondary functions	tertiary functions
of *ʾaqtulu*	imperfectum	imperfective preterite
	futurum	imperfective future
of *qatala*	plusquamperfectum	perfective preterite
	futurum exactum	perfective future

As a matter of fact a context-conditioned perfective future of *ʾaqtulu* is attested in Heb., the so-called *perfectum propheticum*. E. g., Is. 5,13 *lâchẹn gâlâ ʿammī mibbelī-dâʿaṯ* "verily my people will go into exile unexpectedly".

It is probable that the binary system of Ar. has been historically preceded by a ternary one. The old Sem. form expressing anteriority was *ʾaqtul* (as against *ʾaqtulu*). This form is in Akk. the normal preterite. Ousted in Western Sem. by *qatala* it still survives in Ar. as *allomorph* of *qatala*, cf. *qatala* : *lam ʾaqtul* (besides *mā qatala*), or with a modal function (optative or imperative or else potentialis / irrealis in conditional sentences). The substitution of *qatala*

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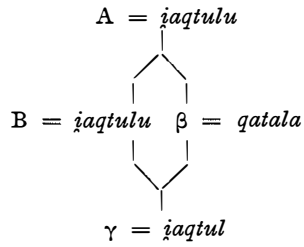
i. e., of γ being only a secondary function of β, the relation β:γ is an expression of genuine aspect, since the latter has *its own forms* in the imperative, in the infinitive etc. Thus whereas Pol. *napisał* denotes primarily anteriority as against *pisze*, and only secondarily perfectivity as against *pisał*, forms like *napisz* (imperative), *napisać* (infinitive) are primarily perfective versus imperfective *pisz*, *pisać*.

for *ʾaqtul* in W. Sem. must have been gradual, i. e., reached by stages, cf. the use of both forms in the Canaanite glosses of el-ʿAmarna. There must have been a period when the functions β and γ were distributed between the two forms; β = *qatala* (*passé indéfini*), γ = *ʾaqtul* (*passé défini* or narrative tense). The underlying system was

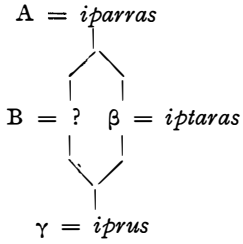
V



The expression of aspect (B: γ) is still context-conditioned, there is no special form for B which is only a secondary of A. But we are a step nearer to aspect than in Ar. where both B and γ are secondary functions.

After Landsberger's discovery of the Akk. perfect it is possible to posit for this languages the following system:

VI



As regards the stative (permansive) *paris* its opposition to the above forms is complex, the complicating factor being *diathesis* (passive versus active with transitive verbs). But *iptaras*: *iprus* may well be compared to the relation *passé indéfini* : *passé défini* of classical French.

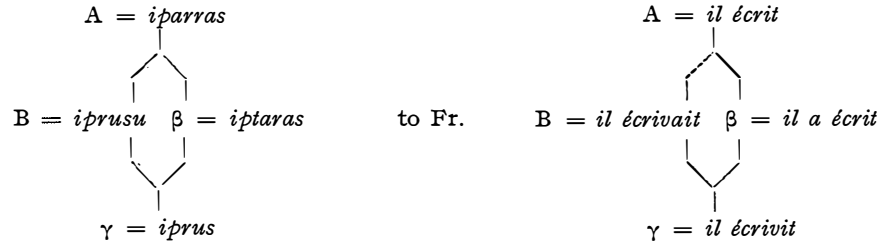
The rendering of B (simultaneity with a moment of the past) is a moot question. The Akk. renewal of A (rendered in Common and W. Sem. by *ʾaqtulu*) [4] did not force *ʾaqtulu* out of the cadre of the conjugation. It was preserved as an expression of certain *secondary* functions of the old form, functions characterized henceforward by the morpheme *-u*. The difference *ʾaqtulu* : *ʾaqtul* became in Akk. preterite (*ʾaqtul*): " subjunctive" (*ʾaqtulu*). The latter form is scarcely modal. It is generally defined by syntactical criteria (relative sentences etc., cf. von Soden, *GAG* p. 211). It is probable that B (originally :

[4] There can scarcely be any doubt about the chronological relation between the types *ʾaqtulu* and *iparras*, the former being radical, the latter enlarged (gemination of R_2). In Common Sem. **ʾaqattalu* had the status of a *derivative*, cf. Engl. *he writes>he is writing*, originally with an iterative, then with a durative meaning.

simultaneousness with a past moment) being in Sem. a secondary function of A (simultaneousness) the renewal of *ʾaqtulu* by *ʾaqattal* (*iparras*), leaving intact the function B, has provided B with a special formative (*ʾaqtul–u, iprus–u*). Whereas in Ar. *ʾaqtulu* serves both for A and B, the two functions can have different exponents in Akk. owing to the formal renewal of A. [5].

The problem now is whether the Akk. type *iprusu* [6] functioned as an equivalent of Ar. *ʾaqtulu* in sentences like *ʾalasū n–nāsu ʾašrabūna l–ḫamra*, i. e., whether it was used to express simultaneity with (another) past action. If so, it may be compared to an European imperfect and the original Akk. system

VII



If the above inference is correct, verbal aspect (B: γ) can be claimed for Akk. though only in the restricted sense applicable to Romance.

[5] The renewal did not consist in the creation of a new form, but in the shift *derivative>inflectional form*, i.e., in the incorporation of a *derivative* into the system of *conjugation*.

[6] The "subjunctive" *iparrasu* must be of course a later formation.

Die Modi beim älteren akkadischen Verbum

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I

Wer sich über die grammatische Kategorie Modus (Aussageweise; Gefühls-, Willens- und Urteilsbeteiligung des Ich) [1] beim akkadischen Verbum unterrichten will, findet in der Fachliteratur keine befriedigende Antwort, sondern viele, einander zum Teil widersprechende Darstellungen. Auch sucht er vergebens nach einer Definition dessen, was unter "Modus" zu verstehen sei. Die Autoren, meint er zu erkennen, orientieren sich vorwiegend an den klassischen indogermanischen Sprachen oder an der arabischen (National)grammatik. Es folgen hier kurze Resümees einiger Arbeiten der Jahre 1952-1969, soweit sie die akkadischen Modi betreffen:

1. W. von Soden, *GAG* (1952) § 74b: "An eigentlichen Modi gibt es nur den Imperativ (§ 81a); der sog. Subjunktiv ist eine Abhängigkeitsform (§ 83)". Ebd. spricht von Soden von besonderen "Wunsch- und Verbotsformen (§ 81)", ohne sie jedoch als Modi zu bezeichnen. Erwähnt werden noch "modale Partikeln" (§ 121d-f). Die Syntax enthält einen Abschnitt "Modale Modifikationen in Aussagesätzen" (§ 152), getrennt von einem Abschnitt über "Befehls–, Verbots– und Wunschsätze" (§ 154); schliesslich ein Kapitel über "Die Ausdrucksmittel für den Eid" (§ 185). Einige Nachträge zu den Modi stehen in *AnOr* 47 (1969) S. 16** f.

2. Th. Jacobsen, *"ittallak niāti"* in *JNES* 19 (1960) 101-116 (= Toward the Image of Tammuz [1970] 271 ff.) beschreibt ein sich überschneidendes "person-gender-number" System beim akkadischen Verbum. S. 114 [289] findet sich eine Tabelle "Modal Suffix System", wo jedem suffigierten kurzen oder langen Vokal eine bestimmte, auch modal relevante Funktion zugeschrieben wird; z.B. *–u* = *a*) "closed individual autonomy", *b*) "predicatively individualizable", *c*) "for the nonce inapproachable". Die letztgenannte Funktion finde sich beim *–u* im "modus iurandi" (s. unten VII 2, 8) "in the sense of expressing a disinterested, detached, hands-off attitude in the speaker suitable to a form of statement demanding strictly objective presentation". Der Gebrauch von *–u* im Subjunktiv wäre nach J. sekundär und "no more than a purely syntactical restriction to use in clauses".

3. B. Kienast, "Das Punktualthema **japrus* und seine Modi" in *Or* 29 (1960) 151-167, bezeichnet Indikativ, Subjunktiv und Ventiv als ursprüngliche Modi des Akkadischen (S. 151, 159). Er schlägt "Relativ" als passendere Bezeichnung für den Subjunktiv vor. Die Endungen *–u* bzw. *–ni/–a* seien vermutlich nachgestellte Demonstrativa. Imperativ, Prekativ etc. werden nicht erwähnt.

[1] Vgl. zum Terminologischen z. B. B. Lewin, *Abriss der japanischen Grammatik* (1959) 172 ff.

4. I. J. Gelb unterscheidet in *MAD* 2_2 (1961) "Old Akkadian Writing and Grammar" S. 169-174 als Modi: "Indicative, Allative [= Ventiv], Subjunctive, Imperative, Precative, Prohibitive [= Vetitiv]".

5. I. M. Diakonoff, *Semito-Hamitic Languages* (1965), beschreibt S. 90 ff. das ursprüngliche Modussystem des Akkadischen so (Übersichtstabelle S. 92): **ja–prus, ji–pras* = 1) "Perfective Indicative", 2) "Jussive, neutral aspect"; **ja–prus–u, ji–pras–u* = "Perfective Subjunctive I (form of the Verb in all subordinate clauses except conditional clauses which require the Jussive)"; **ja–prus–a, ji–pras–a* = "Perfective Subjunctive II (preserved only in one dialect of Old Akkadian, original function unknown)"; **ja–paras, ji–paras* = "Imperfective Indicative"; **ja–paras–u, ji–paras–u* = "Imperfective Subjunctive". Dabei dient der "Jussiv" **ja–prus* auch als Basis für den "Precative" (**lū ja–prus*) und den "Prohibitive" (**ai ja–prus*).

D. schliesst den Imperativ aus als eine "form apart", die "stand in no formal relation to the modal forms under discussion" (S. 91 Anm. 90).

D. folgt (S. 91 Anm. 88) der These von A. P. Riftin, wonach die Subjunktivendungen *–u, –a* ursprünglich die Funktion hatten, Verbalformen zu nominalisieren [2].

Vgl. auch dens. Autor in *Jazyki drevnej perednej Azii* (1967) 253 ff. betr. Modi (наклонения). Der Ventiv ist nach D. kein Modus.

6. E. Reiner, *A Linguistic Analysis of Akkadian* (1966) nennt S. 71 folgende Modi: "indicative (unmarked), subjunctive (suffixed), and ventive, also called allative (also suffixed)". Sie erwägt ferner ("it is possible to consider as a mood" S. 71 f.), auch das "cohortative", "optative", "imperative" und "vetitive" darstellende Paradigma einzubeziehen; doch sie betont, dass sich die letztgenannten strukturell von den ersteren unterscheiden.

7. W. Eilers, "Der sog. Subjunktiv des Akkadischen" in *Gedenkschrift W. Brandenstein* (1968) 241-246, hält den "von den Assyriologen" so bezeichneten Subjunktiv für den eigentlichen Indikativ auf *–u*, der sich nur in Relativsätzen, in anderen Nebensätzen (= verkappte Relativsätze) und im Eid erhalten habe. In Hauptsätzen habe der Indikativ seine Endung verloren. E. vermutet gerade im 'Subjunktiv' beim Eid, "wo die Aussageform den stärksten, man möchte sagen feierlichen Klang hat" (S. 245), alte indikativische Verwendungsweise.

8. I. J. Gelb, *Sequential Reconstruction of Proto-Akkadian* (= *AS* 18, 1969) 69 ff., 75 ff., 98 ff., 112 nähert sich der von den arabischen Nationalgrammatikern vertretenen These, die vokalischen Morpheme, die die Modi des Verbums bezeichnen, seien gleicher Herkunft wie die vokalischen Kasusmorpheme des Nomens (daher S. 69 die Überschrift "Case/Mood"). Er steht damit der These von A. P. Riftin nahe (s. oben I 5). "Semitic languages have two primary moods, indicative and subjunctive, characterized by the markers *u//a*".

Der Imperativ habe keinen Modus. Als "other moods" (S. 112) nennt G. "several secondary 'moods' in Semitic languages variously subsumed under such headings als ventive/allative, (co)hortative/volitive, energic, jussive, and imperative(!)".

9. L. Matouš, *Grammatik des Akkadischen*$_5$ (1969) § 59 (S. 68 f.), schliesst sich eng an W. von Soden an: Indikativ, Subjunktiv, Imperativ und — gesondert — "Wunsch- und Beteuerungsformen" (Prekativ, Vetitiv, Prohibitiv).

[2] Vgl. dazu Verf., *RA* 61 (1967) 149.

II

Angesichts so vieler verschiedener Meinungen erscheint der Versuch lohnend, ja das Vorhaben dringend, den Modus im Akkadischen neu zu analysieren. Wir sahen Inkonsequenzen, die etwa dadurch entstanden, dass man grundsätzlich Modus mit Konjugationsmuster gleichsetzte, so dass man in Kauf nehmen musste, eine Aussageform (Indikativ) im Relativsatz als einen anderen Modus (Subjunktiv) zu bezeichnen. Oder man nahm implicite die Möglichkeit hin, in ein und derselben Form wie *liprusam* zwei 'Modi' (Prekativ + Ventiv) zu kumulieren. Der Verzicht auf eine Definition des "Modus" führte dazu, dass ein Imperativ *alik* "geh!" als Modus, die Formen *lillik* "er möge gehen" oder *lū illik* "er ist bestimmt gegangen" hingegen als "Formen der Beteuerung" oder als "secondary moods" bezeichnet wurden.

Die folgende Argumentation bleibt von komparatistischen Gesichtspunkten zunächst frei; der seit ca. 700 n. Chr. sicher nachweisbare Indikativ des Arabischen wird zunächst nicht herangezogen. Erst unten in Abschnitt XIII und XIV soll das akkadische Modussystem kurz mit dem anderer Sprachen verglichen werden.

III

J. Marouzeau beschreibt den Modus in seinem *Lexique de la terminologie linguistique*$_3$ (1951) S. 147 s.v. "mode" folgendermassen: "Caractère d'une forme verbale susceptible d'exprimer l'attitude du sujet parlant vis-à-vis du procès verbal, c'est-à-dire en un certain sens la manière (lat. modus) dont l'action est présentée par lui, suivant par exemple qu'elle fait l'objet d'un énoncé pur et simple (mode indicatif) ou qu'elle est accompagnée d'une interprétation: modes subjonctif, optatif, impératif, injonctif, conditionnel ... [nähere Definitionen unter den einzelnen Stichwörtern] Il arrive que la valeur propre du mode soit altérée ou supprimée par le jeu du mécanisme syntactique, et que le mode aboutisse à n'être plus qu'une forme grammaticale dépourvue de sens propre, mais imposée par la structure de la langue; c'est ce qu'on appelle proprement le mode grammatical".

Diese Definition lässt bei "forme verbale" offen, wie diese abzugrenzen sei. Der ital. "imperativo" der 1. Pl. andiamo "gehen wir!" ist eine synthetische, nicht auf eine kleinere Einheit reduzierbare Form; wie steht es dagegen mit engl. let's go, das dem ital. andiamo im Gemeinten[3] genau entspricht? Mit anderen Worten: kann eine Sprache nur soviele Modi haben, wie sie synthetische Formen zu bilden imstande ist? Diese Annahme ist unwahrscheinlich[4].

[3] Zum "Gemeinten" vgl. grundsätzlich E. Koschmieder in *Beiträge zur allgemeinen Syntax* (Sammelband 1965) 70 ff.: "Die noetischen Grundlagen der Syntax"; 90 ff.: "Aus den Beziehungen von Sprache und Logik"; 101 ff.: "Das Gemeinte"; 107-115: "Das Problem der Übersetzung" passim.

[4] J. Gonda, *The Character of the Indo-European Mood* (1956) betont S. 5 die Notwendigkeit, zwischen "real moods (φέροι)" und "combinations with auxiliary verbs (he may bear)" zu unterscheiden; er bringt S. 6 ff. Beispiele für Modusbildung in verschiedenen nichtindogermanischen Sprachen. Ebenda werden E. Schwyzer-A. Debrunner, *Griech. Gramm.* II (1950) 304 zitiert: " ... die Beziehungen, die die Modi ausdrücken, können auch durch andere Mittel, und zwar in grösserer Fülle und genauer ausgedrückt werden, durch Hilfsverben wie nhd. wollen ..., oder durch Adverbia und Partikeln wie nhd. wohl vielleicht ... Auch Sprachen, die formal mehrere Modi unterscheiden, können solche Hilfsmittel benutzen".

IV

In jeder Sprache kann man eine oder mehrere Personen zu etwas auffordern und sich selbst dabei einschliessen: "Gehen wir!", "let's go!", "allons-nous-en!", usw. Die sprachlichen Mittel hierfür variieren von Sprache zu Sprache; aber selbst in ein und derselben Sprache sind verschiedene Ausdrucksweisen möglich: "gehen wir!", "wir wollen gehen!", "lasst uns gehen!", "lasset uns beten!". Alle diese Formen sind von der Aussageform "wir gehen", "wir beten" verschieden, unter sich aber in der Bedeutung identisch oder doch nur durch minimale Nuancen unterschieden ("gehen wir!" ist eine etwas eindringlichere, weniger höflichere Form als "lasst uns/lassen Sie uns gehen!"). Das Verhältnis von "wir gehen" zu "gehen wir!", von "we go" zu "let's go!" etc. ist im Gemeinten dasselbe wie das von lat. imus zu eamus, akk. *nillak* zu *i nillik* etc., doch sind die Formen bald synthetisch, bald analytisch gebildet.

Von der einfachen Aussage "ich habe mir ein Motorrad gekauft" ist unterschieden der Satz "ich habe mir ein M. gekauft, aber ehrlich!". Während der erste Satz die Reaktion des Zuhörers nicht weiter in Betracht zieht, setzt der zweite von vornherein Misstrauen, Ungläubigkeit auf Seiten des Zuhörers voraus. Andere Mittel sind die Adverbien "gewiss", "bestimmt" oder, in der Übersetzungssprache noch fest verwurzelt, "wahrlich", "fürwahr", engl. "verily" u.a.m. Das Sumerische besitzt hier eine synthetische Verbalform, gebildet mit dem Präformativ ḫé– und der *ḫamṭu*–Basis des Verbums: ḫ é – ĝ a r [5]. Das Akkadische sagt, wenn positiv, *lū aprus*, wenn negativ, *lā aprusu*, wobei die negative Form identisch ist mit der Form des negativen assertorischen Eides.

Die hier angeführten Beispiele demonstrieren modale Unterschiede. Wir wollen nun versuchen, einen Überblick über die Modi des Akkadischen zu erhalten. Dieser Versuch ist vorläufig, und er macht sich nicht anheischig, einen vollständigen Katalog der Modi "im Gemeinten" zu bieten. Wir ordnen zunächst nach der Form, und zwar *a*) nach synthetisch, *b*) nach analytisch gebildeten Formen [6] ohne Berücksichtigung von Perfekt und Stativ; für diese s. unten X und XI. Wir unterscheiden grundsätzlich die positive und die negative Form und prüfen ferner in jedem Fall, ob bestimmte Konjugationsreihen vollständig sind, d.h. in allen drei Personen Sg. und Pl. vertreten, oder aber unvollständig. In der Regel erscheint nur die altbab. Form; bei Bedarf wird die altass. und oder altakk. zusätzlich notiert.

1. Synthetisch.

11. *aprus, aparras*
taprus, taparras
etc.

12. (*ša*) *aprusu, aparrasu* [7]
(*ša*) *taprusu, taparrasu*
(*ša*) *iprusu, iparrasu*
(*ša*) *niprusu, niparrasu*

[5] Vgl. Verf., *ZA* 61 (1971) 214-216.

[6] Diese Unterscheidung ist konventionell gemeint. *ul iprus* wird als analytisch bezeichnet, da es aus zwei lexikalischen Einheiten besteht. Tatsächlich war *ul* aber wohl kein autonomes, frei produzierbares Element.

[7] An die Stelle des Relativpronomens kann auch eine Subjunktion (*inūma*) oder ein Nomen im st. c. (*bīt, ašar*) gesetzt werden.

13. *aparrasu*
    niparrasu
14. **aprusu*
    taprusu
    **iprusu*
    **niprusu*
15. *luprus*
    liprus
    liprusū/ā
16. *purus*
    pursī
    pursā

11 = Indikativ, bildbar von den Basen /prus/ und /parras/ [8]; vollständiges Konjugationsmuster.

12 = "Subjunktiv", bildbar von den Basen /prus/ und /parras/ [7]; unvollst. Konj.muster [9].

13 = positiver zukunftsbezogener Affirmativ, bildbar von der Basis /parras/; unvollst. Konj. muster. Die Formen sind wahrscheinlich vertauschbar mit *lū aparras / niparras* (V 28).

14 = positiver vergangenheitsbezogener Affirmativ, bildbar von der Basis /prus/; unvollst. Konj.muster. Die Formen (aB nur 2. Sg. bezeugt) sind wahrscheinlich vertauschbar mit *lū aprus* etc. (V 27).

15 = Prekativ, bildbar von der Basis /prus/; unvollst. Konj.muster.

16 = Imperativ, bildbar von der Basis /prus/; unvollst. Konj.muster.

11, 15 und 16 sind vollständig ergänzbar durch die komplementär verteilten Endungen *–am/–m/–nim* des Ventivs. S. dazu unten S. 127.

2. Analytisch.

21. *ul aprus, ul aparras*
    etc.
22. (*ša*) *lā aprusu, aparrasu* [7]
    (*ša*) *lā taprusu, taparrasu*
    (*ša*) *lā taprusī, taparrasī*
    (*ša*) *lā aprusu, aparrasu*
    (*ša*) *lā niprusu, niparrasu*
    (*ša*) *lā taprusā, taparrasā*
    (*ša*) *lā iprusū/ā, iparrasū/ā*
23. *lā iparras*
    lā taparras
    etc.
24. *lā aparrasu*
    lā taparrasu
    lā iparrasu
    lā niparrasu

[8] Zu /ptars/ (Perfekt) s. unten X, zu /pars/ (Stativ) s. XI.

[9] Das Altass. erzielt mittels der komplementär verteilten 'Subjunktiv'-Endungen *–u, –uni/–ni, –ni* ein vollständiges Konjugationsmuster.

25. *lā aprusu*
    lā taprusu
    lā iprusu
    lā niprusu
26. *aj aprus*
    ē taprus
    ē taprusī
    aj iprus
    ē niprus
    ē taprusā
    aj iprusū|ā
27. *lū aprus*
    lū taprus
    etc.
28. *lū aparras*
    lū taparras
    lū taparrasī
    lū niparras
    lū taparrasā
29. (*i taprus*)
    i niprus

21 = der negierte Indikativ, bildbar von denselben Basen wie 11; vollst. Konj.muster; Negation *ul* [10].

22 = der negierte "Subjunktiv", bildbar von denselben Basen wie 11; Negation durchweg *lā*. Die Endung *-u* steht in denselben Fällen wie bei 12 [11].

23 = der Prohibitiv, bildbar von der Basis /parras/; vollst. Konj.muster; Negation durchweg *lā*.

24 = negativer zukunftsbezogener Affirmativ bzw. die Form des negativen promissorischen Eides; bildbar von der Basis /parras/; unvollst. Konj.muster; Negation *lā* und "Subjunktiv"-Endung *-u* [12].

25 = negativer vergangenheitsbezogener Affirmativ bzw. die Form des negativen assertorischen Eides; bildbar von der Basis /prus/; unvollst. Konj.-muster; Negation *lā* und "Subjunktiv"-Endung *-u*.

26. = der Vetitiv, bildbar von der Basis /prus/; vollst. Konj.muster; Negation *aj/ē* in komplementärer Verteilung.

27 = positiver, vergangenheitsbezogener Affirmativ, bildbar von der Basis /prus/; vollst. Konj.muster.

28 = positiver, zukunftsbezogener Affirmativ, bildbar von der Basis /parras/; unvollst. Konj.muster. Die 1. Person ist als Form des positiven promissorischen Eides wahrscheinlich vertauschbar mit *aparrasu/niparrasu* (V 13); die 2. Person ist zugleich auch Prekativ.

29 = Kohortativ Pl. und seltene Form des Prekativs in der 3. f. Sg., bildbar von der Basis /prus/; unvollst. Konj.muster.

[10] Var. *ula*; in Bedingungs- und Fragesätzen *lā*.
[11] Altass. entsteht ein vollst. Konj.muster bereits vermittels der in Anm. 8 genannten Endungen.
[12] S. unten Anm. 38.

21, 23, 26-29 sind vollständig ergänzbar durch die komplementär verteilten Endungen *–am/–m/–nim* des Ventivs. S. dazu unten.

VI

Dieses rein aus der formalen Analyse gewonnene Bild befriedigt nicht. Form und Bedeutung überschneiden einander. *lā aparrasu* "ich werde bestimmt nicht ..." (24) ist formal identisch mit (*ša*) *lā aparrasu* "(..., das) ich nicht ... werde" (22), d. h. einmal negativer Affirmativ der Zukunft und einmal 'Subjunktiv' (= Indikativ im Nebensatz). Der Satz *bītam abni* "ich habe ein Hauss gebaut" bleibt Aussagesatz, d.h. indikativisch, auch nach Umwandlung in einen Relativsatz: → *bītam ša abnû* "das Haus, das ich gebaut habe". Es ist also nicht berechtigt, *abni* und *abnû* (= *abni*+*u*) als zwei verschiedene Modi zu bezeichnen. Vielmehr müssen wir davon ausgehen, bei bestimmten Personen (1. Sg. und Pl., 2 m. Sg., 3 Sg.) im Assyrischen sogar durchweg, z w e i Formen der Indikativs anzusetzen. Dass hier ein ursprünglich gesonderter Modus seiner Bedeutung entkleidet und als "mode grammatical" im Sinne Marouzeaus verwendet wurde, ist denkbar; doch lässt sich dies historisch nicht deduzieren [13].

Mit aller Entschiedenheit ist der Ventiv als Modus abzulehnen. B. Landsberger hatte zwar in seinem grundlegenden Aufsatz zum "Ventiv" [14] von einem Modus gesprochen; dem widersprach er jedoch implicite dadurch, dass er die Ventivendungen hinsichtlich ihrer Funktion als "Richtungsexponenten" bezeichnete. In der Tat lässt sich der gesamte Formenbestand des akkadischen Verbum finitum (den Stativ nicht einbegriffen) aufteilen in Nichtventiv und Ventiv. Ein nichtventivischer Indikativ *illik* "er ging" erfährt, wenn man ihn in den Ventiv umsetzt: → *illikam* "er kam", eine Änderung der Richtung, aber nicht der subjektiven Aussageweise, des Modus. Entsprechend bleibt ein Prekativ *liddin* "er möge geben" bei Umsetzung in den Ventiv: → *liddinam* "er möge mir geben/hergeben" modal dasselbe. Wir können den Ventiv daher im Folgenden ganz ausser Betracht lassen.

B e m e r k u n g 1: Nichts mit dem Ventiv zu tun haben aAK und AB, selten auftretende indikativische Verbalformen, die statt auf zu erwartendes *–u* im Nebensatz ('Subjunktiv') auf *–a* enden [15]. Ihnen entspricht in der 3. Pl. m. eine Formen auf *–ūna* [16]. Diese Formen sind noch zu selten, als dass man sie schon sicher interpretieren könnte. Spekulation darüber, ob akkadischer Dialekt [17] oder etwa amurritisches Adstrat, bleibt einstweilen unergiebig.

B e m e r k u n g 2: Th. Jacobsen hat in *JNES* 19 (1960) 111 Anm. 12 einen "i-Modus" dargestellt, den er vorläufig als "a mode of compelled action"

[13] Zu Rekonstruktionsversuchen eines 'ur–' bzw. 'protosemitischen' Modussystems s. unten S. 140 mit Anm. 68.

[14] Der "Ventiv" des Akkadischen, *ZA* 35 (1923/24) 113-123.

[15] Diskutiert u.a. von I. J. Gelb, *MAD* 2_2 (1961) 170 f.; I. M. Diakonoff (oben I 5). B. Kienast, *Or* 29 (1960) 152 f. Anm. 2 deutete die aAK 'Subjunktive' auf *–a* als mimationslose Ventive.

[16] I. J. Gelb, ebd. S. 170; *UET* 5, 265 Hülle 5-12 (A) = Tafel 4-11 (B) ist mir unklar; dort B 9 *la i–pa–ša–ru–na* (fehlt A); A 12 *la i–qá–bu–na* (B 11 *ú–la i–qá–bi*).

[17] So I. M. Diakonoff, s. oben I 5.

bezeichnete; vorherige Notierung von Formen durch W. von Soden, GAG § 82e sowie *Or* 24 (1955) 386, und durch B. Landsberger (apud J.). Die betreffenden Formen zeichnen sich dadurch aus, dass an konsonantischen Auslaut ein /i/ antritt, wobei der Auslautkonsonant graphisch wiederholt wird: z.B. *i-ḫa-ba-at-ti* (*iḫabbatti*) statt *iḫabbat*. In *Or* 30 (1961) 160 f. lehnt von Soden Jacobsens Deutung ab (eingehende Besprechung auch neuer Belege), gelangt allerdings nur zu dem vorläufigen Ergebnis, "dem *-i* eine stark hervorhebende Funktion zuzuschreiben". Jacobsen bestärkt seine eigene Deutung in *JNES* 22 (1963) 27-29 ("The i-Modus"): Übersetzung der Formen mit "perforce" oder "had to" scheint ihm am besten geeignet. I. J. Gelb, *AS* 18 (1969) 106, notiert zurückhaltend "occurrences of Ind[icative] in *i* in texts from the Old Babylonian period on, where *i* often appears as a form of affectation in the speech or writing habits of women".

Zu ausführlicher erneuter Diskussion fehlt hier der Platz. Daher nur folgendes Argument: Bei Abstrahierung der bisherigen Belege zur Wurzel PRS erhalten wir *a*) 3. Sg. Prät. *iprussi*, *b*) 3. Sg. Präs. *iparrassi*, *c*) 3. Sg. Perf. *iptarassi*, d) 1. Sg. Prek. *luprussi*, *e*) 3. m. Sg. Stativ *parissi*, f) 3. f. Sg. Stativ *parsatti*. Dass die Längung des Endkonsonanten echt ist, zeigt sich daran, dass Perf. und Stativ nicht etwa **iptarsi* bzw. **parsi* lauten. Das bedeutet, dass die Formen durch mehr als nur durch ein Morphem /i/ charakterisiert sind. Kaum Beachtung gefunden hat in der bisherigen Diskussion, dass die Erscheinung überhaupt nicht auf das Verbum beschränkt ist, wie die pronominale Form *mammanni* "irgendwer" zeigt[18]. Wenn wir nur das Verbum heranziehen, ergibt sich eine Verteilung der Formen ähnlich der des Ventivs (nur der Imperativ **purussi*) fehlt noch in unserem Katalog). Ebensowenig wie nun aber das *-am* des Ventivs im Prekativ *liprusam* modaler Natur ist, kann es das *(-s)-i* in *luprussi* sein.

VII

Nach Ausscheidung von Ventiv, 'Subjunktiv', 'a-Modus' und 'i-Modus' aus der Skala der akkadischen Modi bleiben zu diskutieren; 1) der Indikativ, 2) der positive Affirmativ, 3) der Prekativ, 4) der Kohortativ der 1. Pl., 5) der Imperativ, 6) der Prohibitiv, 7) der Vetitiv, 8) der negative Affirmativ. Anschliessend müssen wir noch kurz auf die Ausdrucksmittel für den Irrealis eingehen.

1. Der Indikativ. Er enthält eine positive oder negative Angabe oder Aussage: (*ul*) *iprus* etc., *ša*/*inūma* (*lā*) *iprusu* etc.; s. V 11, 12, 21, 22. In Relativsätzen und sonstigen Nebensätzen, von denen sich viele auch als Relativsätze erklären lassen (*inūma* ..., *ašar* ...), wird ein Morphem /u/ unmittelbar suffigiert, falls nicht bereits ein Morphem /ī/ (2. f. Sg.), /ū/ (3. m. Pl.) oder /ä/ (2. f. und 3. f. Pl.) vorhanden ist. aAK und sehr selten aB kann statt /u/ auch /a/ antreten (s. oben VI Bem. 1). Im Assyrischen tritt das Morphem /ni/ verpflichtend ein in allen Fällen, die im Babylonischen nicht durch /u/ bezeichnet sind; für Bab. /u/ hat das Ass. /u/ oder /u+ni/ nach noch nicht klar erkannten Vertei-

[18] *LFBD* 4,28-30 *ina kīminanna mammanni ana mamma[n(?)] ul iḫabbatti qadum bīti-ja lā amatti* "derzeit kann keiner bei einem anderen ein Darlehen aufnehmen; dass ich und mein Haus nur nicht sterben!". *mamman*/*mammanni* und *iḫabbat*/*iḫabbatti* verhalten sich genau analog zueinander.

lungsregeln [19]. Die Herkunft des Morphems /ni/ ist noch ungeklärt [20]. /ni/ kann auch stehen, wenn wir virtuell eine Kopula ansetzen: *ša ina āli–ni* "der in der Stadt ist" [21]. Insofern ähnelt /ni/ der Partikel *lū*, die ebenfalls ohne Verbalform möglich ist: *lū šarrum anāku* "ich bin gewiss König".

Als Termini für die zwei möglichen und voraussagbaren Formen des Indikativs (*iprus, iprusu; taprusī, taprusīni; iprus, iprusuni*) könnte man vorschlagen "Indikativ 1" und "Indikativ 2"; doch empfiehlt es sich mehr, die traditionellen Bezeichnungen "Indikativ" und "Subjunktiv" beizubehalten. Der "Subjunktiv" im Nebensatz darf jedoch nicht mehr als ein Modus für sich aufgefasst oder — contradiction in adiecto — als "modus relativus" bezeichnet werden. Auch müsste auf jegliche Assoziation mit den Funktionen eines Subjunktivs in anderen Sprachen verzichtet werden.

2. Der positive Affirmativ. Eine Aussage wird bekräftigt, sei es, um echten oder eingebildeten Zweifel des Zuhörers auszuschalten, sei es, um den Inhalt einer Aussage zu beschwören (assertorischer Eid), wobei etwaige Unrichtigkeit der Aussage Fluch oder Strafe nach sich ziehen würde; Bekräftigung kann Erstaunen über die eigene Aussage oder die Selbstverherrlichung des Sprechers zum Inhalt haben [22].

Das Altbab. drückt den Affirmativ der Vergangenheit durch die Basis /prus/ (Präteritum) mit vorangesetzter Partikel *lū* aus. Nur sehr selten steht "Subjunktiv" ohne *lū* [23]; s. oben V 14 und 27. Das Altass. verwendet drei verschiedene — frei vertauschbare? — Formen: *a*) *lū* + Basis /prus/ oder /pars/ (Stativ); *b*) *lū* + Basis /prus/ oder /pars/ im "Subjunktiv"; *c*) /prus/ oder /pars/ ohne *lū* im "Subjunktiv" [24].

lū + Präteritum kann auch noch das modale Verhältnis "hätte sollen" ausdrücken [25]. Vgl. unter VII 7 für *aj* + Präteritum "hätte nicht sollen/dürfen".

Der Affirmativ der Zukunft lautet aB in der 1. Person *lū aparras/niparras* oder aber *aparrasu/niparrasu* ohne *lū*; s. V 13 und 28. Bei den Belegen handelt es sich überwiegend um promissorische Eide; doch war der Affirmativ der Zukunft auch ausserhalb des Eides üblich [26]. Die 2. Person *lū taparras* ist zugleich Suppletivform für die 2. Person des Prekativs. Für die 3. Person fehlen mit aB Belege. Dagegen ist aA *lū iparrasu* bezeugt [27].

Im Gegensatz zum Indikativ sind die Mittel, die für den positiven Affirmativ gebraucht werden, nicht einheitlich. Im Altbab. scheinen *lū* und "Subjunktiv" einander auszuschliessen. Man darf daraus aber wohl keine weitreichenden

[19] K. Hecker, *AnOr* 44 (1968) § 79. *–uni* ist ein diskontinuierliches Morphem; denn es kann durch Pronominalsuffix gesprengt werden: *ša ikšud–u–šu–ni* "der ihn/den er erreicht hat".

[20] B. Kienast führt uns mit seiner Vermutung, es sei ein nachgestelltes Demonstrativum (oben I 3), in nicht nachprüfbare Vorvergangenheit der Sprache. I. J. Gelb, *AS* 18 (1969) 107, führt *–uni* auf pleonastisch gesetzte "Subjunktiv"–Endungen **–u–i* zurück, die durch Gleitlaut *–n–* getrennt worden seien; dann habe sich die Silbe *–ni* verselbständigt.

[21] B. Kienast, *Or* 29 (1960) 153; K. Hecker (s. Anm. 19) §79c.

[22] Vgl. Verf., *ZA* 61 (1971) 214 Anm. 3.

[23] *GAG* § 185b.

[24] K. Hecker (Anm. 17) § 132 a.

[25] *GAG* § 152 f.

[26] Vgl. *lu a–sa–li–im* = *lū asallim* "ich werde bestimmt Frieden schliessen" BaghM 2 S. 55 Nr. 2 I′ 4′.

[27] K. Hecker (Anm. 19) § 132c.

Schlüsse ziehen, da aA *lū* mit "Subjunktiv" vorkommt. Wichtiger ist die Frage, weshalb überhaupt der "Subjunktiv" verwendet wird. Man hat versucht, ihn von elliptischen Sätzen wie "(ich schwöre, dass) ich ..." oder "(ich will verflucht sein, sobald) ich ..." herzuleiten, wo die vollständige Wendung ja den "Subjunktiv" des Nebensatzes erfordern würde [28]. Eine Beweisführung ist kaum möglich. W. Eilers' These (oben I 7) vom spezifisch indikativischen Charakter der Eidesform ist nur dann vertretbar, wenn man einen Affirmativ nicht als einen vom Indikativ verschiedenen Modus ansieht.

Die formale Übereinstimmung zwischen dem "Subjunktiv" im Nebensatz und dem beim Affirmativ ist womöglich nur scheinbar. Eine Differenzierung durch die Intonation ist nicht nur denkbar, sondern sogar wahrscheinlich, wenn wir das Eidesprotokoll *TIM* 4 Nr. 36 mit seiner Häufung von irregulären plene-Schreibungen von Vokalen betrachten; s. dazu VII 8.

Im Zusammenhang mit dem positiven promissorischen Eid ist zu erwähnen der elliptische Bedingungssatz *šumma lā aprus* "wenn ich nicht ge... habe (dann will ich verflucht sein)" = "ich werde ganz bestimmt ..." [29].

Für den Affirmativ der Gegenwart "ich bin gewiss ...", "ich tue gewiss gerade" wurde möglicherweise nur *lū* mit dem Stativ gebraucht; s. dazu unten XI.

3. Der Prekativ (einschl. Kohortativ 1. Pl.). Eine Handlung oder ein Zustand werden gewünscht, erbeten, aber nicht befohlen [30]: *luprus* (ass. *laprus*), *liprus*(*ū*/*ā*) "ich möchte ...", "ich will ...", "er möge ...", "er soll ..." [31]; s. V 15, 29. Für die 2. Person tritt als Suppletivform der positiven Affirmativ *lū taparras* ein.

Eine Nebenfunktion hat der Prekativ als Finalis, oft in dem Syntagma *aqbi–ma līpuš* "ich habe gesagt, und er soll tun" = "ich habe angeordnet, dass er tue". Hier unterscheidet sich das Akkadische von jünger bezeugten semitischen Sprachen (Kanaanäisch der Amarna-Zeit [32], Arabisch), die einen Subjunktiv auf *–a* als modus finalis gebrauchen [33].

Der Kohortativ der 1. Pl. hat eine gesonderte Form (s. VII 4). Es ist nicht ratsam, ihn beim Prekativ einzubeziehen [34]; denn er ist nicht nur formal,

[28] W. von Soden, *StudSem.* 4 (1961) 52 f. (lehnt Ellipse ab); K. Hecker (Anm. 17) § 132b (positiv).

[29] *GAG* § 185g; laut K. Hecker (Anm. 19) S. 221 Anm. 1 und gegen *GAG* 185h noch nicht für das Altass. belegt. Für den wohl selteneren Fall *šumma la aptaras* (Perfekt) s. *AnOr* 47, Nachtrag zu § 185g.

[30] Es empfiehlt sich, "Prekativ" beizubehalten. "Optativ" (so E. Reiner, oben I 6) könnte unerwünschte Assoziationen mit dem idg. Optativ erwecken.

[31] Dialekte und literarische Sprachformen, die eine 3. f. Sg. *taprus* bilden, kennen dazu einen Prekativ *lū taprus* (aA) oder *i taprus* (W. G. Lambert, *Atrahasīs* S. 154; *AS* 16 S. 286 Rs. 23 [*i taprik* nach W. von Soden, *AnOr* 47 § 81c]).

[32] W. Moran, *Or* 29 (1960) 1-19 und bes. 6-13, über 'Amarna-Akkadisch' *iparrasa* und *iprusa* als Reflex von kan. *yaqtula*.

[33] W. von Soden notiert in *GAG* § 178c eine in Mari selten vorkommende Konstruktion *akkīma* (= *ana kīma*) + Präsens "Subjunktiv" zur Darstellung finaler Nebensätze; s.a. *AHw.* 477 r. 8. Sollte hier eine amurritische Konstruktion zugrunde liegen? In *StudSem.* 4 (1961) 52 mit Anm. 43 erwägt er "echt modale Gebrauchsweise" des "Modus relativus auf *–u*" in finalen Sätzen, und er zitiert aB *lā tamaššû* (mit Vorbehalt) sowie Ee 75 f.

[34] Auch das Assyrische hat mit 1. Sg. *laprus* und 1. Pl. *lū niprus* keine vollständige morphologische Parallele. Das Sumerische verwendet zwar sowohl im Sg. als auch im Pl. das Präformativ GA–, differenziert aber möglicherweise bei der verbalen Basis (*ḫamṭu, marû*); s. Verf., *ZA* 61 (1971) 222-225.

sondern auch funktionell anders: Während die 3. Pl. "sie mögen ..." zahlenmässig unbegrenzt ist, stellt die 1. Pl. "wir wollen/lasst uns ..." eine inklusive Form dar, d.h. eine Form, die ausser dem Sprecher nur Anwesende (oder anwesend Gedachte) einschliesst [35].

Die negative Entsprechung zum Prekativ ist der Vetitiv (VII 7).

Exkurs zur Bildung des Prekativs. Vielfach wird angenommen, dass dem Prekativ eine Konstruktion *lū* + Verbalpräfix + Basis /prus/ zugrundeliege[36]; das wäre eine mit dem positiven Affirmativ der Vergangenheit gleiche Form. In vielen Fällen lassen sich die postulierten Lautübergänge konstruieren, in anderen dagegen nicht. Begründbar sind: **lūjiprus* > **lijiprus* > *liprus*; **lūjuparris* > **lijuparris* > *līparris*; **lū'uparris* > *lūparris*; **lūjikūn* > **lijikūn* > *līkūn*. Dagegen scheinen die folgenden postulierten Übergänge zweifelhaft: **lū'aprus* (> **luwaprus*) > *laprus* (ass.) bzw. *luprus* (bab.); **lū'akūn* (> **luwakūn*) > *lakūn/lukūn*.

Wir müssen fragen, ob die rekonstruierten Formen *lū* + ... berechtigt sind. Bei ass. *laprus* ist eine Analyse als /l/ + *aprus* von vornherein wahrscheinlicher, und in den anderen Fällen könnten die mit präfigiertem l-haltigem Morphem gebildeten Formen schon so alt sein, dass sie unserer Analyse nicht mehr zugänglich sind.

4. Der Kohortativ der 1. Pl. Es ergeht seitens des Sprechers eine Aufforderung an sich selbst einschliesslich einer oder mehrerer anwesender (oder anwesend gedachter) Personen: *i niprus* "wir wollen...", "lasst uns..."; s. V 29. Die Form ist, wie übrigens in allen Sprachen, inklusiv (s. Anm. 35). Der Kohortativ ist Komplementärmodus zum Prekativ (VII 3), mit dem er die Basis /prus/ teilt. Seine negative Entsprechung ist die 1. Person Pl. des Vetitivs (VII 7).

Der Kohortativ impliziert nicht immer eine sofortige Auslösung der Handlung; die Auslösung kann aufgeschoben werden: "..., dann wollen wir ..." [37].

5. Der Imperativ. Es ergeht seitens des Sprechers ein Befehl oder eine strikte Aufforderung: *purus*, *pursī*, *pursā*; s. V 15. Der Imperativ kommt nur positiv vor. Seine Negation ist die 2. Person des Prohibitivs. Eine morphologische Scheidung dieser Art findet sich auch in den übrigen semitischen Sprachen, im Sumerischen, in vielen indogermanischen Sprachen etc. wieder [38].

6. Der Prohibitiv. Eine Handlung oder ein Zustand werden verboten ("nicht dürfen") oder als vor dem Sprecher selbst unerwünscht bezeichnet ("ich will nicht ..."): *lā aparras* etc.; s. V 23. Dabei ist die Form der 2. Person zugleich Negation des Imperativs ("du darfst, sollst nicht ..."). Wenn auch Berührungszonen mit dem Vetitiv (VII 7) bestehen dürften, ist doch nicht nur die morphologische Scheidung klar, sondern auch der grundsätzliche Bedeutungsunterschied; beim Prohibitiv liegen fester Wille, Macht und Durchsetzungsvermögen zugrunde, beim Vetitiv dagegen nur der Wunsch.

[35] Sprachen, die bei der 1. Person Dual, Trial, Plural die Kategorien inklusiv und exklusiv formal unterscheiden, bilden den Kohortativ nur mit den inklusiven Formen; vgl. G. B. Milner, *Fijian Grammar* (1956) 53.

[36] Vgl. W. von Soden, *GAG* § 81c; I. M. Diakonoff, oben I 5; R. Hetzron, *JSS* 14 (1969) 4.

[37] Vgl. *AbB* 2,33,14-16 *ina kīma inanna ebūrum warki ebūrim i nillik* "zur Zeit ist Ernte; nach der Ernte wollen wir gehen".

[38] Echte negierte Imperative dagegen "geh nicht!", türk. *yapma* "tu nicht!" etc.

7. Der Vetitiv. Eine Handlung oder ein Zustand werden als nicht erbeten, nicht erwünscht bezeichnet; ihr Nichtsein wird gewünscht, jedoch — im Unterschied zum Prohibitiv — nicht angeordnet: *aj aprus*, *ē taprus* etc.; s. V 26. Der Vetitiv ist die Negation des Prekativs, mit dem er die Basis /prus/ teilt.

Seltene Verwendungsweise des Vetitivs liegt vor in Fällen wie *aj ibluṭ amēlu* "niemand hätte überleben sollen" GE XI 173. Hier zeigt sich, dass der Vetitiv von Hause aus in Bezug auf die Zeitstufe neutral ist, wenn er sich auch in der grossen Mehrzahl der Fälle auf die Zukunft bezieht.

Anmerkung: Die Negationspartikel /AJ/ hat die Komplementärvarianten /aj/ vor Vokal und — kontrahiert — /ē/ vor Konsonant [39]. Altakk. A ist daher wohl nicht "*ā*", sondern defektive Schreibung für *aj* entsprechend altsum. A = aj(a) für späteres *a–a*, *a–ja* "Vater" [40].

8. Der negative Affirmativ. Gemeint ist die Negation des oben, VII 2, behandelten positiven Affirmativs. Im Gegensatz zum Indikativ können pos. und neg. Affirmativ nicht unter einer Rubrik behandelt werden. Sie verhalten sich nämlich nicht rein symmetrisch zueinander, etwa in der Weise, dass einer Anzahl positiver genau dieselbe Anzahl negativer Formen (= Negationspartikel + Positivform) gegenüberstünden.

Der auf die Vergangenheit bezogene negative Affirmativ lautet *lā aprusu* etc.; s. V 25. Er enthält dort, wo anfügbar, das *–u* des "Subjunktivs". Das Altass. lässt ihn auf *–u*, *–uni* oder *–ni* enden [41]. Bei den Belegen handelt es sich um negative assertorische Eide. Der auf die Zukunft bezogene negative Affirmativ ist aB vorwiegend in der 1. Person bezeugt: *lā aparrasu / niparrasu*; s. V 24; dgl. im Altass. [42]. Seltener kommen 2. und 3. Person vor [43]. Es handelt sich auch hier fast durchweg um negative promissorische Eide. Aber, wie oben VII 2 anlässlich des positiven Affirmativs notiert, ist dies wohl nicht die einzige Verwendungsweise gewesen.

Zur Frage der "Subjunktiv"-Endung s. die Diskussion oben VII 2 mit Anm. 28.

Ein wichtiges Beispiel dafür, dass der Affirmativ nicht allein morphologisch gekennzeichnet war, sondern auch in seiner Intonation abwich, gibt der aB Text *TIM* 4 Nr. 36. Hier fallen negative assertorische Eide in grosser Zahl durch

[39] Altass. wird auch vor Vokalanlaut *e* geschrieben. Ob daraus mit W. von Soden, *GAG* § 81 i, und K. Hecker (Anm. 19) § 77d auf *ē* zu schliessen ist, scheint mir nicht sicher. Denkbar ist auch *e iš–ku–un* = *ej–iškun* gegenüber *e ta–aš–ku–un* = *ē taškun*.

[40] I. J. Gelb, *MAD* 2_2 (1961) 173 f., liest *a i–ti–in* = *ajiddin* "ich will nicht geben", interpretiert aber *a taq–bi* "du mögest nicht sagen" nicht weiter. Dagegen W. von Soden, *GAG* § 81 i "*ā iddin*", "*ā taqbi*".

[41] K. Hecker (Anm. 19) § 132b.

[42] Ebd. § 132d.

[43] 2. Person aAK *lā tala''amu* ... *lā tuššabu* "du sollst gewiss nicht essen ..., dich gewiss nicht setzen"; *GAG* § 185 e-f mit Nachtrag in *AnOr* 47. Das aB Beispiel für die 3. Person ist dagegen nicht ganz klar: *MAP* 43 (= *VAB* 5 Nr. 259) 30-33 S. *lā iturru–ma lā ibaqqaru* mu Nanna ... in–pà " S. wird (darauf) nicht mittels Vindikation zurückkommen, hat er bei Nanna ... geschworen". Möglicherweise ist hier kontaminiert aus *a*) S. *ul itâr–ma ul ibaqqar* (Aussage betr. S.) und *b*) S. *lā aturru–ma lā abaqqaru* mu Nanna ... in–pà "S. hat ... geschworen 'ich werde nicht ...' " (Wiedergabe der direkten Rede). Die 3. Person ist daher in Tabelle 2 eingeklammert.

irreguläre Vokal-plene-Schreibungen auf: *la ad–di–i–nu* (Z. 14), *la aš–ku–ú–nu* (Z. 18), *la id–di–i–nu* (Rs. Z. 13′ f.) etc.

Wie der positive kennt auch der negative promissorische Eid die Alternativkonstruktion als elliptischer Bedingungssatz: *šumma aprus* "wenn ich ge... habe (dann will ich verflucht sein)" = "ich werde ganz bestimmt nicht ..." [44].

VIII

Die Übersicht ist zweifellos unvollständig, da sie nur synthetische oder mit den Partikeln *aj/ē, i, lā, lū, ul* zusammengesetzte Verbalformen berücksichtigt hat. Eine erweiterte Darstellung ist jedoch erst möglich nach eingehender Untersuchung der sonstigen sog. modalen Partikeln wie *assurri, minsu, pīqat, tuša, ullaman* u.a. Kurz behandelt seien hier nur noch die Ausdrucksmittel für den Irrealis.

Der Irrealis. Eine Handlung oder ein Zustand werden als nicht wirklich, sondern als nur unter bestimmten (noch zu erreichenden oder auch nicht mehr erreichbaren) Umständen gegeben dargestellt: "Ich ginge/würde gehen, wenn die Sonne schiene" (= ich gehe, vorausgesetzt, dass die Sonne scheint); die Handlung ist noch realisierbar, nämlich sobald die Voraussetzung (Sonnenschein) eintritt. Dagegen "ich wäre gegangen, wenn die Sonne geschienen hätte" (ich bin nicht gegangen, da die Sonne nicht schien); hier ist die Handlung nicht mehr realisierbar; wenn die Sonne jetzt noch hinter den Wolken hervorkommt, ist es schon zu spät. Entsprechend mit umgekehrten Vorzeichen: " ich würde nicht gehen/wäre nicht gegangen, wenn nicht ..." oder mit der Abfolge positiv ... negativ bzw. negativ ... positiv.

Der Irrealis ist — je nach Sprache mit verschiedenen Mitteln — unterschieden von der indikativischen Bedings.form: "ich gehe, wenn die Sonne scheint" (= ich setze voraus: die Sonne scheint; dann gehe ich). Im Akkadischen ist der häufigste Fall für den Irrealis ein Bedingungssatz, dessen Vordersatz mit aB *šumma–man* (gekürzt *šumman*), aA mit *šumma–min* eingeleitet wird, während der Nachsatz *–man* (aB) bzw. *–min* (aA) an einen vom Sprecher hervorgehobenen Satzteil anfügt (Subjekt, Objekt, Prädikat, Partikel): *šumman inanna bēlī [š]aḫim ... libbī–man ṭāb* "wenn mein Herr jetzt wohlauf wäre, dann wäre ich zufrieden" *ARM* 10,92,12-14 [45]. Seltener sind Sätze mit durch *–man* bezeichnetem Irrealis, die keine Bedingungssätze sind.

IX

Wir versuchen, die oben V-VII behandelten Modi in ein System zu bringen, das sich graphisch veranschaulichen lässt. Wir gehen von Indikativ aus und stellen fest, dass sich jeweils zwei Schritte vollziehen lassen: 1) Von der reinen Aussage zur Beteuerung oder zum Wunsch; 2) vom Positiven zum Negativen. Nehmen wir die Form *aprus(u)* als Beispiel: 1a) *aprus* → *lū aprus*, 1b) *ul aprus*

[44] *GAG* § 185 g.

[45] Für das Altass. s. K. Hecker (Anm. 19) § 139; in *GAG* sind Vorder- und Nachsätze von irrealen Bedingungssätzen auf zwei verschiedene Abschnitte (§ 162a, 152d) verteilt.

→ *lā* aprusu; 2a) *aprus* → *ul aprus*, 2b) *lū aprus* → *lā aprusu*. Graphisch lässt sich das so veranschaulichen:

aprus → *ul aprus*
↓ ↓
lū aprus → *lā aprusu*

Es folgt eine Gesamtübersicht für die Basen /prus/ und /parras/, und zwar dort, wo nötig, getrennt nach der 1./3. und 2. Person. Nur assyrisch bezeugte Formen sind durch nichtkursiven Satz hervorgehoben.

Das System ist im Ganzen k e i n System privativer morphologischer Oppositionen, auch wenn das in Einzelfällen, wie beim Indikativ, zutrifft. Mit anderen Worten, die zu vollziehenden und durch Pfeile angedeuteten Schritte kommen nicht grundsätzlich durch den Erwerb eines merkmalhaften Gliedes zustande.

	Pos. Ind.		Neg. Ind.	
1. Sg.	*aprus*	*ša aprusu*	*ul aprus*	*ša lā aprusu*
1. Pl.	*niprus*	*ša niprusu*	*ul niprus*	*ša lā niprusu*
3. Sg.	*iprus*	*ša iprusu*	*ul iprus*	*ša lā iprusu*
3. Pl.	*iprusū/ā*	*ša iprusū/ā*	*ul iprusū/ā*	*ša lā iprusū/ā*

	Pos. Aff.	Neg. Aff.
1. Sg.	*lū aprus* [46]	*lā aprusu*
1. Pl.	*lū niprus*	*lā niprusu*
3. Sg.	*lū iprus*	*lā iprusu*
3. Pl.	*lū iprusū/ā*	*lā iprusū/ā*

	Pos. Ind.		Neg. Ind.	
2. Sg.	*taprus*	*ša taprusu*	*ul taprus*	*ša la taprusu*
2. Sg.	*taprusī*	*ša taprusī*	*ul taprusī*	*ša lā taprusī*
2. Pl.	*taprusā*	*ša taprusā*	*ul taprusā*	*ša lā taprusā*

	Pos. Aff.	Neg. Aff.
2. Sg.	*lū taprus / taprusu* [46]	*lā taprusu*
2. Sg.	*lū taprusī*	*lā taprusī*
2. Pl.	*lū taprusā*	*lā taprusā*

Tabelle 1: Das von *aprus* ausgehende Modussystem.

[46] Für aA *lū aprus/lū aprusu/aprusu* etc. s. VII 2.

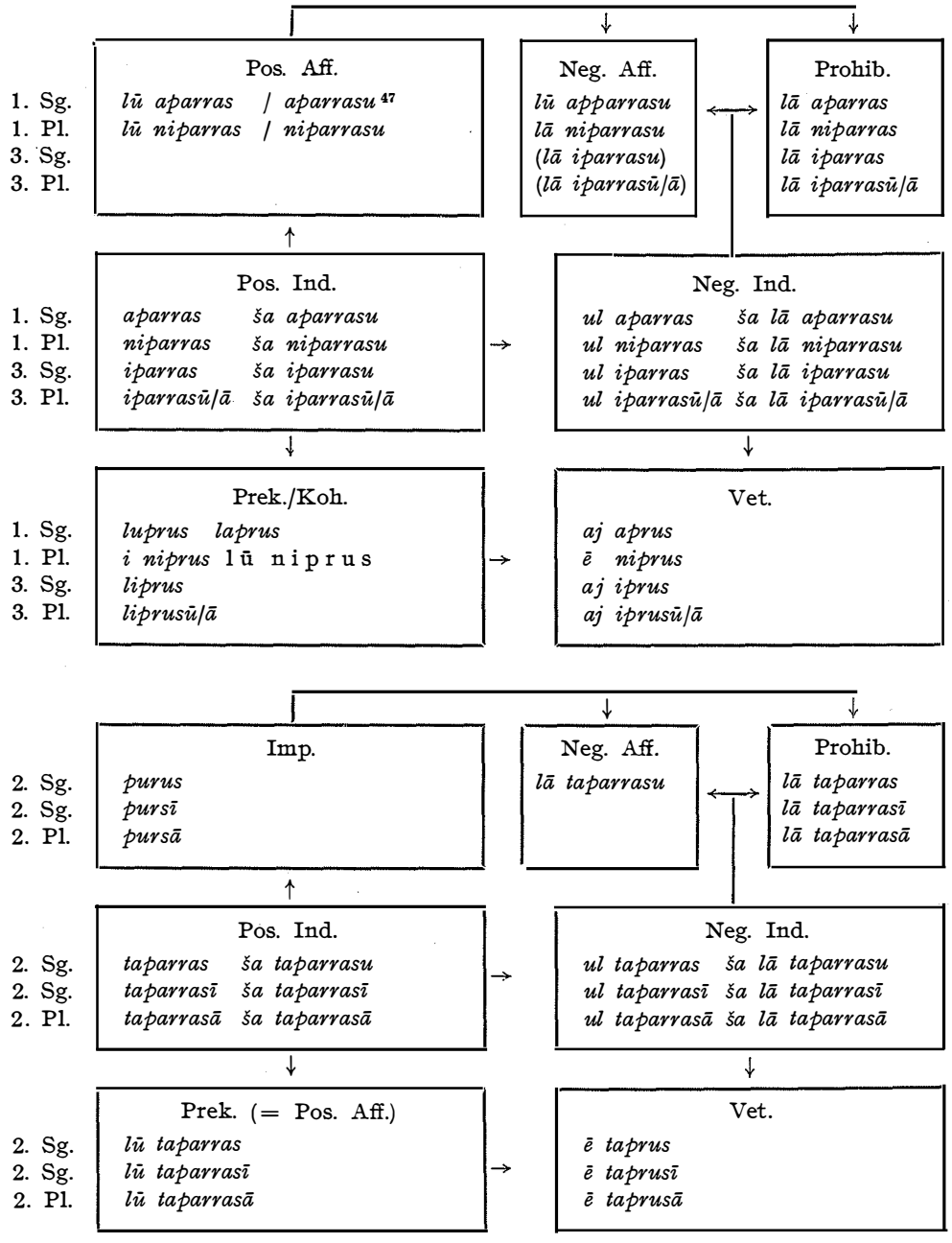

Tabelle 2: Das von *aparras* ausgehende Modussystem.

[47] Für aA *lū aparrasu* s. VII 2.

X

Bei der Basis /ptars/ (Perfekt), die wir bisher ausgespart haben, liegen die Verhältnisse sehr viel einfacher. Das Schema lautet:

Pos. Ind.	Neg. Ind.
aptaras, ša aptarsu etc.	*ul aptaras, ša lā aptarsu*[48] etc.

Erst in der mB Zeit beginnen andere Modi auf /ptars/ überzugreifen: *lupteḫḫir* (= *lū upteḫḫir*) "ich versammelte" als positiver Affirmativ der Vergangenheit[49].

XI

Dagegen sind die Formen der Basis /pars/ (Stativ) wieder modal differenziert.

1. Der positive und negative Indikativ sind als vollständige Konjugationsmuster vorhanden. Wieweit die Formen wirklich bildbar waren, hing natürlich von der Bedeutung der einzelnen Verben ab[50].

2. Der positive Affirmativ ist aB durch vorangesetztes *lū* gekennzeichnet und formal gleich mit dem Prekativ (XI 3). Das Altass. bildet ihn dagegen nach den beiden mir bekannten Belegen mit den Endungen *-u, -ni* des "Subjunktivs": *lū parsu, parsātini*, wobei, wie letztere Form zeigt, *lū* offenbar nicht verpflichtend war[51]. Da jeweils Belege für die 1., 2. und 3. Person vorliegen, darf man ein vollständiges Konjugationsmuster ansetzen[52].

3. Der Prekativ stimmt aB formal mit dem positiven Affirmativ (XI 2) überein. Die modale Bedeutung ist also nur aus dem Zusammenhang zu erkennen. Obwohl mir ein aB Beispiel für die 1. Person fehlt, darf man wohl ein vollständiges Konjugationsmuster vermuten[53]. Die mB bezeugte 1. Pl. wäre nach oben VII 4 konsequenterweise gesondert als Kohortativ aufzuführen. Wir ordnen sie in Tabelle 3 jedoch mit beim Prekativ ein.

4. Der Kohortativ der 1. Person Pl. S. XI 3.

5. Der Prohibitiv ist durch vorangesetztes *lā* bezeichnet. In der 3. m. Sg. ist *lā paris* vom negativen Affirmativ *lā parsu* (XI 7) unterschieden, in den übrigen Fällen dagegen gleich gebildet. Mir fehlen Belege für die 1. Person. Möglicherweise liegt kein vollständiges Konjugationsmuster vor[54].

[48] Zu beachten, dass — wenigstens nach dem Befund des Altass. — der pos. Indikativ im Hauptsatz bei weitem überwiegt; s. K. Hecker (Anm.) § 76, bes. f-h.

[49] *GAG* § 81 f.

[50] Die bisher ausführlichste Darstellung des Stativs findet sich bei M. B. Rowton, *JNES* 21 (1962) 233-303. Die Mehrzahl der nachfolgenden Beispiele verdanke ich seinem Aufsatz.

[51] 3. m. Sg. *libbī lū marṣu* "... ist mein Herz wahrhaftig krank"; 2. m. Sg. *qabi'ātini* "hattest du doch versprochen!" K. Hecker, (Anm. 19) § 132a.

[52] 1. Sg. *kaspam gamram lū nadnāku* "ich habe das ganze Silber bezahlt" Rowton (Anm. 50) Nr. 429; 1. Pl. *kaspam lū ḫubbulānu* "Silber schulden wir in der Tat" Rowton Nr. 242; 2. f. Sg. *puluḫtam lū labšāti* "du bist mit einer Aura des Schreckens bekleidet" VS 10,214 VI 36; 3. m. Pl. PN_1 *u* PN_2 *lū zīzū* "A und B haben geteilt" (Eid) YOS 8,66,25.

[53] 1. Sg. *lū ṣabtāk-ma* "ich will packen" STT 28 IV 58 (jB); 1. Pl. *lū ṭābānu* "wir wollen freundlich sein" (mB) *GAG* § 81b; 2. m. Sg. *lū balṭāta* "du mögest gesund sein" *ABPh.* 96,6; 3. m. Sg. *šulum-ka lū dari* "dein Wohlergehen währe ewig!" *ABPh.* 66, 11; 3. m. Pl. *lū ṣabtū* "... sollen sie innehaben" *AbB* 4,43,18'.

[54] 1. Sg. *lā kaṣṣudāku* "ich will nicht festgehalten werden" (aA) *TC* 3,28,33 (s. J. Lewy, *Or* 29 [1960] 21); 2. m. Sg. *lā wašbāti* "du sollst nicht herumsitzen!"

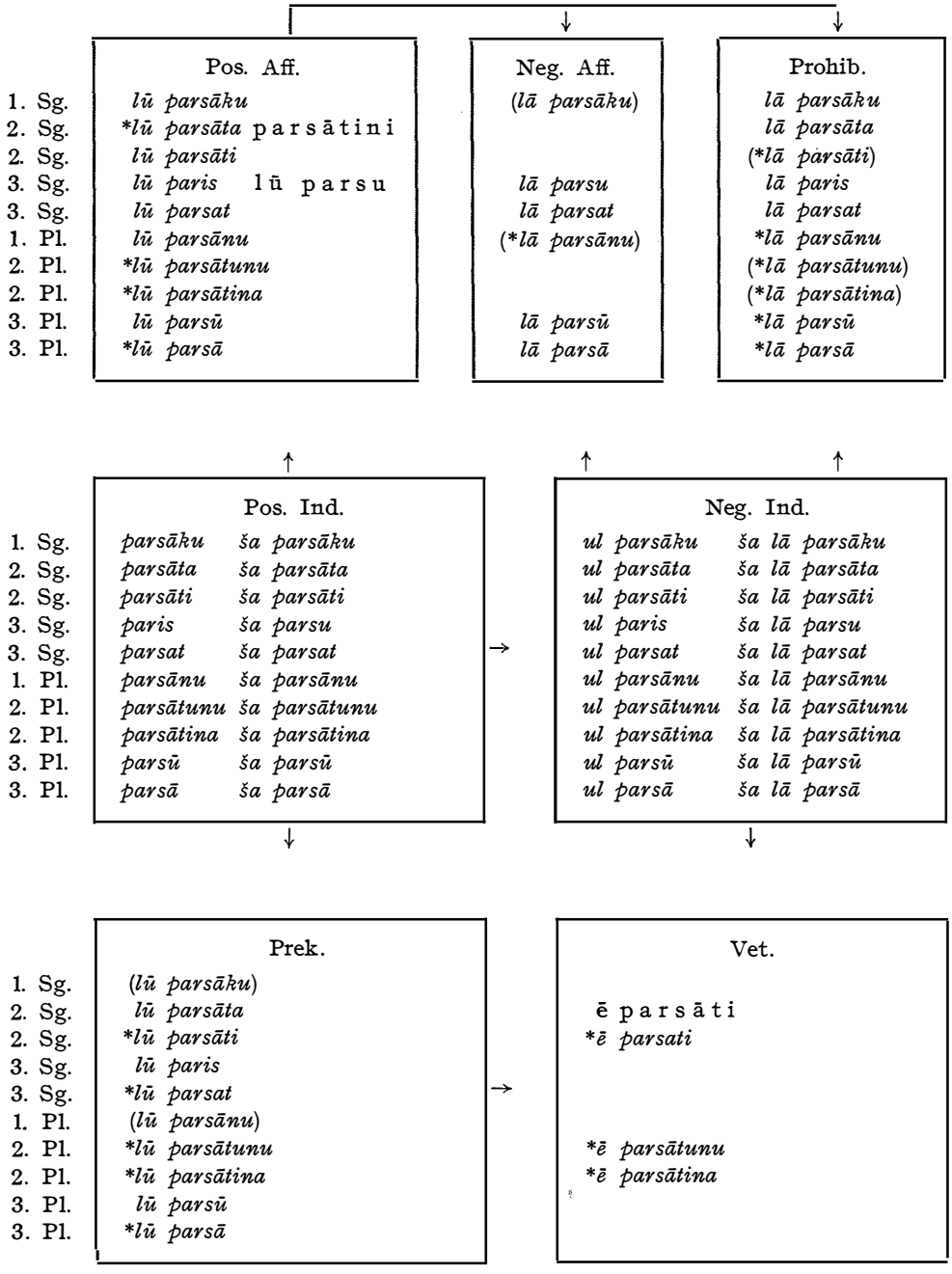

Tabelle 3: Das von *paris* ausgehende Modussystem.

6. Der Vetitiv ist nur einmal aA belegt; *lā parsāti* (3. m. Sg.) [55]. Koexistenz von Prohibitiv und Vetitiv in ein derselben Sprachstufe lässt sich einstweilen nicht beweisen.

7. Der negative Affirmativ. Er ist wie derProhibitiv (XI 5) durch vorangesetztes *lā* gekennzeichnet. Mir fehlen Belege für die 2. Person. Möglicherweise kein vollständiges Konjugationsmuster. Die 3. m. Sg. wurde aB mit dem *-u* des "Subjunktivs" gebildet: *lā parsu* [56]. Für die Altass. darf man wohl analog zum positiven Affirmativ grundsätzlich Bildung mit dem "Subjunktiv" auf *-u, -ni* erwarten. Mir fehlen aber Belege.

Alle Stativformen der 3. Sg. und Pl. können um die komplementär verteilten Endungen *-am, -nim* des Ventivs erweitert werden [57]. Auch hier bestätigt sich, dass der Ventiv k e i n Modus ist.

Im Folgenden soll auch der modale Befund des Stativs tabellarisch veranschaulicht werden. Dabei sind sicher erschliessbare Formen (Analogieschluss von m. auf f. oder vom Sg. auf den Pl. bzw. umgekehrt) mit Sternchen bezeichnet. Altass. Formen stehen in nichtkursiver Type. Wenn mangels älterer belegter Formen auf das mB etc. zurückgeriffen werden musste, so steht die Form in Klammern.

XII

Zusammenfassung. Das mit synthetischen Verbalformen allein oder zusammen mit den Partikeln *aj/ē, i, lā, lū, ul* (sowie ass. *-ni*) gebildete Kernsystem der älteren akkadischen Modi lässt sich grob einteilen nach positiven und negativen Formen sowie nach Aussage, Beteuerung, Wunsch und Befehl.

Positiv; Aussage (Indikativ VII 1), Beteuerung (Affirmativ VII 2), Wunsch (Prekativ, Kohortativ VII 3-4), Befehl (Imperativ VII 5).

Negativ: Aussage (Indikativ VII 1), Beteuerung (Affirmativ VII 8), Wunsch (Vetitiv VII 7), Verbot (Prohibitiv VII 6).

Manche Modi haben Nebenfunktionen (z.B. Finalis beim Prekativ), die morphologisch nicht eigens bezeichnet sind.

Das Modussystem ist — mit Konjugationsmustern von verschiedenem Vollständigkeitsgrad — sowohl für die präfigierenden Verbalformen (Tabelle 1-2) als auch für den Stativ (Tabelle 3) ausgebildet.

Der Indikativ ist bildbar von vier verschiedenen Basen (/prus/, /parras/, /ptars/, /pars/), der Affirmativ von dreien (/prus/, /parras/, /pars/), alle übrigen Modi ausser dem Imperativ von zwei Basen (/prus/ o d e r /parras/ und /pars/) und der Imperativ schliesslich nur von einer Basis (/prus/). D.h. die Modi sind temporal und aspektuell voll oder nur beschränkt realisierbar; sie können auch von Hause aus in Tempus und Aspekt neutral sein.

CCT 5,3b 6 (aA); 3. m. Sg. *libbu-šu lā parid* "sein Herz soll nicht zittern" *CCT* 4,33b 20 (aA); 3. f. Sg. *lā wašbat* "sie soll nicht dasitzen" *ABPh.* 139,13.

[55] 2. m. Sg. *ē naš'āti* "du mögest nicht Transporteur sein" *CCT* 50,13.

[56] 1. Sg. *lā egâku-ma* "ich habe nie gefehlt" *VAB* 4,276 V 24 (nB); 3. m. Sg. *lā watru* "... ist bei Gott nicht mehr als ..." *ZA* 49,170 IV 9; 3. f. Sg. *pūt-ni la el-le-e-et-ma* "unsere Stirn ist nicht rein" Rowton (Anm. 50) Nr. 242 (= *JRAS* 1926, 437 Rs. 3, mir nicht zugänglich; R. transkribiert zusammenhängend *ellēt*).

[57] Z. B. *kankūnim* "sind für nich gesiegelt"; *aššum lā burrakkum* "weil es dir nicht erklärt worden war"; *u ... našûniššum* "wobei sie ihm ... bringen".

K e i n e eigenen Modi sind der "Subjunktiv" als Variante des Indikativs (s. oben VI) und der angebliche "i-Modus" (oben VI Bemerkung 2). Auch der Ventiv, der von jeder präfigierenden Verbalform, vom Imperativ und von der 3. m. Sg. des Stativs gebildet werden kann, ist k e i n Modus.

A n h a n g

XIII

Wir haben uns oben von Bemühungen distanziert, die Modi anderer semitischer Sprachen bei der historischen Erklärung akkadischer Modi heranzuziehen (s. II Ende). In der Tat sind Vergleiche entweder unergiebig, weil die betreffende Sprache kein System mehr hat, das Modi durch morphologisch differenzierte synthetische Verbalformen darstellt (und nur solche kommen bei einem Vergleich in Betracht); oder der Vergleich wird bei einem konsequent durchgeführten Versuch ad absurdum geführt, wie unten am Beispiel des Arabischen gezeigt werden soll.

Das A m u r r i t i s c h e muss beim Vergleich ausscheiden. Wir kennen diese Sprache oder Dialektgruppe [58] fast nur aus Personennamen. Das U g a r i t i s c h e unterscheidet bei Verben tertiae vocalis Formen wie *y'ny* = **yi'nay* V "er antwortet(e)" von *y'n* = **yi'nê* (< **yi'nay*) [59]. Hieraus erschloss man neben *yaqtulu/a* ein *yaqtul*. Wenige Beispiele von Verben tertiae alef erlauben, zwischen *yaqtulu* und *yaqtula* zu differenzieren [60]. Der ugaritische Befund ähnelt dem arabischen stark auf den ersten Blick. Aber die grosse Mehrzahl der Verbalformen gibt, da in der Schrift auf Konsonanten endend, ihren Auslaut nicht preis [61], so dass uns zu wenig Spielraum für die Aufstellung von System und Regeln bleibt [62]. Strittig ist auch noch, ob das Ugaritische neben *yaqtul–* auch *yaqattal–* gehabt hat [63].

Das 'Amarna-Akkadisch' der Briefe aus Byblos lässt einen f r ü h k a n a a n ä i s c h e n Modus *yaqtula* durchscheinen, für den W. Moran die Hauptfunktionen *a*) Wunsch, Befehl und *b*) Zweck (Finalis) eruiert hat [64]. Im *Hebräischen* sind nur Spuren einer synthetisch gebildeten modalen Differenzierung übrig geblieben [65]. Das *Syrische* als Vertreterin der aramäischen Gruppe kennt nur noch ein präfigierendes Konjugationsmuster. Das G e ' e z steht mit seinem 'Indikativ' *yəqattəl* und seinem "Subjunktiv" *jəqtəl* formal, nicht aber in der Funktion dieser Modi, dem Akkadischen nahe [66].

[58] I. J. Gelb, *JCS* 15 (1961) 27-47; W. von Soden, *WZKM* 56 (1960) 187 f.;
[59] C. H. Gordon, *Ugaritic Textbook* (= *AnOr* 38, [1965]) § 9.52; vgl. auch den mit yaqtul gebildeten Prohibitiv al tḫd (ḪDw) "freue dich nicht!".
[60] Ebd. § 9,10.
[61] Eine Ausnahme ist [j]a–ab–ṣi–ru = yabṣiru "er erblickt(e)(?)" in *Ugaritica* 5 Nr. 153 Vs. 1, einem in bab. Keilschrift geschriebenen, gewiss ugaritischen Text.
[62] Gordons Überschrift "Lax Use of Moods" (ebd. § 9.12) ist bezeichnend nicht für das Ugaritische, sondern für die Unsicherheit der Forscher.
[63] Ebd. § 9.2.
[64] S. oben Anm. 28a.
[65] G. Beer/R. Meyer, *Hebr. Gramm.* II (1955) 11-13; B. Kienast, *Or* 29 (1960) 162-164.
[66] Vgl. W. von Soden, *StudSem.* 4 (1961) 41 f.

Ein umfängliches und sicher vokalisierbares System bietet das klassische Arabisch mit seinem Indikativ *yaqtulu*, Subjunktiv *yaqtula*, Jussiv *yaqtul* und Imperativ *(u)qtul* neben *qatala*; nicht ganz sicher ist, ob man den Energicus *yaqtulan(na)* ebenfalls als Modus klassifizieren soll [67]. Wenn wir den — eher spielerischen — Versuch machen, die arabischen Formen ins Akkadische zu transponieren, so ergibt sich etwa das folgende, freilich cum grano salis zu bewertende Bild:

1. *yaqtulu* (Ind., imperfektiv)	*iparras, ša iparrasu*
2. *mā yaqtulu* (Neg. dazu)	*ul iparras, ša lā iparrasu*
3. *qatala* (Ind., perfektiv)	*iprus, ša iprusu*; paris, *ša parsu*
4. *lam yaqtul* (Neg. dazu)	*ul iprus, ša lā iprusu*
5. *an yaqtula* (Subj. als Finalis)	*liprus* (oder nominale Konstruktion)
6. *lan yaqtula* (Subj. als neg. Affirmativ)	*lā iparrasu*
7. *lā yaqtul* (Jussiv als Prohibitiv)	*lā iparras*
8. *liyaqtul* (Jussiv als Prekativ)	*liprus*
9. *layaqtulanna* (Energ. als Affirmativ)	*lū iparras*
10. *qatala* (Prekativ)	*liprus, lū paris*
11. *lā qatala* (Vetitiv)	*aj iprus*

In diesem Bild sind direkte morphologisch-funktionelle Entsprechungen in der Minderzahl (4, 8, gelegentlich 3), und es überwiegen 'Gleichungen', die morphologisch nicht übereinstimmen. Ein solches Bild kann also kaum dazu ermutigen, ein am Arabischen orientiertes ' ursemitisches ' Modussystem zu rekonstruieren. Aber auch andere Versuche erscheinen wenig fruchtbar, da ihre Rekonstruktionen unbeweisbar sind [68]. Nach wie vor ist das zurückhaltende Urteil C. Brockelmanns geboten, das er 1908 in seinem *Grundriss der vergl. Gramm. der semitischen Sprachen*, Bd. I S. 554, abgab: "Das aber die einzelnen Sprachen in den Ausdrucksmitteln für dieses Modalitäten stark von einander abweichen, so lässt sich kein festes System für das Ursemitische aufstellen, wenn es auch wahrscheinlich ist, dass es diese Formen durch vokalische Endungen in den Grundformen und durch Erweiterung der schon zur Personenbezeichnung dienenden Endungen unterschied".

XIV

Verlockend — und vielleicht lohnender — erscheint es mir, einen Blick auf jene Sprache zu werfen, mit der das Akkadische über ein halbes Jahrtausend lang in täglicher, lebendiger, ' hautnaher ' Berührung gelebt hat — das Sumerische. Allerdings kann der knappe Überblick, der hier folgt, nur unvolkommen ausfallen, da es noch keine eingehendere Studie zum Modus im Sumerischen gibt [69].

[67] Da die Energicusendungen sowohl an yaqtul– als auch an den Imperativ (u)qtul antreten können, wäre möglicherweise das oben II aufgebotene Argument der Unmöglichkeit einer Modushäufung ins Feld zu führen.

[68] W. von Soden, "Tempus und Modus im Semitischen", *Akten des XXIV. IOK* 1957 (1959) 263-265, rechnet mit ursemitischem Stativ *qatil/u/al* und Präteritum *yaqtul*; zu letzterem wurde ein Präsens-Futur entweder als *yaqtu/ilu* oder als *yaqatal* gebildet; je nach Sprache verschieden kam modale Differenzierung hinzu. B. Kienast, *Or* 29 (1960), Tabelle S. 166, rekonstruiert auch für das Hebr.

Für die Aussage (Indikativ) werden Verbalformen gebraucht, die überwiegend mit den 'Konjugationspräfixen' m u – oder i– oder aber ohne diese mit einem dimensionalen Präfix (b a –, b i–, n a–) anlauten. Negierung geschieht durch das Negativpräformativ NU– mit seinen Komplementärvarianten n u– und l a–. Diese Formen können durch suffigiertes *–a* nominalisiert werden. Sätzen mit nominalisierten Verbalformen entsprechen im Akkadischen gewöhnlich Nebensätze mit dem Verbum im "Subjunktiv" [70].

Positive Beteuerung wird ausgedrückt durch das Präformativ ḪÉ– mit der *ḫamṭu*-Basis: ḫ é – ĝ a r. Akkadisch entspricht *lū aprus*, der Affirmativ der Vergangenheit. Zukünftiges wird beteuert durch ḪÉ– mit *marû*-Basis oder möglicherweise auch durch ŠA– mit *marû*. Akkadisch entspricht *lū aparras / aparrasu*.

Wunsch: 1. Person mit Präformativ GA– (vgl. Anm. 34); 2. und 3. Person ḪE– mit *marû*-Basis: ḫ é – ĝ á – ĝ á (– a n).

Verbot: Positiver Imperativ (*ḫamṭu*); Prohibitiv vermittels Präformativ NA– mit *marû*– Basis, wobei die 2. Person die Negierung des Imperativs (= *lā taparras*) darstellt.

Negative Beteuerung in der Vergangenheit; Präformativ BARA– mit *ḫamṭu*–Basis; Akkadisch entspricht *lā aprusu*. In der Zukunft: BARA– mit *maru*–Basis.

Das Sumerische hat ebenso wenig wie das Akkadische eine besondere Form für den Finalis. Es kann hierfür den Prekativ verwenden; doch überwiegen nominale Konstruktionen LAL–*eda/ede* [71].

Wir sehen auffällige und wohl nicht zufällige Berührungspunkte zwischen dem akkadischen und dem uns bisher fassbaren sumerischen Modussystem. Die Übereinstimmung bei der Wahl der Mittel für den Finalis ist frappierend. Schon früher wurde angenommen, dass akk. *lū aprus* von sum. ḫ é – ĝ a r beeinflusst sein könnte [72]. Wenn nun zwei in ihrer Struktur so grundverschiedene Sprachen wie das semitische Akkadisch und die agglutinierende Ergativsprache Sumerisch enge morphologische und syntaktische Übereinstimmungen zeigen, so ist man versucht, auf diese Erscheinung den Begriff "Sprachbund" anzuwenden. Damit aber werden Versuche, das akkadische Modussystem allein im semitischen Sprachbereich zu verankern, relativiert.

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und Aram. *yaqtulu, yaqtula* und *yaqtul*. Stark abhängig von rekonstruierten Akzentverhältnissen sind R. Hetzron, "The Evidence for Perfect **y'aqtul* and Jussive **yaqt'ul* in Proto-Semitic", *JSS* 14 (1969) 1-21; s. auch oben Anm. 31; G. Janssens, "The Present-Imperfect in Semitic", *BiOr* 29 (1972) 3-7.

[69] Vgl. vorläufig, besonders für die Zugehörigkeit der Basen *ḫamṭu* und *marû* zu bestimmten Modi, Verf. in *ZA* 61 (1971) 208 ff.

[70] I. M. Diakonoff nimmt im Anschluss an A. P. Riftin (s. oben I 5) auch bei den semitischen auf *–u* oder *–a* endenden Modi das Phänomen der Nominalisierung an, und zwar unter Verwendung des "Lokativzeichens" *–u* oder des "Lokativ-Objektiv-Zeichens" *–a*.

[71] Vgl. Verf., *Heidelberger Studien zum Alten Orient* (1967) 43-47 mit Korrekturen in *ZA* 62 (1972) 25-29.

[72] W. von Soden, *GAG* § 81 f; A. Poebel nahm in seinen *Grundzügen der sum. Gramm.* (1923) § 639 umgekehrte Einflussrichtung an, akk. > sum.

Ein semitisches Wurzelwörterbuch: Probleme und Möglichkeiten

Wolfram VON SODEN – Münster

Anders als für die indogermanischen Sprachen gab es für die semitischen Sprachen bisher kein Wurzelwörterbuch, sondern nur Hinweise auf die verwandten Sprachen in einigen Wörterbüchern für die Einzelsprachen, am häufigsten in den hebräischen Lexika. Das ist um so erstaunlicher, als die Aufgabe, eine vergleichende Grammatik der semitischen Sprachen zu schreiben, immer wieder einmal in Angriff genommen wurde, teils von einzelnen wie Carl Brockelmann, der ausser seinem grundlegenden zweibändigen *Grundriss der vergleichenden Grammatik der semitischen Sprachen* noch zwei Auszugswerke schuf, teils von Forschergruppen wie die neuere *Introduction to the Comparative Grammar of the Semitic Languages; Phonology and Morphology* von S. Moscati, A. Spitaler, E. Ullendorff und W. von Soden (Wiesbaden, 1964). Offenbar gelten die Schwierigkeiten, mit denen der Grammatiker sich auseinanderzusetzen hat, als geringer als die Probleme, vor die sich der Lexikograph gestellt sieht. Es ist daher an der Zeit, sich wenigstens über einige Hauptprobleme Rechenschaft zu geben, die der Ausarbeitung eines vergleichenden Wurzelwörterbuchs ("vgl. Wwb.") der semitischen Sprachen im Wege stehen.

1960 hat St. Segert in *Ar Or* 28, 470 ff. einen Aufsatz "Considerations on Semitic Comparative Lexicography" veröffentlicht, dessen erster Hauptteil überschrieben wurde "Past and Future of Semitic Comparative Lexicons". Wir erfahren aus ihm, dass man im 17. Jahrhundert ein *Lexicon pentaglotton* (von Valentin Schindler; 1612) und zwei siebensprachige Lexika herausgegeben hat, unter denen das *Lexicon heptaglotton* des Edmund Castellus von 1669 mit Vorrang zu nennen ist. Die sieben Sprachen, um die es hier geht, sind Hebräisch, "Chaldäisch" (-Aramäisch), Syrisch, Samaritanisch, Äthiopisch, Arabisch und das indogermanische (Neu-) Persisch. Ausser dem Akkadischen waren also alle semitischen Hauptsprachen schon vertreten[1]. Seither wurde, soweit mir bekannt ist, nichts Vergleichbares unternommen. Den methodischen Ansprüchen von heute würde ein fünf- oder siebensprachiges Lexikon auch bei Hinzufügung einiger weiterer Sprachen ohnehin nicht gerecht werden.

1970 erschien nun die 1. Lieferung eines *Dictionnaire des racines sémitiques ou attestées dans les langues sémitiques*, das auf dem Titelblatt neben dem Verfasser David Cohen auch noch Jean Cantineau nennt[2]. Nach dem Vorwort

[1] Mir ist dieses alte Wörterbuch in Münster nicht zugänglich. Ich kann daher zur Charakterisierung des siebensprachigen Wörterbuchs nichts sagen. Ein Neudruck ist geplant.

[2] Das Dictionnaire erscheint im Verlag Mouton in Paris und den Haag. Lieferung 1 umfasst XXXIV + 36 S. in 4° und kostet 21, 50 Gulden.

ging der Gedanke, ein vgl. Wwb. zu schaffen, von dem grossen Semitisten Marcel Cohen in Paris aus, der in den zwanziger Jahren Jean Cantineau für diese Aufgabe gewann. Cantineau legte eine Zettelsammlung an, gab aber um 1930 den Plan wieder auf. Erst 1955 nahm D. Cohen den Plan wieder auf und erhielt von Cantineau kurz vor dessen Tod (1956) die bis 1930 erstellten Sammlungen. In diese waren nur Wurzeln aufgenommen worden, die sich in wenigstens zwei der Sprachen Arabisch (ʿArabīja), Altsüdarabisch, Ge'ez, Aramäisch (Jüdisch-Palästinisch und Syrisch), Hebräisch und Akkadisch fanden. Cohen hat seit 1955 diese Sammlungen in mehrfacher Richtung stark erweitert. (1) wurden die seither erschienenen Wörterbücher eingearbeitet, vor allem für das Akkadische und Ugaritische. (2) Grundsätzlich wurden alle alten und modernen semitischen Sprachen einbezogen, der Wortschatz der modernen Dialekte allerdings nur in Auswahl nach bestimmten Gesichtspunkten. (3) Mit vollem Recht wurden auch Wörter, die nur in einer sem. Sprache erhalten sind, aufgenommen; denn auch unter ihnen befinden sich viele altsemitische Wörter. (4) Die Beschränkung auf die eindeutig sem. Wurzeln und Wörter wurde aufgegeben. In Auswahl wurden auch Lehn- und Fremdwörter aus nicht-sem. Sprachen wie dem Sumerischen, Iranischen, Hamitensprachen und anderen gesammelt. Unter diesen befinden sich ja Wörter, die später ganz in sem. Sprachen integriert wurden wie sumerisch má-laḫ$_4$ "Schiffer" >akk. *malāḫum* >arab. *mallāḥ* (dazu *milāḥa* "Schiffahrt" usw.). (5) Die Aufnahme von Wanderwörtern wie Pflanzen- und Tiernamen wurde beschränkt. Ein ergänzendes Verzeichnis von auf bestimmte Gebiete beschränkten Bezeichnungen ist für später vorgesehen.

Als Stichwörter wurden nur "Wurzeln" aufgenommen, d. h. Konsonantengebilde nach Art der üblicherweise angesetzten sem. Wurzeln, also z. B. für das sumer. Lehnwort *alimbû* "Bison (?)" eine Fiktivwurzel *'lmb*. Wenn mit denselben Radikalen Wörter bezeugt sind, die ihrer Bedeutung wegen nicht von derselben Wurzel abgeleitet werden können, so werden sie unter demselben Stichwort aufgeführt, aber numeriert. Zwei bis sechs Homonyme werden oft unterschieden; bei *'ll* sind es zwölf, bei *'mr* sogar fünfzehn. Damit vereinigt jedes grössere Stichwort sem. und nicht-sem. Wörter manchmal sehr verschiedener Herkunft. Innerhalb des Artikels werden zuerst die Wurzeln mit ihren wichtigsten Ableitungen in den Einzelsprachen aufgeführt. Der zweite Teil bringt dann für die Wurzeln und Wörter, bei denen das angezeigt erschien, Literaturnachweise und manchmal auch kurze Diskussionen sowie bei Lehn- und Fremdwörtern in der grossen Mehrheit der Fälle Hinweise auf die Herkunftssprache. Die Literaturnachweise konnten natürlich nur in kleiner Auswahl gegeben werden, wobei von Abkürzungen reichlich Gebrauch gemacht wird; das vorläufige Abkürzungsverzeichnis umfasst 22 Seiten. Wurzeln, die aufgrund der bekannten Lautverschiebungen oder orthographischer Gewohnheiten in den Einzelsprachen in verschiedener Lautgestalt erscheinen, werden in ihrer vermutlich ursemitischen Form aufgeführt. Die vergleichende Lauttabelle auf S. XII gibt die Möglichkeit, abweichende Formen in den Einzelsprachen wie *udn-* (ug. und aram.) und *uznum*/he. *ozen* "Ohr" zu *uḏn*(*un*) unter *'ḏn* aufzufinden. Damit wird eine grosse Zahl von blossen Verweis-Stichwörtern eingespart [3].

[3] In der Einsparung von Verweisstichwörtern geht das Dict. freilich manchmal zu weit. So sucht man unter *'ml* vergebens das jünger-babylonische

Bis hierher haben wir nur ganz neutral über die bisher allein vorliegende 1. Lieferung mit den Stichwörtern von ' bis *'tn* referiert. Wenn wir nun zu kritischen Überlegungen übergehen, so soll, dem Charakter dieses Bandes entsprechend, Einzelkritik nur so weit zu Worte kommen, wie sie zur Erläuterung der hier zu behandelnden grundsätzlichen Fragen erforderlich ist. Eine in der notwendigen Weise begründete Einzelkritik würde nicht weniger Raum einnehmen als das Buch selbst und auch die Möglichkeiten eines Einzelnen übersteigen. Sie wird zweckmässigerweise von den Spezialisten für bestimmte Sprachgruppen jeweils für ihre Sprachen vorgenommen, wenn einmal mehrere Lieferungen vorliegen werden. Hier können nur Fragen von allgemeinerer Bedeutung besprochen werden.

1) Dass ein solches Werk für die Alltagsarbeit von Semitisten aller Teilgebiete dringend erforderlich ist, bedarf keiner besonderen Betonung. Wenn dem aber so ist, so dürfen die Forderungen an ein vgl. Wwb. nicht zu hoch geschraubt werden. Denn, wenn man alle Schwierigkeiten, die vorhandenen und nicht vorhandenen Hilfsmittel für die Einzelsprachen und den Stand der Forschung gründlich und gewissenhaft prüft, so kann man eigentlich nur zu dem Ergebnis kommen, dass ein vgl. Wwb. heute und in absehbarer Zeit nicht geschrieben werden kann. Um nur über die durch Texte reichlich bezeugten Hauptsprachen zu sprechen, so gibt es für das Altsüdarabische nur Teilglossare, und die neuen akkadischen Wörterbücher sind noch unvollständig. Die Wörterbücher für einige aramäische und abessinische Sprachen lassen viel zu wünschen übrig. Vor allem aber hat es noch niemand unternommen, die vielen homonymen Wurzeln im Arabischen auseinanderzuziehen und neu zu ordnen. Da überdies, wie immer wieder festgestellt wurde[4], die in die arabischen Wörterbücher aufgenommenen Angaben der einheimischen Lexikographen von ungleicher Zuverlässigkeit sind, kann eine in jedem Fall zuverlässige Trennung der Homonyme vorläufig nicht erreicht werden. Nur mit nachgeprüften, einwandfreien Angaben wird ein sem. Wwb. heute also für keine sem. Sprache arbeiten können. Einen Grund, ein vgl. Wwb. nicht zu schreiben, kann ich darin ebenso wenig sehen wie die Indogermanisten, die für manche Sprachen auch mit unzureichendem und nur hypothetisch zu deutendem lexikalischem Material auskommen müssen. Es stellt sich allerdings die Frage, ob ein Einzelner für alle Sprachen zu dem erreichbaren Mass an Zuverlässigkeit gelangen kann.

2) Ein vgl. Wwb. ist ein Werk der Lexikographie. Die Frage ist nun, wie weit auch bei realistischer Einschätzung der gegebenen Möglichkeiten lexikologische Überlegungen die Anlage des Wwb. bestimmen und damit der Ausarbeitung vorausgehen müssen. Im Vorwort von D. Cohen ist von solchen Überlegungen kaum die Rede. Auch in den Artikeln ist von einem Nachdenken über grundsätzliche Fragen nicht viel zu spüren. Die Einstellung C.s zu den Problemen, vor die er sich gestellt sah, ist eine durchaus pragmatische; auf S. IX ist

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amī/ēlu "Mensch". Nicht jeder wird wissen, dass dieses Wort wegen der alten Form *awīlum* unter *'wl* 2 aufgeführt ist. Auch für das unter *'gs/ṣ* aufgeführte arab. *inǧāṣ* "Birne" fehlt ein Verweis unter *'ngṣ*; das zugehörige akkad. *angaš/su* "Birne" ist überhaupt nicht genannt.

[4] Vgl. z. B. W. Fischer, *Farb- und Formbezeichnungen in der Sprache der altarabischen Dichtung* (Wiesbaden, 1965), wo viele einschlägige Angaben der Wörterbücher korrigiert werden.

die Rede von einer Lösung der Probleme "de manière empirique". Grundsätzlich bejahe ich für den gegenwärtigen Zeitpunkt die pragmatische Einstellung. Ich meine allerdings, dass mindestens ein Nachdenken über die Wurzeltypen im Semitischen und über zentrale Fragen der Wortbildung auch für die Einzelsprachen unerlässlich ist, wenn man zu einer korrekten Abgrenzung der homonymen Wurzeln gelangen und bei der Ansetzung der Grundbedeutung von Verbalwurzeln und Nomina nicht dem Zufall ausgesetzt sein will. Was mir da besonders wichtig erscheint, soll im Folgenden in der gebotenen Kürze besprochen werden.

a) Zunächst einmal müssen die zuerst von B. Landsberger festgestellten, von den *Wortarten* zu unterscheidenden *Hauptwortklassen* des Semitischen beachtet werden, die ich in *GAG* § 52 kurz charakterisiert habe. Nur wenn dies geschieht, können wir wenigstens in der Mehrzahl der Fälle die Verbalwurzeln von den Nominalwurzeln sondern und innerhalb der Verbalwurzeln die fientischen Verben von den Zustandsverben, die als Adjektive auch dekliniert werden, unterscheiden. Wenn wir dann weiter für die altsemitischen Sprachen auf die Nominalformen achten [5] — vgl. für das Akkadische *GAG* § 54-57 — und bei den Verben auf die Wurzelvokale [6] sowie beim schwachen Verbum auf die Bedeutungsklassen [7], so werden wir zunächst einmal einen Grundbestand an sicher semitischen Verbal- und Nominalwurzeln herausstellen können, der innerhalb des sem. Wwb, zweckmässig durch eine etwas grössere Drucktype hervorgehoben würde. Dabei ist die in *GAG* für die Nominalformen durchgeführte Numerierung — I für nicht deverbale (Gegenstands-)Nomina, II für Adjektive bzw. Zustandsverben und III für fientische Verben und deren nominale Ableitungen — gewiss auch für ein Wwb. nicht unzweckmässig, zumal da bei einer gleichbleibenden Reihenfolge I, II, III auch ein gleichmässiger Aufbau der alle Homonyme vereinigenden Artikel möglich wäre.

b) Im Anschluss an den Grundbestand sollten unter IV und V die sicher oder sehr wahrscheinlich semitischen Nomina und Verben aufgeführt werden, die man noch nicht mit einiger Wahrscheinlichkeit den Klassen I-III zuweisen kann. Zu diesen würden vor allem viele der als Neubildungen anzusehenden arabischen Wörter gehören, deren Bildungstypen erst ganz unzureichend er-

[5] Die sem. Grammatiken führen zwar zumeist die Nominalformen auf, gehen aber oft auf deren Funktionen nicht ein. Für das Arabische geben die Wörterbücher bei den Substantiven und Adjektiven sehr zahlreiche Vokalisierungsvarianten an. Solange diese nicht untersucht sind — oft mögen Dialektformen vorliegen! —, wird es nicht möglich sein, das Kapitel Nominalformen für das Arabische ebenso auszuarbeiten, wie es für das Akkadische bereits geschehen und für das Hebräische ebenfalls realisierbar ist.

[6] Vgl. hierzu J. Aro, *Die Vokalisierung des Grundstammes im semitischen Verbum* (Helsinki, 1964 = Studia Orientalia 31).

[7] Die von B. Landsberger zuerst beobachteten Bedeutungsklassen beim schwachen Verbum sind bisher nur für das Akkadische in *GAG* § 100 ff. ausführlicher dargestellt. Einen ganz kurzen Abschnitt "Semantic Categories in "Weak" Verbs" konnte ich auf S. 168 f. der auf S. 142 genannten *Introduction to the Comparative Grammar* einfügen. Über das Wurzelaugment *n* und das teilweise damit wechselnde *w* habe ich unter Aufführung von Beispielen aus mehreren sem. Sprachen eingehender gehandelt in meinem Beitrag "*n* als Wurzelaugment im Semitischen" zu *Studia Orientalia in memoriam Caroli Brockelmann* (Halle/Saale, 1968 = *Wiss. Zeitschrift der Martin-Luther-Universität Halle-Wittenberg*, Band 17, Heft 2/3, S. 175-84).

forscht sind. Erst danach könnte man dann unter VI in kleinerem Druck die Lehnwörter aus nicht-semitischen Sprachen aufführen, die wegen ihrer Verbreitung in den sem. Sprachen in ein vgl. Wwb. aufgenommen werden sollten. Wenn man auch für diese konsonantische "Wurzeln" ansetzen will, um ihre Einordnung in das hebräische Alphabet zu erleichtern, so sollten diese "Wurzeln" etwa durch ein Sternchen deutlich als nur fiktiv gekennzeichnet werden [8]. Unterbleibt das wie oft in diesem Buch, so entsteht eine völlig falsche Vorstellung von dem möglichen Aussehen semitischer Wurzeln. Ein sem. Wwb. soll aber doch gerade auch dazu verhelfen, dass der nachdenkende Benutzer Erkenntnisse über die Gestalt der verschiedenen Typen von sem. Wurzeln gewinnt. Noch günstiger unter sprachwissenschaftlichen Gesichtspunkten wäre übrigens die Herausnahme der nicht ins Semitische integrierten Lehnwörter aus dem Hauptalphabet des Wwb. und ihre Verweisung in ein zusätzliches Wörterverzeichnis.

3) Während die lautlich korrekte Ansetzung der sem. Wurzeln nur in einer Minderzahl von Fällen Schwierigkeiten bereiten dürfte, sind die in einem solchen Wwb. zu bewältigenden semantischen Probleme vielschichtiger. Cohen verzichtet bei Verben mit stark verschiedenem Gebrauch in den Einzelsprachen wie z. B. *'mr* — akk. "sehen"; hebr., aram. und arab. "befehlen"; äth. *a'mara* "erkennen" — auf die Ermittlung einer Grundbedeutung, aus der man die einzelsprachlich bezeugten Bedeutungen ableiten könnte, und zitiert nur einen Vorschlag dazu [9]. Diese Zurückhaltung mag man bedauern; sie ist unter den gegebenen Umständen aber doch wohl das Richtige. Oft genug ist ja selbst einzelsprachlich die Grundbedeutung nicht mit Sicherheit feststellbar. Es ist auch fraglich, ob wirklich bei allen Wurzeln etwas wie eine klar umrissene "Grundbedeutung" in voreinzelsprachlicher Zeit anzusetzen ist. Eine Folge dieser Unklarheit ist, dass wir oft zwischen stark abweichendem Sprachgebrauch bei ein und derselben Wurzel und homonymen Wurzeln nicht sicher unterscheiden können. Bei Beachtung des oben über die Hauptwortklassen und die Bedeutungsklassen beim fientischen schwachen Verbum Gesagten wird man sicher oft weiterkommen können als in diesem Wwb. Fortschritte in der Durcharbeitung des Lexikons der Einzelsprachen werden bei anderen Wurzeln klarere Angaben ermöglichen. In vielen hundert anderen Fällen wird man aber nie mehr tun können, als die Angaben der Lexika für die Einzelsprachen zusammenzustellen.

Ich habe hier bewusst kein im Niemandsland angesiedeltes Ideal eines sem. Wwb. skizziert. Aber auch die Anforderungen, die ich für unverzichtbar halten möchte, sind so hoch, dass ihnen angesichts der Fülle des Stoffes kein einzelner allein entsprechen kann. Jeder, der ein solches Werk plant, sollte nach Mitarbeitern suchen, die für ihm weniger vertraute Sprachen bzw. Dialektgruppen die Verantwortung übernehmen. D. Cohen hat sich ein bescheideneres Ziel gesteckt und meinte daher, der Arbeit im wesentlichen allein gewachsen zu sein, nachdem ihm die Starthilfe von J. Cantineau und M. Cohen

[8] In Lieferung 1 fehlt bei mehreren solcher Fiktivwurzeln jede Kennzeichnung, so z. B. bei *'brn* (das nur altassyrische *abarnīum* ist von dem Ortsnamen *Abarne* abgeleitet!), *'bšm* (*abašmû* ist ein Lehnwort unbekannter Herkunft), *'rṯ* 1 als angeblicher Wurzel von *sirāšu* und *tīrōš* usw.

[9] Dieser Vorschlag ist abzulehnen, weil das transitive Verbum *'mr* nicht auf ein Zustandsverbum mit der Bedeutung "sichtbar sein" zurückgehen kann.

zuteilgeworden war. Er hat damit seine Möglichkeiten überschätzt. Es gibt dadurch in dieser 1. Lieferung mehr an vermeidbaren Irrtümern und Unkorrektheiten, als dem Benutzer lieb sein kann, obwohl C. eine erstaunliche Arbeitsleistung vollbracht hat. So erscheint akk. *(w)at/ṭmānum* "Heiligtum" ohne Querverweise sowohl unter *'dm* als auch unter *'ṭm*, weil beide Ableitungen früher einmal erwogen wurden; auf *wtm* wird nicht verwiesen. Ebenfalls auf S. 9b erscheint ein akk. *adnat-* "parcelle de terrain", das weder *CAD* noch *AHw* verzeichnet. Unter *'dn* wird he. *'ādōn* "Herr" mit akk. *danān-* "puissant" verglichen (Verwechselung von *dannum* und *danānum*)![10]. Eine Hauptursache für solche Versehen ist das unkritische Ausschreiben älterer Literatur, ohne deren Angaben mit neuerer Literatur zu vergleichen. Da es offenbar oft nicht möglich war, die zu den einzelnen Wurzeln gesammelten Hinweise vor dem Druck noch einmal durchzusehen, wird eine Fülle von Literaturverweisen geboten, denen nichts zu entnehmen ist. Hierher gehört z. B. die Liste von akk. Lehnwörtern im Hebräischen, die H. Zimmern zu *KAT*[3] (1903), 648 ff. beigesteuert hat. Sie enthält nur ganz wenige Quellenhinweise und ist durch Zimmern's *Akkadische Fremdwörter* (1917) überholt, das seinerseits heute auch nicht mehr überall ohne Kritik übernommen werden kann[11].

Angesichts der nicht geringen Zahl von Fehlern, die, eben weil der Verfasser allein gearbeitet hat, auch bei den zitierten Wortformen unterlaufen sind, wird der Benutzer den Wunsch haben, die Angaben in den jeweils neuesten Wörterbüchern der Einzelsprachen zu überprüfen. Hier stösst er nun auf die Schwierigkeit, dass er sehr oft nicht weiss, woher die Angaben stammen. Denn es fehlt eine Übersicht über die lexikalischen Hilfsmittel (Wörterbücher und Glossare), die für die Einzelsprachen verfügbar sind und benutzt wurden. Nicht jeder weiss z. B., dass er für das Soqotri das *Lexique Soqotri* von W. Leslau zu Rate ziehen muss. Ein Verzeichnis der benutzten Lexika und Glossare sollte daher, nach dem Alphabet der Sprachen geordnet, baldmöglichst nachgeholt werden.

Jeder Lexikograph weiss, dass sein Werk auch bei intensivstem Bemühen Mängel aufweist; er kommt ohne den Mut zum Irrtum, der nicht mit Leichtfertigkeit verwechselt werden darf, nicht aus. Der Verfasser eines vergleichen-

[10] Nur noch einige weitere Korrekturen für das Akkadische: Die Nebenform *ḫajābu* zu *ajjābum* "Feind" ist zu streichen, weil nur einmal in einem Amarna-Brief bezeugt. — Einen Infinitiv *iḳû* (S. 18b) gibt es nicht. — *ul* (S. 19b) ist keine Prohibitivnegation! — *šuluṣu* "fermer" (S. 21b) gibt es nicht. — Die Unterscheidung zwischen der normalen Vokallänge (*û*) und der Kontraktionslänge (*û*) ist für das Akkadische in einem Wörterbuch unentbehrlich! Cohen verwendet nur *û* usw. Zu hebr. *ebjōn* "arm" wurde mein Artikel in *MIO* 15/1969, 322 ff. übersehen (zu *abijānum* in Māri); die Wurzel *'bjn* ist zu streichen.

[11] 9 Literaturverweise zu sum. *egal* > akk. *ekallum* "Palast" > hebr. *hēkāl* usw sind gewiss nicht nötig. Bei strengerer Auswahl gäbe es sicher weniger Zitatfehler! Aus den Zitaten *JAOS* 35/1953 und *JAOS* 35/377 auf S. 18a das richtige Zitat *JAOS* 73 (1953), 7f. herauszulesen, fällt nicht ganz leicht, wenn man vollständige Serien der Zeitschriften nicht zur Verfügung hat. Auch Namen sind mehrfach verschrieben. Wenn wenig bekannte Arbeiten wie z. B. Noordtrij, *Het Hebr. Voorzetsel 'l* (S. 19b) zitiert werden, sollte die Jahresangabe nicht fehlen, da ohne sie das Buch schwer zu beschaffen ist. Schliesslich sollte auf die Abkürzungen geachtet werden. Nach dem Verzeichnis steht Emp. nur für Empire. Tatsächlich wird es aber anstatt und neben empr. auch öfter für emprunt Entlehnung gebraucht.

den Wörterbuchs ist der Gefahr zu irren noch viel öfter ausgesetzt. Angesichts des etwas enttäuschenden Ergebnisses der vieljährigen Bemühungen D. Cohen's habe ich den Eindruck, dass er nicht nur über die Grundsatzfragen zu wenig nachgedacht hat, sondern auch sich selbst gegenüber zu wenig kritisch war. Er hätte sonst merken müssen, dass sein ursprünglicher Plan seine Kräfte überstieg. Vielleicht hat er es inzwischen erkannt und zögert daher mit der Ausgabe der 2. Lieferung. Ich würde es sehr bedauern, wenn er etwa nun den Mut zur Weiterarbeit verlieren würde. Aber ich meine, dass es unerlässlich ist, die Arbeit jetzt ganz neu zu organisieren. Die Gewinnung einiger für die verantwortliche Betreuung bestimmter Bereiche vorgebildeter und interessierter Mitarbeiter ist vordringlich. Mit ihnen zusammen müsste das gesammelte Material kritisch durchgesehen, gesichtet und unter Berücksichtigung der Erkenntnisse über die Hauptwortklassen neu geordnet werden. Da eine vollständige Erfassung des in den einzelsprachlichen Wörterbüchern enthaltenen Materials ohnehin nicht möglich ist, sollte auf die Aufnahme von Wörtern unsicherer Lesung und von fragwürdigen Deutungen nicht zuletzt auch in den arabischen Wörterbüchern ganz verzichtet werden. Damit wird viel Zeit gespart, die dann für die bessere Durcharbeitung des übrigen Materials und eine übersichtlichere Gliederung der Artikel zur Verfügung stände. Sicher könnte man auch aus den vorliegenden indogermanischen etymologischen Wörterbüchern methodisch einiges lernen[12]. Ich meine, dass bei Zusammenarbeit einiger Semitisten mit verschiedenen Schwerpunkten das Werk in nicht allzu langer Zeit zum Abschluss gebracht werden könnte, einem Abschluss, der die vielen Benutzer und nicht zuletzt auch David Cohen selbst, dem wir für seinen Mut, zu beginnen, zu aufrichtigem Dank verpflichtet sind, zufriedenstellen kann. Dazu eine kleine Hilfestellung zu geben, ist das Hauptanliegen der Überlegungen zu einigen ausgewählten Fragen, die ich hier vorgetragen habe und zur Diskussion stelle.

[12] Das neueste ist J. Pokorny, *Indogermanisches etymologisches Wörterbuch* (Bern-München, 1959-69), dessen erster Band nach den rekonstruierten ur-indogermanischen Formen geordnet ist; Band 2 enthält die Indices für die Einzelsprachen. Ältere Werke sind dort genannt.

A Computer Program for Analysis of Words According to Their Meaning

(Conceptual Analysis of Latin Equivalents for the Comparative Dictionary of Semitic Languages)

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This contribution is based on a co-operative work of both authors. The first has provided the lexical material, and has presented general information and a description of the lexical material used. The second author worked out a program for the computer and proved its validity and usefulness.

I. Preparation of the Material for the Comparative Dictionary of Semitic Languages

Professor Ignace Jay Gelb to whom this contribution is respectfully dedicated was always very much interested in innovative methods in linguistics. With his understanding, advice, support and active participation, he stood at the very beginning of some new applications of mathematics and of computers for linguistic purposes which developed eventually into very important fields of research. The project of the *Comparative Dictionary of Semitic Languages* enjoyed the interest of Professor Gelb since 1960. The participation of the author on Professor Gelb's Amorite project [1] in 1966 provided the opportunity to discuss some theoretical devices for the dictionary project.

The unavailability of complete modern dictionaries of the Semitic languages has limited this project to the Northwest Semitic group consisting of so-called Canaanite and Aramaic languages [2]. Work on the project was undertaken on behalf of the Oriental Institute of the Czechosloyak Academy in Prague, 1960-1968. The initial methods for computer processing were developed in close co-operation with Miro Valach, then research fellow of the Institute for Computer Research in Prague. The pilot project was programmed by Leoš Čapka,

[1] Cf. report by Joyce Bartels, Résumé of Prof. Segert's section of project 2BQ003 accomplished at the University of Chicago, April-October 1966.

[2] Cf. "Considerations on Semitic Comparative Lexicography I-II", *ArOr* 28 (1960) 479-487; "A Preliminary Report on a Comparative Lexicon of North-West Semitic Languages", *Trudy dvacat'pyatogo meždunarodnogo kongressa vostokovedov*, 1, (Moskva 1962) 383-385.

programmer in the Computation center INORGA in Prague for the IBM 1410 computer, and was completed May 17, 1967. More than 3000 lexical items from Old Aramaic dialects, Ya'udic, Early, Imperial, Biblical, were processed at that time [3].

This lexical material was arranged according to three criteria. The first listing showed the material arranged according to tri-consonantal roots, while the words not adapted to the tri-consonantal nominal and verbal patterns were put aside. The second arrangement was according to nominal and verbal patterns. The third, according to the meaning, was done simply by setting the Latin equivalents of the Semitic words into alphabetic order. While the arrangements according to the roots and the patterns fulfilled the requirements of the project, the arrangement according to the alphabetic order of the Latin equivalents was considered provisory, to be complemented by a more satisfactory classification according to the meaning of the Semitic words.

Further stages of the dictionary project are now being carried out at the University of California, Los Angeles. The lexical material from Syriac and other East Aramaic languages is being prepared for computer processing.

The Comparative Dictionary project is based on existing dictionaries of the individual Semitic languages. The meanings of the Semitic words were transferred from these sources except in a very limited number of cases. For practical reasons, if more than one equivalent was indicated in a dictionary it was necessary to select only one. The selection was in many respects affected by subjective considerations. In some cases a meaning which appeared to be fundamental, and from which the other meaning could be derived was selected, while in other cases the meaning which was indicated as most frequent by a dictionary or by a concordance was entered. Also, it was possible to use only one Latin equivalent for a Semitic word. In isolated cases, when the meanings appeared to be too disparate, they were entered together with the Semitic word, as two different items.

Latin was chosen as the language for the equivalent words for more than one reason. The Semitic languages contained in the Comparative Dictionary are attested from Antiquity and from Early Middle Ages. Latin as a language contemporaneous to them is quite suitable for espressing the meaning of the words. The Latin words with their well discernible affixes and prefixes are not so equivocal. If English words were used, some additional information would be necessary. The structure of Latin words proved also very convenient for the semantic analysis, even though this was not planned in the beginning of the project.

The classification of words according to their meaning was used for practical and paedagogical purposes since Antiquity [4]. J. Amos Comenius arranged the concepts of Latin in his *Janua Linguarum* which since 1631 served as a conven-

[3] Cf. "Tendenzen und Perspektiven der vergleichenden semitischen Sprachwissenschaft", *Studia Orientalia in memoriam Caroli Brockelmann – WZ der Martin-Luther-Universität, Halle-Wittenberg,* 17 (1968) 167-173; "Hebrew Bible and Semitic Comparative Lexicography", *Congress Volume Rome 1968* (*SVT* 16), pp. 204-211; "Die Arbeit am vergleichenden Worterbuch der semitischen Sprachen mit Hilfe des Computer IBM 1410", *XVII. Deutscher Orientalistentag,* Vorträge, Teil II, 1969 (*ZDMG,* Supplementa I), pp. 714-717.

[4] Cf. F. Dornseiff, *Der deutsche Wortschatz nach Sachgruppen* (Berlin 1954[4]), pp. 29-33.

ient textbook for more than two hundred years [5]. Words are arranged according to the areas of meaning in which they are used in the Lexicon of German by F. Dornseiff [6]. A similar system presented by R. Hallig and W. von Wartburg is based on French, but was conceived to be used also for other languages [7]. English words were classified by P. M. Roget. His Thesaurus [8] has the advantage that within the areas of use the words are also classified according to the categories of nouns, verbs, adjectives and adverbs. The use of this method in the area of Semitics has been limited to lists of words of Biblical Hebrew compiled for elementary paedagogical purposes [9]. The arrangement of the words according to the meaning was also applied is selective comparative dictionaries, for the Indo-European languages by C. Buck [10] and for Semitic languages by P. Fronzaroli [11]. This method corresponds in principle to the modern linguistic concept of word fields or semantic fields [12]. The other methodical approach uses the analysis of the function of words in the sentence [13].

For application to the Dictionary Project, neither of these two existing approaches seemed fully satisfactory. The basic framework of the extant conceptual dictionaries can be used, while a more systematic classification of the meanings could be interpolated. The recent work in componential analysis frequently hints to a systematic characterization of the meaning of words but none of the proposed methods seems to be suitable for digesting large quantities of lexical material. Further, these methods were developed and tested on limited and somewhat intentionally selected sections of lexicon. Neither approach seems to lend itself easily to computer processing.

The selection of Latin as the Language for the equivalents proved advantageous, because Latin words have many clearly discernible prefixes and affixes conveying a semantic message [14]. These morphemes were therefore used for classification of meanings. There was of course an obvious danger to be avoided: the technique must not present a componential analysis of Latin words but rather use the structure of Latin words to devise some generally valid criteria for the classification of concepts.

[5] *Janua linguarum reserata*, s.l., (1631); *Orbis sensualium pictus*, (Nürnberg 1658); cf. *Orbis pictus*, ed. H. Jarník, (Brno 1929). Cf. Dornseiff, op. cit., p. 34.

[6] Dornseiff, op. cit.

[7] R. Hallig – W. von Wartburg, *Begriffssystem als Grundlage für die Lexikographie* (Berlin 1952).

[8] *Roget's International Thesaurus* (New York 1962).

[9] E.g., J. Weinheimer, *Hebräisches Wörterbuch in sachlicher Ordnung*. (Tübingen 1918).

[10] C. D. Buck, *A Dictionary of Selected Synonyms in the Principal Indo-European Languages* (Chicago 1949).

[11] P. Fronzaroli, "Studi sul lessico comune semitico, I-VI", *ANLR*, ser. VIII, 19, 5-6 (1964) 155-172, 243-280; 20, 3-4 (1965) 135-150; 20, 5-6 (1965) 246-269; 23, 7-12 (1968) 267-303; 24, 7-12 (1969) 285-320. Cf. also P. Marrassini, *Formazione del lessico dell'edilizia militare nel semitico di Siria, Quaderni di Semitistica*, 1 (Firenze 1971).

[12] Cf. e.g., J. Lyons, *Introduction to Theoretical Linguistics*, (Cambridge 1969), pp. 429-433, 488; cf. also pp. 470-481, 489.

[13] Cf. e.g., J. J. Katz and J. A. Fodor, "The Structure of a Semantic Theory", in J. A. Fodor and J. J. Katz, eds., *The Structure of Language* (Englewood Cliffs, 1964), pp. 479-518. Cf. U. Weinreich, "Exploration in Semantic Theory", in Th. A. Sebeok, ed., *Current Trends in Linguistics*, Vol. III, *Theoretical Foundations* (Den Haag 1966), pp. 395-497.

[14] Cf. e.g., V. Pisani, *Grammatica latina storica e comparativa* (Torino, 1952²), pp. 88-136, 229-244.

The final listing of semantically relevant Latin affixes and prefixes used for the project was based on a complete list of Latin words arranged in both progressive and retrogressive alphabetic order by O. Gradenwitz [15].

A computer program was written to extract the prefix and affix from the equivalents and sort material according to these forms. The program developed by Mr. Hall was initially tested on the material selected from the letter *b* of the Syriac Lexicon [16]. The Latin equivalents representing this set can be considered a random selection from the view-point of the Latin lexicon.

The program handled successfully the great majority of the Latin words. It appeared that the prefixes were not so important semantically, but it was necessary to isolate them in order to come to the core of the word. The affixes were characterized as bearers of relevant semantic information and with their help it was possible to classify the overwhelming majority of the nouns and many verbs. Some secondary phonetic changes in Latin obliterated or confounded the original forms of the morphemes, but even here it was possible to use the secondary forms for their identification, e.g. *-sus* and *-sio* as variants of *-tus* and *-tio*. Of course, for homonyms and for some cases where the morphemes appeared identical with some unrelated ones (e.g., *de-mens* looks like a participle on *-ens*) it was necessary to make some manual corrections on an individual basis.

In general, the program developed by Mr. Hall proved to be a very helpful tool for semantic classification of Latin words. It enables classifications according to characteristics of affixes and prefixes. It also isolates the core of words for further analysis and classification [17].

The analysis of the cores remains as a further task. Some criteria used successfully in the above-mentioned conceptual dictionaries will be applied [18].

II. Sorting Lexical Material According to the Meaning

A computer-aided technique has been developed which can assist linguistics in generating semantically ordered vocabularies of natural languages [19]. Given an arbitrarily ordered list of words defined in terms of a word from an intermediate language, a PL/I program will sort suffixes, prefixes, root forms, and special keys to arrive at a "concept ordered" dictionary.

By definition a concept ordered dictionary is a semantically arranged, one-dimensional list containing the words from the vocabulary of a particular language. Since words are only arranged, not translated, the exact meaning of the word is not important to the classification program. However, the mean-

[15] O. Gradenwitz, *Laterculi vocum Latinarum* (Leipzig 1904).

[16] According to C. Brockelmann, *Lexicon Syriacum*, (Halle, 1928²). The key-punching was done by D. Cargille, graduate student at U.C.L.A.

[17] Only after finishing the draft of this article, the authors got the information about a similar project; C. Dudrap and G. Emery, "Sorting the French vocabulary according to word endings", in R. A. Wisbey, ed., *The Computer in Literary and Linguistic Research* (Cambridge 1971), pp. 87-92.

[18] The author expresses his thanks to Professors of the Department of Computer Science at U.C.L.A., M. A. Melkanoff (Chairman), W. J. Karplus and A. Svoboda for providing the opportunity for cooperation.

[19] J. R. Hall, *A System for Automatic Classification of Lexical Material*, Master Thesis, UCLA Library, 1972.

ing relative to other words must be derived. Fortunately the problem can be reduced, since in a comparative dictionary words with the same grammatical usage must be clustered together. The arrangement according to relative meaning must be performed within these clusters. Analysis of affixes and prefixes of Latin words can usually determine their correct grammatical usage. By sorting the Latin words according to the word forms with a computer program, a large portion of the work needed to produce a comparative dictionary can be completed.

Lists of all possible prefixes and endings must be prepared and entered into the program as data. These lists will accomodate the segmentation algorithm which examines each word for prefix and ending. To locate a prefix the program examines a word character by character from left to right. A list of prefixes is searched also. The longest string of characters in the word which has an entry in the prefix list will be used as the prefix. Similarly, a suffix list is searched and the longest string of characters starting from the last character of the word and going right to left will be used as the suffix. A suffix or a prefix may not necessarily be found. The core of the word is the character string bounded by the prefix and the suffix. A special character (%) if found in the character string delimits the word core boundaries in special cases.

Prefix and ending lists need only be prepared once. Since less than one hundred fifty items were needed for this project, the amount of effort to prepare the lists was relatively small.

Once segmentation is correctly performed on all the equivalent words, they can be sorted according to the suffixes to obtain clustering according to grammatical usage. Now semantic assortment must be started inside each cluster. By alphabetically sorting the cores within these groups it is possible to gain even more semantic resolution because all words with the same cores will be grouped together. Another level of sorting on the prefix will slightly improve the ordering.

A "classification key" is a character string which is associated with a particular word in the lexical material. It does not necessarily apply to the Latin equivalent, for it is used to relate semantic data to the source word. A "classification value" is used in the same way as a classification key, but it is always a numeric character string representing a positive integer value. The keys and values are used as sorting information and they are the means by which relative meanings between words are specified to the program. Every word in the vocabulary to be analyzed is represented by at least one equivalent word in the intermediate language, a set of classification keys, and a set of classification values.

The classification program is used in the following manner to generate a comparative dictionary. The lexical material containing the source vocabulary, equivalents, prefix list, and the suffix lists are read into the program from punched cards.

First, a listing of all words in the vocabulary according to the alphabetical sequence of the language is obtained. The program can sort according to the alphabetical sequence of any symbolic language. At the same time, the segmentation results on the equivalent words are checked. Corrections to the input data can be made before the next run. Next, the classification program is employed again to obtain a sorting based on endings, bases, and prefixes resulting in the clustering of words with respect to grammatical usage. Next,

keys and values are added to the results of the last run to obtain more specialized sortings until the desired concept ordered listing is produced. Finally, the program can produce "comparison keys" which will summarize the key-value-ending classifications already available. When more than one comparative dictionary exists, the comparison keys can be used in additional procedures which will correlate word content with word forms among the languages.

The lexical classification system was implemented on an IBM 360 Model 91 operated by Campus Computing Network at UCLA. The URSA MINOR system was used to submit remote batch jobs. URSA's on-line editor was used to modify both input and output data sets.

A single batch program was developed to interpret the input data supplied by the linguist, translate the special characters of the foreign words, segment the meta language word and control sorting. This program will be referred to as ALEXIA, Automatic LEXIcal Analysis. The ALEXIA program was written in PL/I (F). A copy of the complete program is available from the author upon request. The PL/I language was chosen because the input/output and character manipulation features of the language were particularly suitable; the PL/I system sort package was used for the sorting operations.

A description of a program's input and output is usually helpful in describing its primary operations. A single punched card was used for each Syriac word. Each card contains a Syriac word expressed in a substitution alphabet, one or two Latin equivalents (punched in the standard alphabet) and a set of comparison keys which relate special meaning to the Syriac word. Fig. 1 shows a small sample of typical Syriac data. The first two columns coded the usage of the verbs. The eighteenth column was used to code the root type of some of the Syriac words. Alphanumeric codes were added by the linguist in order to furnish additional information about certain Syriac words. The codes may also be used by the classification program as supplementary sorting information.

There are primarily two outputs from the ALEXIA program. The principal output is a listing of the sorted Syriac words and their equivalents. The listing contains cross-referencing information, the results of the segmentation precedure, and semantic information corresponding to the meanings of the suffixes and classification keys. Fig. 2 shows a sample of some classified words sorted according to the suffixes of the Latin equivalents. The second output is a file consisting of Syriac words and their equivalents sorted in the same way as the listing and placed on a data set with the exact format as the input deck. This second output file can be saved for additional passes through the ALEXIA program.

ALEXIA performs the following functions:

a) Interpret user commands
b) Interpret and store data items
c) Decifer user's alphabet and alphabetical sequence
d) Derive prefixes and suffixes from source words
e) Interpret alphanumeric keys and integer values
f) Perform sorting on the basis of a simple SORT BY statement
g) Output page headers, user comments, program limitations, sorting option summaries
h) Output classified words to an on-line data set for subsequent modification.

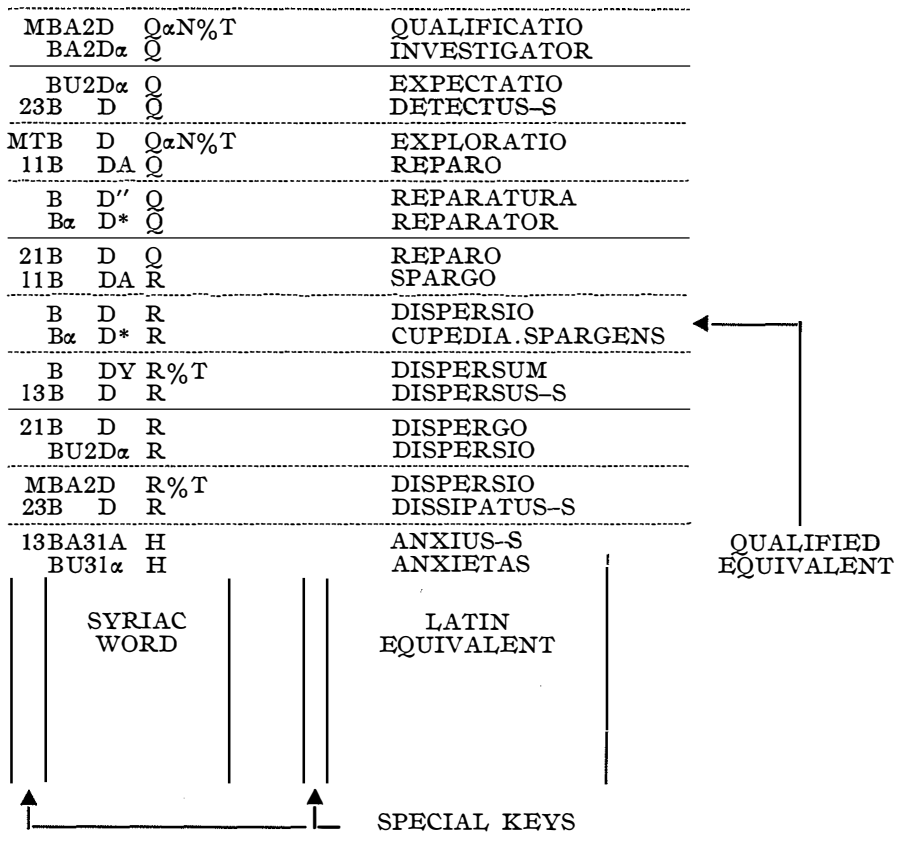

Fig. 1. – The lexical input to ALEXIA contains on each card a word from the source language, an equivalent phrase in the metalanguage, and a set of characters with special meanings.

Work under the comparative dictionary project resulted in the development of a general computer program which classifies lexical material The program produces arrangements of words from a language according to their relative semantic values by applying techniques which derive and examine the prefixes, roots, and suffixes of the words. In addition, a method by which the linguist can develop his own encodings, relate semantic information to them, and use them as a basis for classifications was developed and was a significant contribution.

Automatic classification based on Latin words produced clusters of words into semantically-related groups. A computer-aided modification and rerun scheme was used to refine these groupings into a concept-oriented structure. The actual conceptual framework was not defined by the program but rather by the user through his manipulation of the input data and the control language.

An interpretive control language was developed for the program to promote generality, flexibility, readability and convenience. As a result the system is

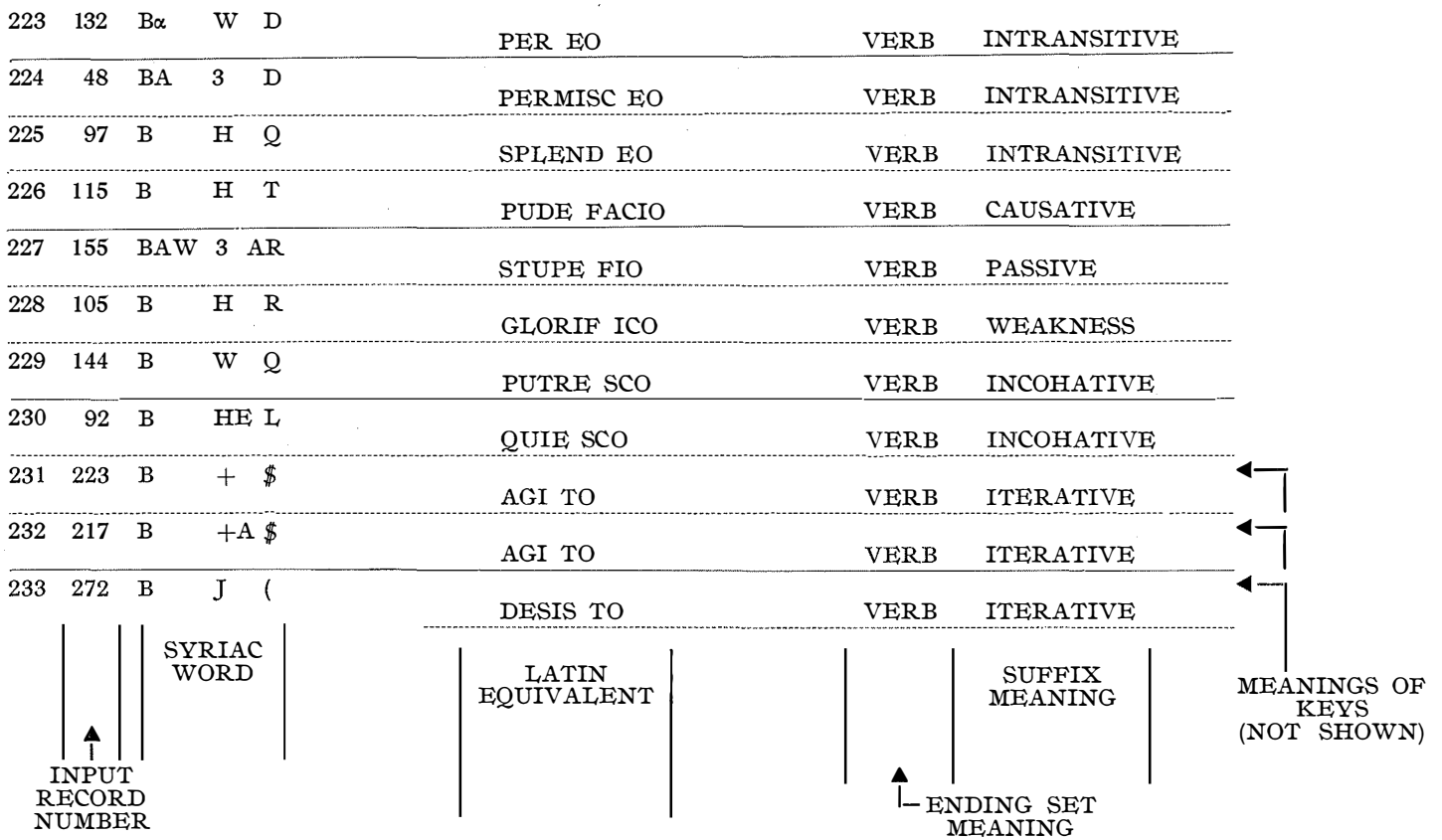

Fig. 2. – The classified words are listed showing the meanings of the ending set and ending of the equivalent [20].

[20] In DESISTO the iterativity is expressed by repeating of S(I), but incidentally the mechanical analysis gives the right characteristic.

quite general with respect to what languages can be examined even for a variety of alphabets and word forms. The technique is limited, however, to alphabetic languages where words are constructed from the symbols of an alphabet.

The compilation and examination of words from the vocabulary of even a small dialect inevitably involves a large volume of data. Up to ten thousand words might eventually be classified for certain languages in the project. ALEXIA was designed to minimize data volume problems to the user. The program makes use of on-line disk storage while the lexical data can be scanned and modified from a remote CRT terminal.

A 300 Syriac word test case proved the feasibility of the techniques used. In fact, the results of the automatic segmentation were better than expected. From 4 percent to 8 percent of the Latin words required special attention from the linguist. The reiteration method along with the key and value sorting items have proven to be quite useful in achieving final classifications, while the restricted key concept enables the linguist to try new encodings and to handle special cases without programming changes. The general features of free format input, interpretive control language, user documentation facilities, controlled selection of data, and the sorting statement all make the system very user-oriented which should prove to be the system's greatest advantage.

ALEXIA is not a concept ordering system in itself. Rather it is a tool which provides a number of features to assist the linguist in classifying lexical material. Thus, the linguist will supply the means and the information necessary to achieve his goal. This computer-aided approach to linguistic problems clearly separates the linguistic analysis from the programming yet it brings the linguist truly closer to utilizing the full capabilities of the computer [21].

[21] The authors will gladly provide more detailed information about this project. Their addresses are: S. Segert, Near Eastern Languages, University of California, Los Angeles, CA 90024, and J. R. Hall, 1808 Spreckels Lane, Redondo Beach, CA 90278.

Problems of Translation

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The Italian saying, "traduttore, traditore", most aptly sums up the difficulties, and dangers, of that particular literary exercise, the rendering of a given text into a different language. Yet if most people agree that there are indeed "untranslatable" pieces (poetry is the common example), few would admit, or even realize, that there is scarcely a text susceptible of impeccable translation. By "translation", of course, I do not mean the mechanical substitution of, say, English words for the corresponding French words. It is more than the mere meaning of words or even sentences that "translation" implies. The good translator must offer, beyond the superficial meaning, the "true" meaning, the style, the mode of thinking of the translated author which is, in fact, conditioned by his philosophical and cultural environment. This, obviously, cannot be pushed to its extreme consequence for it could only result in a simple refusal to translate. The words *pain*, or *maison*, or *déjeuner*, conjure up certain pictures in the mind of a Frenchman which are quite different from the pictures brought to the mind of an Englishman or a German by the corresponding words *bread, house* and *breakfast* or *Brot, Haus* and *Frühstück*. Indeed, when a Frenchman wants to convey the image evinced by the English *breakfast* he uses, in French, the English word. Conversely, an Englishman will say *French bread*, which, however, cannot be retranslated *pain français*, such a translation, albeit "correct", being meaningless (and in any case *French bread* denotes only a certain type of French bread). Clearly, unless one wishes to be facetious, or ridiculous, one cannot but translate *pain* by *bread* or *Brot*. Similar examples pertaining to loftier planes could easily be found. The main point is that the translator's faithful rendering of all aspects of the foreign language cannot be achieved at the expense of the corresponding features of his own language. In fact, the main characteristic of a good translation is that it does not sound like a translation at all. The translator must simply try to rethink his text and re-express it in a different language.

In the case of Sumerian, and even Akkadian, the problems confronting the translator are, of course, considerably bigger; for here, we not only deal with foreign languages, we deal with an alien, and for all purposes dead, culture. We have not only to cope with words which, especially in Sumerian, are only too often just words: we also have to penetrate modes of thinking which we cannot really hope fully to understand. The Sumerian verbal phrase *šu ti*, for instance, is usually translated "to receive", mainly because its Akkadian equivalent is *leqũ*. If we analyse the phrase, however, we find that the literal meaning is "to bring the hand near" and precisely because it is the literal meaning of the phrase, and we aim at "good" translation, we shall put "he received a mina of silver" rather than "he brought the hand near a mina of silver". The

latter, however, might well be thought a better translation since it would show that the Sumerian expression of the concept "to receive" was not synthetic but analytic. Yet if by translating "he received" we may not be doing full justice to the Sumerian, at least we use normal English. I think it is imperative to avoid the pitfall of "poetic" speech of the "Peace be with you!" type when all that is meant is "Good morning".

Mercifully, gone are the days of Oppert who would turn Mesopotamian gods into Graeco-Roman ones, and normally translate Sumerian and Akkadian into Latin—a noble language, I suppose, being only translatable into another noble language. (He once translated a piece of Sumerian hymn into Sanskrit, but that was only to prove beyond any doubt that Sumerian was indeed a real language!). Also on the way out seems to be the strange assumption that the most trivial sentences in Sumerian were pompous statements couched in majestic style: only *thou, thee* and *thy* were good enough for a merchant instructing his employee. Yet, curiously, if literal translations resulting in "poetic" language are on the whole avoided, literal translations resulting in no language at all[1] seem to be still pretty widely used ("barley to PN let him give!").

We have so far dealt with, in a way, extreme cases which can be, indeed should be, easily overcome. But there are a number of hard-core problems which are not all easily solved. They may be grouped under three main headings: problems of vocabulary, problems of syntax, and problems of context. We shall examine them briefly in that order.

The vocabulary of Sumerian is admittedly a stumbling block. Our knowledge of it is still rudimentary and we depend to a very large extent on the Sumero-Akkadian equations bequeathed to us by Babylonian and Assyrian scholars. There is still a considerable body of words which are no more than empty shells. We may perhaps know, or presume, that a given word denotes a tree, or a metal, or a manufactured object, but that is as far as we can go. In too many cases the juxtaposition of *signifiant* and (specific) *signifié* still escapes us. Even when we are reasonably certain of the meaning of a word we have to bear in mind the unavoidable variations in content entailed by three thousand years of history. This is particularly true of names of functions or professions. These, in any language, tend to survive in form through the ages although their connotations, and even their basic meaning, may have changed beyond recognition. Thus the terminology of officialdom of the Roman Republic remained almost intact through the Empire: the form was the same, the substance was not. The word *king* does not mean the same thing in present-day Britain as it did in the sixteenth century, or in archaic Rome, or in ancient Israel. Yet we can still use the word and translate it into other languages because the context supplies the nuances of which, anyway, we are aware. In other cases this is not possible because the meaning of the word has not just varied within the same semantic range but has transferred into another one. Thus *constable* will be translated into French as "connétable" in the case of the "Constable of France", and as "gendarme" or "agent de police" in the case of "Constable Bloggs".

Which brings us to the question, does one translate *ensi* or *išš*a*kku*, for example, by one and the same word regardless of the historical context, thus failing to convey immediately the fact that we may be dealing with an independent ruler, an appointed governor, or a farmer? Or does one use different terms, thus failing to convey the fact that the original language uses one and the same

word? Or take the case of *rabiānum*, usually translated "mayor" although the term may designate the head of a town as well as of a nomadic group, in which case the word "mayor" is hardly apposite. Of course, when our translations are aimed at fellow-Assyriologists we can take an easy way out of the difficulty by retaining the original word, relying on our readers' awareness of the implications. But if "translations" such as "the *sukkal* has given the *ensi* two *ḫalub*-trees" (or, perhaps because it is felt to sound more like a translation, "the *sukkallu* has given the *iššakku* two *ḫuluppu*-trees") may be deemed good enough among the initiate, they certainly won't do for the "general public". The problem is real and I shall not presume to suggest a solution other than the expedient of making a decision and sticking to it, with due warning to the reader.

There is perhaps an easier answer to another problem — that of words belonging to the same general semantic range and being used concurrently or even simultaneously. I have in mind pairs like *dù* and *dím*, *šu ti* and *dab*$_5$, *sumu* and *ba*, or the phrase *in rīšātim u ḫidi'ātim*. Here, I think, the translator, in so far as his own language will allow it, should select semantically related words and decide once and for all which will translate which, enabling the reader not only to get the meaning but to know which word stood in the original. Thus, for the pairs mentioned above, one could perhaps have "to build" and "to fashion", "to receive" and "to take in", "to give" and "to present with", "joy and jubilation".

Conversely, the same word may mean different, sometimes antonymous, things. Here one has no option but to use the word demanded by the context: *gin* may have to be translated "to go" or "to come", *kú* "to eat" or "to consume", *zi*(*g*) "to debit" or "to credit", and so on. There is in fact no great difficulty since, contrary to the case just discussed, the reader can immediately identify the original word. The only snag is that the context must be available for the actual meaning cannot be deduced from isolated phrases in which the words in question occur. The invitation *Entrez*! has to be translated "Come in!" or "Go in!" depending on the situation and a precise translation is possible only if that situation is known. On the other hand, whereas *Come in*! and *Go in*! immediately convey the relative position of speaker and hearer, the translation "Entrez!", which applies equally to either phrase, does not.

Turning now from problems of vocabulary to problems of syntax, one finds that they are at once more difficult to solve and easier to avoid. As a matter of fact, translations of Sumerian texts simply ignore most of the syntactic features of the language, and can do so without missing the point. Some cases are fairly obvious. In a Sumerian sentence, for example, case-relations may be expressed twice, by a suffix appended to the nominal syntagm and by an infix inserted in the verbal syntagm. The translation, unless it be into a language sharing this syntactic peculiarity with Sumerian, will naturally express the case-relation only once, with no loss of sense whatsoever.

The case of the verbal prefixes is somewhat different. Here, even when their true function is well established, we can usually dispense with accounting for them in the translation. This, *a fortiori*, applies to "controversial" prefixes, such as the pair *mu–*:*i–*. Their true rôle is so distinctively Sumerian, they express ideas so alien to our languages, that not only is there no consensus on the nature of their function, but we simply ignore them without impairing, or so it seems to us, our understanding of the text. There is no other translation for *mu–ĝar* and *i–ĝar* than "(he) placed", although it must be pretty obvious that

had there been no difference there wouldn't have been two prefixes. Our inability to grasp the nuance is not, however, entirely due to our poor knowledge of the language. The Babylonian scribes, who knew more Sumerian than we shall ever dream to do, were in this respect in the same predicament and had to be content to translate both *mu–ĝar* and *ì–ĝar* (and also *bi–ĝar*) by *iškun*. Actually, their analysis of the phrases rested on the translations, which in turn were determined by the context. Thus, depending on the context, or on their understanding of it, the scribes translated *mu–ĝar* as *aškun, taškun, iškun*, etc. The constant element in the Akkadian paradigm corresponding to *–ĝar*, obviously *mu-* corresponded to the varying element and was therefore equated to *a–, ta–, i–*, etc. The same would apply to *ì–ĝar, bi–ĝar*, etc. This intellectual process appears clearly in the grammatical texts which list not only large numbers of verbal elements representing the same person, but the very same elements representing them all. So perhaps in this it may be wiser to follow the example of the Babylonian scribes and ignore in translations grammatical or syntactic nuances which, even if we can apprehend them, we cannot possibly translate. It is legitimate to posit that a certain verbal form implies that the action is performed by the subject wishing to indicate that his goal, though within his immediate perception, remains without his actual sphere of physical contact; it is another thing to try and express that in one good English (or even German!) word.

Finally, very briefly, problems of context, by which I mean not merely the textual context, but equally the historic circumstances, the moral and intellectual climate, the physical environment, all of which help to shape the mental processes underlying the use of articulate language and literary expression.

Obviously, our ignorance in this respect does not necessarily impede our understanding of a text and, consequently, our ability to translate it. Take the case of letters, for instance. We may not know the circumstances which led to a letter being written ordering a detained man to be set free; but we still understand the meaning of the letter and can therefore translate it. But if the same letter contains an allusion to some literary text, or to some recent and widely publicized incident, the translator is in trouble unless he happens to know the facts behind the allusion. This, of course, applies to translations from any language. "Kiss me, Hardy!" in the middle of a letter addressed to one Smith makes no sense if we are not familiar with the circumstances of Nelson's death.

In the case of Sumerian, the difficulties are magnified by the very paucity of our information and our only hope to overcome them lies in an ever-increasing knowledge of the various components of what I have called the "context". Thanks to the efforts of scholars, of both the Tammuz and the Onions persuasions, we have made tremendous progress in this, but there is still a wide enough gap in our information to make a solution to *all* our problems seem fairly remote. But perhaps it is just as well. Sumerology would certainly not hold us so totally under her spell were it not for all the many problems yet to solve.

On Canaanite Rhetoric
The Evidence of the Amarna Letters from Tyre

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Stylistic analysis of biblical Hebrew and immediately cognate poetic literatures has resulted in the development of a working hypothesis, the emergence of a growing awareness that their authors must have shared a literary heritage. The virtual identity of rhetorical devices, locutions, and diction of the poetic texts recovered from ancient Ugarit and those of the Hebrew Bible — from which they are separated by barriers of time and distance — necessitated the inference of a relationship between the two bodies of literature that could not be accounted for by recourse to the vagaries of chance [1]. It rather seemed increasingly reasonable to suppose that the relationship was generic, that one had to reckon with a knowing and purposive continuity of expressions and of modes of expression, the source of which had probably to be sought in the oral techniques of pre-writing poets [2], and must have been independent of both the Israelite and Ugaritian authors. Inasmuch as Phoenician inscriptions, too, often evidence the continued existence of this Canaanite literary tradition [3], the hypothesis has gained further support, and bids fair to becoming firmly established as an irrefutable fact.

Verification of the hypothesis of a common, Canaanite, stylistic tradition is more firmly secured, of course, with the admission of every new documentary source that employs some element of the tradition. Now an avowed concern of this volume of studies that is offered in tribute to our teacher and friend, Professor Ignace J. Gelb, is consideration of methods in (and the impact of these methods on the progress of) distinct disciplines; and to us has been allotted the task of evaluating procedures in Northwest Semitic literary study. But, as we have intimated, new sources, rather than new methods, have been chiefly responsible for advances in our understanding of this literature. Accordingly, the assigned task may be best fulfilled by the application of customary methods — the isolation and comparison of stylistic features and rhetorical devices — to documents hitherto only casually and inadequately examined from this per-

[1] Consult the bibliography in *JNES* 20 (1961) 41 n. 2.

[2] Cf. the writer's *Patterns in the Early Poetry of Israel* (*SAOC* 32, [Chicago, 1963], pp. 10-14.

[3] For Phoenician literary connections with Ugaritic and Hebrew, see, e.g., the author's "West Semitic Curses and the Problem of the Origins of Hebrew Law", *VT* 11 (1961) 137-158; *idem*, "The Reprimand of Reuben", *JNES* 30 (1971) 87-98 (particularly pp. 88-90); J. C. Greenfield, "Scripture and Inscription: The Literary and Rhetorical Element in Some Early Phoenician Inscriptions", in *Near Eastern Studies in Honor of William Foxwell Albright*, (ed. by Hans Goedicke, [Baltimore and London, 1971]), pp. 253-268.

spective. We may thereby hope to demonstrate anew the essential validity of these basic, time-honored procedures, to augment the evidence for further investigations of Canaanite literary style, and to broaden, however slightly, our appreciation of Canaanite rhetoric.

One group of texts that, in this regard, has received far less attention than it deserves is the body of diplomatic messages of 14th century B.C.E. rulers of Syro-Palestinian city-states that is contained in the collection generally referred to as the Tell el-Amarna letters [4]. Subject to continuing analyses of historical and syntactical interest [5], the rhetorical content of these missives has, with rare exception, been all but ignored [6]. Not surprising, therefore, was the appearance in a recent essay of the following observation [7]: "... the Hebrew Bible and the Ugaritic corpus are the two great bodies of literature in a 'Canaanite' language that have come down to us. The literature of the vital center is lacking. For Canaan proper during the Amarna Age we can only hear faint echoes and phrases in the Amarna letters from Palestine and Phoenicia..." Certainly, Ugaritic and biblical Hebrew literatures are quantitatively and — the latter in particular — qualitatively unrivalled in significance; but the "faint echoes" from the Amarna correspondence, I venture to suggest, may be more audible to the attentive listener than research to date has indicated. And for the very reason that literature from the vital center of Canaanite culture is lacking, what literary elements these documents contain clamor for notice. Because the Amarna letters bear frequent witness to the Canaanite literary heritage, the kind of stylistic inquiry that has proved so enlightening for our comprehension of Ugaritic, biblical Hebrew, and Phoenician literary efforts may similarly prove illuminating for these writings as well.

The Amarna letters that originated in southern Syria, Phoenicia, and Palestine are, by their nature, communications of vassal kings ostensibly to the ruler of Egypt, who appears to have claimed and, on occasion, to have exercised a measure of control over their senders [8]. Written in learned, Middle

[4] Hereinafter, *EA*. We follow the numbering system as outlined by Anson F. Rainey in his *El Amarna Tablets 359-379: Supplement to J. A. Knudtzon, Die El-Amarna Tafeln,* (*AOAT* Vol. 8 [Neukirchen-Vluyn, 1970]), p. 1.

[5] For historical evaluations of these documents see W. F. Albright, "The Amarna Letters from Palestine", in *CAH*[3] (Cambridge, 1966) fasc. 51; Edward F. Campbell, *The Chronology of the Amarna Letters* (Baltimore, 1964); Horst Klengel, *Geschichte Syriens im 2. Jahrtausend v.u.Z.* Teil 2: *Mittel- und Südsyrien* (Berlin, 1969). For recent grammatical and syntactical analyses, see particularly the several studies by William L. Moran, appearing in *JNES* 8 (1949) 124-125; *JCS* 4 (1950) 169-172; *JCS* 5 (1951) 33-35; *JCS* 6 (1952) 76-80; *JCS* 7 (1953) 78-80; *Or* 29 (1960) 1-19; *The Bible and the Ancient Near East* (ed. by G. Ernest Wright; [Anchor Books, 1965]), pp. 59-84; *Bib* 45 (1964) 80-82; and *Eretz-Israel* 9 (1969) 94-99.

[6] Cf. Hugo Winckler, *Geschichte Israels in Einzeldarstellungen*, I (Leipzig, 1895), p. 123; Franz M. Th. de Liagre Böhl, "Hymnisches und Rhythmisches in den Amarnabriefen aus Kanaan", *Theologisches Literaturblatt*, 35, Nr. 15 (1914) 337-340 (republished in *Opera Minore* [Groningen, 1953], pp. 375-379 with notes on pp. 516 f.); Albrecht Alt, "Hic murus aheneus esto", *ZDMG* 11 (1932) 33-48; Anton Jirku, "Kanaʿanäische Psalmenfragmente in der vorisraelitischen Zeit Palästinas und Syriens", *JBL* 52 (1933), 108-120.

[7] Greenfield, op. cit. (above, n. 3), p. 253.

[8] The view that the Egyptian empire in Asia disintegrated by virtue of the neglect accorded it by the Egyptian government during the Amarna period

Babylonian Akkadian, but betraying in a variety of ways the native dialects and stylistic practices of their authors, they are letters of local dynasts who employed this means to report to the Egyptian government on their own activities and those of their neighbors, to protest their loyalties and to impugn those of their rivals, and often to seek additional military and economic support. Their objective was thus to inform and to persuade. Except when flattery appears to have been the motive they are not, on the whole, examples of *belles lettres* (religious or other), but official government communiqués. And yet, there is nothing in them to suggest that these missives were composed without care or without considerable attention having been paid to (even lavished upon) stylistic or rhetorical niceties. Appreciation of this, however, requires sensitivity to the underlying Canaanite idiom. To permit the informational, predicative or admonitory character of these messages to obscure what literary qualities they possess, or to dismiss the rhetoric which informs so many of them as "pompous, long-winded and excited" [9], is to refuse to accept them on their own terms; it is to set indifferently aside the authors' literary modes as so much embellishment, as tangential rather than essential elements of style. Yet, when ʾIR-Ḫepa of Jerusalem, or his scribe, after an urgent report in which he cites evidence of an impending military and political disaster, implores his Egyptian correspondent to "tell it to the king in good (i.e., eloquent) words" [10], surely the appeal attests to his appreciation of and concern for the efficacy of the well-turned phrase.

Limitations of space and theme impel us to forego any attempt at an exhaustive treatment of every Canaanite stylistic feature in each of the relevant Amarna letters. We propose, therefore, to restrict our attention to the rhetorical elements in the correspondence from one city, Tyre. Tyre has been selected not only because it is unquestionably in "the vital center... (in) Canaan proper", but because, despite the relatively small number of documents available, almost every characteristic of Canaanite rhetoric is represented in them. This observation assumes additional interest when it is recalled that, in a paper published in 1937, W. F. Albright, unaware of these stylistic Canaanitisms, advanced the thesis that the letters from Tyre, all of them the product of a single scribe, contained a number of Egyptianisms, and that the author must have been an Egyptian in the employ of the Tyrian king [11]. The present investigation, therefore, may serve to point up the nature of some of the advances in Northwest Semitic literary study since that time.

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has been challenged by Alan R. Schulman, "Some Remarks on the Military Background of the Amarna Period", *Journal of the American Research Center in Egypt*, 3 (1964) 51-69; particularly, pp. 60 ff.

[9] Thus, A. Leo Oppenheim, "A Note on the Scribes in Mesopotamia", in *Studies in Honor of Benno Landsberger on his Seventy-Fifth Birthday: April 21, 1965* (Assyriological Studies, No. 16 [Chicago, 1965]), p. 255.

[10] *EA* 289:49. Cf. *EA* 286:62-63, 287:67-68, 288:64-65. The suggestion that *awatū banâta* here means "(tell it to the king) in Egyptian" (thus, A. L. Oppenheim, loc. cit., and cf., *CAD* A/2, p. 31) belies both the well-established meaning of the expression (cf. *CAD* B, pp. 80, 82) and common sense. Is it likely that the secretary-scribe would (or would be expected to) convey the contents of the note to the Pharaoh in a language other than Egyptian?

[11] W. F. Albright, "The Egyptian Correspondence of Abimilki, Prince of Tyre", *JEA* 23 (1937) 190-203; particularly pp. 196 ff.

* * *

Repeatedly in the extant reports of the king of Tyre, Abimilki, to the Egyptian Pharaoh, Akhenaton, there is registered complaint concerning the difficulties which the vassal king encounters in attempting to ensure adequate supplies of water for drinking and wood for fuel. He requests that these items be furnished him, and that the city, Uzu, be assigned to him in order to satisfy Tyre's needs for these essential commodities [12].

Although its wealth could be compared with that of Ugarit [13], the letters of Abimilki indicate that, in the Late Bronze Age, Tyre, situated on a tiny island about half a mile from the Phoenician coast in the Mediterranean Sea, depended largely upon deliveries of potable water and fire-wood by boat from the mainland. Confirming this, but only in part, is a statement contained in an Egyptian literary document of a century and a half later [14]: "They say another town is in the sea, named Tyre-the-Port. Water is taken (to) it by the boats, and it is richer in fish than the sands". That water alone is mentioned as being brought by boat to island Tyre, and not wood, may be due to an oversight, to ignorance on the part of the Egyptian author, to the circumstance that it may have been irrelevant for his purpose, or to any of a number of other reasons — not excluding the possibility that it may not always have been necessary for Tyre to import wood. On at least one occasion Abimilki notes that he has exported wood to Egypt [15], but this may have been in the form of manufactured wooden objects.

The need to import drinking water, W. F. Albright has argued, continued until sometime after the beginning of the Iron Age, when slaked lime plaster came to be employed as an effective seal in the lining of cisterns for the collection and storage of rain water [16]. Prior to that development the island of Tyre, dependent upon outside sources for the bulk of its drinking water, was vulnerable. And when, as is specifically noted in two of the letters (149:49 and 154:14-18), the major source of supply for Tyre of water and wood, the mainland town of Uzu, was in hostile hands, and what was perhaps the usual supply-route thereby threatened, the plight of the inhabitants of Tyre, we are led to believe, could be serious. The Tyrian king's letters put the matter in the following ways:

> And now I protect Tyre, the great city, for the king, my lord, until (*or*: as long as) the mighty arm of the king goes out to me to give me water (*me–e*) to drink and wood (GIŠ.MEŠ) to warm me (*EA* 147:61-66).

[12] The motif appears in eight of the ten extant letters (*EA* 146:17, 20; 147:65-66; 148:12, 31-34; 149:51, 75-76; 150:20-21, 33-34; 151:39-40, 43; 154:17-18; 155:10, 16, 19, 25, 63), and of the remaining two, *EA* 152 and 153, the former is too fragmentary to enable us to determine whether it may not have appeared there originally.

[13] By Ribhaddu of Byblos in *EA* 89:48-53.

[14] *Papyrus Anastasi* I xxi, 1-2. The translation is that of John A. Wilson in *ANET*[2], p. 477.

[15] *EA* 151:48.

[16] See his, *The Archaeology of Palestine* (rev. ed.; Pelican Books [Baltimore, 1960]), p. 113; "The Rôle of the Canaanites in the History of Civilization", in *The Bible and the Ancient Near East* (ed. by G. Ernest Wright [Garden City, 1965]), pp. 456, 481 n. 72; *Yahweh and the Gods of Canaan* (Garden City, 1968), p. 220.

> Let the king set his face [17] toward his servant, and let him instruct his commissioner, and let him (= that he may?) give Uzu for water (A.MEŠ *mi-e-ma*) to his servant, for procuring wood (GIŠ.MEŠ), for straw, for clay (*EA* 148:26-34).
>
> Zimri(d)da has taken Uzu ... and neither water (A.MEŠ) nor wood (GIŠ.MEŠ) do we have; and there is no place where we may lay the dead (*EA* 149:49-53).
>
> There is no water (A.MEŠ), there is no wood (GIŠ.MEŠ) (*EA* 149:75-76).
>
> Let the king my lord set [his] face and g[ive] water (A.MEŠ) for [his] se[rvant's] drink and wood (GIŠ.MEŠ) for his servant (*EA* 151:37-40).
>
> There is no water (A.MEŠ) and there is no wood (GIŠ.MEŠ) in our possession (*EA* 151:43-44).
>
> The ruler of Sidon does not allow my people to go ashore to procure wood (GIŠ.MEŠ), to procure water (A.MEŠ) for drinking (*EA* 154:14-18) [18].

Aggravated by the Sidonian capture of Uzu, the problem faced by Abimilki may have been real; it could scarcely have been insoluble, however, even without Egyptian succor or interference in the local controversy. Abimilki's letters span a number of years [19], and the islanders can hardly be expected to have endured so long without fresh water and fuel. If water and wood continued to be imported, then boats must have been dispatched for these items to other sites (along the Phoenician coast?). This procedure, however, were it followed, would almost certainly have entailed a greater inconvenience, perhaps even danger, and a consequent increase in cost. While the need, particularly for water, may have been real, there is something suspiciously contrived, artful, one may say literary, about the way in which it is expressed. It is this literary aspect to which we would now turn.

When both needed commodities are cited, they are invariably paired in such a way as to form a poetic parallelism; while the sequence of terms — with two exceptions that may be explained by reference to what I have elsewhere called the "principle of compensation" [20] — is consistent: *water // wood*. Consistent pairing of terms, of course, is a hallmark of Canaanite poetic style, and this suggests that the author of these letters may have made deliberate use of this inherited device. To confirm this suggestion we need to locate the same fixed pair of words in other, Canaanite, literary texts; and, indeed, it is readily available in biblical Hebrew poetry. Note its appearance, for example, in Lam 5,4:

> Our water (*mymnw*) do we drink for money,
> Our wood (*ʿṣynw*) comes for a price

[17] As recognized by W. von Soden, *AHw.*, p. 702 b, this is a Canaanitism. See the *Excursus*, below.

[18] Cf. *EA* 146:20; 148:8-13; 150:14-21; 155:10, 16-17, 18-21, 25, 59-64.

[19] Eight years (ca. 1365-1358) according to the calculation of Albright in *JEA* 23 (1937) 196; four to five years according to that of E. F. Campbell, *The Chronology of the Amarna Letters*, p. 72.

[20] *Patterns in the Early Poetry of Israel*, p. 36. The two examples may be found in *EA* 149:49-53 and 154:14-18, and have been cited above in translation.

and Eccl 2,6:

> I made me pools of water (*mym*) to irrigate thereby a sprouting forest of trees (*ʿṣym*).

In reverse sequence the pair occurs in Dt 29,10b:

> ... from the hewer of your wood (*ḥṭb ʿṣyk*)
> to the drawer of your water (*šʾb mymyk*)

as well as in Jos 9,21 (cf. 9,23, 27a) [21]:

> they shall be hewers of wood (*ḥṭby ʿṣym*)
> and drawers of water (*šʾby mym*).

These last examples are of additional interest because they attest to another parallelism, *ḥṭb*(*y*) // *šʾb*(*y*), "hewer(s of wood)" // "drawer(s of water)", which has appeared in Ugaritic poetry. Cf. UT *Krt* 214-217 (111-114):

sʿt [22] *bšdm ḥṭb⟨t⟩*	(...) [22] from the fields the hewer(s)
wbgrnm ḫpšt	and from the threshing floors the cullers (?)
sʿt bnpk šibt	(...) from the fount the drawers
wbmqr mmlat	and from the spring the fillers.

The pair, *wood // water*, not yet documented in Ugaritic, may well be expected, for it is clearly at home in the Canaanite poetic repertoire.

Less certainly employed in it as a parallel pair, but nonetheless suggestive, is the passage occurring in the foundation inscription of the West Semitic king of Mari, Yaḫdunlim (*Syria* 32 [1955] 1-28, col. ii 12-18), which may be rendered:

> And in the sea his troops
> with water (*me–e*) washed.
> The mountains of cedar and boxwood,
> the mighty mountains, he entered, and
> the boxwood, cedar, cypress
> and *elammakku* (trees),
> these trees (*i–ṣí an–nu–ti–in*) he felled.

If the appearance of the sequence, "water" and " wood" — as in the Tyrian Amarna and biblical Hebrew examples, in plural form — is not merely fortuitous (cf. col. i 37 – ii 2), and if, despite the intrusion of the tree names, the terms do indeed constitute a parallelism, then the pair may be traced back to the 18th century B.C.E., and the thesis that the 14th century author of the letters from Tyre made use of a standard poetic parallelism gains added support.

* * *

In an unclear context toward the end of *EA* 155, Abimilki notes, presumably for the information of the Egyptian king, the embarkation of the rulers of Beirut and of Sidon and his own departure. An apparently straightforward bit of intelligence, it is nevertheless couched in characteristically Canaanite

[21] See also Jer 17,8; Jl 1,19-20; Ps 1,3.

[22] For recent discussions of Ugaritic *sʿt*, see J. C. Greenfield, "Some Glosses on the Keret Epic", *Eretz-Israel* 9 (1969) 63; and M. Dahood, "Ugaritic-Hebrew Syntax and Style", *Ugarit-Forschungen*, 1 (1969) 20-21.

literary style, employing a combination of rhetorical devices. The lines, 67-69, read:

amur awīl Bērūti ina [1] *elippi alik*
u awīl Ṣidūna ina 2 *el*[*ippi i*]*llak*
u anāku illak qadu gabbi elippika gabbi āliya

and, in accordance with the requirements of the tense system of Akkadian, have been rendered [23]:

The ruler of Beirut has (already) left with one ship
and the ruler of Sidon is going to leave with two ships
and I, myself, am going to depart with all your ships and my entire town.

But the passage incorporates two sets of parallelisms: that of numbers (including an aspect not hitherto noted) and that of varying tenses of the same verb. Whether adherence to the usual manner of rendering Akkadian verbal forms is here justified, then, is doubtful.

The use of numbers in poetic parallelism, a very common feature of Ugaritic and biblical Hebrew poetry, and occurring in Sumerian, Akkadian, and Aramaic literatures as well, is most often structured in such a way that the numeral in the first of the parallel cola is matched by the "next higher" number in the second. Because the phenomenon has been widely discussed [24] we need cite only a few, randomly selected examples from biblical Hebrew, Ugaritic, and Akkadian in order to illustrate the stylistic device. From biblical Hebrew:

How may *one* chase a *thousand*,
Or *two* cause *ten-thousand* to flee? (Dt 32,30)

The days of our years are *seventy* years,
Or even by reason of strength *eighty* years (Ps 90,10)

From Ugaritic:

Seven years may Baal fail,
Eight, the Rider of the Clouds! (*UT* 1 *Aqht* I 42-44)

Sixty-six towns he seized,
Seventy-seven villages (*UT* 51 VII 9-10)

From Akkadian:

I provided her with *six* decks,
Dividing her into *seven* parts (Gilg. XI 60-61)

At *twenty* ["leagues"] they broke off a morsel,
At *thirty* "leagues" they settled for the night (Gilg. XI 300-301)

[23] Cf., *CAD*, A/1, pp. 302 f.

[24] Cf., e.g., H. L. Ginsberg in *Minḥah l'David* (David Yellin Jubilee Volume [Jerusalem, 1935]), pp. 76-82; U. (M.D.) Cassuto, in *Tarbiz* 13 (1942) 206-207; *idem*, in *The Goddess Anath* (tr. by I. Abrahams; [Jerusalem, 1971]), pp. 138 f.; M. H. Pope, "Number", *The Interpreter's Dictionary of the Bible* (New York and Nashville, 1962), vol. K-Q, pp. 561-567: particularly pp. 563 f.; W. M. W. Roth, "The Numerical Sequence x/x+1 in the Old Testament", *Vetus Testamentum* 12 (1962) 300-311; S. Gevirtz, *Patterns in the Early Poetry of Israel*, pp. 15-24, 29 f.; G. Sauer, *Die Sprüche Agurs*, (BWANT, 4 [Stuttgart, 1963]) M. Weiss, "The Pattern af Numerical Sequence in Amos 1-2: A Re-examination", *JBL* 86 (1967) 416-23; D. Freedman, "Counting Formulae in the Akkadian Epics", *Journal of the Ancient Near Eastern Society of Columbia University* 3 (1971) 65-81: particularly pp. 75 ff.

The pattern of number parallelism is thus clear: *n* // *n* + 1, 10 *n* // 10 (*n* + 1), 11 *n* // 11 (*n* + 1), etc. By itself, therefore, the numerical sequence, "1" // "2", in *EA* 155: 68-69, would not distinguish the passage as peculiarly Canaanite; nor, if the figures employed represent the number of boats actually engaged by the rulers of Beirut and Sidon (so that the entire statement is nothing more nor less than a factual report of an observed event) would they even suggest that we have necessarily been presented with a rhetorical device. It is to be noted, however, that the sequence has been extended so as to include as third and fourth members of the parallelism the word "all". The pattern thus formed may be schematized as *n* // *n* + 1 // *all*, which is at least reminiscent of the Ugaritic text, *UT Krt* II 94-95 (cf. 182-183). It reads:

aṯr ṯn ṯn hlk	After(?) two, two march
aṯr ṯlṯ klhm	After(?) three, all of them

The pattern may also find reflection in the admittedly prosaic, but nonetheless literary, text of Jer 36,23, which exhibits the sequence, "three ... four ... all":

> And when Yehudi had read three columns or four he (the king) would cut it off with a penknife and cast it into the fire which was in the brazier until the entire roll (*kl hmglh*) was consumed upon the fire which was on the brazier.

Even more significant as an indicator of Canaanite rhetoric in *EA* 155: 67-69 is the additional, three-fold parallelism with which the parallelism of numbers is combined: that of the forms of the verb, *alāku*, "to go". The first of these, *alik* (written: *a–li–ik*), is permansive in form. As often, in Amarna Akkadian, the permansive here functions in a manner corresponding to the West Semitic "perfect tense", the suffix conjugation, *qtl* [25]. The second of the forms of *alāku*, the 3 m. sg. present-future, [*i*]*llak* (written: [ ]*–la–ak*), certainly corresponds to the West Semitic "imperfect", the prefix conjugation, *yqtl*. Similarly, the third form, *illak* (written: *i–la–ak*), in the phrase, *u anāku illak*, is analogous with the West Semitic 1 c. sg. "imperfect". Schematically reproduced, then, the parallelism employed by the author of *EA* 155:67-69 is *qtl* // *yqtl* // *yqtl* of the verb *alāku*.

Occurring too infrequently to be considered a common feature of Canaanite rhetoric, three-fold parallelism of one particular verb, nevertheless, is in evidence in biblical Hebrew and in Ugaritic poetry [26]. The former utilizes the pattern, *qtl* // *qtl* // *yqtl*. It has been noted in Ps 93,3:

> The streams raise up (*nś'w*), O Yhwh,
> The streams raise up (*nś'w*) their voice,
> The streams raise up (*yś'w*) their roar(?).

To this example we may add Ps 57,9:

> Awake (*'wrh*), O my soul(?)!
> Awake (*'wrh*), O harp and lyre!
> Let me awake (*'yrh*) the dawn!

[25] Cf. Franz M. Th. Böhl, *Die Sprache der Amarnabriefe* (Leipzig, 1909), §27; W. L. Moran, S.J., "The Use of the Canaanite Infinitive Absolute as a Finite Verb in the Amarna Letters from Byblos", *JCS* 4 (1950) 18 f., n. 18.

[26] For examples of two-fold parallelism of the same verb in *EA*, see my study, "Evidence of Conjugational Variation in the Parallelization of Selfsame Verbs in the Amarna Letters", *JNES* 32 (1973) 99-104.

for though the verb appears here in the imperative and cohortative moods, the pattern is obviously similar. Ugaritic verse, on the other hand, evinces the sequence, *yqtl // qtl // qtl*, which manifests itself-in *UT* 75 II 49-52:

> His seventy-seven brothers re[ach him] (*ym[ṯah]* [27]),
> His eighty-eight siblings (?) reach him (*mṯah*),
> His kinsmen reach him (*mṯah*).

The variation in sequence of verbal forms in three-fold parallelism of the same verb in Hebrew, Ugaritic, and in the letter from Tyre is patent but minor. Minor, also, is the fact that in *EA* 155:67-69 each of the verbal forms has a distinct subject: "the ruler of Beirut ... the ruler of Sidon ... I ...", for variation of subject, in otherwise strictly synonymous parallelism, is certainly not unknown in Canaanite poetry [28]. The Amarna passage may readily be acknowledged, therefore, to be an adaptation of the rhetorical device to a specific historical situation; stylistically, it differs in no essential way from the biblical Hebrew and Ugaritic examples to which it is unquestionably related. This, then, brings us back to the problem of how best to translate these verbal forms. Until the tense system of Canaanite is more perfectly understood (particularly when, as here, "perfect" and "imperfect" forms are set in parallel construction), it is perhaps preferable to render the verbs in the "present" tense: " ...the ruler of Beirut is leaving ... the ruler of Sidon is leaving ... and I, I am leaving...". However this may be, the passage, utilizing as it does a combination of rhetorical devices, must be recognized as the work of an author, who, conscious of Canaanite literary style, was able to employ it with considerable skill.

* * *

Another peculiarity of West Semitic rhetoric is the alteration of grammatical person (3d to 2d and 2d to 3d) within one sentence or unit of thought while the reference remains to the same individual. It is a well-documented though often overlooked feature of biblical Hebrew literary style [29]. We find it in poetry, for example, in Ct 1,2:

> O that he might kiss me (*yšqny*) with the kisses of his mouth (*pyhw*),
> For better than wine are your caresses (*ddyḵ*)

and in prose, for example, in Ex 21,2-6:

> When you buy (*tqnh*) a Hebrew slave, six years shall he serve, and in the seventh shall he go out gratis. ... (4) If his master shall have given (*'m 'dnyw ytn*) him a wife ... (6) then his master shall bring him (*whgyšw 'dnyw*) ...

[27] Thus, with Moshe Held, "The *yqtl–qtl* (*qtl–yqtl*) Sequence of Identical Verbs in Biblical Hebrew and in Ugaritic", *Studies and Essays in Honor of Abraham A. Neuman* (ed. by M. Ben-Horin, *et al.*; [Leiden 1962]), p. 289.

[28] Cf. such familiar pairs as: *šm(y)m // 'rṣ*, "heaven"//"earth"; *ṭl // šmn*, "dew" // "oil"; *mym // ṣ(y)m*, "water" // "wood"; *yt(w)m // 'lmnh*, "orphan"// "widow"; *mṣrym//'šwr*, "Egypt"// "Assyria"; and very many more.

[29] Cf., with reservations, J. Sperber, "Der Personenwechsel in der Bibel", *ZA*, XXXII (1918), 23-33.

I have elsewhere noted the same phenomenon in Phoenician [30]. It occurs, for example, in the inscription of Yeḥawmilk, king of Byblos (*KAI*, No. 10:13-15):

> But if you do not set (*w'm 'bl tšt*) my name ... may the Lady, the Mistress of Byblos [remove (?)] that man and his seed (*'yt h'dm h' wzr'w*) from before all the gods of Byblos!

In Aramaic it is encountered, for example, in *KAI*, No. 226:8-10:

> Whoever you (may be) shall wrong and remove me (*mn 't t'šq wthnsny*) may Šhr and Nkl and Nšk make odious his death (*mmtth*) and may his posterity perish (*w'ḥrth t'bd*;)

It is perhaps to be recognized in Ugaritic poetry also. In *UT Krt* 62-82, following two 2 m. sg. jussive forms, there is found a series of ten 2 m. sg. imperatives. These are interrupted, in lines 79-80, with what appears to be a 3 m. sg. imperfect/jussive: *w-yrd*. (It is not impossible, however, that *yrd* may represent an infinitive with imperative force [31]). *UT* 95:14-18, on the other hand, may reflect an "epistolary style" [32]:

> What welfare there be over there with our lady (*adtny*),
> send back (*tṯb*) a report to your servants (*'bdk*).

Cf. also *UT* 2059:24-27:

> And your ship (*anyk*) is moored(?) at Acco ... so let my brother not put care in his heart (*w. aḫy . mhk / b . lbh . al . yšt*).

When, therefore, in *EA* 149:47-48 we observe a change from 3d to 2d person, we are certainly to interpret it as a stylistic nicety that was dictated, perhaps, by rules of etiquet, rhetoric or protocol:

> *šarru idê šumma šaknātani ina rābiṣi ina Ṣurri*
> The king knows whether you have appointed me commissioner in Tyre.

It is not the case that there is some correspondent other than the Pharaoh to whom Abimilki addresses these remarks; rather are we confronted with another example of the change of grammatical person without effecting any alteration in the referent. The subject of *šaknātani* (2 m. sg. permansive [= Canaanite 2 m. sg. perfect] plus 1 c. sg. pronominal suffix) must be the same as the subject of *idê* (3 m. sg. present-future), namely, the king. If further proof were required that the king of Egypt alone is here being addressed, one need only refer to *EA* 148:20-22:

> *enūma ipqidni šarru bēlia ana naṣāri ālišu*
> When the king my lord appointed me to protect his city

and *EA* 149:9-10:

> *šarru bēlia ipqidni ana ⟨na⟩ṣār Ṣurri amti šarri*
> The king, my lord, appointed me to protect Tyre, the handmaid of the king.

Abimilki acknowledges no third party, but the Egyptian king, as having appointed him to his office.

[30] Cf. *VT* 11 (1961) 157.

[31] Cf., H. L. Ginsberg, *The Legend of King Keret* (*BASOR* Supplementary Studies, Nos. 2-3 [New Haven, 1946]), p. 37.

[32] Cf., C. H. Gordon, *UT*, §6.15.

* * *

In response to a request that he inform the Egyptian court of happenings in the land of Canaan, Abimilki reports on the death of the ruler of Danuna, on the succession of the dead king's brother, and on the peaceful state of the land (*EA* 151:52-55). Employing terms, expressions, and syntactical constructions which, though written in Akkadian, are manifest translations of Canaanite originals, and which, confined (as on present evidence they seem to be) to annals or chronicles relating of governmental succession, the report appears to reflect a traditional phraseology of official Canaanite historiography. The text reads:

> *šar Danuna mīt u šarra aḫušu ana arkišu u pašḫat mātušu*
> The king of Danuna is dead; and his brother reigns in his stead; and his land is at rest.

Containing notice of the king's death, reference to his successor's accession, and the observation that "his land is at rest", the statement may be compared with that in 2 Ch 13,23:

> And Abijah slept with his fathers (*i.e.*, died) ... and Asa his son reigned in his stead (*wymlk 's' bnw tḥtyw*); in his (i.e., Asa's) days the land was at rest (*šqṭ h'rṣ*) ten years.

Of particular interest here is the expression, "and his/the land is/was at rest", which is present in both texts. In biblical Hebrew it is found again four times in the Book of Judges. Compare, e.g., Jgs 3,9-4,1:

> And Yhwh raised up .... Othniel, son of Qenaz ... (10) And the spirit of Yhwh was upon him, and he judged Israel, and he went out to war and Yhwh gave Cushan Rishathaim, king of Aram, into his hand, and his hand prevailed over Cushan Rishathaim. (11) And the land was at rest (*wtšqṭ h'rṣ*) forty years; and Othniel, son of Qenaz, died (*wymt 'tny'l bn qnz*) ... (15) And Yhwh raised up ... Ehud, son of Gera ... (30) And Moab was subdued on that day beneath the hand of Israel; and the land was at rest (*wtšqṭ h'rṣ*) eighty years. (31) And after him was Shamgar ben Anath ... (4:1) ... And Ehud died (*w'hwd mt*).

The final phrase cited here, "And Ehud died", obviously misplaced, belongs after the account of Ehud's activities and before the statement of the rise of Shamgar ben Anath; but that a kinship between the annalistic narrations exists is clear. Note should be taken of the fact that, in the Book of Judges, the formula is restricted to those narratives that deal with the so-called "major" judges. (It recurs in Jgs 5,31, immediately following the "Song of Deborah", where, however, it lacks reference to the death of the judge). When, therefore, in Jgs 8,28-32, after an account of Gideon's rise and successful military action against the Midianites, this formula is combined with another, usually associated with the so-called "minor" judges, the latter invites examination:

> So Midian was subdued before the people of Israel, and they lifted up their heads no more. And the land was at rest (*wtšqṭ h'rṣ*) forty years in the days of Gideon ... Now Gideon had seventy sons, his own offspring, for he had many wives. And his concubine who was in Shechem also bore him a son, and he named him Abimelech. And Gideon, son of Joash, died in good old age and was buried in the tomb of Joash his father in Ophrah ...

The second formula, containing reference to the burial place of the judge, is patterned directly after that employed in Jgs 10,1-5 and 11,1 + 12,7-15:

> After PN_1 there arose to deliver Israel PN_2 of GN, and he judged Israel (variant: After him PN judged Israel) *n* years. Then he/PN_2 died and was buried at GN.

On occasion, additional, anecdotal information concerning some activity of (or noteworthy fact about) the judge is inserted before or after the record of the number of years he is said to have judged Israel. These concise biographies (providing the name and genealogy, place of origin, length of rule, and burial place) find their close parallels not only in the canonical Books of Kings and Chronicles — whose accounts of the reigns of the kings of Israel and of Judah are merely elaborations of this basic formula — but also in that section of the Babylonian "Dynastic Chronicle" which relates of the three short dynasties that succeeded the Second Dynasty of Isin [33]. For each of the individual monarchs there is given (1) his name and patronymic (or notice of usurpation), (2) the length of his reign, and (3) his place of burial. Notice of a monarch's burial place is not attested elsewhere in Mesopotamian chronicles [34], so that the similarity of these formulae is too precise to be merely coincidental. What it may signify (beyond the fact that the accounts of the "minor" judges in the Book of Judges are, or reflect, records of authentic chronicles of real if only local rulers) is unknown [35]; a common model, however, is not excluded. That within the Book of Judges the two biblical formulae are restricted to the narratives of the "major" and "minor" judges respectively, and are found in combination only in the narrative concerning Gideon (Jgs 8,28-32) and in the late record of the Abijah-Asa succession (2 Chr 13,23) suggests that they may have originated in distinct archival traditions.

Returning to the expression occurring in *EA* 151:54-55, *u pašḫat mātušu*, "and his land is at rest", we note that the verb, *pašāḫu*, "to rest", in Akkadian is ordinarily used of gods, people, the heart, anger, sickness, and (rarely) of horses [36]. Employed to denote, as here, the state of land, it is peculiar to the Amarna correspondence, and within that corpus, to the letters from Tyre [37] and Byblos [38]. The conclusion to which we are led, therefore, is that (*u*) *pašḫat mātu*(*šu*) — Akkadian neither in idiom nor in syntax [39] — is simply a rendering of Canaanite (*w*)*šqṭ*(*h*) *'rṣ*(*h*/*w*).

Similarly, *u šarra aḫušu ana arkišu*, "and his brother reigns in his stead", may be recognized as a translation from Canaanite. In so far as can be determined,

[33] Cf., L. W. King, *Chronicles Concerning Early Babylonian Kings*, II (London, 1907), pp. 51 ff.

[34] Cf. J. A. Brinkman, *A Political History of Post-Kassite Babylonia* (AnOr 43 [Rome, 1968]), p. 155, n. 394.

[35] See my remarks in *Judaism* 16 (1967) 501-505: particularly, p. 504.

[36] Cf., W. von Soden, *AHw.*, pp. 840 f.

[37] Cf., *EA* 147:12.

[38] Cf., *EA* 107:31, 112:38-39, 118:44, 132:59, and perhaps also 113:31-33, 118:45-46, 127:41.

[39] Contrast this construction with those classic Babylonian examples assembled by M. B. Rowton, "The Use of the Permansive in Classic Babylonian", *JNES* 21 (1962) 233-303: particularly, pp. 279-280, and cf. G. Buccellati, "An Interpretation of the Akkadian Stative as a Nominal Sentence" *JNES* 27 (1968) 1-12, particularly p. 9.

šarra (written: ⌜*ša*⌝*–ar–ra*), 3 m. sg. permansive of *šarāru*, appears to be unique in Akkadian; it compares favorably, however, with West Semitic *(w)mlk* RN [40], *mlk(t/ty) tḥt–/'ḥr–* [41], and variations. Whether *ana arḫišu*, "in his stead", then, in this passage, reflects *tḥtyw* or *'ḥryw* cannot be determined: biblical Hebrew favors the former (though not to the exclusion of the latter) [42], while Moabite attests only to the latter [43].

On the basis of idiom, syntax, and virtually precise Hebrew parallels, *EA* 151:52-55 may be considered Canaanite in form and in content. Because the various components of the report, particularly in combination, find their biblical Hebrew counterparts restricted to records of governmental succession, it is furthermore likely that the passage constitutes an adaptation of a Canaanite archival formulary.

* * *

Continuing his summary of events in the "land of Canaan", Abimilki relates news of a destructive fire that occurred in Ugarit (*EA* 151:55-57), and again employs Canaanite idiom to convey it:

> *u bīt*(!) [44] *šarri Ugarit īkul išātum mišilšu īkul u miši⟨l⟩šu yānu*
> And the house (!) of the king of Ugarit has fire consumed: half of it did it consume and its (other) half not.

The expression, *mišilšu/i ... u mišilšu/i*, "its half ... and its (other) half", is otherwise known to me from Akkadian sources only in an Amarna letter from Byblos (*EA* 138:71-73):

> *anumma ālu* [*m*]*iši*[*l*]*ši ra'im ana mārē Abdiaširti u mišilši ana bēliya*
> Now the city — half of it is devoted to the sons of A., and its (other) half to my lord

and, in a slightly variant form, in a letter from Ugarit [45]. In biblical Hebrew, however, its correspondents occur several times:

> ... as a regular cereal offering: half of it (*mḥṣyth*) in the morning, and its (other) half (*wmḥṣyth*) in the evening (Lv 6,13)
>
> And all Israel ... stranger and native alike: half of it (*ḥṣyw*) opposite Mt. Gerizim and its (other) half (*whḥṣyw*) opposite Mt. Ebal (Jos 8,33)
>
> And to half (*wlḥṣy*) the tribe of Manasseh Moses gave (a possession) in the Bashan, and to its (other) half (*wlḥṣyw*) Joshua gave (a possession) with their brothers west of the Jordan (Jos 22,7a)

[40] Cf., e.g., Donner-Röllig, *KAI*, No. 24:2; Is 24,23; Mi 4,7; 2 Chr 25,1; 29,1, *et passim*. The more usual expression in Akkadian appears to have been *šarrūta(m) epēšu* (cf. *CAD* E, pp. 219 f.), though others are also attested.

[41] Cf., e.g., *KAI*, No. 181:2-3; 2 Sm 16,8.

[42] By far the more common formula, the former occurs in Gn 36,33-39; 2 Sm 10,1; 1 Kgs 11,43, *et passim* in the Biblical books of Kings and Chronicles. The latter may be noted in 1 Kgs 1,13, 17, 24, 30; cf. Jgs 12,8, 11, 13, and see 1 Kgs 1,20, 27.

[43] *KAI*, No. 181:2-3.

[44] Cf. J. A. Knudtzon, *Die El-Amarna–Tafeln*, II, 1597 (in the commentary on *EA* 148:12).

[45] RS 20.33:18-19 (written: MAŠ ... ù MAŠ). Cf. Jean Nougayrol in *MRS* 16, 71.

> ... And half the mountain (*ḥṣy hhr*) shall withdraw northward, and its (other) half (*wḥṣyw*) southward (Zech 14,4)
>
> And on that day living waters shall go forth from Jerusalem; half of them (*ḥṣym*) to the eastern sea, and their (other) half (*wḥṣym*) to the western sea ... (Zech 14,8a)
>
> And from that day (on), half of my servants (*ḥṣy n'ry*) were working on the project, and their (other) half *wḥṣym*) were holding spears, shields, bows and ... (Neh 4,10)

Cf. further, Ex 24,6; 30,13. In view of its occurrence, to my knowledge, only in documents from Ugarit, Byblos, Tyre and in the Hebrew Bible, the locution would appear to be Canaanite.

* * *

In the foregoing examination of Canaanite stylistic features we have attempted to demonstrate that recognition of a common, literary tradition (to which the Ugaritic and biblical Hebrew literatures and the Phoenician inscriptions bear indisputable witness) must be extended to the Amarna letters from Tyre. Not every rhetorical device, known from the other sources, has been located in this limited collection of ten documents, but almost all the more strikingly characteristic traits were found to be present: parallelism of a fixed pair of nouns, of numbers, and of varying tenses of the same verb; alteration of grammatical person; and the use of manifestly Canaanite formulaic locutions. Because Tyre is in the Canaanite "heartland", so to speak, this should not be entirely surprising; but fruitful results, utilizing the same techniques from the same perspective, may also be expected, I believe, from investigations not only of Amarna letters from other Canaanite cities but of other Northwest Semitic documentary materials as well: particularly, those from Mari. I have elsewhere cited one expression common to biblical Hebrew, Phoenician, and the dialect of one of the royal families of Mari [46], another expression found in Old Akkadian, Mari, and biblical Hebrew sources [47], and have drawn attention to a possible parallel between the Canaanite and Marian literary traditions. Others have adduced additional examples from the letters and inscriptions of Mari and still other West Semitic dynasties in the Old Babylonian period [48], and a recent study has evaluated the presence in the letters from Mari of apodictic and casuistic constructions [49], otherwise best known from biblical Hebrew legal phraseology and Phoenician imprecatory formulations [50]. In sum, with the caveat that the missives from Amarna and Mari are not belletristic, the appearance in them of stylistic devices that can be compared, often precisely, with those in

[46] See *VT* 11 (1961) 142 f., n. 4.

[47] See *Judaism* 15 (1966) 248.

[48] Cf. Greenfield, op. cit. (above, n. 3), p. 258. On pp. 266 ff. of the same work, comparison was drawn between *KAI*, No. 26 iv 1-3, a passage in Ugaritic (RS 24.252 rev. 10-12 [*MRS*, 16, 554]), Pss 72,5,17; 89,37-38, and an inscription of Samsuilūna (YOS 9, 35, Pl. XIII:148-154). Apparently independently, Sh. Paul made essentially the same observation in a paper read before the annual meeting of the American Oriental Society held in Cambridge, Mass., on April 6, 1971.

[49] See A. Marzal, "Mari Clauses in 'Casuistic' and 'Apodictic' Styles", *CBQ* 33 (1971) 333-364, 492-509.

[50] Cf. *VT* 11 (1961) 137-158.

evidence in specifically literary texts helps us to expand our otherwise narrower view of Canaanite literature and to appreciate in somewhat greater measure the pervasiveness and utility of Canaanite rhetoric.

* * *

Excursus: The Alleged Egyptian Origin of the Tyrian Scribe

The author of the Amarna letters from Tyre was evidently a skilled practitioner of Canaanite rhetoric; yet, as was noted in the introduction to this study, W. F. Albright concluded that he must have been an Egyptian. These two judgments do not necessarily contradict each other; but, in the light of the findings presented above, the assumption of an Egyptian nativity demands further examination.

The more salient bits of evidence, adduced by Albright in support of his hypothesis, fall into three principal categories: words and glosses of Egyptian (and supposed Egyptian) origin, two poetic passages that may be direct translations from Egyptian (*EA* 147:5-15, 41-56), and what he termed "morphological and syntactic peculiarities" owing to "the scribe's ignorance of idiomatic Canaanite or Accadian" (*JEA* 23 [1937] 196). Upon re-examination of this evidence, however, the conclusion at which he arrived is by no means necessary, and, in view of the results of our own investigation, it is most unlikely.

Beginning with the Egyptian words and alleged Egyptian glosses, it is to be noted that, of the five words which had been identified as Egyptian prior to his study, three are military or administrative titles: *we'u, pawira, uputi*, and one is a designation of a particular kind of vessel: *akunu*. (Such use of foreign words, surely, need signify no more than a concern for precision). Because it has an excellent West Semitic cognate, there is little justification in seeking an Egyptian origin for the fifth, *ḫapši* (*EA* 147:12) [51]. Of the four terms added to this list by Albright, only one, *a-ru-ú* (*EA* 147:28), need be conceded an Egyptian derivation. Of the others, one, *pa-ni-mu* (*EA* 155:46), is highly uncertain; a second, *qu-na* (*EA* 147:36), as Albright recognized, may well be the imperative of Canaanite *kwn* (p. 197, n. 4); and the last, *ia-a-ia-ia* (*EA* 147:38), "an exclamation denoting approval", equated by Albright with late Egyptian *yꜣ*, "'yea, verily, etc.' (repeated for emphasis)", (p. 197), is just as likely to have its origin in West Semitic: cf. Hebrew *y'h* (and Syriac *ya'e'*), "to be befitting", and Punic *y'*, "good". It is, moreover, difficult to follow him when he says of *ia-a-ia-ia* that "such reduplication is apparently unknown in Accadian, Canaanite or Biblical Hebrew" (p. 197, n. 5). If he meant to imply that repetition for the purpose of lending emphasis to a word or expression is unattested in Canaanite and Hebrew, the implication is false; cf., e.g., Ugaritic *dʿ. dʿ*, "know well!" (*UT* 2114:13), Phoenician *'l 'l tptḥ*, "do not, do not open!" (*KAI*, No. 13:3-4), Hebrew *'ûrî 'ûrî*, "awake, awake!" (Jgs 5,12/12), *gᵉbōhāh gᵉbōhāh*, "(talk no more) so proudly!" (1 Sm 2,3), *hôy hôy wᵉnûsū*, "ah, ah, flee!" (Zech 2,10), and *ḥēn ḥēn lāh*, "grace, grace to it" (Zech 4,7) [52].

[51] Cf. Ugaritic *ḫpṯ* in *UT Krt* 90 (parallel: *ṯnn*, l. 91) and perhaps *UT* 2015: 25. See *CAD* Ḫ, p. 85 (*sub ḫapši*), and the literature cited there.

[52] For other examples, cf. E. Kautzsch and A. E. Cowley, *Gesenius' Hebrew Grammar* (Oxford, 1910, 2nd English Edition), § 123 e-f.

In the matter of syntax, I am at a loss to understand why *šaknātani ina rābiṣi*, "you have appointed me to (lit.: you have put me in [the office of]) commissioner", (*EA* 149:47-48), must be regarded as a translation of late Egyptian (*mḥ*) *di·k wim mḥ–ib* (p. 200). The verbal form reflects its Canaanite prototype, the phrase constitutes a characteristic West Semitic shift in person (see above), and the syntactical construction is perfectly at home in biblical Hebrew; cf., *my śmk l'yš śr wšpṭ 'lynw*, "who appointed you to (the office of) commander and judge over us?" (Ex 2,14).

One cannot fault Albright for failing in 1937 to see through the Akkadian guise to the Canaanite nature of the epithet applied to the Pharaoh in *EA* 155:6, 47, *šamaš dārîtu*, "the eternal sun", because its counterparts in Phoenician, *šmš 'lm* (*KAI*, No. 26 iii 19), and in Ugaritic, *špš 'lm* (*UT* 2008:7), were discovered only several years later [53]. One may, however, express astonishment at the weight accorded the Egyptian words appearing in this correspondence, while such undoubted Canaanite glosses as *nu–uḫ–ti* and *ba–ti–i–ti* (*EA* 147: 56) are so lightly dismissed with the assertion that they must have been furnished the "Egyptian" scribe by his Canaanite employer (p. 199, n. 12), and so patent a Canaanitism as *nadānu pāni*(*šu/ya*), "to set (his/my) face" [54], is completely ignored.

The poems may well be translations, at least in part, of Egyptian originals. As Albright admits, however, it cannot be presumed that they were prepared by the Tyrian scribe for the purposes of *EA* 147; rather, as he puts it, "it is much more likely that he had used the Accadian version frequently, and had perhaps obtained it originally from official Egyptian sources in translated form" (p. 198). Neither does this eminently reasonable conjecture warrant postulating Egypt as the homeland of the author. But whether Abimilki's scribe was an Egyptian, who had mastered thoroughly the niceties of Canaanite rhetoric, or a Canaanite with a proficiency in Egyptian rhetoric sufficient to mislead a renowned modern authority, these letters, written in learned Akkadian, display an extraordinary virtuosity, and constitute a remarkable *tour de force*.

[53] See *VT* 11 (1961) 143 n. 5, and Ch. Virolleaud in *MRS* 11 (1965) 15.

[54] In the letters from Tyre it appears in *EA* 148:9-10, 26-27; 150:4, 14-15; 151:19-20, 32, 37, 69-70; 152:55-56; and 155:59-60. For Ugaritic *yṯn pnm*, see *UT* 49 I 4, IV 31; 51 IV 20-21, 84-85; 51 VIII 1-2, 10-11; 67 I 9-10, II 13-15, V 11-12; 76 II 8-9; 129:4; *Krt* 301-302; 'nt IV 81, Pl. vi VI 12-13; and for *pnm lyṯn*, cf. RS 24.244:63 (*MRS*, Vol. 16, 568, 571). For biblical Hebrew *ntn pnym*, see Lv 17,10; 20,3, 6; 26,17; Ez 14,8; 15,7; Dn 9,3; 10,15; 2 Chr 20,3.

Memorandum on the Approach to Historiographic Texts

Mario LIVERANI – Rome

Histoire science de l'Homme, et alors les faits, *oui: mais ce sont des faits* humains; *tâche de l'historien: retrouver les hommes qui les ont vécus, et ceux qui dans chacun d'eux, plus tard, se sont logés en eux avec toutes leurs idées, pour les interpréter.*

L. Febvre

I. Introduction: Toward a "Comprehensive Reading" of the Historical Document

1. A marked progress in anthropological research has taken place during past decades through the substitution of the old method of the informer with direct observation. As is well known, former research by ethnologists was held without knowledge of local dialects and personal insertion in the local communities. The researcher usually remained outside the community and communicated with it by means of an informer, to whom particular questions were posed; and it is only too natural, today, to smile at the presumed authenticity of the answers given. Using this method, it was impossible to surmount a sort of barrier between the scholar and the society under study. The barrier was certainly a technical one: the presence of interpreters caused ignorance of local technical terms; too rapid visits failed to provide information on longer cycles; prearranged questions brought about misjudgement on the normal patterns of life in the communities; interest for differences and oddities made for overestimation of the latter, counter to a generally balanced picture of the environment. But the roots of such lack of communication lay in a cultural gap of obvious eurocentric character. The aim was a collection of exotic facts — of what is different from us — and not a reflection on human behavior — on *our* behavior as seen in others as in a refracting mirror which amplifies to absurdity certain features. The researcher of present times, on the other hand, lives for an extended period on the spot, learns the local language, enters fully into community life so as to derive a picture of that life without asking specific (and therefore inevitably biased) questions. The community becomes accustomed to him, and is hardly disturbed by the presence of an outside observer. The information collected in this way is much freer from the distortion caused by intermediate passages.

A similar shift in methods could also be adopted in regards to historical societies, or "dead" societies as they are usually called with a deceitful and inelegant term. Hitherto written documents which have come down to us from historical societies were generally made to play a role corresponding to the one of the "informers". In other words, it was believed that facts of interest to us lay beyond the texts; that the latter, when questioned, could provide us with pieces of information. This information is however foreign to the texts themselves; their exactitude and very existence should, moreover, be considered largely questionable by the responsible historian, due to the cultural gap between us and the historical society — a gap not filled by the "informer"-text, but rather evidenced by the deforming elements of the latter. The thing to do should be to view the document not as a "source of information", but as information in itself; not as an opening on a reality laying beyond, but as an element which makes up that reality. Or, in keeping with the ethnological comparison made above: not as an informer, but as a member of the community under study.

2. The shift in focus is hardly a small one, and hardly an artificial one. Rather, it corresponds to a completely changed approach by the scholar to the society and to the text considered as a constitutive cell of that society. The view of the document as a "source of information" brings about a quite elementary form of interpretation: information materially present in the text is purely and simply taken out and employed to rebuild a sort of mosaic related to the problem under study. The document is therefore considered *a source for the knowledge of what the document says*. This attitude, apparently obvious and unerring, in fact brings about serious drawbacks: (1) if the textual information is wrong, as might be the case for various reasons the error passes inevitably into the historical reconstruction (and in particular: if the information is contradictory one finds one self at a loss, if it is not contradictory it is difficult to detect possible errors); (2) the type of information in the texts does not always satisfy the needs of the scholar, who has different scopes and interests from the ones of the writers of the documents, and who should like to obtain certain information for which the texts, to his dismay, are uncommunicative or altogether silent.

Let us on the other hand try to view the document as *a source for the knowledge of itself* — i.e., as a source of knowledge on the author of the document, whom we know from the document itself. In this type of approach our attention is no more centered on the events, but on how they are narrated. For the event is foreign, to a certain extent, to the author of the text, is independent of his will and does not aid us in characterizing him. On the other hand, the way the author presents the event is a characteristic feature, since it is *his* way of entering in contact both with the event and with the public (i.e. his public; the modern scholar is a secondary and not expected public). The peculiarity of the narration is the element by which we may hope to gain some enlightenment on the historical environment of the author, and possibly even on the single author in the context of his environment.

This type of approach requires, so to speak, an increased delicacy as regards the document, which must no more be forced, dissected, plagiarized for our aims. Rather, its literary structure, terminology, and implications must be tactfully analyzed toward an understanding as complete and conscious as possible. To speak in the linguist's terms, we need to take a higher interest in the connotational level than in the denotational.

3. In this way every irregular, irrational, inconsistent and even reticent element in the document will cease to seem an obstacle to the reception of the message, and will instead become a privileged sign of the code. From deterring element of analysis to stimulating element of analysis; especially so if we think that the so-called disturbing elements are simply elements of difference between "our" way of presenting information (understood by us as the best possible way) and the author's way. A difference of this type actually helps us to set in a historical perspective the relation between us and the environment under study: i.e., it shows us that every historical society has certain characteristic way of conceiving, of living, of presenting reality.

Considering every "irregular" element as a privileged bearer of meaning, we may bring to mind a quite large series of these "irregularities". It is obvious that intentional (or even sub-intentional) distortions, for example those typical of political (or generally ideological) propaganda, are to be set at one extreme (the most illuminating) in the series. They offer possibilities of analysis (which should be evident, but unfortunately are not, in our field of study) of the cultural environment in which the event is set, and of a complex network of reactions which the event causes among different sections of the public. We shall come back to these problems when giving examples later on. But even the most unsophisticated error, which we may set at the opposite (or least illuminating) extreme, may have its meaning. Let us consider, for example, a copyist's error or a textual variant, in the light of all the conclusions the most refined philologists are able to draw on a historical-cultural level. Or let us consider an error of calculation in an administrative text: even such a simple mistake may be illuminating for us, as regards the methods of calculation if nothing else (certain errors may occur using the decimal system, others the sexagesimal system; certain errors occur doing additions, others doing multiplications, etc.), but also perhaps as regards procedures of the bureaucracy (were the figures obtained by counting, by copying from a former text, or through intermediate calculations? did the scribe write from dictation, or did he count himself?). An analysis of such errors in an entire archive or in an entire administrative system could provide us with information about the conditions in which the documents were written, and about the capacity for tolerating errors on the part of the administration, in relation to the purposes for which the calculations were made and kept.

4. Naturally if a document viewed as a "source of information" may be used in its separate parts, i.e. excerpting information out of context (and here lies the limitation of this procedure), a document viewed as an object of study in itself need be considered in its entirety. Surely it may — and must, as we shall see presently — be decomposed and analyzed, but in a different sense. The breaking up into constituent parts is not done in order to dwell on the individual parts apart from the whole, but rather in order to achieve a more accurate understanding of the whole and of each part in the context of, and in relation with, the entire document.

The concept of total or comprehensive reading of the text must be understood in two ways, both of them equally necessary. The most obvious and simple way is that the document be read *in its entirety*: a completeness in quantity, necessary to understand why the text was written. There is, however, a more complex way of understanding the concept of totality: the texts must be read *from all possible points of view*. This type of completeness, in quality, is

necessary to understand why the text was written in that particular way. Only with this type of analysis can we exploit all the possibilities offered by the text for the understanding of its author — i.e., of its position in relation to the world around and to the specific historical circumstance. One could even establish a whole series of motivations and modalities, from more complex ones for ideologically committed texts, to the opposite for texts of practical or occasional function; and it would be a series only seemingly diversified, in fact perfectly fitting in each case to the type of text.

Any single text may therefore be examined from different points of view, or better at different levels, toward a total understanding of it. The existence of various levels of analysis is more obvious in regard to some linguistic levels (not to speak of the level of palaeography), than for others of an exegetical character; but no real gap separates the ones from the others. It is clear to all that a morphological analysis must be separated from a syntactical analysis; it is clear to most that a semantic analysis denotational in level, must be distinguished from a connotational one; but not yet general is the consciousness that the analysis of the "events" narrated in the text must be set aside from the analysis of the literary and thought patterns according to which the events are presented. Naturally, whoever seeks pieces of information is interested in finding the event, and tolerates with annoyance the burden (if felt) of the pattern which "distorts" the event. But whoever (like the ogre of M. Bloch) is seeking man is much more interested in the pattern (which is a product of man) than in the event, which makes up, so to speak, the rough and occasional material for the pattern.

5. In terms of saussurian linguistics, one could consider the single document as an act of *parole*, isolated and unique and hard to analyze as such; in it, however, various facts of *langue* meet and come into being: any of these may indeed be analyzed as elements of a system. Or, in terms of gurvitchian sociology (on this point inspired by Mauss), we might consider the single document as a "total social fact" and also as a "total psychic fact", in which a number of "structures" intersect: all of these may be analyzed separately but in relation to the others to rebuild a complete understanding of the single "total fact" which alone exists in historical reality.

On the one hand, therefore, we are forced to admit that only the single document exists in reality, and that our final aim has to be the complete understanding of the single document. But on the other hand this understanding can be attained only through the analysis of the many "structures" that are interwoven in the document: structures which do not exist in reality, which are only a fabrication of ours for a certain aim, but which alone allow us to connect the document under study with other documents — and with ourselves. The most productive type of study of the single document towards its total comprehension derives therefore from the setting of the text in a homologous series, chosen so as to enlighten the particular structure under study, and to set apart the paradigmatic variants and the syntagmatic successions (in terms of R. Barthes). Choosing the series in which to set the text is clearly a delicate and consequence-bearing act of the analysis, but it often is quite a natural one: one may form a series from an actual historical group of documents (a letter shall be studied in the context of the correspondence to which it belongs; a legal act in the context of its archive), or from a literary genre (a decree shall be studied in the context of the genre of the decrees, etc.). But it may be quite

useful (i.e., it may prove illuminating) to set the text into a seemingly removed series, either in time and space (Idrimi studied in the context of Russian fairytales) or in literary genre (the letters of Rib-Adda compared to the *Ludlul*).

In the examples which follow (based on studies still in progress), problems connected to only one of the many possible "structures" will be tackled: the search for thought patterns of mythical (I use this term in the sense of preformed) character in historiographic documents. I believe, by the way, that, as for historiographic texts, this structure can most aid us in grasping the total meaning of the single document; also, this has been the most neglected structure up to the present.

II. Exemplification: Mythical Pattern and Historical Event

1. *To the conquest of the throne*

The inscription of Idrimi is a precious remain of a Syrian historiographic tradition, signally different from the contemporary Mesopotamian, Hittite and Egyptian ones. This tradition is almost totally lost to us, although it must have been a rather important one: its existence explains creations such as the story of David, otherwise unjustifiable as to its origins. The isolation of the inscription of Idrimi does not allow us to set its structure into its most relevant framework, i.e. in the context of other examples of the same genre proceeding from the same cultural milieu. Nevertheless, from the very beginning of research, some fablelike elements of the text were evident (more from immediate sensation, I should say, than from accurate analysis). A comparison of the pattern of the text with the pattern of the fairy-tales (as formalized by V. J. Propp) may give exactitude and substance to these sensations. The fairy-tale character of the story of Idrimi appears from the overall pattern in use, from single details of the narration, from the tone of the story itself, from the treatment of temporal determinations, from the stylization of human relations in the story: this character is therefore a solid and well-established fact. One could even rewrite the tale according to Propp's symbols, although I believe this would only result in a game, which adds nothing to our knowledge.

In this case the introduction of the text in a series helps to single out the narrative structure. Of course, immediately after this operation the series used as a framework (in this case the corpus of Russian fairy-tales), being not relevant from a historical point of view, is set aside; or rather, its historical elements are set aside. By means of the comparison the text becomes endowed with a significant structure. What was shapeless when analysed by itself now reveals its compositional elements; formerly unobserved meanings, motivations, connotations may now be easily singled out in the framework of the series.

In turn, the tale of Idrimi, in which the fairy-tale pattern is more conspicuous, may serve as reference point for other texts of related environment, in which the pattern is less clearly shaped, although present. E.g., the apology of Ḫattušili III, or the story of the rebellion which brought Joas to the throne in the place of Ataliah, or some Egyptian texts of the *Königsnovelle* type. And vice-versa a comparison becomes necessary also with texts of different types, closer to the actual fairy-tale, such as the poem of Keret or the Egyptian tale

of the foredoomed prince. A study of differences and of similarities (in pattern, in detail) becomes of course more solid if we limit ourselves to this whole group of texts: there are more possibilities of clarifying correspondences with the historical situation (the event which is related) and with the aim of the composition (the public to which the story is related).

The relation between pattern and event does not so much pose the dilemma: "true or false?" — for which there is a tendency, and an oversimplifying one at that. Rather, the problem posed is that of the interpretation of the event by the protagonist (a person emotionally and socially interested in choosing a specific interpretation) in the light of the presentation of the event to a definite public. It may perhaps be useful to distinguish two different cases: there are some elements which we may term fabulous (preformed and therefore implicitly non-historical) insofar as they correspond to a certain way of narrating and, even more, to a certain way of perceiving reality. This way is common to the whole milieu from which the text is produced; no particular implications of purpose are therefore to be understood. An example of this is the motif of the "seven" years. But there are also elements which are fabulous on a deliberate level (fully conscious or even sub-conscious) in that they intend to introduce the event so as to recall to mind other texts (of mythical, poetic character) and therefore to provide the protagonist with a somewhat heroic quality. An example of this is the contrast with elder brothers. More discrimination is required in the analysis of this type of element; one needs to appreciate (and previously to detect) a play upon semantical innuendoes and intentions. Idrimi sets out on his own, with chariot, horses and personal attendant: this piece of information (in terms of an *histoire événementielle*) may be considered quite ordinary, while in fact it aids in depicting the protagonist according to certain common standards of courage, determination, break with the past and with the family. Idrimi is the youngest of his brothers: the degree of exactitude of this piece of information (in the terms referred to above) is open to doubt, but it helps again to enhance the features of the hero (the one against many, the youngest against the elders). More specific details in the story (the hero sleeping in his chariot, or meeting the Suteans) also have their meaning, their connotative function, although it may be more enigmatical for us to appreciate them at present.

The overall pattern may be analysed in the same way as the details: it is a preformed pattern, just as many details are preformed. But what is important is to try to understand the reason for which it was adopted: i.e. the plays upon innuendoes and sensations it should have stirred up in the public for which it was meant. In the case of Idrimi, of Ḫattušili, of Joas, there is a fundamental and evident historical fact to be read through the fairy-tale plot: in all cases we have to deal with usurpers to the throne, who needed to display their action as heroic (for the hero prevails by himself over all, overcoming all difficulties), as righteous (for the hero restores oppressed rights), and not as a disturbance to a normal and correct state of things, but rather as the re-establishement of a normal state after a period of alteration (see further on, on the motif of the restorer of order). The story of these usurpers is therefore presented according to the pattern of the fairy-tale prince, who sets out to reconquer the lost throne, and regains it by himself. Adopting this pattern Idrimi intends to appear legitimate and heroic to his public. It's *his* version of the story: we may well imagine how different the same story would be if the narrator were the unknown king he drove out from Alaḫaḫ.

2. *The righteous sufferer*

In opposition to the isolation of Idrimi's inscriptions, the exasperating repetition of Rib-Adda's correspondence suggests by itself the possibility of viewing the single letters as constitutive elements of a series; also, of excerpting a constant conceptual pattern from the series, suitable for all single examples. The resulting pattern (or, since a pattern is always the outcome of a voluntary simplification, it would be best to say: the pattern which seems the most illuminating) is the one well known from wisdom literature (Job, *Ludlul*, etc.) as the motif of the "righteous sufferer". The protagonist feels abandoned by divine assistance, and consequently by wealth which used to follow him, and by all friends who used to back him. He examines his own behavior but fails to find any wrong or deceit towards the deity which might explain the change in his fortune. The only solution is therefore to hope and ask of the deity a new turn of fate by which justice and happiness may coincide, and his arrogant and selfconfident enemies may be confounded. Anyone experienced with the correspondence of Rib-Adda is able to see how every single letter may be precisely fitted into this pattern or part of it.

Since Rib-Adda's correspondence spans some ten years in time, the preconceived character of the pattern is evidenced by its diachronical persistence. An element which might be viewed as historical "information", punctual and reliable when considering a letter by itself, shows instead very clearly its biased character upon noting that it persists unchanged through the entire series of letters. The protagonist is always on the brink of final catastrophe, is always on the point of succumbing if reinforcements do not arrive within six months' time. Rib-Adda is always abandoned by all, but he always still holds two last cities; the latter might even be different from one time to the other, but they always are two in number and always the last two remaining; there is always a prince of Ammiya who has been killed by the rebels; Aziru does all that Abdi-Aširta has previously done down to the smallest details; but the age of Abdi-Aširta, which was a time of the blackest despair when present, when past acquires the overtones of happy bygone days in which all was for the best.

The qualifying points of the pattern may be brought down to a series of absolute and preformed oppositions. It may prove useful to force the analysis towards a greater formalization, in the light of models proposed by current structural and semiological analysis. One may set up oppositions of two "spaces," different in quality (the internal space of security and the external one, hostile and variable) and of two "times" also different in quality. The nearby time (present) is disturbed by the non-coincidence of justice and happiness, and the distant time (past and future) is correct, for justice and happiness concur. Also the "spheres" of the single actors — i.e., their ethical and psychological evaluation and the type of action competing to each of them — may be characterized. We have here an opposition of the righteous and unprosperous protagonist, and his unrighteous and prosperous enemies. Crucial element in the "play" between justice and prosperity (a "play" which may be detected through the analysis of time and of the actors) is the god's attitude: if he is present he brings about the correct state of things, if he is absent he gives ample opportunity for disturbance. Oppositions (or at least alternations) of paradigmatic character can be evidenced: e.g., the alternation between *āla naṣāru* "to protect the city," and *āla ezēbu* "to abandon the city," typical of Rib-Adda; or between

qālu "to take no interest" and *idû* "to take care," typical of Pharaoh. There are also links of syntagmatic character: e.g. the typical sequence *wuššuru* → *naṣāru* → *aṣû* → *pašāḫu/balātu*, i.e. "if you provide me with the necessary means → I shall protect the city → until you will come out to solve the situation for good → and then we shall be able to live in peace for ever."

As can be seen, it is possible to build quite elementary structures. By giving graphic symbols to each of the elements detected through analysis we might even condense the single letters into short formulas which clarify the relative position of the elements, their compatibility or incompatibility, the characteristical sequences, their essential or subsidiary quality, and so forth. Obviously (as we have already noticed for Idrimi) these graphic formalizations are not to be over-estimated: a (mathematical or in any case symbolical) formula has value for knowledge only if it allows certain operations which may not be accomplished through language. This is not the case of our formulas, which would only aid in resuming what has been detected. But in any case, the effort of abstraction to arrive at formulas might perhaps help to organize the pattern along stricter lines.

Having excerpted the preformed pattern, one may view its relation with the historical events in various ways, depending on the particular case. For example, the problem might be considered one of literary technique: i.e., the scribe who composed the letters (or the king himself who dictated them?) made use of certain patterns, either clear to his mind at fully conscious level or at least assimilated by familiarity with wisdom texts. I believe however this direction of research to be scarcely fruitful in this case, at least until clear echoes of wisdom literature in Rib-Adda's phrasing and vocabulary are identified and brought forth. The equivalence of the pattern together with differences in terms used should rather induce us to consider the psychological situation as identical. Rib-Adda adopts the pattern of the "righteous sufferer" because he believes himself to be living in a situation, in a network of human relations, similar to the one sensed by the scribes who wrote the wisdom texts. But, being a political figure, he extends this existential feeling of his from the personal to the political sphere; and he expresses it by means of a stateman's messages instead of literary creations.

Should we want to examine the correspondence between Rib-Adda's existential feeling and the historical events in which he is involved, we would soon notice that this type of research proves impossible. We are not in possession of the historical event, only of some interpretations of it: the views taken by the different actors and witnesses, and the opinions of the historians who reconstruct events through those views. I am afraid that the concept of "historical event" — but I hesitate entering into a field inaccessible to me — is a pure abstraction, which in all cases implies a choice in interpretation, a way of understanding and of presenting. The reconstruction of the most artless and common episode of everyday life is already a simplification in itself; beginning with the choice of the people involved, of their physical movements, and even more — naturally — the interpretation of their intentions, of the elements which condition them culturally at an unconscious level, and so on. We must resign ourselves to recognize that the so-called "event" is, upon objective consideration, so complex as to be impossible to describe and in fact unusable: every use of it implies a drastic simplification which is necessarily biased in one direction.

In our case we have the view of events given by Rib-Adda, a view evidenced

by the adoption of a certain pattern. Even analysing this view along internal lines we may be strongly doubtful as to which part of it is the outcome of inner belief and which part on the other hand represents a clever attempt at convincing the addressee. We may also be doubtful as to which part of the interpretation goes back to Rib-Adda himself and which part is shared by a definite milieu. To Rib-Adda's interpretation we may in any case oppose other views, of which we know much less, of course, but manage to catch a glimpse. So we see (from fragments of answers quoted for the sake of arguing) that the opinion of Pharaoh was different from that of Rib-Adda; again different was the interpretation of Abdi-Aširta (e.g. as for the view that peace was the outcome of victory of the *ḫabiru*s and the banishing of the *ḫazānu*s and of the Egyptian troops, exactly the opposite of Rib-Adda's); also different was the opinion of the peasants of Byblos, with their main problem of where to find something to eat; and even the view of Rib-Adda's wife, who hoped for peace in alliance with Aziru, was different.

In this state of things, after having reconstructed the interpretations of the single actors or "witnesses," it is only natural to propose our interpretation, which is handicapped by a much lesser knowledge of events (inadequately selected, moreover) in comparison with that of the actors. We are, however, advantaged over the persons concerned by the cognizance of later developments and of a few documents to which they had no access; also by the very opposition of the different views of the actors, and by the more refined methods of analysis at our disposal. But the result will be *our* interpretation; we must be conscious of this. It is incorrect to believe acritically that the interpretation of Rib-Adda is the "true" way in which events took place, as has been done down to the present, giving concrete substance to what are in fact the stereotyped, mythical characterizations of the pattern: the negative quality of time (from which it has been inferred that the Amarna age was an age of grave crisis), the opposite characterizations of the protagonist and his enemies (from which it has been inferred that pro- and anti–Egyptian parties existed, i.e. "faithful" and "hostile" groups, with no thought to the turnover in roles depending on whom is writing), the absenteeism of Pharaoh (from which misinformation or loss of interest in politics or even personal laziness have been inferred). But vice-versa it is also incorrect to suppose that — since Rib-Adda adopts a preformed pattern — the information he gives is "false." Rather, if Rib-Adda makes use of a certain pattern, it naturally means that the latter is somewhat convenient, somewhat adaptable to events; Rib-Adda adopts the pattern looking for a definite meaning, but also referring to definite facts. The pattern, and in general the way of narrating, is a sort of bridge which the author of the text throws between the events and his public. We may use the text to try to catch a glimpse of the event and a glimpse of the public; but we certainly cannot do so without having clarified in the first place the position of the author himself.

3. *The restorer of order*

In a situation like the one depicted by Rib-Adda the most characteristic feature is the quality of time. The pattern according to which a positive past is followed by a negative present, which it is hoped shall be followed by a positive future (as a repetition of the situation of the past, which serves as model), is generally characterized by a cyclic appreciation of time. This cyclic charac-

ter is provided by the identification of the quality of the *Endezeit* with that of the *Urzeit*, by the *éternel retour* with which the one restores the other. All this is true, and has been (and is yet) essential for understanding the organization of time in texts of "mythical" flavour; especially since we have a clear contrast between this appreciation of time — different in quality — and the one — homogeneous in quality — of linear time in "rational" thought. But the characterization of this time as cyclic could bring about a misunderstanding which it is important to avoid: with the above definition one could think that the propounders of this notion on certain occasions saw the cycle actually completed, i.e. noticed the coming into being in the present of the future which concludes the cycle. It is not so: it would be better to speak of an "arrested time" than of a "cyclic time." In the case of Rib-Adda, as we have said, the diachronic character of the documents evidences in itself the freezing of all historical experience in the immovable pattern: all positive events are framed into past or future, all the negative ones into the present, and the position of the three periods is not subject to rotation, the protagonist being imprisoned in the temporal quality of the present. It is for this reason that the king of Ammiya is always killed "today," even after ten years; it is for this reason that a visit to Pharaoh takes on a quality of salvation if set in the future but one of oppression if set in the present. The strength of the pattern is such as to assimilate all historical content to itself; the permanent datum is the persuasion of living in a negatively characterized present, to live on the brink of catastrophe. The happy images of past and future act like the bar of the tightrope walker, to keep immovable this quite fleeting moment which is the "eve of the end." The wheel does not turn, it is at a halt.

There is another pattern, as widespread and as famous as that of the "righteous sufferer," which could be at face value viewed in terms of a "rotation" of the characteristic qualities of time: it is the pattern of the "restorer of order", as found in the reforms of Urukagina, the edict of Telipinu, or that of Horemhab, just to give a few examples. In this pattern the sequence of the qualities of time is the usual one (good → bad → good), but the subject seems to have moved one step further in the sequence. The happy past is pushed back into a more remote past, a veritable mythical age, and its function of ideal model of a corrected situation is underscored. The phase of corruption and chaos is over, i.e. moved from the present to a nearby past, just finished; while the second stage of order and prosperity is moved ahead from the future to the present. It would seem, therefore, that the cycle had undergone a rotation of one degree.

Actually, the two patterns are used in a completely different manner and especially in completely different texts; these differences are essential for an understanding of the relations between the two patterns and especially between each of them and the existential reactions of their public. The pattern of the righteous sufferer has in all cases an autobiographical character: it is the meditation of an individual on his own condition. The pattern of the restorer of order has instead public character: it is political authority addressing the entire community. Different, in a parallel fashion, is the "verbal" (so to speak) aspect, with which the action is set into the pattern: in the first case we have a narration concerning the present, in the second a volition concerning the immediate future. Result of the comparison is that there is no real rotation of one degree in the cycle, only a different visual direction: in the righteous sufferer what we

call "present" (the phase negatively connotated in which the individual feels he is living) is in fact the immediate past, is a "hitherto" which one hopes shall change; while in the restorer of order what we call "present" (the phase positively connotated which is inaugurated) is in fact the immediate future, is a "from now on."

The two patterns are therefore complementary to one another, in opposition from a certain viewpoint. The study of the one in function of the other, which does not seem to have been attempted as yet, may represent a useful opening to understand the connotative intentions, manifest or sub-conscious as they may be. Naturally the second pattern will be rather provided with such implications, since it is a "public" pattern, i.e. it establishes contact between two different groups (between the king and his subjects, in the case at hand); the pattern of the righteous sufferer — due to its character of personal considerations — will be rather provided with psychological motivations. Now, it seems clear that the political effectiveness of the pattern of the restorer of order is due to the fact of being addressed to an audience in which one finds widespread an attitude of the righteous sufferer type, or more generally a negative appreciation of the present coupled with a longing for the past and with salvific expectations in the future. The king who terms himself restorer of order conveys in essence a message of this type: "The negative present in which all of you used to live is over beginning from now; all the hopes you had are fullfilled beginning from now". In other words the political authority arranges for its subjects to rotate of one degree their existential cycle; but whether the rotation really takes place (apart from an initial enthusiasm) or not, is another matter. In any case the king has one result in mind while delivering this message: to induce his public (his subjects) to recognize him as the "god" from which they expected salvation.

If we have set in evidence the aims of propaganda (in any case well known) of the pattern of the restorer of order, it is not our intention, here as before, to deny its connections with a genuine policy of reform. On the contrary: one may notice that the application of the pattern to the genre of the edicts, and its normative character ("from now on...") assures that the connection exists; or rather that the pattern has not only a verbal but also a factual application. But reforms can be of various types, and upon examining this point in every single case we find ourselves abandoning literary problems to enter the realm of historical questions proper. So, just to give a few rapid examples, reforms can be quite significant as in the case of Horemhab, who intends to rid himself of the Amarna heritage and restore certain cults and milieus in the place of others; other actions are merely personal, as perhaps in the case of Urukagina, if he restricts himself (as I believe may be inferred) to replacing single officials guilty of misuse of power; other actions merely aim at revocations and amnesties, as in the case of remissions of debts (of the type of Ammiṣaduqa's edict); other reforms may be only of the make-believe type, as in the case of Telipinu who proclaims the introduction of a mechanism for succession to the throne which was already in use; and so forth. But in general a more attentive consideration of these texts as media for propaganda brings about a lessening of their value as media for historical information (in the terms referred to above): what they give as regulation edict (of a more or less utopian flavour) we must not alter into narrative record. And I believe this has been done too often, despite the underlying pattern being well-known and manifest.

4. *The city at the center of the world*

The patterns of the righteous sufferer and of the restorer of order are especially characterized in a temporal sense, i.e. they move from a different appreciation of the quality of the different phases. Equally interesting is the analysis of patterns based on space, i.e. based on a different appreciation of the different zones. As in the case of time, the different zones in space are singled out in relation to the position of the subject, and therefore change if the subject moves or upon considering subjects in different positions. And as in the case of time, the different zones in space are characterized by a different evaluation in quality, firmly connected to them. We find usually an opposition between nearby or central space and far-off or peripheral space; to it are connected other oppositions, such as the one between a closed and an open space, between a firm and a movable space, between a light and a dark space, and so on. Differently, however, from the temporal patterns, the qualitative appreciation of space provides generally negative connotations to the far-off (unknown) zone, and positive connotations to the nearby (known) zone. One could think that the qualitative appreciation of time stems from a feeling of dissatisfaction, whereas the spatial one derives from a need for security; that the former is tied up to the sphere of hope, the latter to that of fear. In fact the temporal feelings are rather introspective in character, i.e. stem from self-consideration; whereas the spatial ones are more external in character, come into being through relations with others.

Notice that some evaluations of space of utopian character, in which the far-away zone is considered superior in quality to the zone of residence — evaluations which would appear to contrast with what we have just said — never refer to all the peripheral belt, but to a precise region which the subject considers central although at the moment his personal position does not coincide with it. Such utopias of migration towards a better land (as with the "promised land") are always seen as a return to the original abode. It follows that the upholders of these utopias also view the center in a positive light and the peripheral belt in a negative light; but they believe themselves to have been temporarily transferred to the negative outskirts and long to return to the positive center. As can be seen, we have to deal here with the insertion of the temporal pattern in the spatial one, resulting in a play on relations between the opposite characterizations which corresponds perfectly to the premises of the two patterns.

Evident and well-known applications of the spatial pattern of the type illustrated above are linked to the idea of the center of the world being in a great city. The latter is normally the political and cultic center of the community which upholds the conception (e.g. Babylon, Jerusalem); a fact which makes the idea of the extension of the umbilical function from national to cosmic a reasonable one in the eyes of the internal public. But after all, this idea only makes clearer the feeling of being in the center which we all have, independently from the political, religious, cultural role of the place of residence.

A chance to make use of the pattern of the "city at the center of the world" is given by the foundation of a new capital, designed by will of the sovereign to have the umbilical function. We may recall to mind texts of Amenophi IV on the foundation of Akhet-Aton, of Assurnasirpal II on the foundation of Kalḫu and of Sargon II on the foundation of Dūr-Šarrukīn. These texts do not

adopt identical literary patterns (or at least, this is not the point here), but rather make use of a similar imagery, suggestive and constitutive of the symbolic motif defined as "the city at the center of the world." The critical procedure to adopt is therefore not one of structural analysis but an analysis of archetypes; the aim in this way is towards the understanding of the imagery used, not towards (or rather: before) the understanding of how it is arranged and presented.

Leaving some symbolic elements aside, highly interesting as they are (e.g. the cosmic character of border, the regularity of the city plan, etc.), we may consider by way of example the movements of materials and peoples related with the building of the umbilical city. The most insistent stress is laid on the fact that materials and people came from all the outlying areas (especially opposite zones, to express totality in terms of circumference, and extreme zones, to express totality in terms of radius). This stress obviously is meant to show the dependence of peripheral areas from the center, and the strength of the latter in making its influx felt to the farthest margins of the world, and consequently the uniqueness of the center, the absence of rival centers. Moreover, as a result of this centripetal collaboration, the umbilical city reproduces the whole world, condensing it all in one place: e.g., the *bīt ḫilāni* "as the Hittite palaces," the garden "as on Mount Amanus," etc. Finally, the outlying zone, in itself chaotic, becomes orderly when used and disciplined by the center: so all the deportees which spoke "strange and different" languages may now all be taught to speak Assyrian; so the stone quarries which nobody had ever seen before (and therefore were as non-existing) may now be known and used, etc.

The celebration propaganda which originates from the political center only stresses, or rather acknowledges the existence of those movements which contribute to the prominent position of the center itself. The movements considered meaningful (not only in the specific case of the foundation of a new capital, but in general in the centralized spatial pattern) take place along clear lines, forced by the cosmic character of the world, unlike "rational" space, in which movements take place in all directions, since space is homogeneous. In practice the movements among different points of the peripheral belt have no importance and are ignored, if not included in a general sense of chaos (of which disorderly movement is an essential factor) and so negatively connotated. What counts is the movement between the peripheral belt considered as a whole and the center, and naturally movements from the center are viewed unlike movements to the center. The former take place due to a free act of will of the center, the latter by force, by the force of the whirlpool-center on the outlying zone. Moreover, incoming material is usually of base origin and usually acquires greater prestige by way of the movement to the center; while outgoing material is highly priced and is intended for consumers of lower condition. At times incoming and outgoing movements can be opposed as concerning respectively material (destined for consumption or transformation) and immaterial goods.

The problem, as usual, is that of the ties between the pattern (or the archetypes) and the actual historical experience upon which each text is based. It is immediately clear that what has been said at the level of symbolic values can be connected with economic and political structures. A palace type of organization implies a difference between the capital (the center) and the villages (the periphery); the former is seat of activities of transformation, accumulation, consumption, the latter seat of activities of production; the former is

active subject of decisions and propelling center of actions, the latter passive ground for events. Affluence of materials is directed from the peripheral areas to the center; traffic among different points of the outlying region is to be avoided since it is disturbing; in exchange for the centralization of surplus, ideological values (life, protection) proceed from the center. In practice, the difference between center and periphery is a clear difference in quality. One might infer that the movement of persons from the periphery to the center has to be evaluated univocally as a process which can only make things better, that this movement cannot be subjected to different interpretations depending on points of view.

But if we analyse documents which proceed from different viewpoints, we find that, the pattern being immovable, its relation with the historical situation is reversed, according to a different casting of roles. The case of deportees is a good example in this sense. The deportation, as presented from the viewpoint of the deporters (a viewpoint which we know from the Assyrian annals) is a positive fact, since it is a contribution to world order. The zones from which the deportees come gain from the situation, since they shall be re-structured, repopulated and thus gained to cosmos from chaos; the deportees themselves gain from the deplacement, since they come closer to the center and so are in the part of the world which works correctly. But the same episodes acquire opposite connotations from the viewpoint of the deportees (a viewpoint which we know from the Old Testament). They believe they were previously living in the center of the world and now are being moved to the outskirts, passing from cosmos to chaos; and the arrival of foreign farmers is an element of disorder and not of regulation. Altogether we have to deal not with a marginal portion of the periphery acquired for order, finding its correct and final peace; we have to deal with the cosmic center overturned by spreading chaos. Only one hope is left: hope that this state of things may not be final, rather representing a negative interval limited to the present; hope that an unfailing future turnover may restore forever the past order.

5. *Gifts and tributes*

The analysis of the spatial (center vs. periphery) pattern has brought us to a study of the patterns of personal relations, in particular of those which center around the movement of goods. These relations may be organized (in the type of society under study, in which the market pattern is not feasible) along the two patterns of "reciprocity" and "redistribution" (in the terms of K. Polanyi). The first pattern sees equal movements in both directions among two partners considered of equal rank; the second sees forced movements (i.e. non-reciprocal) from a periphery to a center considered of higher rank. The exchange of gifts is a concrete example of the first pattern, the bringing of tributes of the second. If initially the direct correspondence between pattern and "event" appears to provide the pattern with objective value (i.e. a possibility of being described in physical terms: changes in position, quantitative ratio, etc.) an attentive analysis reveals the ideological character of a construction obtained through voluntary (although sometimes unconscious) selection. A privileged situation for analysis will therefore be one in which the same event, or the same type of events, is presented once according to the reciprocative pattern and once according to the redistributive pattern, depending on the point of view taken and on the public.

A good example of the above is the system of trade at royal level (i.e. of diplomatic, not commercial character) between XVIII dynasty Egypt and the "great kings" of Asia. The diplomatic correspondence which prepares and accompanies the single movements of goods presents these movements as an exchange of gifts, which is conducted among individuals of equal rank, with no coercion but rather with the intention of pleasing the addressee, with no gain but rather with a show of generosity. The documents of monumental Egyptian propaganda instead show the very same movements as incoming tribute, a movement which takes place among persons of different rank, conditioned by fear (of Pharaoh's power) or respect (of his prestige), and to the exclusive advantage of the receiver.

A superficial study could bring about (and has in fact brought about) the opinion that a text such as the Annals of Thutmosi III records "information" about tribute payed to Pharaoh by the kings of Hatti, Babylon, Cyprus, etc., and that political dependence from Egypt on the part of the above rulers may be inferred. But a simple comparison with documents of different character shows the deformation for propaganda aims in the celebrative texts. This is not sufficient, however, to declare the problem solved at this point, simply by singling out a propaganda "fake," or even by translating the key-word *inw* as "gift" instead of "tribute".

First of all, the detection of a character of propaganda for the redistributive pattern does not automatically imply the truthfulness of the reciprocative pattern. It is not that certain texts deform as tribute what was "really" a gift among equals. The pattern of the exchange among equals is a voluntary and biased simplification itself: it displays as equal partners individuals of quite different economic and political-military power, it shows the disinterested character of deeds which tended strongly towards personal gain, it denies political conditioning (in the sense of awe, of fear) which is sometimes evident, and so on. The convention of the "brotherhood" on which the pattern of reciprocal gifts is set is upheld or denied depending on political intentions. The single transactions are quite complex and finally non-repeatable (as acts of *parole* in the terms used in the introduction above), but are presented according to the simplifying and biased patterns of reciprocity in some cases and of redistribution in others.

But the real problem is to clarify why and how the two patterns were applied. The choice of the pattern is directly related to the type of text: the pattern of reciprocity is selected in texts which set two different political centers in contact, while the pattern of redistribution is used in texts through which the political center addresses the internal public opinion. So the opposition is between internal and external audiences: the way of presenting events is the way by which the subject enters more advantageously in contact with the addressee. As for how to adapt the event to the pattern, this is based on a choice of what is to be said and what is not, and on a play of connotations. In saying that Pharaoh receives goods sent by the king of Hatti or by the king of Babylon, the author of the text "skips" saying that Pharaoh on his part sent other goods to the king of Hatti or to the king of Babylon. He intends to make meaningful and interesting one type of movement (toward the center) and not another (from the center), since the first type serves to show the prestige of Pharaoh, the second type does not. In tying up the recording of a "tribute" and of a military victory, or in depicting the donors as awed by Pharaoh's

strength and prestige, the author of the text hints that the donors, although they have not been defeated, are afraid of Pharaoh, and imagine themselves to be vanquished in a possible struggle. They therefore repute themselves inferior, and send gifts of homage and submission to Pharaoh as if they had been defeated. Egyptian public opinion, to which this presentation is aimed, was certainly inclined to consider it "real," since it is an "internal" public opinion, which does not have any outside independent point of comparison: it sees foreign gifts arrive, brought by messengers who kneel before Pharaoh, and cannot see the gifts brought before foreign kings by Egyptian messengers who kneel in the same fashion.

Restricting analysis to a semantic level, and concentrating it on one term only, one could say that the key-term *inw* "what is brought" is indifferent to the patterns on a denotational level; but that the texts of the type of the Annals of Thutmosi III connotate quite heavily this term, implying that what is given is from inferior to superior in rank, that it is a forced contribution, that it heightens the prestige of the receiver and diminishes the prestige of the giver, and so forth; so that our translation of the term has to be "tribute." But it would be incorrect to transfer the meaning "tribute" from the connotational to the denotational level, and more generally it would be incorrect to change the "pattern" into an "event." The text shows us that a certain movement of goods is effectively presented as "tribute" when the receiver speaks to internal public opinion; but the text does not say that it really is a tribute, and actually the donor thinks differently. As can be seen, the problem is the usual one: the text as source of knowledge of its author, and not (or previously to being) source of knowledge of narrated events.

Bibliographical Note. I add here this note, of which I must admit I am unsatisfied, and which will be unnecessary to many, only as a help for a better understanding of the background of this article. In general, on a history having man and not the past as his object, and on the related argument against *évenementielle histoire*, I follow of course M. Bloch, *Apologie pour l'histoire ou métier d'historien* (Paris 1949) and L. Febvre, *Combats pour l'histoire* (Paris 1953). On the concept of *fait total* see G. Gurvitch, *La vocation actuelle de la sociologie* (Paris 1950) and J. Cazeneuve, *Sociologie de Marcel Mauss* (Paris 1968).

Among techniques of literary analysis I bore in mind especially the modern trends of structural and semiological analysis. As a concise introduction I suggest R. Barthes, *Eléments de sémiologie* (Paris 1964) (with proposals of transferring the oppositions *langue* vs. *parole*, syntagm vs. system, denotation vs. connotation, from linguistics, where they are already well established, into other fields). A recent presentation of trends in literary analysis is M. Corti – C. Segre, *I metodi attuali della critica in Italia* (Torino 1970) (see especially the contribution by U. Eco on semiological criticism), with a large bibliography (mostly devoted to Italian criticism) to which I refer the reader. Here I quote only some contributions, e.g. N. Frye, *Anatomy of Criticism. Four Essays* (Princeton 1957); R. Barthes, *Mythologies* (Paris 1957) and *Sur Racine* (Paris 1960); C. Segre, *I segni e la critica* (Torino 1969); U. Eco, *La struttura assente* (Milano 1969) and *Le forme del contenuto* (Milano 1971); and the miscellaneous volume *L'analyse structurale du récit* (= "Communications", 8, 1966). Interesting forerunnings now again in evidence are in the writings of the Russian formalist school; see V. Erlich, *Russian Formalism* ('s-Gravenhage 1955) and

the anthology edited by T. Todorov, *Théorie de la littérature* (Paris 1965). In particular I refer to J.V. Propp, *Morfologia della fiaba* (Torino 1966) (English in "International Journal of American Linguistics", 24/4, 1958, pp. 1-134). In analysing historiographic texts of the ancient Near East it proves more useful to take as model the analysis of texts of simple structure and propagandistic flavour (political addresses, folk literature, mass media, etc.) than of modern literary works, much more refined and often more conscious in structure.

As for the "mythical" or "primitive" (both terms are today disqualified in many ways) conception of space and time, which can be traced back to L. Lévy–Bruhl, basic are of course the classical studies by E. Cassirer, *Philosophie der symbolischen Formen. II: Das mythische Denken* (Berlin 1925); but now chiefly the works of C. Lévi-Strauss, from *La pensée sauvage* (Paris 1962), on. A critical presentation with bibliography in R. Cantoni, *Il pensiero dei primitivi* (Milano 1963). This kind of approach, as is well known, has long been customary in fields related to ours: e.g. in history of religions, chiefly in the works of M. Eliade (from *Le mythe de l'éternel retour* [Paris 1949] on); and in biblical criticism (from the time of Gunkel; see more recently, for one, the presentation by B. S. Childs, *Myth and Reality in the Old Testament* [London 1960]). The relation between mythical pattern and historical event in the ancient Near East is recently treated, in rather traditional terms, by H. Cancik, *Mythische und historische Wahrheit* (Stuttgart 1970). In particular, German Egyptologists, with works such as A. Hermann, *Die ägyptische Kōnigsnovelle* (Glückstadt 1938); S. Schott, *Mythe und Geschichte* (Mainz 1954); E. Hornung, *Zur geschichtlichen Rolle der Kōnig in der 18. Dynastie*: *MDAIK*, 15 (1957), pp. 120-133; E. Otto, *Geschichtsbild und Geschichtsschreibung in Ägypten*: *WO*, III/3 (1966), pp. 161-176, have shown a constant interest in such problems, more neglected by Assyriologists. But Orientalists in general, and especially Old Testament scholars (due to the obvious theological implications of the problem), have centered their discussion on an opposition "myth vs. reality." This opposition may today be surpassed both by a greater knowledge of sub-conscious conditioning (psychoanalitical criticism may help in this direction) and chiefly by an approach of semiological analysis (in terms of author and public, message and code, denotation and connotation, propaganda and ideology, etc.).

Examples given in the above *Memorandum* are based on studies in progress. Only two of these have been completed up to the present moment: *Partire sul carro, per il deserto*: *AION*, 32 (1972), pp. 403-415 (Idrimi), and *Rib–Adda, giusto sofferente*, to be published in "Altorientalische Forschungen", 1 (1973); while a book on *Prestigio e interesse* (also on the gift/tribute problem) and a series of articles on *Storiografia politica hittita* (on Šunaššura, Telipinu, Ḫattušili III) are in preparation.

Statistics in Archaeology and Its Application to Ancient Near Eastern Data

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The increasingly widespread use of statistics as a tool in archaeological research is the motivation for this study. However all statistical models and their possible uses are not covered in this short article. Seriation techniques for purposes of relative dating are discussed and the application of statistical description and analysis to archaeological problems is outlined. Recent literature concerned with a wide variety of statistical methods including the relevance of computer technology can be found in Whallon (1972), Hodson, Kendall and Tãutu (1971), and Gardin (1970).

Vertical Patterning: Seriation

Definition and historical background

A fundamental problem frequently encountered by archaeologists is the establishment of a relative chronology for sites or groups of artifacts with no continuous stratigraphic relationship. One solution of chronological ambiguity is the isolation of distinct, important artifacts, types, or classes and the comparison of levels in which they are found in several sites or over entire regions. This results in an interconnected chronological framework of relationships (e. g. Kantor 1965; Dyson 1968; Gimbutas 1970). Among the sites some chronological evidence is given by stratigraphic position; other temporal indications come from outside sources such as texts and radiocarbon dating.

Another approach to this problem is the use of seriation, the ordering of artifacts in their presumed chronological sequence through the observance of their relative frequencies. Seriation is one of the most useful of the statistical tools dealing with chronological ambiguity. For example, to seriate sites in one region, specific categories of artifacts are chosen and their relative frequencies among the sites are computed. The assumption is that within a single geographical area sites with similar frequency distributions are contemporary (Deetz 1967: 26-30). On this basis the sites are then seriated. This technique may be used when the chronological associations are unknown and thus is applicable, for example, to material from a cemetery where the stratigraphy does not indicate the relative chronology of the graves or to sites within one region if the material is sufficiently homogeneous. The validity of seriation is even greater, obviously, if combined with stratigraphic information from at least some of the sites.

The use of seriation is not new, and in fact was first used on archaeological material from Egypt at the end of the 19th century. At that time Sir Flinders Petrie attacked the problem of chronologically ordering over 4000 graves from the cemeteries of Abadiyeh and Hu near Dendereh (Petrie 1899, 1901). Since the prehistoric sequence in Egypt was then unknown, Petrie established a chronological framework by using his own material exclusively. On the evidence of stylistic and technological differences, nine types of pottery from 900 tombs were analyzed. The results showed that certain variables interacted in a consistent fashion and from these his chronological "Sequence Dating" (S.D.) was developed. He divided the graves into fifty groups of eighteen tombs each, with each unit approximately succeeding the other in time. One sequence date, between S.D. 31-80, was assigned to each of these groups, thereby linking his artificial time to a projected population size. As in any seriation, Petrie had no way of determining from his ceramic evidence the chronological direction of the evolution of his sequence and could only confirm his assumptions through ties with Proto-Dynastic material.

Petrie's study did not take into account the problem of spacial variation among sites and regions. Thus, contrary to his expectations, its application to prehistoric sites in Lower Egypt and Nubia is unsuccessful (Massoulard 1949: 61-9). The necessity to evaluate this dimension was appreciated by Kroeber and Spier in their work in the American Southwest (Kroeber 1916; Spier 1917).

In an important study, David G. Kendall attempted to formulate the statistical problems involved in Petrie's approach. He also pointed out the contacts Petrie had with his colleague, Karl Pearson at University College, London (Kendall 1963). Pearson was interested in applying mathematical and statistical models to other disciplines. This was the first in what is by now an extensive literature by statisticians and mathematicians on the application of and theory behind seriation (Whallon 1972: 42-45). The techniques continued to be refined and tested in the first half of this century (Sterud 1967).

Techniques of seriation

A suitable statistical procedure to determine the relative correlation of types among various sites was designed by Robinson with the help of the archaeologist Brainerd (Brainerd 1951; Robinson 1951). Their approach places data, for which the spacial distribution is limited to a particular site (i.e. burial finds), or area (i.e. survey or excavated material), along a continuum of similarity. The aim, as with all seriation, is to establish relative temporal relationships.

In this method a coefficient of agreement is calculated between each pair of sites, thus giving numerical expression to the measure of difference between two sites. These coefficients are then ordered in a symmetrical matrix bringing the coefficient with the highest magnitudes along the diagonal of self agreement. To illustrate this method, we compare cylinder seals from Tchoga Zanbil found in Chapels III and IV as well as those from the Palace-hypogeum (Porada 1970). These three deposits cannot be dated from the stratigraphy so their chronological position is established by Porada on stylistic grounds (Porada 1970: 127-131). We test here her groups I, II, III, VII, XI and XIII. The total sample comprises 92 seals: 29 in Chapel III, 55 in Chapel IV and 8 in the

Palace. In the construction of the matrix, the initial step is to calculate the percentages of each group in each location.

	CH III	Ch IV	Pal
Gp I	10	20	0
Gp II	7	9	0
Gp III	21	5	0
Gp VII	31	38	12.5
Gp XI	3	13	12.5
Gp XIII	28	15	75
	100%	100%	100%

These percentages are then compared, two sites at a time, by computing the index of disagreement (ID) between them.

	Ch III	Ch IV	ID
Gp I	10	20	10
Gp II	7	9	2
Gp III	21	5	16
Gp VII	31	38	7
Gp XI	3	13	10
Gp XIII	28	15	13
			58 TOTAL ID

	Ch III	Pal	ID
Gp I	10	0	10
Gp II	7	0	7
Gp III	21	0	21
Gp VII	50	12.5	37.5
Gp XI	6	12.5	6.5
Gp XIII	44	75	31
			113 TOTAL ID

	Ch IV	Pal	ID
Gp I	20	0	20
Gp II	9	0	9
Gp III	5	0	5
Gp VII	38	12.5	25.5
Gp XI	13	12.5	.5
Gp XIII	15	75	60
			120 TOTAL ID

The coefficient of agreement is obtained by subtracting the ID from 200, which is the maximum amount of agreement to be seen between two sites. A 200 figure indicates that the sites are identical and zero would be the maximum disagreement score. In each of our examples the coefficient of agreement

is: Ch III-Ch IV:142; Ch III-Pal:87; Ch IV-Pal:80. Next, these coefficients of agreement are placed unordered in a matrix.

	Ch III	Ch IV	Pal	
Ch III	200	142	87	
Ch IV	142	200	80	
Pal	87	80	200	Diagonal of Agreement

From this simple matrix, the other possible variations can be seen quickly: Ch III-Pal-Ch IV; Pal-Ch III-Ch IV. The best matrix, shown below, is the last:

	Pal	Ch III	Ch IV
Pal	200	87	80
Ch III	87	200	142
Ch IV	80	142	200

The chronological order indicated in this matrix is either Palace-Chapel III-Chapel IV or Chapel IV-Chapel III-Palace since the chronological direction is not given by the seriation.

The disadvantages of this sample are: (1) the typology was not designed for this type of study; (2) the Palace-hypogeum had few seals. Nevertheless, the seriation confirms the opinions of both Porada and Ghirshman as to the chronological sequence: Chapel IV, Chapel III, Palace (Porada 1970: 129).

In practice, the ordering of any matrix is a long, tedious task, and computer programs have been developed which reduce the required time (Ascher and Ascher 1963; Kuzara, Mead, Dixon 1966; Hole and Shaw 1967; and current work discussed by Whallon 1972).

Another technique also aimed at finding the best seriated order using a correlation matrix was developed by Dempsey and Baumhoff (1963). Their correlation coefficient is based on whether or not a type is found at a pair of sites; thus the name commonly used is Presence-Absence method. The advantage of employing this type of coefficient is that it does not give a bias in the matrix to types which are numerically important, but less so chronologically. In this technique, each type has equal weight. Hole and Shaw found that in one of their data sets, Pa Sanger flints, this procedure was more sensitive to non-lenticular variation (1967: 64, 78). However, it should be used only with caution. Methodologically, it is less misleading to work with the actual numerical occurrence than to tabulate presence/absence. In most cases, absence takes on too great a significance; a type may not be recovered on an archaeological site only because it is scarce (Cowgill 1968).

An accurate mechanical tool, easily used in the field, is the Meighan or Three–Pole system (1959). It incorporates percentages of the three main types in any population group. The percentages of these three types are computed as if they equaled 100% of the assemblages. These are plotted on triangular coordinate paper with a straight line drawn through the points which represent an approximate ordering. Meighan's system is well suited to preliminary analysis in the field because it is quick and requires no special equipment.

Underlying concepts of seriation

The assumption basic to seriation studies is that of stylistic replacement through time and space. A type begins its popularity at one site with few examples, reaches a maximum and slowly dies out. Another form reaches its zenith while the original type is losing its vogue. During the same developmental period, the type spreads in popularity to other sites at varying rates with the furthest sites receiving the original model last. The relative popularity of the class is usually shown by computing the percentage of each type at each site and plotting this on a graph. If replacement is taking place, this appears as a lenticular curve (for replacement of pottery wares in Korucutepe, see Kelly-Buccellati n.d.).

This model was tested in an interesting study analyzing dated gravestones in Colonial New England (Dethlefsen and Deetz 1966). The styles of these gravestones from a number of cemeteries were noted; their location and date plotted on a graph. The lenticular curve was produced, confirming assumptions about successive stylistic replacement in space, time and form (Dethlefsen and Deetz 1966: 504-5). The study demonstrated the diffusion rate of each type and underscored the problem of distinguishing between the effects of time and space on seriation. Both of these factors are influenced by a phenomenon known in physics as the Doppler effects (Deetz and Dethlefsen 1964; Clarke 1968: 426-7 and 462). The action of the Doppler effect can be seen only over a number of sites. The rate of change increases above the true rate as the initial site is approached from the most distant site; it decreases at a rate substantially below the true rate going away from the initial site.

Tests of the reliability of seriation techniques appear in an important study by Hole and Shaw on excavated material from Deh Luran (1967). They reasoned that if seriation was a reliable tool, it should arrange their data in the same order as it had been found in the stratigraphy, unless some other plausible explanation was forthcoming from the data itself. Their results showed that certain of the tests duplicated the stratigraphy, others only approximated it, and the remainder were not suitable. Ceramic and stone tool data from their sites were all seriated with the same five techniques, two of which they developed. Data which did not seriate (i.e. bad data) resulted from several factors: insufficient variation during the time involved, random occurrence at the site, a temporal change which did not follow the expected lenticular pattern (Hole and Shaw 1967: 36-7). Other factors to be considered include the possibility that specialized areas or 'activity areas' were sampled, thus reflecting a functional difference rather than a chronological one. Hole precludes this from his data by stating that all his samples came from midden areas (p. 6).

Implications for Ancient Near Eastern material

Dethlefsen and Deetz also show how useful seriation can be in assessing stylistic change (1966). In terms of Ancient Near Eastern material, this means that seriation may be extended to various quantifiable sets or classes of artifacts such as stone vessels or cylinder seals. The latter are particularly suited to seriation for the following reasons:

1) the body of material is large and thus, quantifiable;
2) the basic stylistic chronology is understood;
3) a large number from some periods can be dated by reference to their inscriptions or the dated tablets on which they have been rolled; and
4) their publication tends to be fuller than other classes of artifacts because glyptic holds interest for a variety of scholars.

First, the analysis of dated seals and impressions from one city should show their precise chronological position. A second step based on these results incorporates seals dated stylistically to the same period from that city or regions immediately surrounding it. Work is already underway in developing the technical codes needed for this analysis by the Centre d'Analyse Documentaire pour l'Archéologie (Gardin 1967). Encouragement is given by the excellent results of the Colonial gravestone study which go beyond chronological and stylistic change to shed light on political and social dynamics (Dethlefsen and Deetz 1966). Other research has begun with an analysis of the co-association of symbols and materials of Minoan seals to show regional variation (Reich and Morgan 1967, 1968, 1969).

Horizontal Patterning: Non-Random Distribution

Definition and historical background

A large body of literature is now available describing the theoretical basis and practical application of statistical techniques to give a more precise answer to a variety of questions (Sackett 1966; Clarke 1968; Binford and Binford 1968; Binford 1972). These questions are partly based on the view that the combination of artifacts and their spacial distribution reflect behavioral patterns, as well as general cultural patterns. All observable data are used to reconstruct ancient societies. In addition, the horizontal and vertical distributions of the data are as important as the data itself. Statistics is a necessary tool in the assimilation of this vast amount of information.

Some of the earliest applications of statistics in archaeological investigation are found in studies focusing on two widely separated areas: Predynastic Egypt (Meyers 1950) and prehistoric America. The impetus for the latter may have come from the need to organize and interpret the assemblages, artifacts and features from the pre-World War II government sponsored Works Progress Administration (WPA) projects in archaeology. Heretofore the implication had been that inferences about the past were limited because of the absence of adequate data (Smith 1955). Yet, new explanations about American pre-history were not generated by the wealth of detail resulting from the WPA projects. Perhaps stemming from this imbalance, archaeologists were urged to shift their focus from intersite comparisons to intra-site analysis (Taylor 1948). This meant a change from descriptions of artifacts from different sites to descriptions and explanations of the co-associations of artifacts and features from a single site. The design and testing of innovative approaches stressing problem-oriented research ('new archaeology'; e.g. Watson 1972) is a current development in the continuing re-evaluation of the theory behind and methods of archaeological research.

A good example of problem-oriented research is Hill's work at a pueblo of the 12-13th C., A.D. in the American Southwest. Some of his questions revolved

around explanations of the formal variability of rooms (1968). A statistically representative sample of rooms was selected for excavation and study in order to ascertain the varying pattern of cultural remains.

On the basis of this evidence, Hill then offered predictions to explain variability and tested these inferences. His field research was designed to collect comparable data for statistical testing. The associations between and among the classes of artifacts and features were examined in order to isolate significant patterns of co-variation. For example, the following classes were tabulated: room size, floor area; presence/absence of fire pits, mealing bins (for grinding), doorways; style of masonry; sherd types per room, density of sherd per room, density per square meter on site; faunal and floral remains. Statistical evaluation of all of these paired and grouped co-associations was undertaken. Hill was able to explain the room variability in functional terms by referring to the attributes measuring these differences and to the tests of statistical inference. He further tested his explanations by reference to ethnographic evidence. His work yielded a wealth of information about past pueblo habitation.

Preliminary tabulation and description

Certain methods are basic once statistics are employed. Most researchers begin with the design of an attribute system, an organized code for tabulating and describing features or artifacts. Such a program forces the worker to examine the material, to record parameters carefully and explicitly state the criteria for each attribute. The attribute system presents what the scholar proposes as the limits of variability against which he measures the collection. There are no rules for selecting attributes; it is a matter of testing for rejection or inclusion, just as in a traditional trial sorting. But there is one overarching rule; the code must be mutually exclusive and mutually inclusive at the same time. Each set of attributes *must include* variables allowing for measurement of each and every member of the collection; but at the same time, an individual item may only be exclusively measurable by *one* of these attributes in a given set (Sackett 1966: 359-361; Elster 1971: 19-20). The attributes outlined in the system are those which are selected to best describe the collection in terms of the questions posed. For problems of prehistoric technology, one set of attributes applies; for stylistic variation, another set of variables is summarized. Once the attribute system is adopted, the data processing is explicit and objective with observations converted into numbers appearing as frequency distributions.

The techniques of statistical description generally utilize percentages of groups, industries, types, etc. based on frequency distributions. This data is summarized on bar histograms, cumulative frequency graphs or pie charts. Such aids visually present comparative differences and similarities.

Isolation of significant patterns: the Chi square test

Methods of statistical analysis may be employed if the relationship between the samples and the population from which they are derived is ambiguous. Such procedures select formulae which assist the archaeologist in evaluating and and analyzing the data. Basic to this is the Chi square test of significance, very

useful in dealing with ambiguous associations (Spaulding 1953: 305-313; Sackett 1966: 365). For example, pottery types are observed in rooms and other loci in the excavation of a site. The question then arises: are we dealing with a random distribution or with a significant non-random pattern of association which should be investigated further? The reader may have noted Chi square referred to elsewhere as the 'null hypothesis' which only means that there is no relationship between, for example, the pottery types and the locations. If an association does exist, then such patterning will yield a frequency which is much greater than may be explained by chance alone. The Chi square test contrasts the observations reported by the archaeologist (the tabulated distribution) to that which chance alone dictates. With this statistical tool, the observed frequency is measured against the expected figure (due to chance alone) and this deviation becomes the Chi square score. Its significance is easily judged by reference to any standard Chi square table found in all basic statistics books (e. g. Blalock 1960: 452).

Every acceptable Chi square score is the result of an underlying pattern of co-variation. The tests, Cramer's V or Phi both assess the strength of this association (Sackett 1966: 367).

The raison d'être of the Chi square test is the hypothesis of chance. But since the hypothesis *is* chance or randomness, sampling error distorts the results if the total sample number is low. For example, with one hundred tosses of a coin, heads or tails probably appear in a 50:50 ratio. But with only 25 tosses of a coin, the ratio does not hold. Thus Chi square testing is not as reliable with small sample numbers. However, there is a formula defined as the Yates correction (Blalock 1966: 220-221), which is applied to serve as a control for small sample. In the Korucutepe examples given below, the Chi square test procedure and the application of Phi is outlined.

Technique of Chi square testing: Korucutepe examples

A combined team from the Universities of Chicago, California and Amsterdam joined during the seasons 1968-70 to investigate the mound at Korucutepe (van Loon 1969; van Loon and Buccellati 1970; van Loon and Güterbock 1970; van Loon 1971; van Loon and Güterbock 1972). The tell is sited in the Altinova plain near Elazig in the Keban Dam area, Turkey. Occupational debris represents settlement of the Chalcolithic, Bronze Age, Hittite and Seljuk periods. Our sampling area consists of the archaeological units reported as representing Early Bronze II and III. Radio-carbon determinants place the occupation in the second half of the third millennium (van Loon and Güterbock 1970: 126). The EB II and III areas excavated are spacially separated on Korucutepe (van Loon and Güterbock 1970: 3; van Loon 1971: 60, 61; van Loon and Güterbock 1972: 128).

Preservation and recovery is such that architectural features are seen to delineate specific areas. For the EB II, the team uncovered 4 rooms or houses surrounded by yards and 1 courtyard with features generally associated with food preparation. For the EB III there were 3 rooms or houses, 1 Shrine or hall joined to a corridor with a row of hearths and associated outside areas. These areas must reflect only a fraction of the occupation and are not to be compared to the kind of sample defined in Hill's investigation (1968). Furthermore, Hill's project carried a quantitative approach since he first isolated all of the rooms in the pueblo and then chose a statistically representative sample to excavate.

The Korucutepe areas were excavated under different conditions, but the pottery is quantifiable since all sherds were collected and careful tally kept of recovery from the numerous loci either from within a room (henceforth Inside) or from without (Outside). The sample consists of: type I — black burnished; II – red burnished; III – brown burnished; IV – painted; V – cream slipped; VI – unburnished, coarse; VII – imported; VIII – red/black burnished; IX – miscellaneous [1].

An example is given of the application of statistics to this quantitative data. Our question is whether the ceramic types are found in a patterned nonrandom distribution with the various Early Bronze II and III areas on Korucutepe. The pottery counts from each area are transferred to a frequency distribution table, which then becomes the data base for all subsequent computations. In Figure 1 the sample from all EB II units separated into Inside or Outside areas is tabulated.

EB II	I	II	III	IV	VI	VII	VIII	IX	Row Totals
INSIDE	321	99	416	14	45	14	130	208	1247
OUTSIDE	2692	265	1395	36	188	59	933	300	5868
(Column Totals)	3013	364	1811	50	233	73	1063	508	7115 (N)

EB III	I	II	III	IV	V	VI	VII	VIII	IX	Row Totals
INSIDE	164	32	253	18	165	212	3	101	25	973
OUTSIDE	103	45	149	5	54	188		130	5	679
(Column Totals)	267	77	402	23	219	400	3	231	30	1652 (N)

Fig. 1: Distribution Table of Pottery Types for EB II and EB III

A visual statistical description of these observations is reproduced in bar-histogram form in Fig. 2.

The question as to whether these frequencies represent random patterning or significant co-associations is taken up by means of the Chi square test. All of the computations may be performed manually on a desk calculator. In this case, use is made of the Olivetti Programma 101. The 101 is a mini-computer using the specific Programma language. The program for Chi square, Yates' correction and Phi (to measure the magnitude of association) was developed by Harold Kushner. The Olivetti 101 reduced many hours from the time required to compare ware types from area to area and determine the significance of co-association.

Fig. 1 exhibits the observed pattern of co-variation of all EB II Inside or Outside areas with ware types. In Fig. 3, comparison is made of what is *observed* (O column) against what would be *expected* (e column) on the basis of randomness or chance using the Chi square statistic; R stands for Row Total.

This contingency table shows the addition of the *expected* frequencies. Computation of the expected frequency (e) for each pair utilizes the formula:

[1] (The sample used in this study incorporates only those sherds found in association with rooms, thus the totals in this article differ from those in Kelly-Buccellati n. d.)

$$e = \frac{C \times R}{N}$$

(Blalock 1960: 215, 216). Here the border totals (from Fig. 1) of columns (C), e.g. 3013, and rows (R), e.g. 1247, are used while 'N' refers to total number in sample, in our case 7115. The observed frequency (the "O" column) and the expected frequency (the 'e' column) are contrasted in computing the Chi square score using the formula:

$$\chi^2 = \frac{(O - e)^2}{e}$$

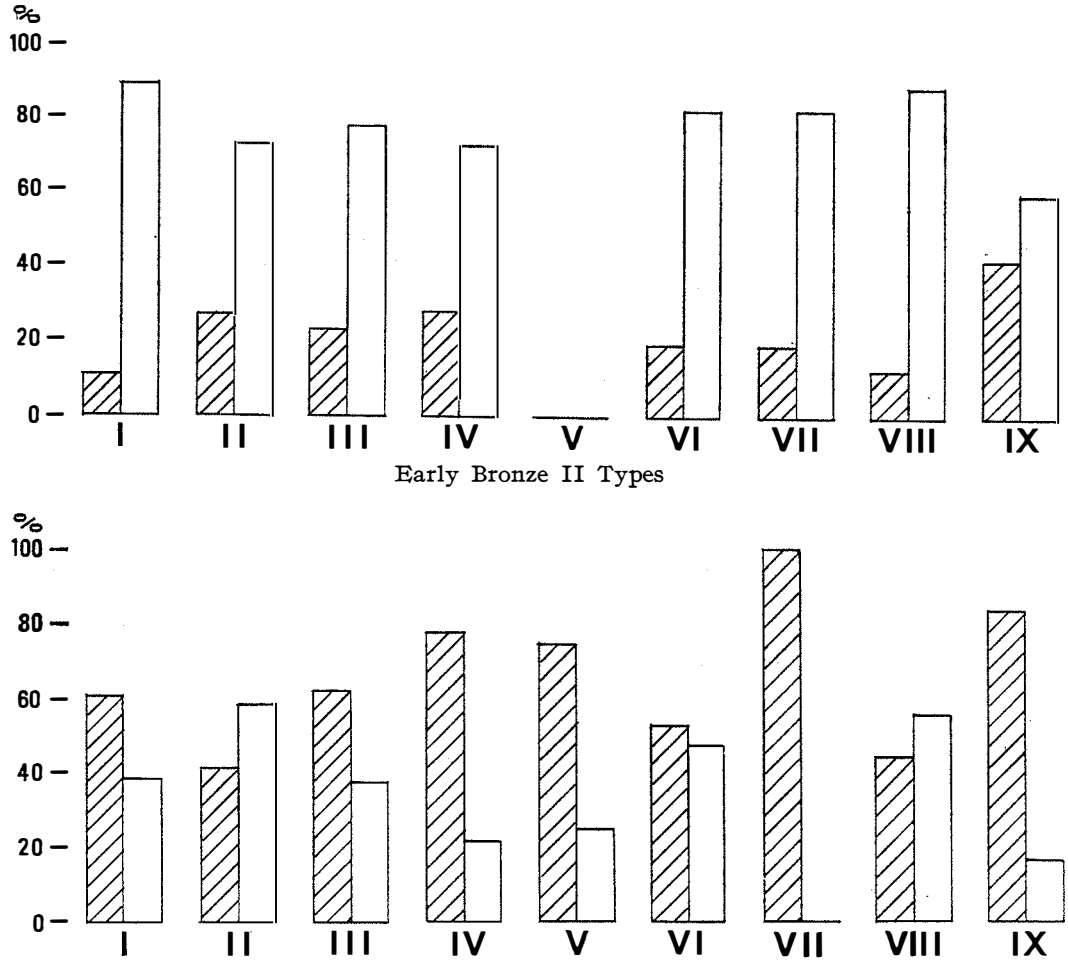

Fig. 2: Histograms of Korucutepe Pottery Ware Types

		O	C	R	e	χ^2
I	I	321	3013	1247	528.07	81.19
N	II	99	364	1247	63.79	19.43
S	III	416	1811	1247	317.40	30.62
I	IV	14	50	1247	8.76	3.13
D	VI	45	233	1247	40.83	.42
E	VII	14	73	1247	12.79	.11
	VIII	130	1063	1247	186.30	17.01
	IX	208	508	1247	89.03	158.97
O	I	2692	3013	5868	2484.93	17.25
U	II	265	364	5868	300.20	4.12
T	III	1395	1811	5868	1493.58	6.50
S	IV	36	50	5868	41.23	.66
I	VI	188	233	5868	192.16	.09
D	VII	59	73	5868	60.20	.02
E	VIII	933	1063	5868	876.69	3.61
	IX	300	508	5868	418.96	33.71

N = 7115 χ^2 = 376.90

Fig. 3: Contingency Table General Test I, EB II

(Sackett 1966: 367). Following the formula, the difference between observed frequency and expected frequency is squared, then divided by the expected frequency, giving the Chi square value for that pair. The sum of the individual Chi square scores for each contrasted pair results in the total Chi square value for the test (in our case, 376.90).

The significance of the total Chi square value of Test I, 376.90 is scored by reference to a standard table of Chi square values (e.g. Blalock 1960). At this point the 'degrees of freedom' (df) for the distribution table (Fig. 1) are required as an index to the Chi square score. The formula is: df = (C-1) × (R - 1), or degrees of freedom equals total number of columns (here 8) minus 1, times total number of rows (here 2) minus 1, the total here being 7 df. This indicates that if expected frequencies for 7 cells in the distribution table (Figure 1) are known, the 9 remaining may be established by substraction. This rule applies to any size distribution table.

Test I, for the EB II, scores at the .001 'confidence level' which means there is but one chance in a thousand that the association of ware types and locations is due to pure chance. In other words, we have been questioning whether the distribution of pottery is random or non-random and with a level at .001 it can be confidently stated that non-randomness has been demonstrated.

If the score were significant at the .01 confidence level, there would be 10 chances in a thousand (or one in a hundred) that this distribution is due to chance alone. The confidence level considered significant is an arbitrary decision (Sackett 1966: 376); in this study both the general and restricted tests have a confidence level of over one percent.

Examination of Fig. 3 indicates which types and associations contribute most to the Chi square score. These are summarized in descending order of their contribution to the total Chi square score in Fig. 4. Pairs below the dotted line represent observed and expected frequencies so close as to be virtually random.

If observed frequency is much less than expected, a negative-inflated Chi square score may be the result (see Fig. 3, type I/Inside). Such pairs are

not referred to in our summary, although their scores add to the total. As a control, inflated scores are added together, then subtracted from the Chi square total. This new total is compared to a Chi square table of values to see whether the confidence level is acceptable and therefore the test results. Of course the absence or low frequency of certain types may have archaeological significance and should be examined.

The continuity requirements for Chi square testing are (1) none of the expected frequencies may fall below 1.0; and (2) no more than 20% of the expected frequencies may fall below 5.0 (Sackett 1966: 369). If the requirements are not fulfilled, the test may not be reliable. However, if examination shows no inflation (of total Chi square score), results are probably acceptable. As the worker experiments with the Chi square statistic, many of these points fall into place.

χ^2 .001 ϕ .23	χ^2 .001 ϕ .21
IX : Inside	VIII: Outside
III : Inside	V : Inside
II : Inside	II : Outside
I : Outside	VI : Outside
VIII: Outside	IX : Inside
IV : Inside	IV : Inside
. . . . .	III : Inside
VI : Inside (very low score)	. . . . .
VII : Inside (very low score)	VII : Inside (very low score)
V : Not present	I : Inside (very low score)
General Test I	General Test II
Early Bronze II	Early Bronze III

Fig. 4: Rank Order of Chi Square Scores and Significant Associations

Phi measures the association between variables such as ware types and locations, but the Phi value is not the measure of the strength of just one pair (for example, type II: Inside). Rather, Phi assesses all the underlying paired co-associations which contribute to the Chi square score. The formula is:

$$\phi = \sqrt{\frac{\chi^2}{N}}$$

Scores are scaled from 0 to 1. A series of Phi scores is useful for comparing the strenght of association from a group of tests. In addition, Phi controls for sample size because, in the formula, the Chi square score is divided by the number (N) in the sample; thus Phi scores may be compared although they derive from Chi square tests on populations of various sizes. Chi square, on the other hand, is sensitive to sample number and may yield higher Chi square scores with larger populations.

Contrast between descriptive and analytic statistics

A graphic example of the difference between descriptive statistics and analytic statistics appears with the comparison of Fig. 2 to Fig. 4. Both deal

with the same data but the bar histograms of Fig. 2 describe precisely what percentage of each pottery type is recovered from Inside or Outside locations. The greater recovery of a ware type from a particular location does not necessarily predict a significant co-association. For example, types II, III, IV, IX hold higher percentages from EB II Outside locations (Fig. 2); yet the statistically significant non-random association of these types is with Inside areas (see Fig. 4). A parallel situation can be seen for the EB III with type VI and the Inside. Fig. 4 summarizes the significant co-associations of types and locations (in rank order) as isolated in the Chi square test in Fig. 3. The order of these co-associations could not have been inferred from the descriptive bar histograms of Fig. 2.

The patterns demonstrated are clearly seen on the following chart, Fig. 5, rearranged from Fig. 4 to point up the parallels. The figure in parenthesis to the left of the type numeral refers to the total percentage of this type in the population (from *all* loci).

EB II	EB III
Inside	Inside
(.26) III	(.24) III
(.07) IX	(.02) IX
(.001) IV	(.01) IV
(.05) II	(.13) V
Outside	Outside
(.42) I	(.14) VIII
(.15) VIII	(.05) II
	(.24) VI
Random	Random
(.03) VI	(.001) VII
(.01) VII	(.16) I
Not Present	
V	

Fig. 5: Inside-Outside Type Associations for EB II and III

To summarize from EB II to III:

1. No change is demonstrated in location and very little in percentage for types III, IV, VIII, IX.

2. The standard EB II household wares, III, IV, and IX continue into the EB III period. Likewise type VIII maintains its relative percentage and association with Outside areas through time.

3. Type V enters the inventory of houses with EB III. This development is explained as a function of time.

4. Changes seen for both common and uncommon types:

a) Type I loses in percentage through time (.42 in EB II; .16 in EB III) and changes association from Outside to Random.

b) Type II (.05) changes association from Inside to Outside but maintains degree of percentage in both populations.

c) Type VI (.03), randomly distributed in EB II, increases eight fold (.24) in the EB III population and is found in association with Outside areas.

Restricted Chi square tests

With these associations demonstrated, two restricted tests are presented (for EB II and III) which compare the incidence of ware types from specific Inside and Outside locations. The expectation is that the associations isolated in the general tests will duplicate in the specific tests comparing a single room or a few rooms to a related outside area. Recovery from EB II Rooms 1, 2, 3, 5 is compared with that from the Courtyard. For the EB III, wares collected from Room 15 are contrasted with those from the area outside. Test summaries are seen in Fig. 6; the calculations are omitted.

χ^2 .001 ϕ .32	χ^2 .001 ϕ .37
Rms 1, 2, 3, 5 to Courtyard	Room 15 to area Outside
I : Courtyard	II : Outside
IX : Rooms	VI : Outside
III : Rooms	V : Room
II : Rooms	III : Room
VII : Rooms	IX : Room
VI : Rooms (very low score)	I : Outside (very low score)
IV : Rooms (very low score)	VIII: Outside (very low score)
	VII : Room (very low score)
Test III, EB II	Test IV, EB III

Fig. 6: Restricted Tests III and IV in Rank Order of Chi Square Score and Significant Associations (Rooms 1-3,5 are in squares 012, N11, N12, 016 and 017; the courtyard is in N11, 12; Room 15 is in O14 and P14).

Comparison of Fig. 6 to Fig. 4 shows fair duplication of General Tests with the following points to note:

1. In Test III type VII has a significant co-association with the Rooms, whereas in Test I the Chi square score for this pair is very low. Thus Test III adds needed support to this type-association.

2. The anomaly in Test IV is Type I which again scores very low but this time with Outside. Since Type I scores low on both Test II and IV we are inclined to see the random distribution of this type as a function of sample size.

To summarize: the distribution of certain pottery types in room-houses and outside areas of Early Bronze II and III Korucutepe is non-random. The ambiguous pattern has been clarified using a method of statistical analysis. The question of patterning cannot be completely explained but is carried as far as the data allows.

Conclusions

All archaeologists are concerned with the co-associations exposed through survey and excavation. Here we have tried to show how statistical techniques can clarify ambiguous patterning: both vertical and horizontal. The inferences thus generated are not necessarily different from those reached without this tool. In fact, statistics does not always show something new nor does it use different logic. Still, an inference thus derived has an added dimension which is

not negligible: it is the addition of a precise and explicit quality to those inferences.

The insistence upon explicitness and explanation is underscored as the discipline of archaeology broadens to include the aims and methodologies of the natural sciences (Adams 1968; Hammond 1971; Watson, LeBlanc and Redman 1971). Not all archaeologists will agree with this diversification in the process and practice of archaeology (Hawkes 1968). But a report on the quest for an expanded relevance for archaeology is germane to the central concern of this volume. Scholars will want to examine these expanded goals and their bearing upon the manner and means of excavating in the Ancient Near East.

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Un modellino votivo di Malta

(TAB. I-II)

S. MOSCATI – Roma

Nel corso degli scavi da noi effettuati nell'area sacra fenicia di Tas Silg, a Malta, è venuto alla luce un piccolo monumento, unico nel suo genere e di spiccato interesse per la sua connessione a modelli orientali finora inedito. Offro la sua pubblicazione in modesto omaggio all'insigne studioso che si onora con questa raccolta di scritti, nel ricordo dell'eccezionale contributo da lui recato alla conoscenza degli apporti orientali alla civiltà dell'Occidente.

Campagna di scavi 1970, inv. n. 2475.
Tas Silg, area nord, trincea di depredazione del muro M 31.
Calcare tenero biancastro. Tracce di colore rosso sull'architrave.
Alt. mass. cm 10, min. cm 8; largh. cm 8,5; spess. cm 6,6.
Molte scheggiature nella parte inferiore e nelle due fasce superiori del sopraspecchio. Piano di base mal conservato.

Il piccolo monumento è lavorato sulla faccia anteriore e su quelle laterali; la faccia posteriore è liscia e reca un foro non passante profondo cm 1 a circa cm 3 dal piano di base. La parte posteriore del coronamento ha andamento di poco discendente e convesso. La faccia anteriore presenta a rilievo un'edicola, fiancheggiata da due pilastri rettangolari che sostengono un architrave con gola egizia; al centro della gola è un disco solare aptero fiancheggiato da urei (?); il sopraspecchio ha tre fasce orizzontali a sezione convessa. Nell'interno dell'edicola è rappresentata a rilievo una figura umana schematizzata. Le due facce laterali recano solchi orizzontali che suggeriscono filari di blocchi.

Sembra evidente che il piccolo monumento fin qui descritto sia il modellino di un sacello. Della costruzione architettonica che riproduce, esso conserva appieno la tridimensionalità (è infatti quasi cubico) e alcuni particolari costruttivi sulla faccia anteriore e su quelle laterali, mentre la faccia posteriore liscia e il foro che vi si trova indicano che l'oggetto era verosimilmente applicato a una parete con un piccolo chiodo, in funzione votiva.

L'impiego di modellini di sacelli come *ex-voto* è ben noto nel Vicino Oriente [1]. In modo specifico, la tipologia si richiama a quella del sacello cubico nota in Fenicia da Amrit e Ain el-Hayat [2]. Nell'ambito della tipologia, il sacello è chiaramente a una sola faccia architettonica, registrando le fiancate solo i filari

[1] Cf., per un esempio assai vicino al nostro e di ambiente fenicio, il modellino da Cipro in D. Harden, *The Phoenicians* (London 1962), fig. 23.
[2] Cf. da ultimo, dello scrivente, *I Fenici e Cartagine* (Torino 1972), pp. 176-177.

dei blocchi. La pendenza del coronamento verso la parte posteriore trova riscontro nel noto frontoncino di Nora [3] e viene riprodotta abitualmente nelle stele [4]. L'unico vano che si apre sulla fronte è occupato dal simulacro divino.

Il modellino, per quanto mi risulta, è unico nel mondo fenicio d'Occidente. Esso è anche notevole per l'uso della pietra rispetto a quello, più comune in oggetti del genere, della terracotta. La provenienza dal riempimento di una trincea di depredazione moderna non consente una datazione su basi archeologiche. La tipologia e l'iconografia non indicano, di per sé, elementi posteriori al V secolo; occorre però ricordare che a Tas Silg l'uso di elementi architettonici egittizzanti è documentato fino alla tarda età ellenistica.

I vari dati mostrano che il modellino non deve intendersi come una rappresentazione templare « compendiaria », bensì come l'immagine di una singola cappella o sacello nell'ambito di un'area sacra. Cappelle del genere dovevano esistere a Tas Silg nei cortili antistanti al grande santuario, almeno a giudicare da alcuni basamenti conservati nella pavimentazione del cortile 20[5] e da vari frammenti di gole egizie di medie dimensioni [6] (mentre per altri molto grandi occorre pensare alla collocazione in strutture di maggior mole [7]).

È comprensibile, del resto, che il modellino votivo si riferisca a tali cappelle piuttosto che all'insieme del santuario di Tas Silg. Questo, infatti, utilizzava le strutture di un grande tempio preistorico esistente sul luogo, del tutto diverse dall'architettura fenicia di tipo egittizzante. Taluni adattamenti, che pure vi furono [8], non dovettero mutare di molto il generale aspetto del santuario, né abituale né rappresentativo per le genti fenicie. Queste, dunque, ricorsero volentieri alla dedica di sacelli; e da tali sacelli, pienamente aderenti alla loro tradizione orientale, trassero il presente modellino, che destinarono a *ex-voto* e che gli scavi hanno riportato alla luce.

[3] G. Pesce, *Nora. Guida agli scavi* (Cagliari 1972), pp. 96-98, fig. 82.

[4] S. Moscati – M. L. Uberti, *Le stele puniche di Nora nel Museo Nazionale di Cagliari* (Roma 1970), pp. 26-27.

[5] A. Ciasca, in *Missione archeologica italiana a Malta. Rapporto preliminare della Campagna 1967* (Roma 1968), p. 25, fig. 1; tavv. 1,1; 6,1. Non è tuttavia escluso che si tratti di basamenti di altari.

[6] A. Ciasca, in *Missione archeologica italiana a Malta. Rapporto preliminare della Campagna 1966* (Roma 1967), p. 33, tav. 21,2; Ead., in *Missione archeologica italiana a Malta. Rapporto preliminare della Campagna 1968* (Roma 1969), p. 45, tav. 4,2.

[7] F. D'Andria, in *Missione archeologica italiana a Malta. Rapporto preliminare della Campagna 1970*, in stampa, tavv. 19,1; 20,1.

[8] A. Ciasca, in *Ricerche puniche nel Mediterraneo centrale. Relazioni del colloquio in Roma, 5-7 maggio 1969* (Roma 1970), pp. 96-98.

The Domestication of Plants and Animals in Europe and the Near East

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Ever since V. Gordon Childe termed that particular part of prehistory when food-production started the "Neolithic Revolution" the interest of many archaeologists has centered on the causes of this revolution and the time it began [1]. With the advent of age dating techniques, such as radiocarbon, one became increasingly aware of the fact that the Neolithic had started much earlier than heretofore assumed. Overwhelming evidence found in numerous sites in the Near East convinced early investigators that the origin of agriculture and domestication of animals began in the lower hills and adjacent plains of the Zagros mountains. Here wolf, goat, sheep, pig, and cattle existed as native animals. Indeed, all of them occur in their wild forms in the lowest levels of many Near Eastern sites. It was for this reason that these species were thought to be the earliest domesticants in the Neolithic. When several radiocarbon dates on charcoal from the lower levels of these sites were run they placed the time of domestication for some animal species to around 6500 B.C. expressed in conventional radiocarbon years. Understandably, all of these early charcoal dates were then applied to stratigraphically associated bones of such domesticates as goat, sheep, pig, cattle, as well as to some of the cultivated cereals. For comparison, the oldest non-Near Eastern Neolithic charcoal date comes from a ceramic level at the Argissa-Magula in Greek Thessaly (5500 B.C., conventional radiocarbon years) [2]. Here was an obvious indication that this particular site was actually much older, since it contained several aceramic levels with domesticated animal bones below. Apart from the Argissa-Magula other contemporary sites are known in the same area such as Nea Nikomedeia [3].

A second important consideration for this study was mounting evidence from a variety of sources showing that conventional radiocarbon dates need to be calibrated by tree-ring chronologies in order to give dates which correspond to Julian years normally used in history. When such an adjustment is made for the previously mentioned conventional radiocarbon dates of 6500 and 5500 B.C. they actually need to be placed roughly one millennium earlier. This development in itself is of great importance, as it presents a new perspective in Neolithic research. In the following report, except where specifically noted, all radiometric ages are stated as tree-ring calibrated radiocarbon dates synonymous with Julian years.

[1] V. G. Childe, *The Dawn of European Civilization* (New York 1925).
[2] "Groningen Radiocarbon Dates VII", *Radiocarbon* 9, 129 (1967).
[3] R. J. Rodden, *Sci. Amer.* 212, 82 (1965).

Another improvement made by this study concerns the elimination of some archaeologic uncertainties. Even though previous Near Eastern charcoal dates suggest when and where some species were first domesticated, they may not yield the best answers. This is due to the fact that dating by association can be fraught with danger since contemporaneity is not always assured. Therefore, it is inherently better to date specific artifacts or bones directly as was done in this study.

Domestication of Plants

Until recently, most specialists maintained that the domestication of plants and animals occurred simultaneously yet newer evidence seems to support the theory that plant domestication predates animal domestication. This view, however, is solely based on recent radiocarbon dates of domesticated plants from Southeastern Asia predating the earliest domesticated animal dates from Europe and the Near East by roughly 1200 years.

In the dating of domesticated plants several factors must be taken into consideration which could change our view completely. The main point is the mode of preservation of different plant species. Usually their identification is more difficult than that of bones unless they are macrofossils. On the other hand plant remains, especially pollen grains, may offer important clues to past climatic conditions. In general mostly two types or groups of domesticated plants are encountered, vegetatively reproducing species and seed plants. Preservation of either group depends often on favorable environmental circumstances.

The arid Near East was probably one of the most advantageous locations for the preservation of some of the earliest cultivated plants. They occur in this area together with the earliest domesticated animals in the lowest levels of many sites. Besides the arid environment favorable for their preservation, most of the "noble grasses", the wild ancestors of cultivated cereals, were confined to this particular area[4]. Even where no actual remains of these cultivated cereals were found, imprints of them in building materials in the earliest levels of most sites made it possible for archaeologists and botanists to identify them by species[5]. First of all there are the three groups of wheat: the diploid group with einkorn, the tetraploid group with emmer and hard wheat, and the hexaploid group with bread-wheat, club-wheat, and spelt. Whereas the origin of some plant species like spelt is obscure, other domestic species are thought to have arisen from chromosome aberration. Einkorn thus originated from *Triticum aegilopoides*, emmer from *Triticum dicoccoides* and cultivated barleys from *Hordeum spontaneum*. Of these emmer is also found in Europe in a small area of southern Greece. Millet also occurs wild in Europe and the Near East and is divided into two cultivated species-broomcorn millet (*Panicum miliaceum*) and Italian millet (*Seteria italica*) whose wild ancestor *Seteria viridis* is specifically found in southern Europe. Another important cultivated plant is flax which is derived from *Linum bienne* found wild only in the Near East.

[4] J. Murray, *The First European Agriculture*, (Edinburgh 1970), p. 6.
[5] H. Haelbaek, in *Science in Archaeology*, D. Brothwell and E. Higgs, eds., (New York 1970), p. 212.

Moreover, various legumes were found wild in the Near East and also in Europe consisting mainly of peas, lentils, vetch and beans.

The earliest of these plants except for the legumes, left clearly identifiable remnants in sites like Jericho, Jarmo, Ali-Kosh, Nimrud, Babylon, Ur, Warka and others dating to *ca.* 8000 B.C. based on radiocarbon dates of associated domesticated animal bones. Consequently, it was believed that they were also the earliest domesticated plant species.

It is now important and crucial to look at the environment of the Near East which is so favorable for the preservation of plants and animal remains.

Since the wild ancestors of cultivated cereals can still be found in the uplands of Mesopotamia, most specialists thought that the low-lying adjoining river plain would be the natural area for irrigation agriculture of these plants, ultimately resulting in their dispersal from this domestication center into Europe and other adjacent areas.

Present evidence supports the idea of the Near East being the center for the domestication of cereals, but it is not necessarily true for all domesticated plants. Even in the favorable arid environment of the Near East legumes did not survive except for a few exceptions. In fact, Sauer suggested in 1952 that not only were leguminous plants the first domesticated, but also pointed to Southeast Asia as a possible area of origin [6]. His theory appears to have been verified by recent evidence from North Thailand [7] where various domesticated leguminous plants were found in Spirit Cave and dated to around 9200 B.C.

Besides several types of nuts, bottle gourd (Areca), a cucumber type (Cucumis), chinese water chestnut (Trapa), leguminous beans (Phaseolus, Vicia) and the pea (Pisum) were found in its lowest layer indicating a post-food-gathering stage.

The date from the lowest level predates Asiab in the Near East by approximately 1200 years. Until earlier remains of cereal plants are found, the domestication of plants began with the growing of vegetatively reproducing species. It remains to be seen whether similar sites in this geographical area could possibly yield cultivated cereal crops and dates equal to those of the leguminous plants found in Spirit Cave. Yet it is quite likely that different domestication centers apply to different plant species. Since vegeculture seems to be more prevalent in humid climates its origin in early Neolithic times is likely to be found there, while cereal crops originated in regions commensurate with the native habitat of their wild predecessors. It is quite clear, however, that the total history of plant domestication at this point in time remains far from complete.

Sampling Considerations for Faunal Remains [8]

Prior to 1964 several attempts had been made to date bones directly, using their carbonate content but were soon after abandoned, since most dates either proved to be too recent or too old and could not be fitted into the previously established sequence of events based on charcoal dates. The reason for these

[6] C. O. Sauer, *Agricultural Origins and Dispersals*, (New York 1952)

[7] C. F. Gorman, *Science*, 163, No. 3868 (1969) 671-673.

[8] The data used in this portion on animal domestication will also be discussed in a forthcoming article in *Science*.

erroneous dates was contamination by carbonate exchange due to ground water. In 1964 Berger, Horney, and Libby published a method for the proper dating of bones from their organic portion - collagen [9]. Even though this collagen method has been improved since then, it had not been applied so far to bones from morphologically recognized domesticated animals of the lowest levels of some of the earliest Neolithic sites in Europe and the Near East [10]. It was the object of this study to investigate by reliable methods the origins of domestication of several animal species in these geographic areas. (Table I).

The present project started with the dating of bones from the aceramic levels of the Argissa-Magula in Greek Thessaly which contained several domesticated species such as goat, sheep, cattle, and pig [11]. The project was soon extended to include several Russian sites where domesticated horse was abundant. Finally some sites in Yugoslavia and one site in Southern Germany were added. When material from some Near Eastern sites became available for dating it opened up the possibility for a comparison of earliest domesticated bone material of that area and Southeastern Europe. Based on the dates of this study some ideas can be obtained as to the distribution of earliest domesticated animals. But it is emphasized, that the data are not complete and simply provide a good indication as to the existence of possible nuclear areas for domestication. An ultimately exact picture can only be presented once all of the Neolithic sites in Europe and the Near East, as well as the other " nuclear" areas and their domesticated bone material have been dated.

In recent years dating of charcoal from early Near Eastern sites has become one of the prime targets of archaeologists and dating specialists. Many of the dates in the stratigraphy of some sites showed quite a regular sequence in age. But, if for example, one examined the dates from the same stratigraphic levels in sites like Jarmo, Ali Kosh, and Jericho one can easily understand the criticism of some towards radiocarbon dating and its accuracy. The dates from Jarmo, many determined in the early days of radiocarbon dating, differ for example, in the same stratigraphic level by as much as three thousand years [12].

It has been suggested that such differences might be rooted in bitumen contamination of charcoal [13]. Apart from natural contamination another more archaeological problem should be considered and caution the specialist trying to apply C^{14} dates based on charcoal to stratigraphically associated bones of domesticates. The situation can arise that there may be a discrepancy in age between separately and accurately measured charcoal and bones taken from the same level. The physical nature of bone (weight and compactness) allows it to become intrusive into lower stratigraphic levels. Vertically positioned long bones may be penetrating several excavation units and thus extend over a considerable length of time. The location of other heavy objects above bones may drive them also into lower levels. It must be assumed then that bone

[9] R. Berger, A. G. Horney, W. F. Libby, *Science*, 144, 999 (1964).

[10] R. Protsch, *Anthropology* UCLA, 2, No. 1, (1970).

[11] V. Milojcic, J. Boessneck, H. Hopf, *Die deutschen Ausgrabungen auf der Argissa-Magula in Thessalien*, (Bonn, 1962), 1; R. Protsch, *Radiocarbon Dates for Some of the Earliest Domesticated Animals in Europe*. Thesis for the Degree of Master of Arts, Dept. of Anthropology, UCLA (1970).

[12] R. Braidwood, personal communication, T. Waterbolk, Intl. Congress Pre-and Protohistoric Sciences, Belgrade, 1971.

[13] Ibid.

found in sites occupied continuously and densely over long periods of time would be prone to greater intrusive effects.

Contrary to bone, charcoal crushes easily and does not intrude readily into lower levels once deposited, except for rodent and root holes. Thus, most charcoal remains more so than bone can be assumed to represent actual locations of deposition. Consequently, the assumptions of early excavators who attempted to date some of the earliest domesticated species must be evaluated with care since they posited that the age of bone in the same stratigraphic level must be equal to that of the charcoal. In this study, to avoid stratigraphic errors, it was decided to date bones directly.

However, bones were often artificially contaminated due to the abundant use of preservatives. All preservatives are composed of organic compounds and if not removed would yield erroneous dates. The few bones which were saved from early excavations were thus often useless since they were usually totally covered with preservatives difficult to remove completely. Thus they could not be subjected with confidence to radiocarbon dating. Moreover, since frequently most bones from early excavations were thrown away, investigators had unknowingly disposed of the only available material for dating, given that environmental conditions in many sites did not allow for the preservation of any other organic material for dating.

With these considerations in mind, the collection of the following UCLA samples was made under certain stringent conditions. All bone selected was positively identified as fully domesticated on morphological grounds with the exception of some sheep bones and handled in such a way that no contaminants would affect the dating. All samples came directly from the excavator or bone specialist and positively from the lowest level of the site, or the lowest level containing morphologically-defined domesticated species, except in those sites where the total stratigraphy or several levels were dated to check the accuracy of dates in a stratigraphic sequence.

A total of thirty-five bone samples collected for this study came from nineteen sites and six geographically different areas: the Ukraine, Greek Thessaly, Bavaria, Yugoslavia, the Near East, and Saharan Niger. Not all of the sites contained domesticated animal bones, but they were added to the number of sites selected, because they seem to be important in solving questions such as (1) when the terminal phase of a full hunting and gathering economy occurred or for that matter (2) if there was a possible transition stage from the latter to domestication often called " incipient domestication". The species collected for dating included goat, sheep, horse, and cattle, indirectly also dating pig and dog.

Dating Procedures

Prior to the processing of the bones for C^{14} dating microanalytical tests were made for an assessment of the organic carbon present in the bones [14]. In addition the fluorine and nitrogen content was used for relative placement as suggested by Oakley [15]. Such tests were mainly devised for the purpose of checking the age-relation between bones and the deposit in which they were found, or to which

[14] Cf. n. 4.
[15] K. P. Oakley, *Adv. of Science*, II, 3 (1965).

they were attributed. The relative age of the bone in question is usually determined by comparing its chemical composition (F, U, N) with that of other fossils found in the same stratum (UCLA-1714A/sheep, goat, dog; UCLA-1657D/cattle, pig; UCLA-1714E/sheep, goat, onager; UCLA-1657G/deer, horse) and which appeared to have been preserved under similar conditions.

All bones were mandatorily treated for contamination such as preservatives, because some archaeologists who had collected several samples many years ago could not recall whether or not they had used preservatives. This included the removal of 0.5 mm of primary bone by scraping and treatment for several days in a solution of warm distilled water and acetone. Further processing included treatment of HCl in order to dissolve the inorganic portion while retaining the organic portion. In order to eliminate numerous pollutants and to obtain purer collagen the latter was converted in all samples to gelatine [16]. Those samples however, which had come from sites and stratigraphic locations like Jarmo, where previous C^{14} dates on charcoal had yielded spurious and confusing results were processed differently. They were treated according to a liquid-chromatography method for the separation of amino acids with the same specific activity in radiocarbon from bones impregnated with isotopically dead petroleum compounds (bitumen) [17]. Indeed, for Jarmo collagen dates provide an internally consistent chronology superior to charcoal derived dates. The results obtained have been listed in Table 2. It is suggested here to always process bones from sites like Jarmo, Jericho or Ali Kosh by this method, since they are reported to yield bitumen in their inventories.

After processing, all samples were counted in a 7.5 l proportional counter as CO_2 at 1 atm and analysed for at least 1000 minutes to a statistical accuracy of a one sigma standard deviation [18]. The radiocarbon age of all samples was calculated on the basis on a C^{14} half-life of 5568 ± 30 years. These dates were then converted to bristlecone-pine calibrated ages taken to correspond to Julian years used by historians using the relationship discussedby Suess which shows fluctuations of C^{14} graphically over the past millennia [19].

Animal Domestication Dates

Bos primigenius, the wild ancestor of domesticated cattle, *Bos taurus*, was widely distributed in the Near East and Europe. Since identification of both forms of cattle by most specialists was mainly based on size differences, the domesticated cattle in the Near East were usually identified as those being of smaller stature. Thus, wherever small cattle appeared in the archaeological context in the Near East it was simply regarded as domesticated cattle. Soon specialists became aware that this domesticated form was a possible small wild form of cattle, some even suggesting the possibility of sexual dimorphism. Thus the cattle found at the site of Jarmo, which are rather small, are identified by

[16] R. Longin, *Nature* 230, 241 (1971).
[17] T. Y. Ho, L. F. Marcus, R. Berger, *Science* 164, 1051 (1969).
[18] R. Berger, W. F. Libby, "UCLA-Radiocarbon Dates IX", in *Radiocarbon* 11, No. 1, 194 (1969).
[19] H. E. Suess, "Bristlecone-pine Calibration of the Radiocarbon Time-Scale 5200 B.C. to the Present", in *Nobel Symposium* 12, 202 (1970).

Stampfli as wild [20]. The cattle in other early Neolithic sites like Tepe Sabz in Iranian Khuzistan or Banahilk in Northern Iraq are claimed to be fully domesticated [21]. If their identification as domesticated cattle is true, only direct dates on their bone could determine their exact chronological position, Until such time the European dates presented here on domesticated cattle are the earliest anywhere in the Neolithic. The very early date of 7000 B.C. (UCLA-1657D) at the Argissa-Magula in Greek Thessaly is also a date for the preceramic layers of this site. The dates of these preceramic levels of the Argissa-Magula should not differ much from another site with preceramic levels in this area, Nea Nikomedeia. Further north the site of Obre I supplied a date of 6300 B.C. (UCLA-1605I) from level 12, which is underlain by two more levels also containing domesticated cattle, still to be dated. Judging from the thickness of the underlying stratigraphic layers it is possible to suggest an earliest date in this location of about 6500 B.C. Consequently earliest domesticated cattle could have existed further west in Central Europe perhaps some thousand years later if diffusion of cattle domestication spread from Greece through Yugoslavia into Central Europe and then at a later stage into Russia with perhaps completely independent development in the Near East. Or were all of the above-mentioned locations independent from each other and developed cattle domestication by themselves?

It appears that domestication of cattle was a very late occurrence in Russia. The site of Luka-Vrublevetskaja shows dates from 4210-3950 B.C. on domesticated cattle while other sites like Novo-Rozanovka II still contain wild cattle a few hundred years later around 3700 B.C. [22]. We also know by looking at the date of Ambrosievka that the European bison still existed in this area around 6300 B.C. [23]. On the continent of Africa a quite early date of 3750 B.C. (UCLA-1685) was measured for domesticated cattle at J. D. Clark's site at Adrar Bous in Saharan Niger [24]. If other sites are found on the northern fringes of the African continent with dates intermediate from 7000 B.C. to 3750 B.C., perhaps cattle domestication radiated out from Greece through the Near East to Africa. This of course depends on whether or not those found at sites like Jarmo were actually domesticated, and not a small form of wild cattle.

Domesticated sheep were dated to 7200 B.C. (UCLA-1657D) in a lowest level of Argissa-Magula, below the one containing cattle. These sheep are undoubtedly domesticated since no wild form of this species has been found so far in Europe after the close of the Pleistocene. It has been argued, however, that sheep occur in several late Mesolithic Tardenoisian and Azilian sites in Western Europe, mainly in France [25]. Their presence is usually explained as a possible survival of wild sheep from the Final Pleistocene into Post Glacial times and not as a reintroduction. The date of 7200 B.C. at the Argissa-Magula could, however, be used as an argument for diffusion from Greece into Central and

[20] H. R. Stampfli, *The Fauna of the Prehistoric Archaeological Sites of Jarmo, Matarrah, Karim Shahir, and the Amouq in South-western Asia* (Manuscript to be published by the Oriental Institute, Univ. of Chicago).

[21] C. A. Reed, in *Domestication and Exploitation of Plants and Animals*, P. J. Ucko and G. W. Dimbleby, Eds., (Chicago 1969), 375.

[22] Cf. R. Protsch in n. 11.

[23] Ibid.

[24] J. D. Clark, "British Expedition to the Äir Mountains", *Science in the Southern Sahara*, 12 (1971).

[25] J. Murray, *The First European Agriculture*, (Edinburgh 1970), 25.

Western Europe over a time span of perhaps 1000 years. These sheep at the Argissa-Magula had to be imported and most specialists agree that it must have been from the Near East. There is a date on sheep bones from one of the lowest levels at Jarmo, but Stampfli was not certain whether these sheep were wild or domesticated. This date of 7000 B.C. (UCLA-1714E) would be, however, still more recent than the one from Greece. The basal floor of Sarab, from which sample UCLA-1714A came yielded definitely domesticated sheep around 6900 B.C. It remains to be seen whether a site like Cayönü yields dates several hundred years younger than those in Greece. The site of Anzabegovo in Macedonian Yugoslavia, only a few hundred miles away from the Argissa-Magula, supplies a date on the bones of domesticated sheep in an upper level (262-274 cm) at 5750 B.C. (UCLA-1705C), yet, domesticated sheep bones are also found in the lowest levels. Judging by extrapolation of the stratigraphy, and a charcoal date in a lower level of 5950 B.C. (UCLA-1705A), sheep must have arrived in this area around 6400 B.C.

The oldest goat date comes from Asiab in Central Western Iran dated to about 8050 B.C. (UCLA-1714F, depth 150-160 cms). Other fractured goat and sheep bones from 140-120 cms yielded dates of 7900 B.C. (UCLA-1714B) and 7700 B.C. (UCLA-1714C).

Whereas some doubt remains as to the authenticity of domesticated sheep at Jarmo, goats were identified without doubt as domesticated. Jarmo gives us its earliest domesticated goat date as 7000 B.C. (UCLA-1714E). The site of Sarab in Central Western Iran has a similar date of around 6900 B.C. (UCLA-1714A) in the same level (S-I/4) which also yielded domesticated sheep.

The comparison of these dates is interesting because the archaeological assemblages at Sarab are more advanced typologically than those at Jarmo [26]. No direct dates exist yet on goat from Europe, but Nea Nikomedeia which contains goat in its lowest levels seems to indicate that these animals were probably in this area around 6900 B.C. also.

The domestication of the dog took place rather early among European Mesolithic peoples [27]. This suggests that the dog was domesticated by people still subsisting on a hunting-and-gathering economy. The date for basal Sarab of 6900 B.C. is indirectly based on the stratigraphic association of dog bones with sheep and goat dated to ca. 6900 B.C. (UCLA-1714A). The question if the bones of these different species were indeed associated with each other was solved by chemical microanalysis which showed matching composition among the different bones of each species.

Pigs have been dated by stratigraphic association, from the preceramic layers of the Argissa-Magula to ca. 7000 B.C. (UCLA-1657D), the same as for domesticated cattle. It has been pointed out by some specialists that pigs in the lower levels at Jarmo were still wild and probably only became domesticated during the time represented by the upper levels [28]. It will be interesting to see dates for domesticated pig found at Cayönü in Turkey, since they seem to

[26] R. J. Braidwood, B. Howe, C. A. Reed, "The Iranian Prehistoric Project", *Science* 133, 2008 (1961).

[27] M. Degerbol, *Proc. Prehist. Soc.* 27, 35 (1961).

[28] K. V. Flannery, *Skeletal and Radiocarbon Evidence for the Start and Spread of Pig Domestication.* Thesis for the Degree of Masters of Arts, Dept. of Anthropology, Univ. of Chicago, (1961).

appear in levels relatively dated to be several hundred years earlier than Jarmo. Their date could rival the very early date at the Argissa-Magula in Greece. As it looks presently, domesticated pigs seemed to have appeared in both Southeastern Europe and Asia Minor at approximately the same time, 7000 B.C.

One of the most recently domesticated of today's major herd animals is the horse. The earliest suggested occurrence of the horse ranged from 2000 to 4000 B.C., estimates which were strictly based on archaeological finds alone. A sketchy representation of a horse from Khafaje near Bagdad is dated to the Jamdat Nasr period around 3000 B.C. The first actual osteo-morphological find are two molars from Sialk in Central Persia somewhat earlier than 3000 B.C. The most abundant finds of equid bones in the last few decades have been made in sites of the Tripolye and Srednij-Stog cultures in the Russian Ukraine. Two samples of domesticated horse bones were dated from the sites of Dereivka and Evminka at different locations. The undoubtedly oldest date is one from Dereivka of 4350 B.C. (UCLA-1466A) followed by another date at the same site of 3720 B.C. (UCLA-1671A). The site of Evminka yields two roughly contemporary dates of 3730 and 3640 B.C. Surprisingly, horses seem to have existed at the same time also at the site of Polling in Bavaria (ca. 3670 B.C., UCLA-1657G) [29], even though there seems to be some doubt as to whether they were domesticated or not.

Conclusions

Our dates show that cattle and pigs were first domesticated in Europe. Sheep, which were thought to have become extinct in Europe during the terminal Pleistocene also appear first in Europe. However, there remains little doubt today that they originated as domesticates in the Near East or Turkey since no wild sheep appear to have existed in Europe with the beginning of the Holocene. Dogs were domesticated both in the Near East and Europe at virtually the same time. In the Near East Asiab at *ca.* 8000 B.C. qualifies as the first center of goat domestication if no earlier remains are found elsewhere. It is also the earliest domestication center for any animal species dated in this study. Last, horses were first domesticated by man in the steppes of the Ukraine, perhaps even earlier than our present dates indicate, since the Polling finds are virtually contemporaneous.

Plants appear to have been domesticated earlier than animals by more than a thousand years at the time of this writing. Leguminous plants found in Spirit Cave, Thailand, (9200 B.C.) indicate that vegetatively reproducing plants seem to predate the domestication of seed plants.

Undoubtedly future research may alter our overall impressions in detail, especially, after earlier sites like Nea Nikomedeia have been dated directly. But, on balance, there can be no doubt that Southeastern Europe was as much an early center for animal domestication as the Near East [30]. For plants, Southeast Asia must be added to the classical areas of plant domestication.

[29] W. Blome, *Tierknochenfunde aus der spätneolithischen Station Polling*, Inaugural-Dissertation, Ludwig-Maximilians-Universität, München (1968).

[30] We would like to thank R. J. Braidwood, J. Boessneck, S. Bökönyi, J. D. Clark, M. Garasanin, M. Gimbutas, W. F. Libby, V. Milojcic, R. J. Rodden and D. Telegin for samples, advice and appraisal. Supported by NSF GA 24781. Publication 1005 of the Institute of Geophysics and Planetary Physics.

Table 1. – Most Important Domesticated Animals and Their Areas of Origin

	Pasang Capra aegagrus	Dom. Goat Capra hircus
Central-Western Iran	*ca.* 8050 B.C. (UCLA-1714F)	Asiab (150-160 cm Level)
Central-Western Iran	*ca.* 7900 B.C. (UCLA-1714B)	Asiab (140 cm Level) (dom?)
Central-Western Iran	*ca.* 7700 B.C. (UCLA-1714C)	Asiab (120-140 cm Level) (dom?)
Eastern Palestine	*ca.* 7200 B.C. range*	Jericho (Older Preceramic Village)
Northern Iraq	*ca.* 7000 B.C. (UCLA-1714E)	Jarmo (J-I/7)
Eastern Turkey	before 7000 B.C.*	Catal Hüyük East (below Level X)
Central-Western Iran	*ca.* 6900 B.C. (UCLA-1714A)	Sarab (S-I/4)
Southwestern Iran	*ca.* 6900 B.C. range*	Ali Kosh (Brick Wall Zone)
Western Macedonia	*ca.* 6900 B.C.*	Nea Nikomedeia
Northern Iraq	*ca.* 6800 B.C. (UCLA-1723A)	Jarmo (PQ-14/5a)

	Wolf Canis lupis	Dog Canis familiaris
England	*ca.* 7500 B.C.**	Star Carr
Thessaly (Greek)	*ca.* 7000 B.C. (UCLA-1657D)	Argissa-Magula (Preceramic Neolithic)
Eastern Turkey	*ca.* 7000 B.C. range*	Hacilar Aceramic
Central-Western Iran	*ca.* 6900 B.C. (UCLA-1714A)	Sarab (S-I/4)
Eastern Palestine	*ca.* 6800 B.C. range*	Jericho (Younger Preceramic Village)

	Mufflon Ovis musimon	Dom. Sheep Ovis aries
Thessaly (Greek)	*ca.* 7200 B.C. (UCLA-1657A)	Argissa-Magula (Preceramic Neolithic)
Northern Iraq	*ca.* 7000 B.C. (UCLA-1714E)	Jarmo (J-I/7) (dom?)
Eastern Turkey	before 7000 B.C.*	Catal Hüyük East (below Level X)
Central-Western Iran	*ca.* 6900 B.C. (UCLA-1714A)	Sarab (S-I/4)
Western Macedonia	*ca.* 6900 B.C.*	Nea Nikomedeia
Northern Iraq	*ca.* 6800 B.C. (UCLA-1723A)	Jarmo (PQ-14/5a) (dom?)
Southeastern Yugoslavia	*ca.* 5950 B.C. (UCLA-1705A)	Anzabegovo (282-274 cm Level)
Southeastern Yugoslavia	*ca.* 5750 B.C. (UCLA-1705C)	Anzabegovo (262-274 cm Level)

	Ur Bos primigenius	Dom. Cattle Bos taurus
Thessaly (Greek)	*ca.* 7000 B.C. (UCLA-1657D)	Argissa-Magula (Preceramic Neolithic)
Eastern Turkey	*ca.* 7000–6000 B.C.*	Catal Hüyük (Level IX-O)
North-Central Yugoslavia	*ca.* 6300 B.C. (UCLA-1605I)	Obre I (Level 12)
Northern Iraq	*ca.* 6300 B.C. (UCLA-1723B)	Jarmo (PQ-14/2) (dom?)
Northern Iraq	*ca.* 5600 B.C. (UCLA-1723D)	Jarmo (K-21/3) (dom?)
Northeastern Iran	*ca.* 6000–5000 B.C.*	Hotu Cave (Older Painter Pottery Neolithic)
Northern Baluchistan	Mid-5th Millennium*	Kili Ghul Mohammad I
Ukraine	*ca.* 4210 B.C. (UCLA-1642C)	Luka-Vrublevetskaja (Pre-Cucuteni)
Lower Egypt	*ca.* 5000 B.C.*	El Omari
Southwestern Sahara	*ca.* 3750 B.C. (UCLA-1685)	Adrar Bous

	Sow Sus scrofa	Dom. Pig Sus domesticus
Thessaly (Greek)	*ca.* 7000 B.C. (UCLA-1657D)	Argissa-Magula (Preceramic Neolithic)
Northern Iraq	*ca.* 6500 B.C. range*	Jarmo (Level 5-I)
Northwestern Iran	*ca.* 5500 B.C. range*	Hotu Cave (Older Painted Pottery Neolithic)
Northwestern India	*ca.* 3000 B.C.*	Mohenjo Daro/Harappa
Denmark	1st half 3rd Millennium**	

	Onager Equus hemionus	Dom. Onager Equus?
Northern Iraq	*ca.* 7000 B.C. (UCLA-1714E)	Jarmo (J-I/7) (dom?)
Southwestern Turkistan	*ca.* 4800 B.C.*	Anau (Level I)

	Ass Equus africanus	Dom. Ass Equus asinus
Lower Egypt	*ca.* 4th Millennium B.C.*	Maadi

	Horse Equus ferus	Dom. Horse Equus caballus
Ukraine	*ca.* 4350 B.C. (UCLA-1466A)	Dereivka
West Ukraine	*ca.* 3730 B.C. (UCLA-1671B)	Evminka (Tripolye C_1)
Ukraine	*ca.* 3720 B.C. (UCLA-1671A)	Dereivka
Bavaria	*ca.* 3670 B.C. (UCLA-1657G)	Polling (Linear Ceramic) (dom?)

* Date based on radiocarbon date(s) from materials other than bones.
** Date based on archaeologic considerations.

Table 2. – Internal Consistency of Radiocarbon Dates Based on Collagen with their Stratigraphic Levels

Chronology of Jarmo

UCLA-No	Species	Level	C^{14}-Age	Calibrated Age
1723C	Dom. Cattle?	K-21/1	6180±300	*ca.* 5100 B.C.
1723D	Dom. Cattle?	K-21/3	6550±200	*ca.* 5600 B.C.
1723B	Dom. Cattle? Dom. Goat Dom. Sheep?	PQ-14/2	7270±200	*ca.* 6300 B.C.
1723A	Dom. Goat Dom. Sheep?	PQ-14/5a	7800±120	*ca.* 6800 B.C.
1714E	Onager Dom. Goat Dom. Sheep?	J-I/7	7980±140	*ca.* 7000 B.C.

Chronology of Palegawra

UCLA-No	Species	Level	C^{14}-Age	Calibrated Age
1714D	Wild Goat and Sheep	80-100 cm	13600±460	
1703A	Wild Cattle and Sheep	120 cm	14350±280	

Chronology of Asiab

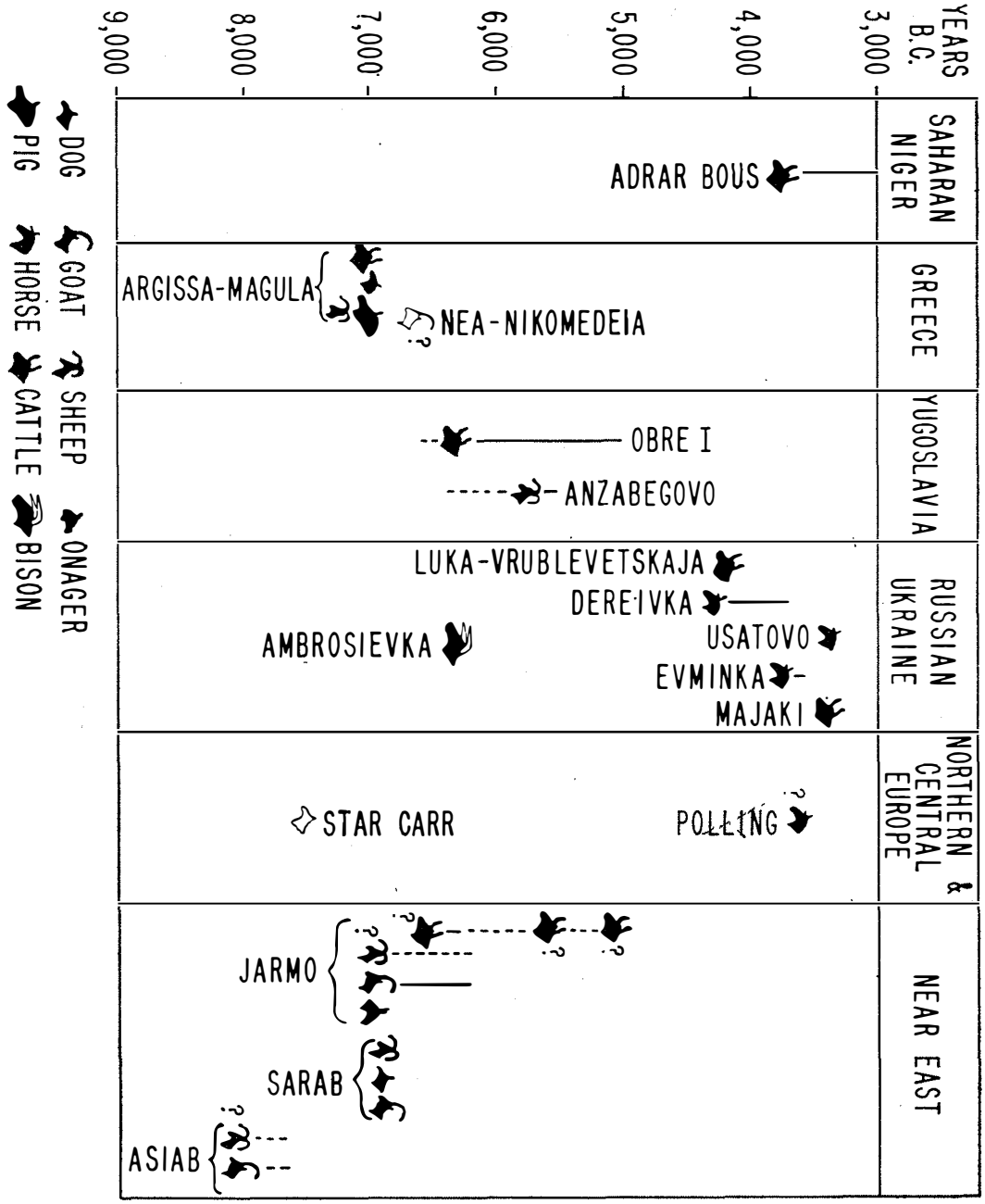
UCLA-No	Species	Depth	C^{14}-Age	Calibrated Age
1714C	Dom. Goat and Sheep?	120-140 cm	8700±100	*ca.* 7700 B.C.
1714B	Dom. Goat and Sheep?	140 cm	8900±100	*ca.* 7900 B.C.
1714F	Dom Goat and Sheep?	Below 140 cm	9050±300	*ca.* 8050 B.C.

Chronology of Obre I

UCLA-No	Species	Level	C^{14}-Age	Calibrated Age
1605H	Dom. Cattle	8	6150±60	5050 B.C.
1605G	Dom. Cattle	11	6710±60	5750 B.C.
1605I	Dom. Cattle	12	7240±60	6300 B.C.

Chronologies based on collagen dates show internal consistency and give for some sites very reliable stratigraphic sequence dates. This applies particularly to locations where charcoal contamination by bitumen can be a problem as in some Near Eastern sites.

Table 3. – Comparison of tree-ring calibrated radiocarbon dates based on the organic components of bones from various sites in Africa, Europe and the Near East determined at UCLA.



The Date of the Fara Period
A Case Study in the Historiography of Early Mesopotamia [1]

William W. HALLO – New Haven

Modern historiography, like ancient mythography, satisfies one of man's most basic curiosities: the quest into origins. The more distant and obscure the origins of anything seem, the more fascinating the quest. And if history be defined as the written record of mankind, then Egyptologists and Assyriologists have had much the best of this quest, for they control the first written sources. And yet the beginnings of recorded history have had a peculiar fate at their hands. For it might be thought that the advances of knowledge and of techniques of investigation would extend the range of our historiographic vision, much as each generation of astronomers can peer further into the spatial distance. In fact, however, these advances have had the opposite effect; gradually they have contrived to shorten the temporal span that we call history, or at any rate our subjective estimate of its length. Indeed, while obscurity and distance may vary together with the fascination they exercise on our imagination, they seem to vary inversely with the progress of knowledge, so that accretions in knowledge have the effect of contracting the span of time postulated by historiography for the placement of ever-increasing numbers of facts, data and events. The knowledge explosion, at least in Ancient Near Eastern history, therefore seems reconcilable with a contracting universe.

As a familiar example to illustrate, or perhaps I should say elucidate, these somewhat abstruse generalizations, let me recall the historiographic impact of the discoveries at Mari, which proved Hammurapi of Babylon a later contemporary of Šamši-Adad of Assyria, and reduced all prior Babylonian dates as previously calculated by no less than 275 years. It was inadequacy of textual data that had until then permitted such a distortion of chronology. The increased documentation resulted, not in a lengthening of the time-span needed to accomodate it, but a shortening. A less familiar example is provided, some three centuries before Hammurapi, by the end of the Sargonic period. Here the shortening of the chronology now espoused, e.g., by Jacobsen and Sollberger, is less drastic, but I believe equally valid. I have reduced the interval between the Sargonic and neo-Sumerian periods by about 65 years

[1] This paper is dedicated with affection and admiration to my teacher I. J. Gelb. It incorporates the edition of a new text (see Appendix) which has had the benefit of his interest and suggestions. Except for this appendix, and the footnotes, the paper reproduces without significant change a talk delivered on April 24, 1968, at the Center for Ancient History, University of Pennsylvania, in the framework of a panel discussion on "Early Sumerian History and the Beginnings of Historiography".

in an article on Gutium which is to appear soon [2], but will not repeat here what I have stated on the subject there or previously at the American Oriental Society meeting of 1961. I also believe that a further shortening of the (by now) traditional chronology is required at the beginning of the Sargonic period, where new texts have brought to light important new synchronisms that tend to prove considerable overlap in dynasties once thought of as more or less consecutive [3].

However, I would like to go back still further in Sumerian history, and to take up the comparative chronology of the Fara texts, a question hardly even debated since 1931, when Landsberger, in a review of *UET* 1, dated these texts as prior to Ur–Nanše of Lagaš [4]. He was led to this conclusion largely on the basis of orthography, even though the terms of the comparison are barely commensurate in orthographic terms: for the written evidence of the Ur–Nanše dynasty at Lagaš comes entirely from monumental texts, many of them on stone, while that for Fara and contemporary sites comes almost entirely from archival texts on clay tablets. Landsberger himself called attention to this fact [5]. His dates, however, were followed by most of the archaeologists working from artifactual materials even though there is not to my knowledge a single site where materials of the so-called Fara and so-called Lagaš periods occur in a demonstrable stratigraphic sequence. It thus seems not unreasonable to reopen the entire question. Let me begin by once more reviewing the current arguments on the comparative ages of Lagaš and Fara, as these have developed since Kramer's first summary of the data in 1932 [6].

A fresh survey of the evidence was given by Falkenstein in 1936 [7]. Basing himself on a combination of factors — stratigraphic, palaeographic, glyptic and in one or two cases even prosopographic — he concluded that the Fara tablets are older than Ur–Nanše, perhaps by as much as 100 years [8]. Yet the actual evidence which he himself cited in almost every instance seems to show that Lagaš broke with the Fara traditions *after*, not before, the time of Ur–Nanše. (1) The greatest part of the Fara texts was found in buildings of plano-convex bricks; at Lagaš such bricks went out of use between Eannatum and Entemena [9]. (2) Stratigraphically, there is no basis for comparison, since there are no "Fara-type texts" from Lagaš [10] nor Lagaš monuments at Fara. (3) Palaeographically, Falkenstein reiterates Deimel's views regarding the differences in ductus between Fara and Lagaš. But caution is in order here, for as Biggs has recently observed on the basis of an examination of photographs and originals, Deimel's own copies of the Fara texts were less accurate, e.g., than those of Jestin, and "have tended to make us think of

[2] See now W. W. Hallo, "Gutium", *RLA* 3 (Berlin/New York 1971), pp. 708-720, esp. § 7 (pp. 713 f.).

[3] See for now William W. Hallo and William Kelly Simpson, *The Ancient Near East: a History* (New York 1971), pp. 54-58.

[4] B. Landsberger, *OLZ* 34 (1931) cols. 117-127.

[5] Ibid., p. 122.

[6] *JAOS* 52 (1932) 120-124.

[7] *Archaische Texte aus Uruk* (Leipzig 1936), pp. 16-22.

[8] Ibid., p. 22.

[9] Ibid., pp. 16 f.

[10] Those published by Thureau-Dangin, *RTC* 9-15, were acquired by purchase; cf. Falkenstein, *ATU* p. 19.

the Fara script as looking more 'archaic' than it actually is "[11]. More decisive, according to Falkenstein, is the fact that the Fara scribes still arranged their signs arbitrarily within each case, something which Ur–Nanše's scribes tolerated only sporadically and Eannatum's not at all. I find this argument less than convincing. The extent of "sporadic arbitrariness"[12] is hard to measure: (a) often we cannot be sure *how* the signs are to be read in the Ur-Nanše texts, especially in the case of personal names; (b) in a particularly clear case like za:gìn, the "arbitrary order" is equally true of Ur–Nanše's text as of the Fara[13] and Abu Salabikh ones, as shown by Biggs[14]; (c) the case for "systematic departures" from the norm (e.g., ^{d}Nanše–Ur) is more complex than Falkenstein realized[15]. In any case, the changeover under Eannatum was so systematic that it looks like a conscious innovation fostered by the great *ensi* or his scribes, and thus introduced first at Lagaš. It would be hazardous to conclude that the same reform was instantly adopted also at Fara, and that it can therefore be used as a sure chronological index. But even if this argument were granted, as well as the further one about more systematic indication of (certain) grammatical elements beginning with Eannatum[16], this still would rank the Fara texts earlier than Eannatum at most, but not necessarily earlier than Ur–Nanše.

There remain the arguments from the glyptic and prosopographic evidence, essentially both based on a single cylinder or cylinder-type[17]. I leave it to others to weigh this evidence and content myself with echoing the caution advised by Biggs: "It is difficult to evaluate the differences in writing between the Fara period and the time of Ur–Nanše of Lagaš, and still more difficult to estimate the intervening time span", and his tentative conclusion: "That we should assume a difference of more than one or two generations between the Fara–Abū Ṣalābikh tablets and the time of Ur–Nanše is, in my opinion, doubtful "[18].

Thus between the Fara texts and Ur–Nanše, Falkenstein could conceive of a gap of as much as a century, Biggs no more than a generation or two at most. Perhaps it seems hopeless to achieve greater precision for such a remote period. But there is at least one line of evidence which has so far hardly been thrown into the balance[19] and which, moreover, is not subject

[11] *JCS* 20 (1966) 75, n. 20.

[12] Falkenstein, *ATU* p. 20: "vereinzelt(e) Freizügigkeit".

[13] See e.g., R. Jestin, *Tablettes Sumériennes de Šuruppak* (Paris 1937) No. 126 iv 9.

[14] Robert Biggs, "Le lapis-lazuli dans les textes sumériens archaïques", *RA* 60 (1966) 175 f. To the examples collected there, one may add the "door of lapis lazuli" (ig–za.gìn) in an even earlier Lagaš text, the so-called "Figure aux Plumes"; cf. E. de Sarzec, *Découvertes en Chaldée* 2 (Paris 1912), pl. 1*bis* "obv." i 1. For such doors, see A. Salonen, *Die Türen des alten Mesopotamien* (Helsinki 1961), pp. 18 (8) and 22 (19) = Kramer, *PAPhS* 107 (1963) 495 f. (line 30).

[15] Cf. Hallo, *HUCA* 33 (1962) 22 f., note 198.

[16] Falkenstein, *ATU* p. 20.

[17] Biggs, *JCS* 20 (1966) 75, n. 19. (Cf. also below, note 50).

[18] Ibid., p. 76.

[19] See however F. J. Stephens *apud* G. G. Hackman, *BIN* 8 (New Haven 1958), p. 6, and Robert D. Biggs, "Semitic Names in the Fara Period", *Or* 36 (1967) 55-66.

to the shortcomings already noted above in connection with the difference in the nature of the documents from Lagaš and Fara. This is the onomastic evidence, and it is to this that I would like to appeal here. (Onomastics is here used for the characteristics of name-giving in general, as distinguished from prosopography, which attempts to identify specific individuals on the basis of personal names).

My object, then, will be to compare the onomasticon of pre-Sargonic Lagaš with that of Šuruppak, with an occasional side-glance at the personal names in the archaic texts from Ur. We may safely leave out of account the few attested names from sites such as Umma as they are largely limited to royal or throne names and these, from all indications, were assumed upon accession and are not necessarily indicative of naming practices in general. For this reason the royal names of Lagaš will also be left out of account. We may also largely ignore the materials from Adab, Nippur, Abu Salabikh etc. as long as this has not been independently dated. Admittedly, this leaves a very limited corpus of personal names from the Lagaš of all but the last two or three pre-Sargonic reigns. But this disadvantage is offset, for chronological purposes, by the genealogical particulars available for this corpus. We have, in fact, some six successive generations of onomastic data *before* the great mass of personal names appearing in the time of Enetarzi, Lugalanda and Urukagina. We may disregard the name of Gur–sar, the alleged grandfather of Ur–Nanše; this name, which recurs neither at Šuruppak nor at Ur, more likely identifies Ur–Nanše's ancestral city [20].

The first generation is represented so far solely by the father of Ur–Nanše, Gu–ni–du. His name is not attested in the publications of Deimel and Jestin, as it happens. On the other hand it occurs in *PBS* 9: 3, which is almost certainly from Fara [21]; and again in texts from Adab and from Lagaš itself in the inscription of Lummatur, the son of Enannatum [22].

This by no means exhausts the prosopographic material from Lagaš antedating Ur–Nanše [23], but the rest is less easily dated, even by the rough and ready standard of "generations". I will therefore single out here only the stone of Enhegal who shares with Urnanše the title of "king of Lagaš" and like him mentions the Inanna temple called Ebgal; he certainly cannot be much earlier than Ur–Nanše; otherwise there would be little time left to accommodate Mesilim, under whom the rulers of Lagaš (and Adab) bore the more modest title of *ensi* [24]. Of its recognizable personal names, at least four recur with some

[20] E. Sollberger, "Gunidu and Gursar", *RLA* 3 (1971) 700.

[21] Cf. the new edition by D. O. Edzard, *Sumerische Rechtsurkunden des III. Jahrtausends* (München 1968), No. 28. Edzard notes the parallels with Fara-texts, though he weighs a Nippur provenience.

[22] Ibid., Nos. 117, 119, 120. For the identity of Lummatur's father, see Sollberger, *Corpus des Inscriptions "royales" présargoniques de Lagaš* (Genève 1956), p. 28, En. I 10.

[23] For a bibliography and descriptions, see A. Parrot, *Tello* (Paris 1948), pp. 70-77.

[24] See the chronological charts in Hallo and Simpson, op. cit. (note 3) p. 47 and D. O. Edzard, *Fischer Weltgeschichte* 2 (Frankfurt am Main 1965), p. 59. For a different view, see C. J. Gadd, *The Cities of Babylonia* (= *CAH*² fasc. 9, Cambridge 1962), chart inside back cover.

frequency in the Fara texts: Lugal–ki–gal–la, Šeš–geštin [25], Si–dù [26], and Maš [27], and of these four, three occur otherwise only in the Fara texts, as far as I can determine. Only one identifiable name does not seem to recur in the Fara texts at all [28]!

The generation of Ur–Nanše himself is represented, apart from the royal name, by those persons depicted and named on his "family reliefs" who are not designated as (his) sons. But they can best be considered together with the next generation, that of his "sons". And here the evidence is particularly telling: of the fifteen "captions", twelve may safely be read and identified as personal names, and all but one has an exact or equivalent parallel in the onomasticon of Šuruppak, the apparent exception being Akurgal, the successor of Ur–Nanše, who may, however, have borne a "throne-name" already as heir apparent; indeed there is a very similar name at Fara (A–ki–gal), just as the frequent personal name Abzu–ki–gal at Fara may be compared to Abzu–kur–gal at Lagaš [29]. Let us study the parallels more closely.

There are, to begin with, Adda–tur, Lugal–ezen and Dudu, but these are such common names throughout all periods that we should probably attach no particular significance to them.

Secondly there are a couple of names which survived in the onomasticon of the late pre-Sargonic or Sargonic periods, but passed out of fashion by neo-Sumerian and Old Babylonian times. Thus, e.g., the name SAG.AN.TUG, which already occurs in archaic Ur in the form SAG.AN.TÚM and at Šuruppak in the form SAG.AN.GÙB?, recurs occasionally in late pre-Sargonic and (early?) Sargonic texts [30], and found its way into the canonical lists of personal names which formed the basis of instruction in the Old Babylonian period, the so-called Silbenalphabet A [31] and the Personal Name List B [32]. But it is no longer in actual use after the Sargonic period.

[25] See the latest edition of the text (with previous literature) by Edzard, *Sumerische Rechtsurkunden* No. 114, but read v 3 f. as a single case: šeš–geštin ugula eb-gal (Edzard: ŠEŠ.IB.GEŠTIN/NIGÍN?–gal) and similarly in v 10 (Edzard: ŠEŠ–x–x–RU engar). In Fara III 70 vi, note Šeš–geštin of Adab!

[26] For the name Si–dù (read DÙ–si by Edzard), see Hallo, *JAOS* 83 (1963) 175 and n. 67 (equated with Enlil–ibni); H. Sauren, *Wirtschaftsurkunden... des Musée d'Art et d'Histoire in Genf* (Napoli 1969), No. 136 (Sauren reads Si–rú); T. Donald, *MCS* 9 (1964) No. 238 i 6 ("Gutian period" text). The name found its way into "Silbenalphabet B"; cf. Landsberger *apud* M. Çiğ and H. Kizilyay, *Zwei altbabylonische Schulbücher aus Nippur* (Ankara 1959), p. 103 (44).

[27] For Maš at Fara, cf. e.g., Jestin, *TSŠ* No. 467 i. He occurs both as farmer (engar) and as NUN.SUR.GU in the Enhegal inscription; for the latter profession, see Edzard, *Sumerische Rechtsurkunden* p. 20 *ad* No. 1 vi 8 and cf. perhaps SUR.GU.ŠE in A. Deimel, *Schultexte aus Fara* (Leipzig 1923), No. 72 v 10.

[28] Lugal–nim–gen, interpreted as Lugal–nimše–gena(?) by Edzard.

[29] The name Á–kur–gál is probably to be kept quite distinct from these. Cf. however the variation between á–nun–gál and a–nun–gál in the Nisaba–hymn nin–mul–an–gim for which see Hallo, *Actes de la XVII^e Rencontre Assyriologique Internationale* (ed. A. Finet [Bruxelles 1970]), p. 124 line 6.

[30] M. V. Nikolsky, *Dokumenty* 1 (St. Petersburg 1908), No. 4 and Hackman, *BIN* 8 No. 131 respectively.

[31] J. Nougayrol, *AS* 16 (= Landsberger Volume [Chicago 1965]), p. 32, line 64; cf. Sollberger, ibid., p. 23, line 63 where it is "explained" as *awilum* x–x; respectively *aškan*, variant (p. 26) ^{d}AN.MAR.TU.

[32] Çiğ and Kizilyay, ibid., p. 44, line 15.

More instructive for our purposes are those names of the Ur–Nanše family plaques which went out of use even before late pre-Sargonic times. These are no less than half of our group. Three of them, Nam–azu, A–nun–pàd and Men–sù–ud, recur in precisely this form at Šuruppak, and nowhere thereafter. A fourth, Mu–ta–kur-re, may be compared with names like Mu–ta–kur and Mu–ni–kur–ra at Šuruppak [33]; if a guess may be hazarded as to its meaning, it may allude to the name of Enlil which is said to be "over the mountain (or foreign land)" in a liturgical tradition which can be traced from Old Babylonian to Seleucid times [34].

The other two names also recur at Šuruppak, but with variant spellings which are less apt to reflect chronological than local differences in orthographies. Both are complex names of a type not likely to have weathered the winds of change in matters of onomastic taste for very long. Bearing in mind that the order of the elements is uncertain we may read the one as Hur-sag-šè–mah$_x$, "Lofty with respect to the mountain, loftier than the mountain" [35], and the other as Bára–sá–gan–nu–di (for later ság [sàg–gan]–nu–di), "The throne (is) not overthrown". Both names recur at Šuruppak; if this has not been recognized before it may be because, in the first, maḫ is written with the MAḪ sign instead of the AL sign [36] and, in the latter, sa–gan is written with the DI sign instead of the SA sign. This is also the spelling in an early text from Adab [37], while a still earlier Ur text [38] writes simply Bára–GAN–nu–di, omitting sa/sá entirely.

The negative evidence must also be considered briefly. At first glance, three names from the Ur–Nanše reliefs have no equivalents in the abundant Šuruppak onomasticon. They are Áb–da, Á–ni–ta, and Gu–la. But on closer inspection, these exceptions seem less telling. As far as Áb–da is concerned, the identification of the signs and their reading is in considerable doubt. The photographs suggest that the second sign may be šu, not da, and as for the first, there is no other attested occurrence of its use as a syllabic value áb in the pre-Sargonic syllabary of Lagaš [39]. As for Á–ni–ta and Gu–la, while one

[33] Mu–ni–kur–ra in turn recurs as a geographical name in Ur III, perhaps equivalent to mu–bi-kur–ra in Entemena 41; cf. Sollberger, *Or* 28 (1959) 345 and n. 1; G. Pettinato, *Mesopotamia* 5-6 (1970-71) 318. Cf. also the personal name Mu–ni–ḫur–sag in *BRM* 4:45 (Early Dynastic III).

[34] A. Poebel, *ZA* 37 (1927) 249; R. Kutscher, *YNER* 6 (forthcoming), *ad* a–ab–ba ḫu–luḫ–ḫa, lines 62-72, 95-98, 122-124. Cf. also Myth of the Pickaxe, line 90: ^{giš}al–àm mu–bi du$_{10}$–ga ḫur–sag–gá mu–un–gál.

[35] For maḫ as a kind of comparative, see J. J. A. van Dijk, *Sumerische Götterlieder* 2 (Heidelberg 1960) p. 122; for impersonal nouns with locative in sense of "than" (not terminative as here) see Falkenstein, *Grammatik der Sprache Gudeas von Lagaš* 2 (Roma 1950), pp. 106 f., n. 4; Hallo and van Dijk, *YNER* 3 (1968) 73 s.v. diri.

[36] The reading AL = mah$_x$ was not recognized by Sollberger, "Le Syllabaire présargonique de Lagaš", *ZA* 54 (1961) 1-50, but is confirmed by the later syllabaries and by the writings gu$_4$–mah$_x$ and áb–mah$_x$; see Landsberger, *MSL* 8/1 (1960) 60 and n. 1 and cf. H. A. Hoffner, *JAOS* 87 (1967) 185.

[37] D. D. Luckenbill, *OIP* 14 (1930) 49 (= Edzard, *Sumerische Rechtsurkunden*, No. 119) iv 8.

[38] E. Burrows, *UET* 2 (1935) 2 v. For the interchange of SÁ.GAN and PA (= SÀG).GAN in early literary texts cf. M. Civil and R. D. Biggs, "Notes sur des textes sumériens archaïques", *RA* 60 (1966) 5 f.

[39] Cf. Sollberger (above, note 36) who interprets the name as Akkadian 'abda' (*ZA* 54:35).

of them is attested as a personal name as late as Sargonic times [40], it is not excluded that they are used as titles or epithets here: "at his side, attendant" [41], and "big, chief" respectively. Even if this interpretation be rejected, a very significant number of correlations remain between the names of Ur–Nanše's time and those of the archives of Šuruppak.

The correlation is the more significant as it changes radically in the succeeding periods. For the three generations from Eannatum to Enannatum II [42], our best source of non-royal names are the contracts for the purchase of real estate from large numbers of sellers [43]. These have been studied by Lambert [44], Diakonoff [45] and Edzard [46] and, in addition to a number whose exact date is uncertain, include in particular the stone of Lummatur son of Enannatum. In addition, I am able to utilize for this purpose a hitherto unpublished real estate deed from Lagaš dated to the priesthood of Dudu, the contemporary of Entemena (see below, Appendix). Both of these documents thus appear to date from the third generation after Ur–Nanše. Between them they contain close to eighty different personal names. Of these, I have found only four or five to go all the way back to the archaic Ur texts (Amar–abzu, Amar–tùr, É–zi, Lugal–Anzu [Imdugud], Dada). Three or four of these names are still among the most common personal names at Fara. The fifth (Lugal–Anzu) also recurs at Fara, interestingly enough with the designation "Lagaš" behind it [47]! Some fifteen others may be identified first in the Fara texts. That still leaves sixty names, or three-fourths of the onomasticon, changed as between the Fara texts and the Lagaš texts of Entemena's generation.

The contrast is even more striking when we study the evidence of the last three rulers of Lagaš, Enetarzid, Lugalanda and Urukagina, who reigned but a single generation [48]. Here the Lagaš material provides us with a super-abundance of personal names, and it cannot be my task in this preliminary survey to compare all of them with the older onomasticon. Even so, a glance at the old but still serviceable compendium by Dhorme [49] will quickly show an almost total break with the onomastic fashions of both Šuruppak and the earlier Lagaš periods.

What then is the significance of all these, admittedly provisional, findings? In its simplest form it may be put thus: a comparison of the Fara personal

[40] *BIN* 8:155 (= Edzard, *Sumerische Rechtsurkunden* No. 93) line 12.
[41] Note his position next to the king and his small size in the actual representations.
[42] In absolute terms ca. 2470-2380 B.C. (cf. Edzard, *Sumerische Rechtsurkunden, passim*), or *ca.* 2455-2365 B.C. (cf. Sollberger and Kupper, *Inscriptions royales sumeriennes et akkadiennes* [Paris 1971], *passim*).
[43] Ibid., Nos. 14, 31-35, 60, 115-118; cf. also Nos. 43 (slave sale) and 105 (list of witnesses?). For another analysis of such texts, and an Old Akkadian example, see Sollberger, *JCS* 10 (1956) 13-17.
[44] M. Lambert, "Textes commerciaux de Lagash (III)", *ArOr* 23 (1955) 557-574.
[45] I. M. Diakonoff, "Sale of land in pre-Sargonic Sumer", *Papers presented by the Soviet delegation at the XXIII International Congress of Orientalists* (Moscow 1954), pp. 19-29.
[46] Above, note 43.
[47] Deimel, *Wirtschaftstexte aus Fara* (Leipzig 1924), No. 70 xii.
[48] *Ca.* 2380-2355 or 2365-2340 B.C. (cf. above, note 42).
[49] P. Dhorme, "Les plus anciens noms de personnes à Lagaš", *ZA* 22 (1909) 284-316.

names with datable (non-royal) personal names from Lagaš shows that their statistically most significant correlation is with the two generations immediately preceding and succeeding Ur–Nanše. The correlation rises from about 80 % in the Enhegal inscription just before Ur–Nanše to almost 100 % in the generation immediately following Ur–Nanše. The correlation drops to about 20 % as early as the time of Entemena, and to a negligible figure by the end of the Lagaš I dynasty. The onomastic evidence therefore does not seem to permit a large gap between Fara and Ur–Nanše. Indeed, I propose to reassign the Fara texts tentatively to the time of Ur–Nanše or even a generation later. Needless to say, this hypothesis will have to be tested against other lines of evidence, glyptic [50] and legal [51] to mention only two, and against evidence from other sites. But it deserves relatively more weight than such other evidence because patterns of styles in personal names appear to be considerably less conservative than styles in seal designs or legal formulations.

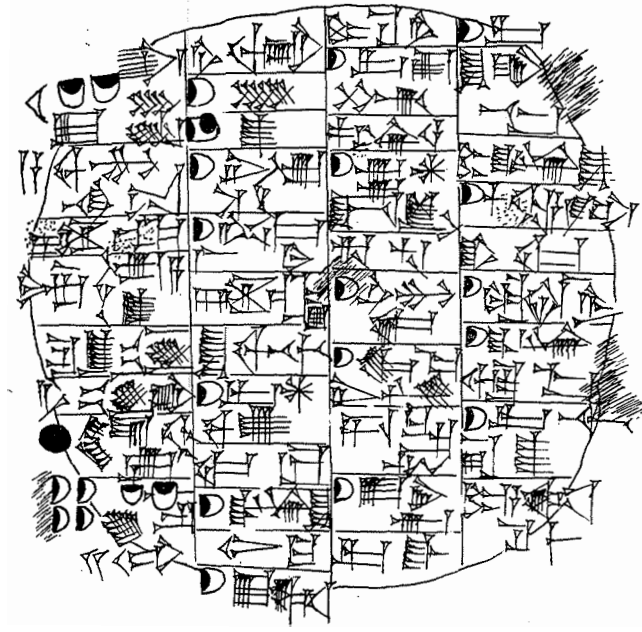
To return to our starting point: we may compare our problem to a picture puzzle. Our first efforts, the beginnings of our own historiography with regard to the early Sumerians, tend to result in restorations of coherent but fragmentary parts of the total picture, and the tendency is to exaggerate the open spaces separating these parts. But as we succeed in joining the separate parts, the open spaces disappear one by one and the total picture begins to emerge. To resolve the analogy: the fragmentary parts of our puzzle are the disparate archives of Lagaš, Šuruppak, and other sites that a previous generation of Assyriologists have to some extent succeeded in reconstructing. The spaces between them are the chronological gaps which our predecessors felt constrained to posit between them. The puzzle will be solved when these chronological gaps are eliminated from our reconstruction either by direct synchronisms or, failing that, by the type of evidence adduced above. Nor need we fear that the end of the puzzle will mean an end to our labors, like geographers who have no more "blank spaces" to fill on their maps. On the contrary, true historical research will only begin when the chronological cartography has finished its work.

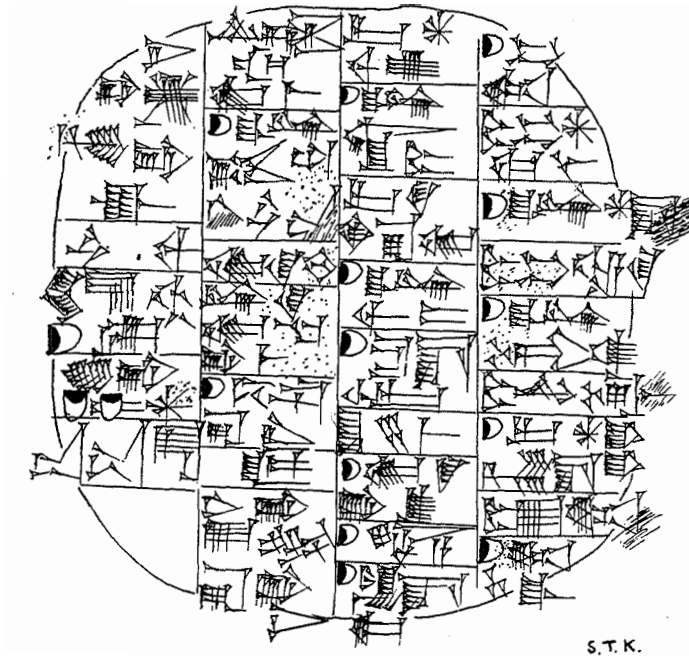
Appendix

In return for its contribution to the excavations at Tello, the William Rockhill Nelson Gallery of Art, Kansas City, Mo., long ago received a number of inscriptions from that site. Some of these were published by H. de Genouillac, *Fouilles de Telloh* (Paris 1934-6) (cf. e.g., pl. xlix TG. 1549). For some years they have been on deposit at the Yale Babylonian Collection for study and cataloguing. The bulk of those not previously known will be published by Norman Yoffee. It seemed appropriate, however, to single out for prior publication the text presented herewith. It provides important additional illustrations not only for the onomasticon of the middle portion of the Lagaš I period (see above, note 42) but also for real estate transactions

[50] See now R. M. Boehmer, "Zur Glyptik zwischen Mesilim- und Akkad-Zeit (Early Dynastic III)", *ZA* 59 (1969), 261-292.

[51] See e.g., Elena Cassin, "Symboles de cession immobilière dans l'ancien droit mésopotamien", *L'Année Sociologique* Series 3 vol. 5 (1952) 145 f., who concludes that the changing formularies for the sale of real estate support a gap of a century or more between the Šuruppak texts and Eannatum of Lagaš.





Nelson Gallery–Atkins Museum, Kansas City, Missouri (Nelson Fund)

typical of that interval (above, notes 44-46), and recently edited by Edzard, *Sumerische Rechtsurkunden* (above, note 43). The preceding article, though written before Edzard's book became known to me (above, note 1), has been aided and stimulated by the latter volume, and may be regarded as a kind of surrogate review article to it (see above, esp. notes 21, 25, 27, 37, 40, 42 f., 46, 52)[52]. The copy of the new text was prepared by Shin Theke Kang, and the transliteration has benefited from the interested and detailed comments of I. J. Gelb.

	TRANSLITERATION		TRANSLATION[53]
(i)	2/3 ša é SAR		2/3 SAR of "house"
	A–ba–mu–na–túm		Aba–munatum
	Ḫé–ezen		from He–ezen
	dam DUN.A–šé		wife of DUN.A
5.	e–šè–šám	5.	bought.
	níg–šám–bi		Its price:
	10 kù–luḫ–ḫa gín		10 šiqil purified silver
	[2+]4 (gur) 1 (UL) 1 (bán) še gur–2–UL		(the equivalent of?) ⌈13⌉ 1/6 ′(UL)[b] barley in bushels of 2 UL (each).
(ii)	níg–ba–bi		(As) its (associated) gift:
10.	1 še (gur)	10.	1 (bushel?) barley
	1(?)zíz (gur)		1 (bushel?) emmer wheat
	1 ì–šaḫ sìla		1 quart lard
	1 níg–sag–lá–munus		1 woman's headdress
	Ḫé–ezen–ke$_4$		He–ezen
15.	šu–ba–ti	15.	received.
	ʼUr–^{d}Ba–ú		Ur-Ba'u
	sag–sug$_5$		the surveyor,
	ʼLugal–šu		Lugal–šu
	udul		the oxherd,
20.	ʼÉ–abzu	20.	E–abzu
(iii)	dumu Si–gar		the son of Sigar
	ʼUr–é–muš		Ur–emuš
	ugula lú–10–me (!)[a]		the foremen of ten men,
	ʼNir–an–da–gál		Nir–andagal

[a] Collated

[b] Restoration based on lines 66 f. The "correct" figure would be 13 1/3 (UL).

[52] To the texts edited by Edzard, add also L. Legrain, *RA* 32 (1935) 126.

[53] Lines 30, 48 and 50 are taken to represent professional designations, since they lack the "personal" wedge and the names they follow would otherwise (except for line 26) be the only ones among all the witnesses identified neither by patronymic nor by profession. Tentatively, we may compare UR in these lines with lú–ur–ra or lú–ur–e = *zabbu*, an ecstatic (*MSL* 12:166:271; 178:34 and *CAD* Z s.v.). Possibly, then, line 50 specifies the kind which "paws the ground" (for ki–har in this sense, cf. most recently M. Tsevat, *Oriens Antiquus* 1, 1962, 9 f.; W. Heimpel, *Tierbilder* 273 f.). Alternatively, the reference may be to a surveying function; cf. ḫur = *eṣēru*, "draw", in connection with ki = *qaqqaru*, "ground" (ŠL 461:288; *CAD* s.v. *eṣēru* A).

25.	RI–mušen	25. the bird–trainer (?),
	ʻUtu–mu–kúš	Utu–mukuš,
	ʻInim–ma–ni–zi	Inimani–zi
	gala–maḫ	the chanter,
	ʻÉ–ir–nun	E–irnun
30.	ur(?)[c]–gal	30. the senior UR,
(iv)	ʻUr–šu–ga–[lam]–ma	Ur–šugalamma
	dumu Lugal–šu	the son of Lugal–šu,
	ʻMaš–ḪA.A.KI–ta	Maš–subarata
	bur–sag	the priest,[d]
35.	ʻNin–šà–lá–tug	35. Nin–šalatug
	ʻLugal–ù–ma	Lugal–uma
	ʻUr–ti–ra–áš	Ur–Tiraš
	dumu A–abzu–si–me	the sons of A–abzu–si,
(v)	ʻAmar–abzu	Amar–abzu
40.	dumu Amar–^{d}SAG.KUD	40. the son of Amar–Madan,
	ʻLugal–an–da	Lugal–anda
	dumu Amar–ḪA.A.KI	the son of Amar–subara,
	ʻLugal–kur–dúb	Lugal–kurdub
	dumu Šeš–kur–ra	the son of Šeš–kurra,
45.	ʻUr–^{d}KINDA–zi–da	45. Ur–^{d}Kindazida
	dumu É–abzu	the son of E–abzu,
	ʻAbzu–kur–gal	Abzu–kur–gal
(vi)	ur ^{d}Ba–ú	the UR of Ba'u,
	ʻLugal–pirig–tur	Lugal–pirigtur
50.	ur ki–ḫar–ra–ka	50. the UR of KI.HAR.RA,
	ʻLugal–igi–tab	Lugal–igitab
	ʻE–da–ḫul	Edahul
	šu–i(!)–me	the barbers,
	ʻKA–ki–bi–šè	KA–kibiše
55.	ʻKa$_5$–a	55. Ka'a
	ʻSAL.EN–da–kúš	SAL–EN–dakuš
(vii)	šeš Hé–ezen–ka–me	the brothers of He–ezen,
	ʻLugal–íb–ta–ni–è	Lugal–ibtani'e
	ka–ki–ʻkam	being the recorder,[e]
60.	lú ki–inim–ma–bi–me	60. were the witnesses to it.
	ʻUr–ra–ni	Urrani
	nimgir	the herald
	kak–bi é-gar$_8$–ra bi–KAK	nailed its nail into the wall,
(viii)	ì–bi zà–ge bi–a$_5$	its oil he "did" at the side.
65.	u$_4$–ba	65. At that time
	kù–luḫ–ḫa 1 gín	1 šiqil purified silver,
	še–bi 1(UL) 2 (bán)–am$_6$	its barley (equivalent) was 1 1/3 UL.
	Du–du sanga	Dudu (was) priest.

[c] The sign actually begins with three verticals, not two as copied.

[d] Cf. *CAD* s.vv. *abru* C, *burrû*.

[e] Cf. *CADK* s.v. *kakikku*.

Sumerian and Akkadian in Sumer and Akkad[1]

Jerrold S. Cooper – Baltimore

Professor I. J. Gelb's major studies of language use in Early Dynastic and Sargonic Babylonia[2] led him to conclude that the north (Akkad and beyond) was peopled by Akkadian speakers, and the south (Sumer) by Sumerian speakers, with increasing Akkadian intrusions. Recent discoveries, notably the Abu Ṣalabikh tablets[3], have occasioned re-examination of Sumero-Akkadian language contact in early Babylonia[4]. The following discussion, which will attempt to further this re-examination, owes much to the stimulating ideas which Professor Gelb has expressed both in print and in his classroom.

The earliest unambiguous evidence for the presence of significant numbers of Akkadian speakers in southern Mesopotamia is the scribal names from Abu Ṣalabikh, at the beginning of Early Dynastic III (*ca.* 2600)[5]. The earliest indisputable attestation of the Sumerian language antedates this evidence by little more than a century (end of ED I)[6], although arguments for the existence of Sumerian in the preceding archeological period ("Protoliterate") have been widely accepted[7]. If we assume that the Abu Ṣalabikh scribes were not recent immigrants, but rather represented a population group that had been in the land long enough to have become involved in the production of rather sophisticated literary texts in a language not their own, then Sumero-Akkadian

[1] Abbreviations follow *CAD* K vi ff. Kraus = F. R. Kraus, *Sumerer und Akkader: Ein Problem der altmesopotamischen Geschichte* (Amsterdam, 1970).

[2] *MAD* 2[2] 1 ff., and *Genava* 8 (1960) 265 ff.

[3] See R. D. Biggs, *JCS* 20 (1966) 73 ff., *Or* 36 (1967) 55 ff., *ZA* 61 (1971) 193 ff.; R. D. Biggs and M. Civil, *RA* 60 (1966) 1 ff.

[4] Especially the work of Kraus cited in footnote 1.

[5] Biggs, *Or* 36 (1967) 55 ff. Note p. 56 fn. 3, where Biggs, following Gelb, *Genava* 8 (1960) 265, rejects the attempt to characterize certain names from the archaic Ur texts as Semitic. Despite Kraus' well-reasoned objections to using the language of a personal name or document as evidence for labeling its bearer or writer a Sumerian or Akkadian (Kraus 17 f.), it is difficult to avoid interpreting incipient Akkadian personal names and texts as evidence for the presence of Akkadian speakers. A language can be used for both personal names and writing after it is no longer spoken, but it is unlikely that a language would be used for such purposes *before* it was spoken. Thus, throughout this paper, the incipience of Akkadian in areas of written language use and personal names will be considered significant evidence for the use of Akkadian as a spoken language, whereas the survival of Sumerian will not be considered significant for the use of Sumerian as a spoken language.

The chronology used in this paper follows D. O. Edzard and J. Bottéro in J. Bottéro *et al.*, *The Near East: The Early Civilizations* (London, 1967), pp. 53 f., 94 f., and 152 f.

[6] The archaic Ur texts. Cf. E. Burrows, *UET* 2 p. 22, and A. Falkenstein, *OLZ* 40 (1937) 96 f.

[7] A. Falkenstein, *ATU* 37 ff., and see now the contribution of Kraus 55.

language contact in Mesopotamia can be projected back literally to the dawn of history. Our evidence is, of course, textual, and any statements about the spoken languages of early Babylonia are necessarily extrapolations from written sources. The case of Abu Ṣalabikh is paradigmatic for the difficulties involved: the scribes who wrote our earliest Sumerian literary texts had Akkadian names. Of the possible conclusions one might draw from this fact, the most extreme, for so early a period in Mesopotamian history, is that the scribes represent an Akkadian speaking population group that uses Sumerian as its vehicle for written expression [8]. Yet, extreme though this position may be, much of our evidence can be interpreted to support it, and little can be persuasively marshalled against it.

Although Pre-Sargonic sources for Akkadian are indeed "scant" [9], the abundant Sargonic material [10] represents a northern population with predominantly Semitic names writing in Akkadian, and a southern population with predominantly Sumerian names writing exclusively in Sumerian [11]. That the northerners with Akkadian names and writing in Akkadian spoke Akkadian is an assumption that few will challenge, but did the Sumerian writing southerners with Sumerian names actually speak Sumerian in ED III and the Sargonic period? Again, few would object to the proposition that at some time, the inhabitants of what we call Sumer spoke Sumerian. In an exhaustive and stimulating study, which one hopes has decisively laid to rest several myths and misconceptions, F. R. Kraus concludes that whereas the existence of the Sumerian and Akkadian languages makes it certain that Sumerians and Akkadians did at one time exist as separate groups, our sources do not permit us to define two distinct language groups in any period, and that a "highly developed symbiosis" possibly existed at the time of our earliest sources [12]. Nowhere do these sources mention or even imply ethno-linguistic differences, and the historian must interpret the evidence of written language and personal names with regard to subsequent historical development and general linguistic theory, in order to reconstruct the hypothetical language contact situation and the process which led to the eventual replacement of Sumerian by Akkadian [13].

This process was virtually complete by the early Old Babylonian period (*ca.* 1900). Gelb's assertion that the fact that all known OB letters are written in Akkadian [14] "is the best evidence that Akkadian became the commonly spoken

[8] Kraus 98. But note the expression of the more conventional view on p. 91, where Nippur, just 12 miles south-east of Abu Ṣalabikh, is considered "wohl sicher" a Sumerian speaking area in the Sargonic period.

[9] *MAD* 2² 5; Kraus 83.

[10] *MAD* 2² 6 ff.; additions in J. Roberts, *The Earliest Semitic Pantheon* (Baltimore, 1972), p. 9 fn. 5.

[11] *MAD* 2² 11 ff.; *Genava* 8 (1960) 268 f.

[12] Kraus 99.

[13] The objections of Kraus (p. 20) to this type of hypothesizing, while unreasonably harsh, should be kept in mind.

[14] The archive studied by S. Walters in *YNER* 4, from the time of Abisare and Sumuel of Larsa (*ca.* 1900-1850), already manifests the Sumerian documents — Akkadian letters syndrome. But from the preceding century there are at least three Sumerian letter-orders datable to Ishbi-Erra, Ishme-Dagan, and Lipit-Ishtar of Isin (see Hallo, *BiOr* 26 172 for references and discussion), as well as one half-Akkadian letter-order from the same period (*BIN* 9 475, edited by Hallo, op. cit., 175). Thus, Gelb's generalization may not hold true for the reigns of the earliest Isin kings, who, in any case, are known for their conscious preservation of Ur III forms (see e.g., Edzard in Bottéro, op. cit., p. 175).

language of the country" [15] is still valid. These Akkadian letters were written for the same individuals represented by legal documents still composed, to a great extent, in Sumerian. Unless we are willing to assume that some letters were dictated in Sumerian and then translated into Akkadian and written down by a scribe [16], we must conclude that Sumerian was not spoken by anyone who sent letters, or at least, if they did speak Sumerian, they were bilingual. The survival of Sumerian in legal contexts, where phraseology is highly formalized, and not in epistolary contexts, which require greater flexibility of expression and hence greater linguistic competence, suggests that Sumerian was no longer a living language [17].

This extinction of Sumerian as a spoken language was the end result of a process that began with the first contact between Akkadian and Sumerian speakers [18]. Barring violent incidents, such as wholesale annihilation, deportation, or deliberate supression, language displacement is a slow process, and occurs when the bilingual community expands to include all members of one mother-tongue group, who then neglect to teach the mother tongue (here Sumerian) to their children. If this process was substantially complete by early OB, then it must have been well underway during the preceding Ur III period (*ca.* 2100-2000). In reviewing the evidence for this period, Kraus observes that "es scheint sich ein Staatsvolk, eine Nation akkadischer Sprach herauszubilden" [19]; his review of the Akkadian loanwords in Sumerian during Ur III gives the impression that Sumerian was a "tote Schriftsprache oder die absterbende Sprache einer Minorität" [20]. Kraus' reluctance to be decisive on this point (p. 96) seems unwarranted in light of his own presentation. Sumerian as a spoken language was in all probability dead or nearly so in Ur III.

The only counter-indication for this is the extraordinary amount of Sumerian literary composition in this period [21]. While it is difficult to imagine this kind of activity without roots in a mother-tongue community, it is not without historical parallel, as the history of Latin literature in Europe or the Hebrew renaissance in Spain amply demonstrate. In any case, this composition of Sumerian texts continues into OB and beyond, and cannot be used to justify the existence of a Sumerian speaking population group. Nor do the overwhelming number of Sumerian administrative and legal documents [22] and the comparatively miniscule number of Akkadian texts found from the Ur III period warrant the designation "Neo-Sumerian" for this period. The Ur III texts come almost exclusively from southern sites, where such texts were written in Sumerian prior to Ur III, and continue to be written in Sumerian in the Old

[15] *Genava* 8 (1960) 271.

[16] The reverse most probably did occur. Cf. Kraus 96 f.

[17] It continued to be spoken in scribal schools (see below), but as such was nobody's native language, but only an elite, acquired language without a mother-tongue group.

[18] Kraus 19.

[19] Kraus 89.

[20] Kraus 93.

[21] See, e.g., Hallo, *JAOS* 83 (1963) 167 ff. and *CRAI* 17 (1970) 116 ff.

[22] Whereas the absence of Sumerian letters in OB is taken to indicate that Sumerian is extinct as a spoken language in that period (see above), the presence of Sumerian letters in Ur III cannot be used as evidence that Sumerian was still spoken then. There are numerous historical examples for letter writing in a tongue other than the vernacular; Akkadian itself was used to write letters long after most, if not all, of the population spoke Aramaic.

Babylonian period. No Ur III texts have yet been published from northern sites, which, based on evidence of previous and succeeding periods, one would expect to yield Akkadian texts [23]. Gelb's view that "the Sumerian renaissance affected only the written language, while the country in general continued in the direction of total Semitization and elimination of Sumerian elements" [24] is valid, albeit on the conservative side [25]. By the end of the Ur III period, Akkadian was so solidly entrenched as the spoken language in southern Mesopotamia that it was able to resist the pressures of large numbers of Amorite speaking settlers on the one hand, and absorb any remnants of the Sumerian mother-tongue group on the other.

The question remains: how far back should the dominance of Akkadian as a spoken language in Sumer be projected? If Sumerian could be used almost exclusively for writing in Ur III, when spoken Akkadian was probably widespread, then the use of Sumerian in Sumer for the same purposes in ED III and the Sargonic period does not necessarily indicate that Sumerian was the dominant spoken language during those periods. The same may be said of the overwhelming number of Sumerian names found in southern documents [26]. Naming customs "survive even a total displacement of the language in which they originated" [27], and the use of Sumerian names in the south continued into Ur III [28], OB and beyond, although with a gradually increasing and eventually superior number of Akkadian names.

In the absence of evidence to the contrary, it does not seem unreasonable to posit a situation in which the displacement of Sumerian as a spoken language in Sumer was in progress in ED III [29]. The subsequent Akkadianization of

[23] Cf. Edzard in Bottéro, op. cit., 148, and Kraus 93 f.

[24] *MAD* 2^2 18; cf. *Genava* 8 (1960) 270.

[25] The term "renaissance" is misleading. We now know that many Sumerian compositions are very old, and there is evidence for vigorous literary activity in both the Early Dynastic (see most recently Biggs, *ZA* 61 [1971] 193 ff.) and Sargonic (see Hallo, *YNER* 3 1 ff.) periods. The tremendous number of Sumerian administrative texts do not indicate a revival of the language, but rather an enormous increase in bureaucratic documentation, using the language that had always been used for such purposes in Sumer.

[26] Note Biggs' suggestion in *Or* 36 (1967) 66, that the use of Sumerian and Akkadian personal names in early ED III might correlate with differences in socio-economic status.

[27] E. Haugen, *Bilingualism in the Americas* (University of Alabama, 1956), p. 104. One might argue that sentence names of the Sumerian type require some knowledge of the Sumerian language on the part of the giver of the name. But since Sumerian names were given in OB, when the likelihood of a Sumerian speech community is minimal, and Akkadian sentence names were given in later periods, in what must have been Aramaic speech communities, it must have been possible for such names to be given without the ostensible giver having a speaking knowledge of the language. Simple names (e.g., *lú-^{d}nanna*) were doubtless known and understood by all; lexically and syntactically more elaborate names may have been composed with the aid of a scribe or other person with the requisite knowledge of Sumerian personal names and their elements. In this respect, note that in his "Répertoire des termes entrant dans la composition des anthroponymes" (H. Limet, *L'anthroponymie Sumérienne* [Paris, 1968]), Limet lists only 152 items.

[28] Cf. the discussion of the distribution of Sumerian and Akkadian personal names in Ur III by Limet, op. cit., 49 ff., who would insist on some ethno-linguistic significance in the choice of language.

[29] Cf. note 53, below, for the reflection of ED bilingualism in Sumer in a literary text.

language use can be illustrated by the following table. The use of parentheses indicates a *significant*, but smaller amount of usage of the language in question.

Language Use in Sumer

	Speech	*Personal Names*	*Letters*	*Documents*	*Literature*
ED III	Sum./Akk. [30]	Sum. [31]	Sum.	Sum.	Sum.
Sargonic	Akk. (Sum.?)	Sum. (Akk.) [32]	Sum [33]	Sum.	Sum.
Ur III	Akk.	Sum. (Akk. and Amorite)	Sum.	Sum.	Sum.
OB	Akk. (Amorite) [34]	Akk. (Sum. and Amorite)	Akk. [35]	Sum. (Akk.)	Sum. (Akk.)

An assumed absence of language contact on the spoken level after ED III would explain why there is relatively little non-lexical Sumerian interference in Akkadian, and why what interference that has been postulated is found already in Old Akkadian [36]. This kind of interference in a living language is not imaginable without intensive contact between the affected language and speakers of the language that is the source of the interference. The contact produces a bilingual community through which the interference can pass from one language to the other [37]. Because of the absence of comparative materials for Sumerian, non-lexical Akkadian interference in early Sumerian is more difficult to detect [38]. Only with the adjustment of the case government of certain

[30] No estimate of the relative size of the mother-tongue groups is implied.
[31] For Akkadian names, see Kraus 83 ff.
[32] For the Akkadian names, cf. Kraus 88.
[33] For some Akkadian letters, see Gelb, *MAD* 2² 12.
[34] The question of Amorite as a spoken language in OB cannot be discussed here. Cf. *CAD* A s.v. *amnānû*; J. Renger ZA 61 (1971) 26 f.
[35] For Sumerian letters, see note 14, above.
[36] On the phonological level, the reduction of Semitic laryngals and pharyngals, supposedly under Sumerian influence (e.g. W. von Soden, *GAG*, p. 2 and A. Falkenstein, *Genava* 8 [1960] 303 f.; cf. E. Reiner, *A Linguistic Analysis of Akkadian* [The Hague, 1966], 33 f.), was already underway in Old Akkadian (*MAD* 2² 119 f.). Morphological interference resulting in the creation of the Akkadian "t-perfect" and ventive has been suggested by W. von Soden, *AS* 16 (1965) 103 ff., and W. Eilers, *Innsbrucker Beiträge zur Kulturwissenschaft* 14 (1968) 241 ff., has suggested that Sumerian influence may have led to the development of the Akkadian subjunctive. Sumerian interference on the syntactic level may have led to the placement of the Akkadian verb at the end of the sentence (von Soden, *GAG*, pp. 2 and 183; cf. Kraus 98 f.). While most of these suggested examples are plausible, the argument for Sumerian interference rather than strictly internal development is in no case conclusive.
[37] For the passage of interference from the speech of bilingual individuals into the language of an entire mother-tongue community, see U. Weinreich, *Languages in Contact* (The Hague, 1966), 11. For phonological and grammatical interference arising from bilingual contact situations, see ibid,. 14 ff., and M. Emeneau, *PAPS* 106 (1962) 430 ff. (this last reference supplied by E. Reiner).
[38] For the possibility of Akkadian interference in early Sumerian, see the remarks of W. von Soden, *AS* 16 (1965) 105. Intensive study of the forthcoming Abu Ṣalabikh tablets may shed light on this problem.

Sumerian verbs on the model of their Akkadian counterparts and the use of the Akkadian conjunction *u* in the inscriptions of Gudea and his predecessors [39], does the structural influence of Akkadian on Sumerian become apparent [40]. This begins after we assume Sumerian to be dead, for all intents and purposes, as a spoken language, and thus highly vulnerable to interference from the mother tongue (Akkadian) of the scribes who wrote it. Akkadian interference continues and increases in the Sumerian of the Ur III and Old Babylonian periods, although the amount of interference varies considerably, depending on the provenience, genre and date of any given text.

It is in the Old Babylonian period that the scribal schools begin to treat Sumerian as a foreign language [41]. Lexical texts, originally intended as aids in learning the writing system, were eventually transformed into language learning aids by the addition of an Akkadian translation column [42]. Similarly, the first explications of Sumerian grammar were composed [43]. Occasionally, Akkadian glosses are found in OB Sumerian literary texts, but their infrequency testifies to the high quality of Sumerian education in the OB schools, as does the paucity of bilinguals dating from this period. In contrast to subsequent periods, when Sumerian texts had relatively fixed Akkadian translations that often, if not always, appeared together with them on the same tablet, the rare bilinguals of the Old Babylonian period were school exercises or the work of individual and apparently less competent scribes [44]. This kind of "pony" was not needed by the great majority of literary scribes, who not only composed in and understood Sumerian, but spoke it in the schools as well [45], much as their analogues spoke Latin in the academies of medieval Europe. There is, however, no evidence at all for spoken Sumerian in Post-Old Babylonian schools, and we may suppose that Sumerian as a spoken language died out completely in its last bastion, the scribal school, a thousand or more years after it had begun to be displaced by Akkadian. It continued to be studied and written for another millennium and a half.

[39] A. Falkenstein, *AnOr* 29 (1950) pp. 42 fn. 3 and 81 ff.

[40] Note that the assumed phonetic shift in Sumerian at the end of Ur III (A. Falkenstein, *Das Sumerische* [Leiden, 1959], p. 25) can be more plausibly explained in terms of innovations in the Akkadian syllabary (J. Renger, *ZA* [1971] 61 31 ff.). Cf. also H. Sauren, *ZA* 59 (1969) 63 f.

[41] Note the exhortation to a scribe in the fictitious Sumerian letter in Letter Collection B 20:3 (F. A. Ali, *Sumerian Letters* [Ann Arbor, 1964], 153): *eme-gi$_7$-šè gú-zu na-ab-šub-bé-en* "Don't neglect your Sumerian!" Other OB references to learning or proficiency in Sumerian can be found in Gordon, *Sumerian Proverbs*, 2.47, 49 and 55, and the commentaries thereto. Additional references will be available in M. Civil's forthcoming *Sumerian Dialogues and Debates*.

[42] For a sketch of the development of the lexical texts, see A. L. Oppenheim, *Ancient Mesopotamia* (Chicago, 1964), 244 ff.

[43] Published in *MSL* 4.

[44] Cf. *ZA* 61 (1971) 7. The bilingual literary texts, which first appear in OB, did not necessarily develop from glossed texts (so e.g., Oppenheim, op. cit., 249), since there is a full-fledged bilingual among the earliest OB Sumerian literary texts yet descovered (Cros, *Tello*, 212 [AO 4322]; for the age of the text, see Thureau-Dangin in Cros, *Tello*, 198 and 201, and J. Krecher, *ZA* 58 [1967] 19). Rather, glossed texts and bilinguals were two separate methods developed by some OB scribes to aid their comprehension of Sumerian literary texts. I hope to deal with this problem in greater detail elsewhere.

[45] References to speaking Sumerian will be found in the forthcoming work of Civil mentioned in note 41, above.

Linguists who study bilingualism recognize that numerous variables are involved in any contact situation, and that facile explanations for the eventual dominance of one language and the extinction of another are inadequate [46]. Because of our ignorance of the socio-economic distribution of the Sumerian and Akkadian language communities [47], and our inability to estimate the relative size of the two groups at any point in history, we are hardly in a position to offer an explanation for the displacement of Sumerian by Akkadian. "Prestige" was hardly a factor here, since Sumerian was and remained a "prestige" language long after it had ceased to be spoken. Sheer numbers may have made the difference, though the only evidence for an assumed "inundation" [48] of Akkadians would be the eventual dominance of the language, and such circularity is not enlightening. One also reads that Akkadian eventually prevailed, or at least that the rise of Agade was made possible, because of the large-scale influx of other Semitic groups, notably "Old Amorites", into Mesopotamia during ED III [49]. The evidence for this influx has never really been presented, and in any case, it is difficult to imagine that massive immigration of Amorite speakers into a predominantly Sumerian speaking area would result in Akkadian dominance, any more than Swedish settlement in Belgium would strengthen the position of Flemish in Walloon strongholds, or the immigration of large numbers of Brazilians into Canada would result in the increased use of French in traditionally English speaking areas [50]. The underlying premise of such explanations of Akkadian dominance is "all Semites stick together", which any casual observer of the contemporary, or, for that matter, ancient Near East knows is absurd [51].

Kraus, in summarizing the textual evidence for Sumerians and Akkadians, correctly insists that the difficulty in even finding designations for Sumerians and Akkadians in Pre-Ur III texts, and the vague usage of these terms from Ur III on, make the existence of competing or antagonistic ethno-linguistic

[46] Weinreich, op. cit., 106 ff.; Emeneau, op. cit., 432 ff.; L. Bloomfield, *Language* (New York, 1933), 463 ff.

[47] But cf. the remarks of Biggs, referred to in note 26, above.

[48] In view of the universality of the flood motif, one must be highly skeptical of the attempt to interpret that motif in Mesopotamia as a metaphor for hordes of invading Semites (A. Falkenstein in Bottéro, op. cit., 51, followed, but modified by Van Dijk in S. Hartman, *Syncretism* [Stockholm, 1969], p. 179).

[49] W. von Soden, *WZKM* 56 (1960) 185 ff. and *Iraq* 28 (1966) 144. Kraus, p. 21, is also inclined toward this view.

[50] Moving from hypothetical examples to fact, the recent expansion of speakers of one Germanic language (German) into Eastern Europe led to the virtual extinction there of another Germanic language (Yiddish) and its mother-tongue community of many millions.

[51] The relative closeness of two Semitic languages and their remoteness from Sumerian would play little or no role here. An immigrant group would learn the language that would be most useful to it; that is to say, language use would be determined by socio-economic rather than linguistic criteria. In any case, it is not certain that languages close to one's own are more easily learned that those that are more remote (V. Vildomec, *Multilingualism* [Leiden, 1963], 19 ff.), and for children learning Sumerian in a contact situation, it would be irrelevant. Turkish, a language frequently compared to Sumerian, was learned as a second language by many speakers of both Semitic and Indo-European languages during the Ottoman period.

groups in early Mesopotamia highly unlikely [52]. The historical memory of that land reached back to the days of Enmerkar, Lugalbanda and Gilgamesh, and nowhere in the literature concerning the Pre-Ur III history of their land did the Mesopotamians reveal even a trace of ethno-linguistic conflict [53]. Certain aspects of this problem. notably the interpretation of the Curse of Agade, will be discussed elsewhere. Let it suffice here to indicate that "Gilgamesh and Agga" [54], the only Sumerian text dealing with the Early Dynastic north-south political division and conflict, never suggests *any* ethno-linguistic differences between the protagonists or their followers. And it is not unthinkable that in their confrontation beneath the walls of Uruk, Gilgamesh and Agga addressed each other in an earlier form of the Old Akkadian language that we know through the masterly works of Professor Gelb.

[52] Thus vindicating and extending the conclusions reached by Jacobsen in his seminal article in *JAOS* 59 (1939) 485-95, now reprinted in *Toward the Image of Tammuz and Other Essays on Mesopotamian History and Culture* (Cambridge, Mass., 1970), 187-192.

[53] Consciousness of language difference is manifest in the famous passage in Enmerkar and the Lord of Aratta 141 ff. (S. N. Kramer, *Enmerkar and the Lord of Aratta* [Philadelphia, 1952], p. 14; new texts courtesy Sol Cohen):

u_4-ba kur šubur^{ki} ḫa-ma-zi^{ki}
eme-ḫa-mun ke-en-gi kur gal me nam-nun-na-kam
ki-uri kur me-te gál-la
kur mar-tu ú-sal-la ná-a
an-ki nigín-na un sag-sì-ga
^{d}en-líl-ra eme-aš-àm ḫé-en-na-da-ab-du$_{11}$

"In those days, the mountain lands Šubur and Ḫamazi,
... -tongued Sumer, great mountain of princely *me*'s
Akkad, the mountain that has all that is befitting,
The mountain land of Martu, reposing securely,
The whole world, the tended people,
Spoke together to Enlil in one language".

If *eme-ḫa-mun*, whether translated "discordant tongued" or "harmonious tongued" (see the bibliography given by A. Sjöberg, *TCS* 3 83), is understood as referring to two languages, not simply to Sumerian alone, then it would reflect Sumero-Akkadian bilingualism in Sumer proper in ED II (see already W. von Soden, *BiOr* 16 [1959] 132; for another possibility, see Sjöberg, loc. cit.) If this later reflection of Sumer's "heroic age" is accurate, it would support the position taken in this paper that Akkadian began to displace Sumerian in ED III.

[54] S. N. Kramer, *AJA* 53 (1949) 1 ff. Add *TuM* NF 4 5 and 6, 3NT 487, CBS 15164, and N 1250 (unpublished texts courtesy M. Civil). Most relevant literature is quoted by A. Falkenstein, *AfO* 21 (1966) 47 ff. A new edition is being prepared by A. Shaffer.

Autonomy and Nomadism in Western Asia

M. B. ROWTON – Chicago

A polity which forms part of a sovereign state will be termed here autonomous. In so far as the situation is accepted by the population, or at least its ruling elite, autonomy implies some measure of divided loyalty. There is loyalty to the autonomous polity and loyalty towards the sovereign state. When autonomy is not merely a matter of subjection by force, as is often the case in its initial phase, this divided loyalty is its most characteristic feature [1].

Dormant since feudal times autonomy seems to be reviving as a major factor in world history. In Eastern Europe it is already taking shape within the U.S.S.R. and, in the form of limited sovereignty, within the Soviet block of nations. In Western Europe autonomy seems ultimately inevitable if federation is to be a success.

This means that we historians must be prepared for renewed interest in the subject. In the history of Western Asia, except for the Hellenistic and Roman periods, we have little to fall back on by way of previous research. Some work has been done on the Islamic period, notably an outstanding contribution by Cahen; for the Ancient Orient we now have a pioneer study by Oppenheim [2].

Both these studies deal exclusively with autonomy in the city. Yet the cities are only part of the story. A short journey beyond the city walls, throughout much of the history of Western Asia, the home of autonomy has been the tribe, its champion the nomad. The fierce loyalty of the tribesman for his tribe is one factor, another is the nature of the physical environment. Most of the pastoral country, whether in mountain or steppe, could hardly be kept under really effective military control. The situation changed only with the advance in military technology which began with the introduction of the breech-loading repeating rifle and the machine gun.

[1] As used here the term "polity" denotes the political infrastructure of a social group or system: both the social group as such, in its political aspect, and also the authority structure within that group. We shall be reverting to the subject in the sixth article, of the present series, once all the evidence has been seen.

This usage is close to Smith's and Belshaw's, though with some reservations. For the relation between polity and authority structure see G. M. Smith in the *International Encyclopedia of Social Sciences* 12 (1968) 197-199; for the polity as one of three basic aspects of a social system, the political as compared to the cultural and economic, see C. S. Belshaw, *The Conditions of Social Performance* (New York 1970), p. 1-4. Smith notes that the concept of polity tends to encompass both the group itself and the group's characteristic political structure.

[2] C. Cahen, "Mouvements populaires et autonomisme urbain dans l'Asie Musulmane du Moyen Age", *Arabica* 5 (1958) 225-250; 6 (1959) 25-56, 233-265; published also as a monograph. A. L. Oppenheim, "A New Look at the Structure of Mesopotamian Society", *JESHO* 10 (1967) 1-16.

Not only did the more powerful tribes and tribal confederations constitute autonomous polities within the sovereign state, the tribal system itself had a similarly autonomous pattern of tribes within a tribe. The town also formed part of this pattern, in the form of urban autonomy in a nomadic environment. A number of problems are involved. It will not be possible to deal with all of these in a single article since detailed discussion of the evidence will be necessary. There will be a total of six articles, with the first three now ready for publication [3].

Nomad and sedentary, tribe and town, tribal confederation and state, all have coexisted in Western Asia for thousands of years, often in close interaction. Yet little has been done to study relations between them. A preliminary study, such as we shall be undertaking in these six articles, should throughout aim at keeping in view the problem in its fullest proportions, both in time and in space. It should also take full account of the physical environment, always a vital factor where nomads are concerned. This means that the problem involves the history of several civilizations, sources in several languages, also anthropology, ecology and geography. It therefore exceeds the competence of any single writer. But someone, from whichever field it be, has got to get the discussion started. That is the main object in these six articles, not proof of a solution, but formulation of a problem in the hope of stimulating interest on the part of experts in various fields of study.

The present article is introductory. Texts will be quoted only where it is necessary to do so in order to bring a new problem into clear focus. Here, the main purpose is to show how different aspects of the problem fit together. In later articles these several basic factors will be discussed separately and in detail.

I. The Enclosed Nomadism of Western Asia

Historical research should start from topology. Topology is the effect of the physical environment on the history of a given region [4]. As such it represents the area in which the fields of history, geography and ecology overlap. It is best therefore to have for it a term which does not explicitly relate to any of the three, as would be the case with a term such as "historical ecology". The latter would place undue emphasis on the ecological factor, and topology is concerned with issues which far transcend the accepted bounds of ecology. For instance most ecologists would probably be dismayed at the prospect of having to study military strategy, tactics and technology, yet that is precisely one of the fields in which the effect of the physical environment is most acutely felt. Nomadism is another.

The terms "Western Asia" and "Southwest Asia" were for some time used synonymously. Geographers have now opted for the latter term; the former becomes therefore available for new and more specific use. Here we propose

[3] See further p. 257 f. below.

[4] For a discussion of topology as one of several factors in a specific problem see M. B. Rowton, "The Topological Factor in the *Ḫapiru* Problem", *AS* 16 (1965) 375-387; for preliminary comment on the physical environment in relation to nomadism in Western Asia see "The Physical Environment and the Problem of the Nomads", *RAI* 15 (1967) 109-121. For the section in that article dealing with the woodlands see subsequently "The Woodlands of Ancient Western Asia", *JNES* 26 (1967) 261-277.

to use it for the western part of Southwest Asia; as such it also denotes the part of Asia which lies furthest west.

Draw the shortest line from the Caspian to the Persian Gulf; between the latter and the Gulf of Aqaba continue along a line which marks the limit to which sheep can be taken on seasonal migration into the arid steppes. Looking at it this way Southwest Asia consists of two very different parts: to the east oasis country, to the west river country. In the eastern part the Persian Gulf separates two great oasis regions. To the west lies "Western Asia" in the sense in which that term will be used here, namely to denote the fluvial region of Southwest Asia as distinct from its two oasis regions.

Thus defined Western Asia represents one of the most remarkable regions on the face of the globe. It joins three continents: Europe, Africa and Asia. In the huge arid belt which runs from China to the Atlantic, and which represents the global habitat of the pastoral nomad, Western Asia has the only significant concentration of rivers. Five inner seas converge on it: the Caspian Sea, the Persian Gulf, the Red Sea with the Gulf of Aqaba, the Mediterranean with the Gulf of Issos, the Black Sea [5]. In Western Asia, between these five seas, western civilization was born and nurtured.

More comprehensive discussion of the topology of Western Asia will have to be postponed to a later publication. In these six articles attention will have to focus only on one of Western Asia's singularly characteristic features. In much of that region, inland from the coastal ranges, agricultural land and pastoral land were closely interwoven. Great cities, nomads roaming the countryside — civilization tended to oscillate between these two extremes. Sometimes the urban morpheme was upmost, sometimes the pastoral. Full exploitation of the environment's economic potential was possible only where a concerted effort on the part of nomad and farmer took the place of their traditional rivalry. Hence a built-in tendency towards symbiosis is profoundly characteristic of nomadism in Western Asia.

In Western Asia, therefore, the usual distinction between the realm of the nomad and that of the sedentary does not apply. In many areas the pastoral land was encircled by urban settlement, either partly or completely; the grazing lands visited by the nomads constituted enclaves partly or completely within the sedentary zone. This enclave nomadism needs to be sharply distinguished from nomadism in the great open steppes of Central Asia or the deserts and the arid steppes of Arabia. It is best to think of nomadism in those two regions as external nomadism. Lattimore devoted a lifetime of exploration and research to the interaction between nomad and sedentary in the frontier regions of China. He notes that nomadism in the regions we are concerned with here is very different from nomadism in Central Asia, "... a major phenomenon being that pastoral nomads were ... enclosed in blocks of desert, semi-desert and highland country within the general sweep of civilization" [6]. He goes on to speak of "excluded nomads" and "enclosed nomads". Following his lead we shall use the term "enclosed nomadism" here.

[5] For brief comment on the importance of these five seas in historical geography see W. C. Brice, *South-West Asia* (London 1966), p. 20 ff.

[6] O. Lattimore, *Studies in Frontier History* (1962), p. 487.

II. Seasonal Migration and the Political Aspect of Enclosed Nomadism

Some years ago Oppenheim suggested that the anthropological approach to history is well worth exploring [7]. That approach amounts essentially to comparing different regions, different periods, or both. An instructive recent example is the comparison of early Mesopotamian civilization and the civilization of Prehispanic Mexico by Adams [8]. Comparison of Mari and early Israel is another example; a host of writers have touched on it in varying detail and in widely scattered comment.

Here, in these six articles, we shall be comparing Mesopotamia, and the territories adjacent to it, in the second millennium B.C. and the second millennium A.D.; the reasons for concentrating on these two millennia will be explained in a later article. This anthropological approach to history involves, by its very nature, a potentially dangerous procedure. Comparison entails, to some extent at least, projection from a later to an earlier period. This can be useful but on one condition only. Nothing from the later period can be allowed in evidence as proof of a solution in the earlier period. Material from the later period should function only as a trail marker in unfamiliar territory, indicating the possible location of evidence, either entirely new or evidence the significance of which has not been appreciated.

Anthropology and history do not ask quite the same questions and cannot therefore expect quite the same answers. Take the tribe and the nomad. Anthropology has hitherto been mainly concerned with the tribe in relatively primitive conditions [9]. In that environment the tribe was seldom genuinely integrated with the state even in colonial times. Here we shall be concerned with tribes which form part of established, deeply rooted states within which they constitute autonomous polities. Our question then is what was the relation between tribe and state?

There has been very little discussion of this question even in political anthropology. More than twenty years ago, and that is a long time in political anthropology, Steward discussed the problems anthropology faced in moving from study of the tribe to the vastly more complex problem of the national state [10]. He notes that the tribes which anthropology had been dealing with were mostly independent. Yet he has no comment on the tribe within the state which, nevertheless, constitutes a vast unexplored field of research between the primitive tribe and the national state.

Nowhere has that kind of tribe played a greater role than in Western Asia. Its history extends over a period of several thousand years, long outliving its

[7] A. L. Oppenheim, "Assyriology – Why and How?" *Current Anthropology* 1 (1960) 409-423, particularly p. 419 = *Ancient Mesopotamia* (Chicago 1964), p. 29 f.

[8] R. McC. Adams, *The Evolution of Urban Society* (Chicago 1966).

[9] For the tribe as viewed by an anthropologist see M. D. Sahlins, *Tribesmen* (Englewood Cliffs 1968). The suggestion there that only acephalous tribes merit the name, as distinct from chiefdoms, does not apply either in Southwest Asia as a whole or in Western Asia. Consequently in these articles we shall be using the term "tribal chiefdom". So already, in speaking of the bedouin, Sweet in L. E. Sweet (ed.), *Peoples and Cultures of the Middle East*, 1 (New York 1970), pp. 265-289; e. g., p. 268: "tribal chiefdom structures of ranked lineages".

[10] J. H. Steward, "Levels of Socio-cultural Integration: An Operational Concept", *Southwestern Journal of Anthropology* 7 (1951) 374-390.

early rival, the city state. One of the most essential functions of the tribe has been its role in conditioning the loyalty of the child. Among the tribes of Western Asia this was mostly on the basis of putative kinship. Loyalty to the tribe came first, loyalty to the state a poor second. As mentioned at the very outset, this divided loyalty is profoundly characteristic of the autonomous polity. It was the tribesman's fierce loyalty to the tribe which enabled the latter to function as a primitive political institution within the sovereign state [11]. Thus we start not only from topology but also from political anthropology [12]. Writers in that field have not hitherto had much to say about Western Asia. One objective here is to enlist their help and interest.

As for nomadism, there too anthropology and history have different questions to ask. The anthropologist views nomadism primarily as an economic institution. He is concerned therefore with forms of nomadism, particularly the distinction between true nomadism and seminomadism. The answer has proved to be extremely elusive and the problem of definition is still not resolved [13]. The historian can safely afford to leave this issue to the anthropologist; his concern is different. It centers on the social and political effect of seasonal migration.

Enclosed nomadism includes both village tribesmen who spend a few months of the year out in the steppes or in mountain pastures, as well as nomads who live the year round in camp. The latter undertook seasonal migration over distances often comparable to the migration of external nomads such as the bedouin; two or three hundred miles was by no means uncommon [14]. Whether or not in the interval between these migrations the nomad lived in a house, shack or tent, whether during that interval he did or did not practice agriculture, these are not the crucial issues for the historian. It is the migration

[11] The term used here will be "gentilism", in the sense of "tribal feeling, devotion to one's gens"; cf. Webster's *New International Dictionary of the English Language* (Springfield, Mass. 1941), p. 1047. The roots of tribal autonomy are gentilism, topology and the tribe's military potential. Topology involves both seasonal migration and the rugged nature of the terrain, the latter a military factor.

[12] For an introduction to political anthropology see G. Balandier, *Anthropologie Politique* (Paris 1967); Engl. transl. by A. M. Sheridan Smith (1970); also E. A. Winckler, "Political Anthropology", in B. J. Siegel (ed.), *Biennial Review of Anthropology*: *1969* (Stanford 1970), pp. 301-392. The latter has an extensive bibliography, though largely confined to publications in English.

[13] For the problem of definition see P. C. Salzman, "Political Organization Among Nomadic Peoples", *Proceedings of the American Philosophical Society* 111 (1967) 115-131. Discussion is confined however to publications in English. For writers in other languages, whose contribution is at least equally important, see W. D. Hütteroth, *Bergnomaden und Yaylabauern*, *Margburger Geografische Schriften*, 11 (Margburg 1959), pp. 10-12. Salzman discusses the political effect of seasonal migration in so far as it has a bearing on the authority of the tribal chief. In these articles we shall be chiefly concerned with the effects of seasonal migration beyond the confines of the tribe.

[14] So for instance in recent times some of the Qashqai and Khamseh tribes in the southern Zagros; a few centuries ago Turkmen tribes in the Eastern Taurus. For these seasonal migrations see the following sketch maps: H. Field, *Contributions to the Anthropology of Iran*, (Chicago 1939), p. 201 (Qashqai); F. Barth, "The Land Use Pattern of Migratory Tribes of South Persia", *Norsk Geografisk Tidsskrift* 17 (1959-1960) p. 4 (the Basseri tribe of the Khamseh); X. De Planhol in L. Foldes (ed.), *Viehwirtschaft und Hirtenkultur* (Budapest 1969), map after p. 84 (the Boz Ulus confederation ca. 1540 A.D.).

itself that matters, not the question of habitation or agriculture. Hence the term "nomad", as used here, denotes any kind of pastoral nomad, provided the migration involves a tribal community and provided the nomads owned the livestock. Where only professional shepherds are present, in charge of livestock which belongs to sedentary owners, the migration represents transhumance, not nomadism. Hybrid forms blur the distinction; so for instance when professional shepherds join the migration of a tribal community or when the latter take with them livestock belonging to the nontribal sedentary.

Seasonal migration is important in all forms of nomadism. But in enclosed nomadism it leads through or into areas inhabited by the sedentary, raising therefore greater problems than it does in other forms of nomadism [15]. It is seasonal migration that was chiefly responsible for the close interaction between nomad and sedentary, tribe and state, which was so characteristic of enclosed nomadism. Its effects were both varied and widespread, with repercussions on the individual as well as the tribe. It could make or break a tribal chief [16], it could be the determining factor in a matrimonial alliance [17], it could profoundly affect tribal structure. The disputes and conflicts between nomad and sedentary, often arising from seasonal migration, would be attenuated or solved if both nomad and sedentary belonged to the same tribe. In the third article we shall see that tribes of this kind were indeed common in Western Asia.

Seasonal migration also had an impact on Mesopotamian history. The Mesopotamian alluvial plain, relatively narrow, is bordered on both sides by pastoral country. In the steppes, water and pasture are lacking in the summer season and then the sheep have to be brought into the sedentary zone or to its fringe. In the mountains lack of pasture and lack of stabling made it necessary to move sheep down to the fringe of the alluvial plain during the winter months.

Thus from both sides nomads seasonally penetrate right into the sedentary zone or to its fringe. When the power of the state was weak this ingressive migration could only too easily become aggressive. Mesopotamian civilization was particularly vulnerable, mainly because the irrigation system, under threat of siltation, required constant maintenance. If due to insecurity the farmers fled the countryside, sclerosis of the watercourses could be the outcome. Dislocation of the highly centralised administrative system due to insecurity and falling tax income would further aggravate the situation. We must therefore reckon with periods in Mesopotamian history that were marked by tribal penetration accompanied by a sharp recession of urban civilization [18].

The role of seasonal migration in Mesopotamian history goes back at least four millennia and perhaps even more. It is already a prominent factor in Mari. Although Mari has no word for nomad it has another closely related term, *nawû*, which occurs in widely varied context. As Edzard has shown, *nawû*

[15] For the problems involved in organizing seasonal migration and its bearing on the authority of the tribal chief see F. Barth, loc. cit., pp. 1-11 and especially *Nomads of South Persia* (Oslo 1961), pp. 79 f., 90, 127; also Salzman, loc. cit. for seasonal migration in external nomadism.

[16] F. Barth, op. cit., (1961), pp. 84-90, especially 89 f.

[17] W. Eberhard, *Settlement and Social Change in Asia* (Hong Kong 1967), p. 318.

[18] See already M. B. Rowton, "The Role of the Watercourses in the Growth of Mesopotamian Civilization", *AOAT* 1 (1969) 307-316, especially 311 ff. We shall be reverting to the subject, after all the evidence has been seen, in a later article.

denotes a migratory group, the group which undertook seasonal migration, a curiously collective concept denoting tribesmen, livestock and camp [19]. At that time in Babylonia proper the usage was somewhat different. The meaning there reflects the effect which seasonal migration had beyond the confines of the tribe; *nawû* denotes the countryside between the cities, including settlements, villages and livestock.

In enclosed nomadism seasonal migration could even become a matter of direct concern to the state, so for instance when the office of paramount chief had been abolished. Thus in Mari, under Jahdunlim and Zimrilim, there was no paramount chief among the Haneans. The result was that the Mari kings, as well as some of the vassal rulers further afield, became closely involved in the problems arising from seasonal migration. We find these urban dynasties concerned with the safety of the migratory group, the *nawû*, and they are also involved in mediating disputes and conflicts between the Haneans and the sedentary population. It is in these Mari archives that enclosed nomadism first emerges clearly into the limelight of history, nearly four thousand years ago, and it is significant that the political aspect of enclosed nomadism should be so clearly in evidence already then.

Seasonal migration is only one factor. Another was sedentarization, which will be discussed, in preliminary form, in the next section of the article. In enclosed nomadism the joint effect of these two factors was considerable; like seasonal migration the process of sedentarization also had an impact on social and political structure beyond the confines of the tribe. Conversely, enclosed nomadism was also conditioned in some measure by social and political structure outside the tribal system, among the sedentary. The effect was mutual. To quote Lattimore: "In addition to the anthropologist's differentiation of types of nomadism, what is needed is a study of the variations of each type under historical conditions of change and in contact with varying types of sedentary Society" [20].

Enclosed nomadism encompasses types of nomadism in closest contact with sedentary society. This brings us to one of the most difficult aspects of the problem, the link between the nomad and the town. As we have seen, seasonal migration could be one factor. More commonly the dominant factor was sedentarization; but sedentarization of a very special kind which in no way implies that all of the nomads in a tribe were in the process of sedentarizing.

III. The Link between the Nomad and the Town

Enclosed nomadism in modern times has been extensively studied only by Barth [21]. One of his most interesting observations concerns sedentarization [22].

[19] D. O. Edzard, "Altbabylonisch *nawûm*", *ZA* 53 (1959) 168-173.

[20] Lattimore, op. cit., 487, n. 14.

[21] Barth himself does not use the term "enclosed nomadism". But his work in the field, both in Western Asia and in Pakistan, concerned nomads who fall within that category, in the sense in which the term is used here. There will be frequent reference to Barth in these articles; see already nn. 14, 15, 16 above, 22, 26 and 34 below.

[22] F. Barth, op. cit., (1961), pp. 101-128; also "Capital, Investment and the Social Structure of a Nomadic Group in South Persia", in R. Firth and B. S. Yamey (eds.), *Capital, Saving and Credit in Peasant Societies* (London

A process of continuous sedentarization has to be recognised as a normal aspect of nomadism. The richest and the poorest among the nomads tend to sedentarize: the richest, when the size of their flocks exceeds the capacity of the grazing land available to the tribe; the poorest, when loss of livestock reduces their flock below the minimum needed to support a family. There are other secondary reasons which we need not go into at this point.

This affects social and political structure in a number of different ways. Sedentarizing members of the tribal elite who become landowners, officials, or military officers form a link between the nomads and the town; some even may function as hostages for the good behaviour of the tribe. Or a tribal chief might become simultaneously the ruler of a town, or conversely, an urban dynasty might come to function as chiefs of a tribal confederation.

Since the capital city was seldom involved, this usually took the form of urban autonomy in a nomadic environment. However, sometimes the town in question did function as an independent capital and the dynasty was fully sovereign. In that case one has to speak of a state, not a chiefdom. Instances of this kind have been rare in the second millennium A.D. but the evidence suggests that in the second millennium B.C. states of this kind were more common. The outstanding example is of course Mari, on which a good deal of information is available. Elsewhere, though the evidence on the nomadic and tribal factor is not as strong as in the case of Mari, it does nevertheless suggest a similar pattern.

For example, the kings may be referred to, whether by title or not, in such a way as to indicate that they were rulers of a territorial state but that at the same time they were viewed as the paramount chiefs of a tribe or tribal confederation. So for instance Mari and the Haneans, Kurda and the Numhâ tribe, Larsa and the Amorites, particularly the tribe of Jamutbal, probably also Babylon and Amnānum.

Sedentarization of the particular kind envisaged here could have still other effects. It could lead to part of a tribe being fully nomadic, part fully sedentary; it would tend to augment social stratification; and there is also the military factor, for the tribe was a paramilitary unit in a permanent state of partial mobilization. This intricate urban-pastoral pattern represents the political aspect of enclosed nomadism. Nowadays not much of it is left and anthropologists, such as Barth, have had little to say about it. The main reason is that tribal autonomy is declining sharply, the tribe's military potential no longer counts.

But things were very different only a century ago. We shall be comparing, and also contrasting, the situation in recent centuries and in the Old Babylonian period. There is a difference both in the amount and in the quality of the evidence. The Old Babylonian evidence seldom, in either respect, amounts to proof. Often all that can be said is that the recent material suggests a plausible solution, that the ancient material is compatible with that solution, but that more evidence is needed.

Urban autonomy in a nomadic environment is the single most important factor. The whole of the second article is devoted to that subject. Here we

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1963), p. 69-81, especially 77 ff.; Stauffer, "The Economics of Nomadism in Iran", *Middle East Journal* 19 (1965) 284-302, especially 292 ff.

shall glance briefly at some of the other factors involved. The object is to focus attention both on the interest of comparing material ancient and recent, as well as on the element of uncertainty inherent in the method. Space will allow only three examples, three texts of the Old Babylonian period, all carefully chosen to bring out more than one of the social and political aspects of enclosed nomadism. The texts in question are *ARM* II 1, *ARM* VIII 11 and TA 1930, 615, the latter one of the many important texts published by Professor Gelb [23].

Conclusive evidence is available only on one issue, namely the tribe which consists in part of nomads, in part of sedentary. For *ARM* VIII 11 explicitly distinguishes between a tribal group resident in a town and another group, belonging to the same tribe, which was with the migratory group (*ḫibrum ša nawîm*). Moreover this is no casual distinction, *ARM* VIII 11 is a legal text; it is discussed in greater detail in the third article [24].

Social stratification within the tribe is reflected in all three texts, and significantly, each refers to a different tribal people: Haneans in the Šubat-Enlil region of Northern Mesopotamia (*ARM* II 1), Jaminites in the Mari region of the Euphrates (*ARM* VIII 11), Amorites in the Ešnunna region of the Diyala (TA 1930, 615). The fact that different tribal peoples are mentioned over a sizeable area suggests that this feature of tribal structure is likely to have been widespread at that time.

In each case different terms are used to denote the tribal elite. In *ARM* VIII 11 they are called *ša* ŠÀ.GA.DU.MEŠ *maḫrū* "who (as) ŠÀ.GA.DU are foremost". In TA 1930, 615 they are summed up as MAR.TU *ellūtum*.ME "Amorites of noble descent". Most explicit is *ARM* II 1. There distinction is made between those among the Haneans who are well off (*mārū awīlī damqūtim*), whose fathers are resident in a town, and those who are able bodied but poor (*awīlū eṭlūtum lapnūtum*). Moreover the text specifies that the latter could even be destitute (*naqdū*) [25].

We thus have to reckon with the probability that in the Old Babylonian period continuous sedentarization may have had an impact not only on tribal structure but also on social structure beyond the confines of the tribe. Hoary problems such as the *ḫapirû* and the *awīlu – muškênu* problem will have to be re-examined. Note for instance that the Old Babylonian term for the social elite, *mār awīlim*, is, to say the least, reminiscent of the term *mārū awīlī damqūtim* in *ARM* II 1 above.

Sedentarization, or partial sedentarization, may also be due to quite different factors. For instance, the state may force or encourage members of the tribal elite to reside in a town. Even the chief of a nomadic tribe may have an urban residence; for with a stake in city life and in city property a prominent tribesman would be more amenable to government policy. Moreover there would be someone ready at hand to act as intermediary with the tribe [26].

[23] I. J. Gelb, "An Old Babylonian List of Amorites", *JAOS* 88 (1968) 39-46.

[24] Cf. G. Boyer, *ARMT* 8 (1958) 190 f.; A. Malamat, "Mari and the Bible: Some Patterns of Tribal Organization and Institutions", *JAOS* 82 (1962) 146.

[25] *[n]a-aq-du-ú* LÚ.MEŠ *eṭ-lu* (text LI)-*tim la-ap-nu-tim a-na-ku i-na* É.GAL-*lim dam-qí-iš a-pa-qí-id* "They can (even) be destitute! The able bodied poor men I will provide for well in the palace". *ARM* II 1:18-20. So also *na-aq-du-ú* in *ARM* V 35:37. Both passages quoted *AHw*, 743a.

[26] On the role of the tribal chief as an intermediary between the tribe and the authorities of the state see Barth, op. cit. (1961), pp. 71-90, 96 f.

Chiefs or members of their families would often be held formally or informally as hostages, sometimes even organized in military units. Thus in the centuries preceding the Islamic conquest Al-Hira provides an interesting example of urban autonomy in a nomadic environment. There, tribal hostages were organized into a military unit [27]. The situation is probably similar in TA 1930, 615. There, Amorite tribesmen reside in the city of Ešnunna. They are members of the tribal elite. They are organized into sections (*babtu*). Most significant is the fact that, with one exception, their own names are not stated. Instead, each is designated by the name of his father. An obvious explanation could be that these Amorites were sons of prominent tribesmen held unofficially as hostages for the good behaviour of their fathers.

Now in these three texts social stratification is clearly in evidence; hostages are less certain but probable. However there is no direct evidence that nomadism, and hence sedentarization, was the underlying cause. But since Akkadian had no unambiguous term either for nomad or for sedentary explicit evidence could hardly be expected. Moreover, because of lack of pasture and stabling during certain seasons of the year, nomadism had deep roots in Western Asia; and furthermore, repeated reference to the migratory group, the *nawû*, shows that nomadism was indeed a factor in the Old Babylonian period. Hence to conclude that nomadism was absent because there is no term for nomad would be almost as illogical as to postulate that the sedentary were absent because there is not a term for them.

The absence of terms for nomad and sedentary merely reflects the fact that in enclosed nomadism the distinction between the two is much less sharp than in external nomadism. In enclosed nomadism there is an inherent tendency towards symbiosis, an aspect of the problem we shall be reverting to in a moment. Thus the tribes often included both nomads and sedentary in the same tribe and this tended to blur the distinction between the two. In the Ancient Orient tribes and tribal peoples were felt to be foreigners, as Buccellati's recent study of the Amorites clearly shows [28]. That was the distinguishing factor, not whether they were nomads or sedentary.

The link between the tribe and the town is reflected even in tribal names. In marked contrast with bedouin nomadism, it is not uncommon in enclosed nomadism for town and tribe to share a name in common, with the tribal name in the form of a gentilic. Examples are, among Turkmen tribes in Western Asia: the Bagdadī, the Baharlū and many others [29]; among tribes of Iranian origin

[27] Cf. M. Kister, "Al-Ḥira", *Arabica* 15 (1968) p. 166 f.

[28] G. Buccellati, *The Amorites of the Ur III Period*, (Naples 1966), pp. 336-339. The use of the term MAR.TU as a gentilic with reference to sedentary Amorites resident in Babylonia strongly suggests that even these continued to be regarded as foreigners. The term "foreigner" is used here in the sense of "different from the local population", not "coming from a foreign land". The Amorites will be discussed in a later article.

[29] For the Baharlū see V. Minorsky, "The Clan of the Qara-Qoyoulu Rulers", in *Mélanges Fuad Köprülü* (Istanbul 1953), p. 391-395. For the Baghdadī see H. Field, op. cit. (1939), p. 171. Minorsky emphasizes that this type of toponymic tribal name was common among the Turkmen of Western Asia; cf. V. Minorsky, *Taḏkirat al-Mulūk*, "A Manual of Ṣafavid Administration", *E. J. W. Gibb Memorial Series*, NS 16 (London, 1943), p. 194 with n. 2.

whether Kurd or Lur: Āwī, Dumbelī, Garisān, Gurgillū, Ḥālaba, Humeidī, Kerkukī, Mihrāuī, Silaḫur [30]. Except for the Bagdadī and the Kerkukī these are all names of small towns and villages.

Brinkman has shown that tribal names of this kind are attested also among the Aramean tribes nearly three thousand years ago; so the Rapiqu, Hiranu, Rabilu, Hudadu, Radê, Karma', Amlatu, Amatu; perhaps also the Naṣiru and the Adilu [31]. There is even an indication that in the first millennium B.C. the Assyrian scribes may have thought that most tribal names were town names. For in a number of instances they wrote Chaldean and Arab tribal names with the determinative URU for town [32], just as the Mari scribes often used the determinative KI after tribal names. We do not know what the role of nomadism was among the Aramaens and the Chaldeans, but there is no doubt that it was significant among the Arabs and in Mari [33].

By and large the pattern of the evidence can be said to reflect symbiosis between nomad and sedentary as well as a link between the nomad and the town. The link between nomad and town may go back far beyond the Mari period. It may well have been a factor already in prehistoric times, or at least in protohistory. We shall be discussing in a later article the evidence which suggests that the Amorites included nomads, and the Amorites appear on the scene only several centuries after the end of the protohistoric, period. Hence it may be no coincidence that a term which in Akkadian means "town" in Hebrew means "tent".

To sum up then, enclosed nomadism involves four basic factors. First, the town in nomadic territory, highly characteristic of this form of nomadism. The town acts as a link between the nomad and the state. This aspect of the problem is discussed in the second article. It is to be published under the title "Urban Autonomy in a Nomadic Environment" in a special issue of *JNES* commemorating the late Professor K. Seele.

Second, seasonal migration. Because the migration goes through, or into, regions inhabited by the sedentary, it is chiefly responsible for close inter-

[30] For the Dumbelī, Humeidī, Gurgillū, Kerkukī, see Charmoy's translation and commentary on Bitlisī: F. B. Charmoy, *Chèref-Nâmeh*, 1/1 (St. Petersburg 1868), pp. 172, 225, 592, 150, 52; for the Dumbelī cf. also B. Nikitine, "Les Afšārs d'Urumiyeh", *JA* (1929) 110; the Garisān: Hütteroth, op. cit., (1959), p. 49 f.; Ḥālaba, Mihrānī, Āwī P. Schwarz, *Iran im Mittelalter*, (Stuttgart 1896), p. 861; Silaḫur: K.-M. Röhrborn, *Provinzen und Zentralgewalt Persiens im 16. und 17. Jahrhundert*, in *Studien zur Sprache, Geschichte und Kultur des islamischen Orients*, NF 2 (1966) 80, n. 512. Schwarz notes that names of this kind were not uncommon among the Kurdish tribes.

[31] J. A. Brinkman, *A Political History of Post-Kassite Babylonia*, (Rome 1968), pp. 271 f. and 396.

[32] For the Arab tribes of the Neo-Assyrian period cf. the determinative for town, URU, which precedes a number of tribal names, e. g., ^{URU}*Ba–da–na–a–a*, ^{URU}*Ha–a–a–ap–pa–a–a*, ^{URU}*Ḫa–at–ti–a–a*, ^{URU}*I–di–ba–'i–il–a–a*, ^{URU}*Sa–ba–'a–a–a*, ^{URU}*Ma–as–'a–a–a*. For a brief discussion of these names see W. F. Albright, "The Biblical Tribe of Massa' and Some Congeners", *Levi della Vida Volume* I (1956) 1-14, especially p. 3 f.

[33] The Arabs in the Neo-Assyrian period were not of course true bedouin, they were still in the "proto-bedouin" phase. On the latter see W. Dostal, *Die Beduinen in Südarabien*, in *Wiener Beiträge zur Kulturgeschichte und Linguistik*, 16 (1967) 11-24, 149-165; Henninger in L. Földes (ed.), *Viehwirtschaft und Hirtenkultur* (Budapest 1969), p. 35-38.

action between nomad and sedentary, tribe and state. Again, a very characteristic feature of enclosed nomadism.

Third, the tendency towards symbiosis. Enclosed nomadism has its roots in a characteristic physical environment, namely in regions where pastoral land and agricultural land are closely interwoven. There is therefore a natural tendency towards symbiosis between nomad and sedentary. As shown by Barth, the two economies complement one the other[34].

The tendency towards symbiosis finds its fullest expression in the tribe which includes both full time nomads and full time sedentary. As we have seen, *ARM* VIII 11 provides an example from the Old Babylonian period. This kind of tribe has received hardly any attention, either in anthropology or history. It is discussed in the third article in conjunction with seasonal migration and the *nawû*. Under the title "Enclosed Nomadism" the article is to be published in a forthcoming issue of *JESHO*.

Fourth, continuous sedentarization. Together with certain other, related factors this will be the subject of the fourth article, now in preparation. That study centers on the effects of these several factors on social and political structure beyond the confines of the tribe.

Two further articles will follow, each dealing with still a different aspect of the problem. Ultimately it is hoped to bring all six articles together, considerably amplified, in book form[35]. This raises the question why publish even part of a book in the form of articles? The answer is that it seems prudent not to be unduly optimistic in the matter of long range publication plans at a time when one is approaching the age of retirement.

One final remark. It is to me a matter of special pleasure that an article introducing the results of several years' work should appear in a publication honoring Professor I. J. Gelb. I owe him much by way of friendly encouragement and intellectual stimulation.

[34] F. Barth, "Nomadism in the Mountains and Plateau Areas of South West Asia", *Arid Zone Research* 18 (1962) 341-355; excerpted Rowton, op. cit. (1967), p. 115 f.

[35] Dr. J. A. Brinkman has very kindly agreed to read and comment on all six articles before publication.

Who Are all Those People? *

Johannes RENGER – Chicago

The last few years have seen a rising interest among Assyriologists and other scholars of ancient Near Eastern Civilizations in tackling problems of social structure and social institutions in the ancient Near East. Professor Gelb has been involved in these discussions from the beginning; it was a paper he presented at the XII[ème] Rencontre in London in 1963, through which he introduced the term "Onionology", as opposed to "Kuyunjicology" into Assyriological literature when urging scholars to concern themselves more intensively with the material culture and the social institutions of the ancient Near East [1]. What he also wanted to express on this occasion was his dissatisfaction with a certain attitude in Assyriology which can best be described as antiquarian in outlook. This particular antiquarian approach was certainly not restricted to the study of Mesopotamian society, for a similar attitude can be found in other areas of Assyriology. For instance, particular types of texts like Neo-Assyrian royal inscriptions, epics, myths, hymns and prayers, to name only a few, are used solely as source material, as a kind of quarry for all kinds of undertakings, legitimate as they may be. But these texts have seldom been studied in their own right. Investigations into their

* I had the opportunity to discuss the anthropological and legal problems with J. Fabian (Northwestern University, Evanston) and H. Petschow (University of Munich) and acknowledge with pleasure my indebtedness to them.

Besides the abbreviations used in *CAD*, the following abbreviations are used: CE = Code of Ešnunna; CH = Code of Ḫammurapi; CL = Code of Lipit-Ištar. Greengus, *Marriage Ceremonies* = S. Greengus, "Old Babylonian Marriage Ceremonies and Rites", *JCS* 20 (1966) 55-72. Greengus, *Marriage Contract* = S. Greengus, "The Old Babylonian Marriage Contract", *JAOS* 89 (1969) 505-32. Koschaker, *Eheformen* = P. Koschaker, "Die Eheformen bei den Indogermanen", Deutsche Landesreferate zum II. Internationalen Kongress für Rechtsvergleichung im Haag 1937 = *Sonderheft des elften Jahrgangs der Zeitschrift für Ausländisches und Internationales Privatrecht*, (1937) 77 ff. Koschaker, *Eherecht* = P. Koschaker, "Über einige Probleme des Eherechts im Lichte der Vergleichenden Rechtsgeschichte", *Deutsche Rechtswissenschaft* 4 (1939) 67 ff. Koschaker, *Eheschliessung* = P. Koschaker, "Eheschliessung und Kauf nach alten Rechten, mit besonderer Berücksichtigung der älteren Keilschriftrechte", *ArOr* 18/3 (1950) 210 ff. Koschaker, *Fratriarchat* = P. Koschaker, "Fratriarchat, Hausgemeinschaft und Mutterrecht in Keilschriftrechten", *ZA* 41 (1933) 1-89. Koschaker, *Studien* = P. Koschaker, *Rechtsvergleichende Studien zur Gesetzgebung Hammurapis* (Leipzig, 1917). Landsberger, *Jungfräulichkeit* = B. Landsberger, "Jungfräulichkeit: Ein Beitrag zum Thema 'Beilager und Eheschliessung' ", *Symbolae iuridicae et historicae Martino David dedicatae*, vol. 2 (1968) 41 ff.

[1] Cf. also his article "Approaches to the Study of Ancient Society", *JAOS* 81 (1967) 1-8.

typology, structure, literary history and other relevant problems are still lacking to a large degree.

While deploring this state of affairs, one has to make one exception: the field of legal history. Since Josef Kohler, a professor of Roman law, introduced cuneiform law into the study of comparative law [2], an ever-increasing amount of research on the legal history of Mesopotamia has been conducted. Most prominent among those who participated in this endeavor were Paul Koschaker and Marian San Nicolò. Through their influence the study of cuneiform law is the one discipline within Assyriology which was for a long time well ahead of all the others in its methodology and achievement. Thanks to the exacting and rigorous standards set by these scholars who received their training in another discipline, Roman law, most of the monographic studies written by them and their pupils — in quite a number of cases as much as fifty years ago — are not really dated, even considering the numerous new finds made in the meantime. This cannot be said, for instance, of the early attempts to deal with Mesopotamian religion or literature.

Since this volume is concerned with new methods in ancient Near Eastern Studies, it is fitting to remain for a moment with the subject of legal history and Paul Koschaker. Koschaker, being a scholar of jurisprudence, approached a text under the assumption that a legal case or prescription has, by definition, to have a remedy. Grammatical, lexicographical, and other ambiguities may obstruct an immediate understanding. To overcome these obstacles, Koschaker often employed most successfully the comparative method. His writings are ample evidence for his extensive use of data provided by comparisons to other systems of law [3]. This becomes particularly apparent in his writings on family and marriage law [4]. Still another fact emerges here which has a direct bearing on the present discussion: One of Koschaker's most prominent articles in this field, "Die Eheformen bei den Indogermanen", was delivered in the section "Ethnologie juridique" of the II. International Congress for Comparative Law at The Hague, The Netherlands, in 1937. Not only on account of this one contribution, which in no way represents an exception to Koschaker's approach to the problems of family and marriage law, we must credit him with introducing social anthropology or, better, ethnography as a viable means for solving the problems which confront students of ancient Near Eastern civilizations [5]. And it does not diminish the importance of this earlier

[2] See J. Kohler and L. Wenger, *Allgemeine Rechtsgeschichte*, Erste Hälfte: *Orientalisches Recht und Recht der Griechen und Römer* (Leipzig-Berlin, 1914); this volume also deals with the laws of "primitive" peoples.

[3] Koschaker, *Eherecht*. Note especially p. 70 with note 3 where he states that he could not have written his "Babylonisch-Assyrisches Bürgschaftsrecht" without having had recourse to studies on comparable institutions of the old Germanic laws.

[4] Koschaker, *Studien*; Koschaker, *Eheformen*; Koschaker, *Fratriarchat*.

[5] But note for his critical approach his critique of the Vienna School (P. W. Schmidt and his Kulturkreislehre). The reader's attention should be drawn to the works of two scholars in related fields: W. Eberhard, the Sinologist, has employed a variety of methods from the realm of the social sciences in elucidating many aspects of Chinese social history. S. Morenz, the Egyptologist, has written as his last major work a booklet on Egyptian economy which he explains in terms of an institution well known among ethnographers and social anthropologists: the potlatch.

attempt when one points out that his comparisons were largely restricted to those societies whose laws had found the attention of scholars of comparative law, that is, basically the Hebrew, Greek, Old English, Old Germanic, and Medieval societies and their legal institutions [6].

The study of legal history and of legal institutions is sometimes viewed as restricted in approach and promise. This would certainly be true if the investigation of legal institutions and of the formulary of legal records were to be considered as sufficient, as the ultimate goal. Again it is Koschaker's example which proves the opposite. He was always aware of the fact that such investigations are only a beginning, that the real task of those concerned with the legal institutions of the ancient Near East was to discover the underlying social conditions which found their distinct expression in a legal regulation or institution. There are many facts in the cuneiform sources which can not be explained from the legal point of view alone. For these, only social anthropology can be of help. And the same has to be said in the opposite case: there are facts which cannot sufficiently be elucidated by the social anthropologist's approach, where the mind of the legal historian is needed. Therefore, legal history and comparative law on the one hand, and social anthropology on the other, are complementary to each other in the study of ancient Near Eastern societies. For there are facts which follow customary regulations and have not been elevated to a level where they are legally binding, or those which are not of legal relevance. But there are also phenomena which at first seem of little or minor significance to the social anthropologist, but which nevertheless can contribute much to the understanding of a social institution after being duly scrutinized in legal terms.

After having pointed out the potential of using data supplied by studies of a social anthropological orientation we have to call into account the problems necessarily inherent in such an approach. It is again Koschaker's example which can help and guide us in this survey. There are basically three clearly distinct steps for a social anthropologist in investigating and eventually describing a particular unit of social life: to collect, present, and interpret his data. Since social anthropology deals mostly with contemporary non-western societies the collection of data is usually done where these societies exist [7]. The anthropologist lives among these people, learns their language and asks his native informants his questions. The main problem at this point is to avoid the emergence of a distorted picture of the facts because of questions wrongly put or biased by a preconceived concept or working hypothesis [8].

[6] The emergence of the Anglo-Saxon school of social anthropology represented by Radcliffe-Brown and his colleagues and pupils came too late to find its echo in Koschaker's writings.

[7] See P. Murdock, *Social Structure*, p. xiv.

[8] The rôle of a working hypothesis in investigating a social institution can best be viewed and evaluated in connection with the discussion of whether marriage in ancient Mesopotamia at the turn of the third to the second millennium was marriage by purchase or not, for ref. cf. Koschaker, *Studien*, 197 ff.; Koschaker, *Eheformen*; Koschaker, *Eheschliessung*; Landsberger, *Jungfräulichkeit*, 93 f. and opposing this view E. Cuq, *Études sur le droit babylonien, les lois assyriennes et les lois hittites* (Paris, 1929), p. 26; G. R. Driver and J. C. Miles, *The Babylonian Laws* (Oxford, 1952-5), I 264; A. van Praag, *Droit matrimonial assyro-babylonien* (Amsterdam, 1945), 139 ff.; R. Haase, *Einführung in das Studium Keilschriftlicher Rechtsquellen* (Wiesbaden, 1965), 58 ff.

In exploring a dead civilization or society, the equivalent to the social anthropologist's fieldwork is to read the written records of such a civilization. In the same way that the anthropologist when studying North American Indians, for instance, learns their language, he should also not rely on second-hand information when directing his interest toward an ancient Near Eastern society. It is usually at this point that difficulties arise, not too different from the experiences Koschaker had as a legal historian although he had a remarkably good command of Akkadian. But nevertheless, in difficult cases he turned to the expert philologist for advice. It was a most fortunate circumstance which brought Koschaker together with B. Landsberger who as a philologist was imself deeply interested in what Koschaker was doing [9]. Since the ideal of an anthropologist well trained in the languages of the ancient Near East is rather the exception than the rule, it falls upon the philologist-scholar devoted to an ancient Near Eastern civilization to learn and to train himself in the methods of social anthropology [10]. This certainly is an arduous task and very often falls short of the desired goal. But constant exchange with one's colleagues from social anthropology may help to avoid some of the pitfalls and mistakes in such an endeavor. These remarks, of course, are less important with regard to the collection of the data than for their interpretation. But certainly one would overlook quite a number of data without an anthropologically oriented mind.

Given that the problems observed at the data-collecting stage are somehow overcome to mutual satisfaction, the real problems begin to confront us when it comes to interpreting the data collected. Studying social institutions in a given ancient Near Eastern, or for that matter in any dead civilization, the major obstacle facing us is the incompleteness, scantiness, and accidental character of the data available. This problem does generally not exist for the anthropological field worker, who often can return to his native informant for clarification and additional data, a possibility not open to scholars involved in ancient Near Eastern studies. To understand their data as they are, and present them in a comprehensive way, students of ancient Near Eastern civilizations have to look at the models or concepts provided by social anthropology. They are usually the result of generalizations made by social anthropologists based on their field work. The result of applying such a model or concept for comparison will usually be a hypothesis or theory of the institution under investigation. Their correctness is a variable of many factors. The two most important of these are, the amount and explicitness of the extant data, and to an equal degree the validity of the model or concept used for comparison. The validity of the models or concepts used for comparison poses another — twofold — problem: social anthropology is basically concerned with tribal societies, and only rarely with more sophisticated cultures (Hochkulturen). And secondly, because of the concern with contemporary tribal societies, quite naturally, more emphasis is put on the description of the functioning of a given society at a particular moment in time, rather than on its

[9] Landsberger, *Jungfräulichkeit*, 41.

[10] Landsberger has often pointed out the need for Assyriologists to acquaint themselves with the necessary juridical tools to be able to understand and interpret the abundance of cuneiform texts of legal content. Falkenstein was equally insistent in this regard.

evolution, or on its history [11]. But if an Assyriologist investigates an institution like marriage in the Old Babylonian period (2017-1594 B.C.), for instance, he deals with evidence from several hundred years and is even likely to include data from the preceding Neo-Sumerian and Old Akkadian periods (2340-2016 B.C.). It would contradict all experience if there should not occur some changes in the social institutions in general, and marriage in particular under the circumstances prevailing in those periods. The situation becomes even more complicated considering that these periods bear the stamp of a constant interaction between members of an urbanized or settled society, in itself the product of a long history integrating ethnic and socially different groups, with the members of nomadic tribes which were in the process of becoming settled and integrated [12]. The transformation of a tribal society into a complex and highly sophisticated society or "Hochkultur", or the integration into these, makes the use of models more difficult and risky because many more variables have to be reckoned with. Thus, a social institution or a single phenomenon of such an institution, like marriage during the Old Babylonian times, has to be understood as a function of the social structure and the processes of the actual social life at that time [13].

Using models provided by social anthropologists, one is, of course, not standing aloof from the internal discussion within this discipline concerning the right method. Among the different methods used by social anthropologists the structuralistic method of Levi-Strauss has become the most prominent outside the field [14], which cannot be said of the functionalistic method of the Anglo-Saxon school of social anthropology or the methods applied by the Vienna School (P. W. Schmidt) [15], or the "cross-cultural" method of P. Murdock [16]. The outsider has, of course, to be aware of these discussions unless

[11] Note the critique of Koschaker, *Eheformen*, 68 f. on the pseudohistorical explanations of the Vienna School, and, closer to his own field, of the Wenger School of "Antiker Rechtsgeschichte" which operates on a similar path. In both cases the main point is, that similar institutions in different societies are explained as the result of borrowings which occured when the peoples comprising these societies had contact with each other in the course of their history.

[12] These changes must not necessarily be explained in a narrow evolutionistic way viewing them in terms of progress or improvement.

[13] This is the approach developed by Alfred Radcliffe-Brown and best formulated in his introduction to *Structure and Function in Primitive Society* (London, 1952); he transformed ethnology and ethnography into social anthropology as it is today. But note also, for instance, Hans Julius Wolff, "Die Grundlagen des Griechischen Eherechts", *Tijdschrift voor Rechtsgeschiedenis* 20 (1952) 12, who states that phenomena of Greek law of marriage "ergeben sich vielmehr folgerichtig aus der Struktur einer gentilizischen Gesellschaftsordnung".

[14] Note for critical evaluations of this method J. P. B. de Josselin de Jong "Levi-Strauss's Theory on Kinship and Marriage", *Mededelingen van het Rijksmuseum voor Volkenkunde*, Leiden, No. 10 (1970); E. R. Leach "Claude Lévi-Strauss" (Munich, 1971); the German edition lists on pp. 134 f. a number of contributions concerning the general discussion of structuralism; add now W. Hädecke, "Structuralismus — Ideologie des Status quo?" *Neue Rundschau* 82 (1971) 45 ff. (discussing marxism versus structuralism).

[15] See above note 4 for Koschaker's critique.

[16] See e. g. his *Social Structure* (1949). His comparative research technique is based on his Cross-Cultural Survey comprising data, classified by sub-

he wants to find himself caught in the middle of the quarrel. But it has turned out to be most fruitful for me, when drawing on anthropological inspiration for my own research, to turn to the studies of the functionalistic school.

But even when following the "right" school of social anthropology one still faces numerous pitfalls. The main danger is, of course, to place the admittedly scanty data at hand into the Procrustes' bed of a model which seems to fit, without considering its validity. A short remark in Urukagina's reform law which speaks at one point of the times when woman still had two husbands may serve as an example. This was, and may be still is, interpreted as a reference to polyandry in ancient Sumer. And I cannot say that I have not at one time found this to be an interesting and appealing interpretation. But a little study of the institution of polyandry convinces even the most ardent proponent of this theory that the circumstances fostering such an institution are far from being proven for early Sumerian society [17]. Another perennial problem is the question of land tenure in early Sumer. Here we are faced with the problem of the application of theories, that is, of the marxist view versus the non-marxist view concerning the emergence of the class society, or to use another term, the transformation of a tribal society into a more complex society. This is not the place to discuss this, but it may suffice to point out how incomplete information can be interpreted in quite different ways, depending on the ideological standpoint of the interpreter [18].

These two examples are, of course, rather obvious in their implications. But there are enough possibilities where this may not be the case. It is here that the Assyriologist using anthropological material for comparisons has to be careful in order not to produce a rather badly concocted picture. The study of "primitive" societies all over the world teaches us that they are different even if they have a number of similar or identical institutions. Therefore, we should not only look at those facts which can also be found elsewhere; even more effort should be devoted to the search for those phenomena which comprise a society's unique character. In short we should try to discover what Landsberger called "Die Eigenbegrifflichkeit der babylonischen Welt" [19].

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ject, on some 150 human societies. But his work shows his indebtedness to behavioristic psychology, psychoanalysis, historical anthropology (F. Boas, A. L. Kroeber) and sociology.

[17] For the latest discussion of this highly controversial phenomenon, among social anthropologists, see E. R. Leach, "Polyandry, Inheritance and the Definition of Marriage", *Man* 55 (1955) 182 ff. (with bibliography); R. Fox, "Kinship and Marriage" 100 ff. (referring to the Nayar of Malabar in Southwest India, an often cited example for polyandry, he speaks of "plural marriage"); J. Henninger "Polyandrie im vorislamischen Arabien", *Anthropos* 49 (1954) 314 ff.

[18] I. M. Djakonov, *Obščêstvênnyj i gosudarstvênnyj stroi drêvnêgo dvurječ'ja. Šumer* (Moscow, 1960), 45-83, 293 f.; also "Sale of land in presargonic Sumer", *Papers presented by the Soviet Delegation at the 23. International Congress of Orientalists* (Moscow, 1945), 19 ff.; I. J. Gelb, "On the Alleged Temple and State Economies in Ancient Mesopotamia", *Studi in Onore di Edoardo Volterra*, 6, 137 ff.; and J. Renger, "Grossgrundbesitz", *RlA* 3 (1971) 647 ff.

[19] B. Landsberger, "Die Eigenbegrifflichkeit der Babylonischen Welt", *Islamica* 2 (1926-1927) 355 ff. One step in this direction has been taken recently by H. Petschow in his two articles "Zur Systematik und Gesetzestechnik im Codex Hammurabi", *ZA* 57 (1965) 146 ff., and "Zur 'Systematik' in

There has already been occasion to point out how legal history and comparative law, ethnography and social anthropology can help to elucidate the institution of marriage in ancient Mesopotamia at the turn from the third to the second millennium B.C. The following remarks are intended to be a demonstration of this. When first looking at S. Greengus' article dealing with *UET* 5 636 [20]. I was puzzled by the many persons mentioned in the document who brought and received gifts [21]. Hence the title of this article "Who Are all Those People?" Later, when reading Anita Jacobson's monographic study on the customs of marriage and divorce in the Congo-Gabon region (West Central Africa), I was intrigued by her description of a rather elaborate system of small gifts exchanged between the involved families during the period in which the future marriage was arranged. The customs varied as to the persons participating in this mutual transfer of gifts [22] according to the kinship structure (matrilineal or patrilineal). The underlying principles of reciprocity have been described by Marcel Mauss in a general way [23].

The study of the laws governing marriage in Mesopotamia at the turn of the third to the second millennium B.C. has from the outset centered to a large degree on the question of whether marriage in that period was by purchase or not. We are at this point not interested in this emotionally loaded question [24]. Suffice it to note that there existed two types of marriages during the Old Babylonian time: (a) a marriage where the constituting factor was the transfer of the brideprice (bridemoney, bridewealth) called manus-Ehe or Muntehe by Koschaker and (b) a marriage without the transfer of a brideprice, called manusfreie Ehe or muntfreie Ehe by Koschaker. Many of the proscriptions in the law codes deal with the consequences of the termination of a marriage of type (a), be it by death, divorce or other events [25]. The main concern is to make a financial settlement. That is, to proscribe who gains rights over the brideprice and over the dowry or other gifts given to the bride by her father on the occasion of her marriage (where such payments or gifts were made). There are, however, other proscriptions in the law codes (CL § 29; CE §§ 17 and 25; CH §§ 159 and 161) which speak of the dissolution or termination of a not yet concluded marriage (inchoate marriage) [26], where initial arrangements for a marriage had already been made, but the marriage itself was not yet concluded. These proscriptions are:

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den Gesetzen von Eschnunna", *Symbolae iuridicae et historicae Martino David dedicatae*, vol. 2 (Leiden, 1968), 131 ff.

[20] See Greengus, *Marriage Ceremonies*.

[21] Gift is here used, to some extent anticipating our results, as a transfer of goods based on rules of custom in contrast to payment, which is understood as transfer of goods or money as part of a legal transaction.

[22] A. Jacobson, "Marriage and Money", *Studia Ethnographica Upsalensia* 28 (1967) 40 ff., 93 ff.

[23] M. Mauss, "Essai sur le don", *L'Année sociologique*, second série (1923-1924).

[24] Cf. e.g. Koschaker, *Eheschliessung*, 211 f.

[25] CH §§ 138, 149, 161-164, 171.

[26] G. R. Driver and J. C. Miles, *The Assyrian Laws* (Oxford, 1935), 166 f., 173 f.; H. Petschow, *Symbolae iuridicae et historicae Martino David dedicatae*, vol. 2, 136 with note 2.

CE § 17 (B) *mār awīlim ana bīt emim terḫatam lībilma šumma ina kilallēn ištēn ana šīmtim ittalak kaspum ana bēlišuma itâr*
Should the son of a man bring the brideprice to the house of (his) father-in-law, if then one of the two (i. e. bride or groom) should die, the silver will return to its owner.

CE § 25 *šumma awīlum ana bīt emi issima emušu ikšišuma mārassu ana [šanîm i]ttadin abi mārtim terḫat imḫuru tašnâ utâr*
When a man claims (his marriage) from (his) father-in-law's house [27], but his father-in-law rejects his claim [28] (and) should (even) give his daughter to another man, then the father of (this) daughter shall return the brideprice he received twofold.

CH § 159 *šumma awīlum ša ana bīt emišu biblam ušābilu terḫatam iddinu ana sinništim šanītim uptallisma ana emišu māratḫa ul aḫḫaz iqtabi abi mārtim mimma ša ibbablušum itabbal*
If a man who has had a gift brought to the house of his father-in-law, (and) who has paid the brideprice, becomes distracted to another woman and says to his father-in-law: "I will not marry your daughter", the father of (this) daughter shall take everything which was brought to him.

CH § 160 *šumma awīlum ana bīt emim biblam ušābil terḫatam iddinma abi mārtim mārtī ul anaddikkum iqtabi mimma mala ibbablušum uštašannama utâr*
... but then the father of (this) daughter should say: "I will not give you my daughter (into wedlock)", whatever has been brought to him he shall return twofold.

CH § 161 *šumma awīlum ana bīt emišu biblam ušābil terḫatam iddinma ibiršu uktarrissu emušu ana bīl aššatim mārtī ul taḫḫaz iqtabi mimma mala ibbablušum uštaannama utâr u aššassu ibiršu ul iḫḫaz*
... and then his (own) friend should slander him (and) his father-in-law (therefore) should say to the (future) husband: "You shall not marry my daughter", whatever has been brought to him he shall return twofold, the friend shall not marry his (prospective) wife.

It is the wording of the protases in CH §§ 159-161 which interests us because they mention in addition to the brideprice, a *biblum* (gift) which the groom had brought (*ušābil*) to the bride's father's house. The *biblum* is not mentioned in the comparable sections of CE §§ 17 and 25. Also CL § 29 mentions only a single transfer, that of níg.munus^{ús}.sá. According to lexical texts it is the equivalent to *terḫatum*, but factually it has been interpreted as an equivalent to *biblum* [29]. A discussion of the inherent problems would ne-

[27] See Landsberger, *Jungfräulichkeit*, 74 f.; J. J. Finkelstein, "Ana bīt emim šasû", *RA* 61 (1967) 127 ff.; R. Yaron, *The Laws of Eshnunna* (Jerusalem, 1969), 123 ff., and see note 35 below.
[28] Landsberger, *Jungfräulichkeit*, 75.
[29] See e. g. C. Wilcke *WO* 4 (1968) 153.

cessitate the reopening of the entire dispute whether we have to reckon with different, that is Sumerian and Akkadian, laws governing marriage [30]. This is impossible at the present time.

That the *biblum* seemed not to have the legal significance of the brideprice (*terḫatum*) may in part account for the little attention this term has found among those who dealt with marriage in Old Babylonian times: Koschaker, *Studien*, 134 makes some remarks which obviously have since been neglected. His interpretation of *biblum* as "Verlobungsgeschenk", according to ethnographic comparisons, lacks perhaps some precision, but he clearly defines *terḫatum* (brideprice) as being within the realm of the law, and *biblum* as belonging to the realm of custom. Van Praag [31] takes *biblum* and *terḫatu* as synonyms in CH. Landsberger, *Jungfräulichkeit*, 97 f. deplores the lack of attention paid to *biblum*, but postponed his own discussion for later. Finkelstein [32] says: "It need not be concluded from line 33 of the myth just cited ["Enlil and Sud", see p 268 with note 34 J. R.] or from the entry in the ḪAR-ra series that the mu.pà.da-gift is always distinct from the níg.dé.a = *biblum*. Very likely the two terms are largely synonymous, denoting the same customary marriage gifts, and the use of both terms in the myth serves rather the purpose of poetic parallelism. Similarly, in the marriage contracts, which generally serve as summaries of long and involved negotiations entailing exchanges of gifts of various kinds, the *terḫatum* (which is the one payment that invariably appears in the OB contracts) may be presumed to subsume all antecedent gifts and payments including the *biblum* (see the fourth entry in the lexical list just cited) which technically might have been distinct from the *terḫatum* in the strict sense". This obscures the difference between the legal and customary implications of the two terms *terḫatum* and *biblum* respectively, in an unwarrented way, even though he distinguishes carefully between payment and gift in their translation. Greengus, *Marriage Contract*, 523 note 87 also makes no clear distinction between *terḫatum* and *biblum*.

A look at the lexical passage Hh. XIII 128 ff., to which Landsberger and Finkelstein have drawn our attention [33], provides beside the lexical equation

93	udu.mu.pad.da	sheep given at the mentioning of the name*
94	udu.kadra	sheep given as kadra-gift**
95	udu.níg.dé.a	sheep given as níg.dé.a (*biblum*)-gift
96	udu.níg.dé.a.munusᵘˢ.sá.e.ne	sheep given as níg.dé.a-gift by the in-laws***
96a	udu.níg.dé.a.munus.e.ne	sheep given as níg.dé.a-gift by the women

*Or: at the oath-taking (= betrothal agreement), for which interpretation see below note [34]. Whether mu-pad-da should be interpreted as referring to a custom — assumed by Stamm, *Namengebung*, 272 f. — of giving a new name to a woman in connection with her marriage cannot be decided on the basis of the evidence at hand. – A [tú g.g] ú.è *ša zikir šumi* is mentioned in the OB dowry list *CT* 47 83:23′. Because of the damaged state of the tablet no pertinent conclusions can be drawn. For a full discussion of *zikir šumim* "gift" see F. R. Kraus, "Akkadische Wörter und Ausdrücke, VI-VIII", *RA* 65 (1971) 99 ff., and especially 109 ff.

[30] Cf. Landsberger's critique in *Jungfräulichkeit*, 94, note 4.
[31] *Droit matrimonial*, quoted, p. 156.
[32] J. J. Finkelstein, "The Laws of Ur-Nammu", *JCS* 22 (1968-69) 75, n. 5.
[33] Landsberger, *Jungfräulichkeit*, 95, n. 2; Finkelstein, "Laws of Ur-Nammu", quoted, p. 75, n. 5. Hh. Forerunner (*MSL* 8/1 84) to Hh. XIII

of Sum. n i g . d é . a with Akk. *biblum* the names of a number of other gifts related to a marriage. This is either self-evident or proven by two lines in the myth of "Enlil and Sud" which run as follows: d u m u . m u n u s . z u n a m . d a m . š è g a . du_{12} g i . n a . z u s u m . m a . a b / m u . p a d . d a . m u g a . m u . r a . a b . i l n i g . d é . a . m u š u . t e . m a . a b = *ma-rat-ki a-na áš-šu-ti lu-ḫu-uz kit-ta-ki id-din / a-na zi-kir šu-mi-ia lu-uš-ši-ki-im-ma bi-ib-la muḫ-rin-ni* "I would have your daughter to wife, give me your consent; I will bring you my *mupaddû*-gift, accept my (Akkadian: the) *marriage present*" [34]. This considerably enlarges the number of gifts transferred when arranging a marriage. The precise meaning and the function beyond what is indicated in the tentative translations, however, escape us.

Little is known of what kind of things the *biblum* and the other related gifts were comprised. We can only surmise from the Hh. XIII passage which states that they could consist of sheep. But to conclude from Hh. XIII that these gifts consisted only of sheep would be wrong since Hh. XIII is simply a list concerned with sheep used or given on special occasions. A good possibility is, therefore, that some or all of these gifts consisted also of other foodstuffs and beverages [35]. If we could prove that the still enigmatic passage from the Gilgamesh Epic: "They have invited me to a wedding ... for the table of the (wedding) feast I have loaded on me delicious wedding cakes" [36] belongs in this context we would have evidence for that contention.

Whether we can call *biblum* a betrothal gift (Koschaker, *Studien*, 132 f.) as distinguished from the brideprice (*terḫatum*) which (that is the *biblum*) would be one of the requirements for the conclusion of a marriage, depends on how we can define its function in the marriage arrangement. Some binding function seems to be indicated by CH §§ 159-161 where the *biblum* is forfeited together with the brideprice by the bride's

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**M. Civil informs me that he has the impression that according to Sumerian sources k a d r a is always given to superiors.

***For a discussion see below note 44.

Hh. I 35 ff. lists the following words some of which have their equivalent in the OB Hh. Forerunner to Hh XIII. The pertinent Akkadian sections in Hh. XIII are destroyed (reference is given to them by indicating the line numbers in parentheses).

Hh. I			
35	k a š . d é . a	= *qé–re–e–tum*	
36	n i g . d é . a	= *bi–ib–lu*	(131)
37	n í g . m u m u s^{ús} . s a	= *ter–ḫa–tum*	(132)
38	n i g . š u . s u m . m u	= *ni–din–tum*	(133)
39	n i g . š u . d u g_4 . g a . a	= *šu–bul–tum*	
40	n i g . m u . p a d . d a	= *za–kar šu–mu*	

Note also the footnotes in *MSL* 5 11 f. with reference to parallel sections in the vocabularies (see *CAD biblu* lex. for ref.).

[34] M. Civil, "Remarks on 'Sumerian and Bilingual Texts'", *JNES* 26 (1967), p. 203: 32 f., and Finkelstein, "Laws of Ur-Nammu", quoted, p. 75, n. 5; see Kraus, "Akkadische Wörter", quoted for the difficult *ana zikir sumiia luššikima*.

[35] So already Greengus, *Marriage Ceremonies*, 61 and section (3).

[36] Gilg. P. iv 22-26, see Landsberger, *Jungfräulichkeit*, 83, Greengus, *Marriage Ceremonies*, 61. We refrain from discussing the examples from Sumerian sources quoted by Greengus, loc. cit., 69 f. because we have no room to follow up the controversial question of whether n í g . m u n u s^{ús} . s á is bride-price or a gift, equivalent to *biblum*.

father or is to be repaid twofold according to who is at fault in case the intended marriage is not concluded. The binding character of *biblum* (n í g . d é . a) and/or m u . p a d . d a in the myth "Enlil and Sud" can not be proven by the text. These two gifts are presented by the groom's emissary in order to receive the consent of the prospective bride's guardian, that is, they are intended to induce a betrothal. Whether they, by being accepted, bind the two parties remains open, but it is possible. But one should also be aware that only one gift would be necessary as a pledge concerning a future marriage, at least if we think in our terms. If this is so, one of the two gifts might serve another purpose.

According to some translations the *biblum* was brought by the groom. But a close look at the references shows that it was brought by either an unspecified number of persons (*UET* 5 636: 9), probably including the groom's brother, or that the groom had it brought (by others: *ušābil*) to the house of the bride's father (CH §§ 159: 36, 160: 50). In the myth "Enlil and Sud", Enlil's vezier, Nuska, is the emissary who brings the *biblum*.

The recipient of *biblum* and related gifts is the bride's guardian, as is indicated by the myth "Enlil and Sud". In CH §§ 159 f. *biblum* was brought to the house of the bride's father. If we take the expression at face value these words can be simply interpreted as the place where the *biblum* was taken. But *bīt emim* can also mean the bride's family [37]. What significance the latter interpretation may have will be discussed later.

In *UET* 5 636 the presentation of the *biblum* by the groom's emissaries is recorded in section (3), whereas section (1) records a gift (n í g . b a) to the groom, given by the bride's father. If the sequence of events is correct, an interpretation of *biblum* as betrothal gift becomes doubtful. After all, one would expect that an agreement having the binding power of a betrothal would have been reached before the groom was given such a valuable gift. The last quoted text especially suggests another look at CH § 159 ff. One should note that no explicit mention is made as to the function of the *biblum*, no hint is given how the transfer of the *biblum* bound the two parties involved to fulfill their respective obligations with regard to the intended marriage. An interpolation of *terḫatam iddin* as a way to resolve the problems of interpretation has been rejected by Koschaker, *Studien*, 133 note 13. Or should *ana bīt emim biblam ušābil* be without legal significance, just referring to an act of custom as originally anticipated by Koschaker? This seems to be a possibility and would not be contradicted by the regulations of CH § 159 ff. where, if correctly assumed, the forfeiture or twofold indemnification of the *biblum* is included in the words *ša ibbablušum itabbal* and *uštašannama utâr*. Also, no regulation dealing with the termination of a concluded marriage refers to *biblum*, but only to *terḫatum* (brideprice). This would indicate its limited function, that it expires with the conclusion of the marriage.

All this leaves us to doubt the function of *biblum* as a betrothal gift considering that such a gift should be expected to be made by the groom or his

[37] Finkelstein's interpretation in "Ana bīt emim", quoted, of *bīt emim* as nuptial chamber is not satisfactory, cf. e. g. CH § 159 which is concerned with events before the bride and groom would enter the nuptial chamber but nevertheless it refers to *biblum* and *terḫatum* brought *ana bīt emišu / emim*. See also the criticism by R. Yaron, *Laws*, quoted, p. 126 f.

emmissary at an very early point in the courtship. One should therefore consider the possibility that mu.pad.da is a betrothal gift and *biblum* has another function. This will have to be done at a later point of our discussion.

We now must examine in greater detail the text *UET* 5 636. This text has been extensively treated by Greengus, *Marriage Ceremonies* and additional remarks were made by Landsberger, *Jungfräulichkeit*, 77 ff. But since Greengus was mainly interested in marriage ceremonies and rites, it is necessary to elaborate on those sections which are of importance in the present discussion. The text consists of 14 sections listing expenditures given at particular occasions before, during, and after the marriage. Sections (1), (3)-(7), and (13) [38] record foodstuffs and beverages taken to persons which seem to belong to the groom's family:

(1) Gold, a silver ring and garments (together worth 14 2/3 shekels of silver) and a piece of headwear (without money equivalent) as níg.ba (gift) for Aplum.

(3) "For those who brought the *biblum* to me [39] one sila of oil with which they anointed themselves" (no money equivalent given). Flour, beer (without money equivalent), two sheep, butter and oil worth together three shekels of silver, and cakes (without money equivalent) "I let PN, his brother, take at the occasion of the ... of the tray".

(4) When the two women Šāt–Ilabrat and Ea–Lamassī, whose relation to the groom is not clear, came (*illikānim*) they received one sheep (worth one shekel of silver), beer (without money equivalent), ... and oil (worth one shekel of silver).

(5) "When ... came (*illikam*) to Ur they took to him (*ublūšum*)" ... (worth one half shekel of silver) and beer (without money equivalent).

(6) "When his (i. e. the groom's) mother came (*illikam*) to Ur they took to her (*ublūšim*)" one sheep (worth 1 1/2 shekel of silver), and also beer and flour (both without money equivalent).

(7) The groom's mother performed some rite in the Enki–gate. At this occasion "they took (*ublūšim*) to her" bread, beer (without money equivalent) and one ... -sheep (worth one shekel).

(13) "[P]N came to me to Ur and they brought for him" bread, beer (without money equivalent) and fine oil (worth one shekel of silver). PN's relation, who came after the bride left her father's house, to any of the other persons remains unknown.

The recipients can be identified — under the assumption that Aplum is the groom [40] and that the pronominal suffixes in šeš.a.ni and *ummašu* refer to Aplum — as the groom, his brother, and his mother. No specific relation to the groom can be established for those who brought the *biblum*, for Šāt–Ilabrat and Ea–Lamassī and for the persons in sections (5) and (13). We assume with Greengus that they are all relatives of the groom.

In section (3) it is stated that "I (= the bride's father) had PN, his brother, take" a variety of foodstuffs. Since the document is basically written

[38] I follow the arrangement given by Greengus, *Marriage Ceremonies*.

[39] All ventives in this text can, of course, also be translated "to me" rather than "to Ur" in the wider sense.

[40] See Greengus, *Marriage Ceremonies*, 58.

from the point of view of the bride's father (see lines 36 and 39: *ana bītija*) it is reasonable to assume that the gift (níg.ba) for the groom, Aplum, in section (1) also came from the bride's father, although no verb expressly stating that is found. Nor is there a verb indicating who gave the oil for anointment to those who brought the *biblum* in section (3), or in section (4) where expenditures for Šāt–Ilabrat and Ea–Lamassī are listed. But in both cases we may safely assume that the bride's father distributed these things. But who are the people who took (*ublūšum, ublūšim*) foodstuffs and beverages to the persons in sections (5) and (13) and to the groom's mother in sections (6) and (7)? Two possibilities are open: (1) The text is formulated unprecisely and the third person plural simply expresses that these things were taken to those persons on order of the bride's father by members (even servants) of his household. (2) The other interpretation would be that "they" are persons, related to the bride's father, who took these things to members of the groom's family in their own right, perhaps required by custom. It should be noted in this context that the bride's father is the one who gives to the groom, to those who brought the *biblum*, and to Šāt–Ilabrat and Ea–Lamassī, whereas "they" give in sections (5)-(7) and (13) to two persons, whose relations to the groom remain unknown, and to the groom's mother.

The total value of the things distributed to presumed members of the groom's family is more than 30 shekels of silver consisting of 25 2/3 shekels for those things whose prices are given and more than 4 1/3 shekels for those where no money equivalent is given [41]. This is a considerable amount. The amounts of foodstuffs listed on each occasion suggest that they could well provide for a good meal for several persons [42].

We now have to come back to two suggestions made in the previous discussion: (1) that the expression *biblam ana bīt emim wabālum / šūbulum* could mean to bring or have the *biblum* brought to the "house" (= family) of the bride's father, indicating that not only he, but also other members of his household were recipients of the *biblum* [43]. (2) That *ublūšum / ublūšim* indicates that members of the bride's family other than her father transfer gifts consisting of foodstuffs to members of the groom's family. At this point we have to mention again the words udu.níg.dé.a.munus^{ûs}.sá.e.ne and udu.níg.dé.a.munus.e.ne in Hh., Forerunner 96 and 96a, to be translated as "sheep given as *biblum* by the in-laws / by the women" [44].

[41] Based on W. Schwenzner, *Zum altbabylonischen Wirtschaftsleben* (*MVAG* 19/3, Leipzig, 1915), 104, and CE § 1.

[42] Note however, that sheep given to the members of the groom's family were given, presumably, alive which may be concluded from *UET* 5 636: 37 where, at another occasion, the slaughtering of a sheep is mentioned explicitly.

[43] Our interpretation of the text, however, does not rest on this interpretation of *bīt ēmim* alone. Comparative material could be quoted (cf. e. g. the biblical account in Gn 24, 53 where Eliezer presents gifts to the bride's brother and mother, discussed by Neufeld, *Ancient Hebrew Marriage Laws* 115 as complementary gifts (*migdanôth*); also in A. Jacobson (above note, 22), but there is also an inner logic which suggests that members of the bride's family other than her father were receiving some of these gifts.

[44] Landsberger, *Jungfräulichkeit*, 95 translates: "Hochzeitsgaben, gegeben von den Brāutigamen bzw. von den Frauen (Brāuten)". For the rendering of munus^{ûs}.sá cf. Inanna's Descent 354 (S. N. Kramer, *JCS* 5 [1951] 14, and also *SEM* 88 II 2 + dupl., both quoted by Greengus, *Marriage Ceremonies*, 70 with note 100) where Dumuzi says to Utu, Inanna's brother, "I am your

The interpretation of the evidence discussed so far permits the following conclusions. On the occasion of a marriage gifts were exchanged between the two families involved following some rule of reciprocity [45]. Different members of both families were the recipients, and there is also the possibility that members from the two families other than the groom (or his father) and the bride's father, were givers in their own right, perhaps required by custom. These gifts, as far as can be established, consisted of foodstuffs and beverages. This distinguishes them from the brideprice which consisted of money (silver). Exceptional is a gift consisting of jewelry and clothes given to the groom by the bride's father. These gifts were apparently exchanged according to rules of custom, not of law, since they do not figure in settlements following the termination of a legally concluded marriage [46].

Finally we have to ask what function this mutual exchange of gifts had at the occasion of a marriage. It has been noted repeatedly that marriage in Old Babylonian times is not an arrangement between the groom and the bride, not even between the groom and the bride's father, but between the two families [47]. Therefore, the exchange of gifts between the two families has a function which goes beyond the marriage of a young man and a young woman. We may now refer to the proposition made earlier [48] in the discussion that *biblum* was not a gift binding the two families to the betrothal but that it served another function. This becomes even more suggestive when we consider

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(brother)-in-law (munus^{ús}.sá.zu.me.en)". This would result in a translation "sheep, (given) as (wedding) gift(s) by the in-laws" as opposed to "sheep, given as (wedding) gift(s) by the women (i. e. the bride's [?] family)".

[45] Note, that the brideprice is not reciprocated in goods, but only in giving the groom the right to marry (*aḫāzum*) the bride. The dowry cannot be understood as a counterpart of the brideprice since they do not originally occur together. The reciprocity of the gift exchange is vaguely anticipated by Greengus, *Marriage Ceremonies*, 72; more explicitly but too generally stated by Landsberger, *Jungfräulichkeit*, 98, n. 1, on the basis of udu.níg.dé.a.munus^{ús}.sá.e.ne and udu.níg.dé.a.munus.e.ne. The custom of reciprocal (gift) exchange on occasion of a marriage is also indicated by the proverb munus^{ús}.sá.TUR ta mu.un.ir.ra.bi uš bar(UR$_7$).e ta bi.in.búr.ra.bi "What has an fiancé brought, what did a father-in-law give for it?" (Gordon, *Sumerian Proverbs*, 1.169, quoted by C. Wilke, "ku–li", *ZA* 59 [1969] 76, n. 46, and *WO* 4 [1968] 155), but is too far removed in time to be considered as direct proof.

[46] The question could be raised what happened to the gifts received by the groom's family in case a marriage was not concluded. The CH §§ 159 ff. stipulate forfeiture or twofold indemnification of *biblum* and *terḫatum* but do not mention any gift received by the groom's family. It seems that these gifts were not mentioned because they were not yet given: This may be deduced from the following: if the sequence of events as given in *UET* 5 636 is a fair reflection of what would ordinarily happen prior to a marriage then the *biblum* was handed over before all the gifts went to members of the groom's family. If therefore at this point either party decides not to conclude the marriage only *biblum* and *terḫatum* could be subject to the subsequent financial settlement. But in any case one has to be aware that numerous questions will remain unanswered. The law codes, for instance, are also not interested in two other gifts: níg.ba (*UET* 5 636: 5, see above (1)) and *mupaddû* (above note 34). Is this the case because these gifts were only made according to rules of custom? But why then is *biblum* — which seems to fall into the same category (see above, p. 266 and 268) — mentioned in CH §§ 159 ff.?

[47] See Koschaker, *Eheschliessung*, 280.

[48] See above p. 270.

that the *biblum* was not brought by the groom but by others including his brother(s) [49]. These people are thus participating in the process of an exchange of goods. This exchange is a means of building or maintaining an alliance, a close relationship, between these two families [50]. Only on this basis can the custom of transferring gifts between two families be fully understood. "Maintaining alliances" would of course imply that a custom of preferential marriage existed. There is only a slight indication that such a custom existed among tribal groups in northern Babylonia [51], but nothing is known about its existence among the settled population. "Building alliances" would imply a society of people without a custom of preferential or prescribed marriage. Such conditions would most likely prevail in towns or cities, if it could be shown that urbanization resulted in a large scale break-up of family ties or alliances [52].

The discussion and interpretation of an unique document, *UET* 5 636, in the light of a concept obtained from elsewhere has resulted in a surprisingly coherent picture which I had not originally anticipated. It seems, therefore, unnecessary to quote extensively from other sources as proof [53]. The evidence used in this discussion covers only a limited span of time and only one marriage type, marriage by purchase. It must be left to another occasion to find out whether in previous or later periods this custom of gift exchange on the occasion of marriage existed.

[49] See above p. 269.

[50] Landsberger, *Jungfräulichkeit*, 82 "Verschwägerung"; Greengus, *Marriage Ceremonies*, 65 f. refers to *kirrum* "as a rite which symbolized the change of relationships and the incorporation of new family members". *kirrum* has not been treated in our discussion, since it seems to have no specific meaning in *UET* 5 636: 33 other than "drinking party" for which the bride's father provided 60 sìla beer (ca. 50 litres). *kirrum*, provided by the groom for the bride's parents, as a constituting element in concluding a marriage occurs in CE §§ 27 f. which, however, do not refer to a marriage by purchase (with *terhatum*).

[51] See my article "mārat ili" in *AfO* 24 (1973).

[52] See my remarks CRRAI 18 181 end of note 53.

[53] E. Westermarck, *The History of Human Marriage* (1922), vol. 2, 397 ff. gives reference to the customs of transfer of gifts on the occasion of a marriage among numerous societies. The whole process of mutual gift exchange becomes, however, much more transparent in A. Jacobson's book (see above, note 22) because of its restriction of describing only the evidence coming from a limited area.

Notes on Nintur

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The invitation to contribute to this volume in honour of Professor Ignace Gelb called for a brief paper dealing with method, specifically with the phenomenological method, as it applies to the study of Ancient Mesopotamian religion. Seeking to comply as nearly as possible with this request we have chosen, since a fulldress discussion of method would hardly be feasible in the space available [1], merely to restate briefly what to us appears as the fundamental methodological distinctions, and then to illustrate and comment on — subjectively and without systematic intent — some of the further problems that arise as one approaches a group of related data in these terms. We have chosen to take a cursory look at some of the names, epithets, and belongings of the Ancient Mesopotamian goddess Nintur that are mentioned in Gudea's Statue A. We lay no claim to completeness. Nor do we attempt in dealing with them to furnish anything resembling a rounded study of the divine figure; these are only random notes looking toward such an undertaking.

Fundamental Distinctions

The methodological distinctions that seem to us basic for a rewarding study of religion we have on previous occasions stated as follows:

1. *The Ground of Religion and the Study of Religion*

"Basic to all religion, formal or otherwise, is a unique experience which with a term coined by Rudolph Otto, is usually called the 'numinous' experience. Otto has analyzed it as the experience of a Mysterium Tremendum et Fascinosum, a confrontation with the 'Wholly Other', outside normal experience and indescribable in terms of normal experience, terrifying, ranging from sheer demonic dread through awe to sublime majesty, and fascinating, with irresistible attraction, demanding unconditional allegiance. It is the human response to this experience in thought (mythology and theology) and action (cult) which constitutes religion, and it is with the manifold forms which the response takes that the study of religion properly concerns itself. The aim of such study must be, beyond all, to understand and to interpret the various religious

[1] The interested reader will find such a discussion in G. van der Leeuw, *Religion in Essence and Manifestation* (Gloucester, Mass. 1967), chapters 107-110, vol. II p. 671-695.

forms as response, as ultimately meaningful in relation to the underlying numinous experience only" [2].

2. *The Religious Metaphor*

Since the Numinous is altogether other, not of this world, it cannot be described; for all available terms are based on experience in this world and so do not apply. At most it is possible to evoke the response of the numinous experience by analogy, in metaphors [3]. Because of this, the particular metaphors which a given culture or cultural period lights upon as especially meaningful and effective are of central interest, for they represent its way of conveying its understanding of the Numinous and forms a bridge from immediate experience in the religiously gifted to mediate experience in the great masses, and are also the means by which one generation hands on its religious insights to the next. As such metaphors must count names, forms and functions of the gods, even the term god itself; and they are presumed in, and underlie, the building of temples and the performing of rituals.

In dealing with religious metaphors the fact that they are metaphors should not be forgotten, they are Janus-faced. On the one hand they are firmly grounded in experience of this world as conditioned by the culture out of which they come, and must first be understood in those terms. On the other, their function is precisely to point beyond themselves to something not of this world. They can serve, Janus-like, as doors to ultimate understanding only if that is kept in mind.

3. *The Secular Grounding of the Metaphor*

The religious metaphor is taken from culturally conditioned, culturally determined, experience and so must first of all be understood in terms of function within that culture [4]. In the case of Ancient Mesopotamian religion, the data for which are conditioned by a culture so far from us in time and place, such understanding is no easy matter. There is, however, only one road toward it: continued study seeking to grasp the culture and its value organically and as a whole, trusting to insights that light up and clarify beyond what one expected, checking and rechecking in empathy against new data, new possibilities as

[2] "Formative Tendencies in Sumerian Religion", in *The Bible and the Ancient Near East: Essays in Honor of William Foxwell Albright*, ed. G. Ernest Wright (New York 1961) p. 267 = Anchor Books edition (Garden City 1965), p. 353 = *Toward the Image of Tammuz* (Cambridge, Mass. 1970; henceforth *TIT*), p. 1.

[3] See Rudolph Otto, *The Idea of the Holy* (London 1950) p. 7.

[4] We are happy to find ourselves in agreement on this point with Professor Gelb's statement: "As all man's ideas about things divine are human, it is my firm belief that we shall never know what was the nectar of the gods until we learn what was the daily bread of the people" *AS* 16 (Chicago 1965) p. 62. It is not quite clear to us, however, why, as stated just before the passage here quoted, a study of the resurrection of Tammuz (arranged for by Inanna at the end of the Myth of Inanna's Descent) or of Sumerian beliefs in afterlife (described in detail in the story Gilgamesh, Enkidu, and the Nether World) must be fruitless. Since a culture is an organic whole, a system, it would seem to us that one should try to understand it as a whole, not in fragments, and that no cultural element should be deliberately neglected.

presented by philology and archaeology both [5]. What Usener so marvelously states for the former is equally true for the latter.

> Nur durch hingebendes versenken in diese geistesspuren entschwundener zeit, also durch philologische arbeit vermögen wir uns zum nachempfinden zu erziehen; dann können allmählich verwandte saiten in uns mit schwingen und klingen, und wir entdecken im eignen bewusstsein die fäden, die altes und neues verbinden. Reichere beobachtung und vergleichung gestattet weiter zu gehen, und wir erheben uns vom einzelnen zum ganzen, von den erscheinungen zum gesetz. Es wäre übel mit menschlicher wissenschaft bestellt, wenn wer im einzelnen forscht fesseln trüge, die ihm vervehrten zum ganzen zu streben. Je tiefer man gräbt, desto mehr wird man durch allgemeinere erkenntnisse belohnt [6].

4. *The Sacred Pointing of the Metaphors*

The other side of the metaphor, its function of pointing to, and evoking, numinous experience is clearly dependent for its realization both on the degree to which the student is able to enter with valid empathy into the culture to which it belongs, and on the degree to which he is capable generally of religious response — in fine on his openness to symbolization other than his own.

To state that the student of religion needs must be capable of religious sensitivity is nothing very extraordinary, it is not different from saying that a student of literature must be capable of literary sensitivity, of sensing e. g., what is poetry and what is not poetry; or that a student of art must be capable of sensitivity to art. In the study of religion it means specifically that one is willing to draw on one's own religious experience to understand sympathetically forms that may at first seem empty or even repellent, while at the same time guarding against reading into the data things that are not there.

A characteristic feature of the oldest Mesopotamian metaphors is what we have termed "intransitivity" [7]. The Ancient Mesopotamian tended, immediately and unreflectedly, to see a numinous experience as a revelation of power to, and in, a dominant phenomenon of the situation of the experience. The power was, so to speak, the numinous *élan vital* of the phenomenon, a will in it to be and thrive in its particular form and manner. Only with the advent of the ruler metaphor did a new "transitivity" of certain gods appear: Will to social justice and morality, will and power to victory over enemy armies etc.

In seeking to understand the pointing of such situationally conditioned religious metaphors it is obviously of help to seek to clarify to oneself what was the situation, in nature or society, from which the metaphor was taken. As we have stated earlier in dealing with the figure of Dumuzi: "We must try

[5] A. Leo Oppenheim, *Ancient Mesopotamia* (Chicago 1964) contains a chapter entitled "Why a 'Mesopotamian Religion' should not be Written" in which these difficulties are stressed to the point of defeatism. Oppenheim gives as reasons: "the nature of the available evidence, and the problem of comprehension across the barriers of conceptual conditioning" (op. cit., p. 172) but these same reasons for giving up could with equal right be applied to any other aspect of Ancient Mesopotamian civilization and even to any attempt at making a dictionary. If the early decipherers, who faced far greater difficulties, had let themselves be thus easily discouraged, cuneiform would still be a closed book.

[6] Hermann Usener, *Götternamen* (Bonn 1896), p. vii.

[7] In "Toward the Image of Tammuz", *History of Religions* 1 (1961) 190-192 = *TIT*, p. 74-76.

to identify the concrete loci in the external world in which a feeling of being in the presence of divine power of that particular kind, suggestive of that particular symbolization, may naturally have arisen. For it is only there that we may hope to find common ground in reality with the myth. Once we have identified the loci in the external world in which the particular experience that is symbolized and given form in the god came to the Ancients, we can try, out of our own human experience to recapture in sympathetic understanding such value and truth as that symbolization may possess as an expression of the 'unchanging human heart' " [8]. Guiding for understanding and evaluation must always be the fact that in so far as data are religious data they reflect, directly or indirectly, numinous experience.

Nintur

The Name Nintur

Statue A of Gudea, the divine names, epithets etc. of which we wish to consider, was destined for a Temple built for a goddess — a very well known one — who is called by two different names. Once she is Ninhursağa(k) and once Nintur [9]. Omitting Gudea's cartouche on the shoulder of the statue the inscription reads [10]:

i ^{d}Nin-hur-saĝ
nin-uru-da-mú-a
ama-dumu-dumu-ne
nin-a-ni
Gù-dé-a
ensik (PA-TE-SI)
Lagaša^{ki}-ke$_4$
é-uru-Ĝír-su ^{ki}-ka-ni
mu-na-dù

[8] *History of Religions* 1 (1961) 189 = *TIT*, p. 73. A former pupil, Mr. B. Alster, has taken exception to this statement in his study *Dumuzi's Dream* (Copenhagen 1972) p. 10-12. He says (p. 12): "As a result of the emotional approach which is especially apparent in the final statement of the quotation above, that myths contain certain values which may be grasped by the sympathetic understanding, the evidence of the different sources is not evaluated systematically in Th. Jacobsen's study". It is not clear to us why sympathetic understanding must act to preclude systematic evaluation of sources — we should have thought, rather, it might have helped — and we find Alster's frowning on sympathetic understanding and unwillingness to allow that ancient materials might be bearers of values oddly conformable with the methodology recommended, not on best authority, to the student in Faust: "Wer will was Lebendigs erkennen und beschreiben, / Sucht erst den Geist heraus zu treiben, / Dann hat er die Teile in seiner Hand, / Fehlt leider! nur das geistige Band".

[9] In favor of reading ^{d}Nin-TU as ^{d}Nin-tur$_5$ is the gloss tú r in An: *Anum*, *CT* XXIV pl. 12.16 and writings showing the final -r such as [^{d}][N]in-TU-re, *TRS* 12.24 and ^{d}Nin-TU-ra (anticipatory genitive), *PBS* V no 1. i. 39; also Adab^{ki}-uru-^{d}Nin-TU-ra-ta Falkenstein, *ZA* 55 p. 23 note 80 etc. See also Å. Sjöberg, *The Collection of the Sumerian Temple Hymns* (Locust Valley 1969; henceforth *TH*, p. 72.

[10] *Dec.* pl. 20 and 15 no. 5, *Dec. ep.* p. vi ff., cf. *SAKI*, p. 66a.

ii ^{urudu}x a l a l - k ù - g a - n i [11]
m u - n a - d í m
^{geš}d ú r - ğ a r - m a h - n a m - n i n - k a - n i
m u - n a - d í m
é - m a h - n i - a m u - n a - n i - k u r$_4$
k u r - M á - g a n^{ki} - t a
iii n a$_4$ - e s i i m - t a - e$_{11}$
a l a n - n a - n i - š è
m u - d ú
n i n - a n - k i - a - n a m - t a r - r e - d è
^{d}N i n - t u r$_5$
a m a - d i ğ i r - r e - n e - k e$_4$
G ù - d é - a
l ú - é - d ù - a - k a
n a m - t i - l a - n i m u - s u d
m u - š è m u - n a - s a$_4$
é - a m u - n a - n i - k u r$_4$

For (divine) Ninhursağa,
the queen contemporary (lit. "sprouted forth") with the city,
the mother of all children,
his mistress,
did Gudea,
ruler
of Lagash,
build
her temple in the city Girsu (lit. "her Girsu-city-house").
Her holy (copper) water pail
he fashioned for her.
Her august queenly seat
he fashioned for her,
and brought it in for her in her august temple.
From the highlands (of) Magan
he brought down dolerite
and shaped (lit. "gave birth to") it
into his statue.
"The queen (entitled) to make decisions in (all) heaven and earth,
Nintur,
mother of the gods,
has lengthened the life
of Gudea,
the man (in charge) of building the temple"
he named it for her
and brought it in for her into the Temple.

Taking the information here given as a starting point for trying to understand the character of the goddess to whom the statue was dedicated we may begin

[11] For DUB as graphic variant of URUDU in older time see *CAD* E p. 321 s. v. *eru* A quoting A III/5:11.

by considering the second of the two names used for her, Nintur. This name, it may be noted first of all, is written with a sign t u r $_5$ (TU) which, as shown by its earlier forms [12], was originally the picture of a reed-hut with a tall decorative spar or spire, similar to the reed-huts pictured in early representations of sheepfolds and cowpens [13]. Such a picture also underlies another sign with value t u r, the sign t ù r, "cowpen" (Akk. *tarbāṣu*) [14], and the difference between this sign and t u r $_5$ in their original forms is so slight — in t ù r the spar points straight up, in t u r $_5$ it tilts — that it is tempting to assume that they were originally mere orthographic variants of a single original sign picturing the hut of a cowpen and representing the Sumerian word t u r "cowpen".

In a study of the huts shown in early representations of sheepfolds and cowpens P. Delougaz has called attention to the small and weak lambs or calves that are typically shown emerging from them, and he argues convincingly that they are meant to represent newborn animals coming out of a hut to which the mother animals were brought to give them needed protection and help in lambing or calving [15].

The suggested function of the hut in fold and pen as a "birth-hut", a shelter for the pregnant ewes and cows when about to give birth and, presumably, a shelter also for animals needing protection and care because of sickness, goes far to explain and unify the values and meaning attested for the sign t u r $_5$ (TU). When read t u r $_5$ it can denote "baby" (*šerrum*), "weak" (*la'u*), "young" (*ṣehrum*), "delicate" (*lakû*), and "goat-kid" (*laliu*); when read d ú it means "to shape", "construct" (*banû*) and "to give birth" (*walādu*), while, significantly, when read d u r $_x$ it denotes "illness" (*murṣu*), "ill" (*marṣu*), "to fall ill" (*marāṣu*), and "to distress" (*murruṣu*) [16].

The word for the birth-hut in the cowpen, t u r, was also used metaphorically to designate the organ which in a woman's body fulfilled a similar function, the womb. This metaphorical use is strikingly attested to in an incantation to aid childbirth [17] which begins:

m u n u s – e é – t ù r – a m a š – k ù – g a i m – d a – a n – z é – e b – b a – n a
é – d ú – u d – ğ á l é – t ù r – a m a š – k ù – g a i m – d a – a n – z e – e b – b a – n a
n u m u n – z i – n a m – l ú – u $_x$ – ⌜k a⌝ š à – g a b a – n i – i n – r i
^{e}e $_4$ – š à – g e – r i – a k a – k é š $_{ki}$ – š $_i$ – d u l ú – r a d u m u s u m $_{šu}$ – m u
m u n u s – e ù – l à l – e z ú ⌜z u⌝ – b i – b i – i n $_{gu}$ – u $_b$ – g u b š à – g a l – d a b i – i n – g u r

[12] See A. Falkenstein, *Archaische Texte aus Uruk* (Berlin/Leipzig 1936), sign no. 213.

[13] See P. P. Delougaz, "Animals Emerging from a Hut", *JNES* 27 (1968) 196 and 186 f., figs. 2-11.

[14] See Falkenstein, *Archaische Texte aus Uruk*.

[15] Delougaz, *JNES* 27 (1968) 184-197.

[16] See á – A – *nāqu* VII$_4$ 60-70 published by Goetze, *JCS* 13 (1959) 120.

[17] VAT 8381 quoted W. W. Hallo and J. J. A. van Dijk, *The Exaltation of Inanna* (New Haven 1968), p. 53 [22]. We know the full text from a xerox of van Dijk's copy which he generously placed at our disposal. The text strikingly confirms Delougaz's interpretation of the birth-hut motif in the article quoted above notes 13 and 15.

ú-làl-níğ-ki!-áğ-ğá-nizuzú bíbí-ingu-ub-gub
šà-gal-da bíbí-in-gur
munus-e u4-da-ni iti-da-ni [n]a-nam

This woman, as she sweetened with it the birth-hut-house of the pure fold,
as she sweetened with it the house causing birth-giving to be, the birth-hut-house of the pure fold,
had (the makings) of a rightful human seed ejected in (her) insides,
a semen ejected in (her) insides, contracted for (by marriage contract), giving the man a son.
This woman tasted the honeysweet pasturage and from the fodder she grew thick,
Tasted the honeysweet pasturage the thing she loves, and from the fodder she grew thick
This woman – it is verily her month, her day.

The metaphorical use of the word for birth-hut to denote uterus, womb, here met with helps to clarify the etymology of the usual Sumerian word for "womb", "uterus", which was written šag4-tùr and was borrowed into Akkadian as *šassūrum* "womb". It apparently constitutes a partitive apposition[18] and denotes "the birth-hut (tùr) of the insides/innards (šag4)". Also the use of the sign TÙR with inscribed MUNUS "woman" to denote the word arhuš "womb", "compassion", thus becomes clear.

Returning, then, to the sign tur5 (TU) and its use in the name Nintur, we may assume an original meaning "Lady (nin) Birth-hut (tur5)" with a potential metaphorical use also for "Lady (nin) Womb (tur5)". This latter meaning for the name is directly attested by the equations given in the god-list An:*Anum*[19]

dNin(!)-tur5[20]: d*Šassūrum* (ŠÀ–TUR)
dŠag4-tùr : " "

and is further confirmed by the Akkadian name for the goddess, *Šassūrum*, used e. g., in the Atra-hasīs myth[21].

With the philological evidence agrees the archaeological testimony. It has long been realized that a curious omega-shaped (Ω) emblem found on boundary stones and on Old-Babylonian clay plaques is an emblem of Nintur/Ninhursağa. In a detailed study of this emblem Henri Frankfort was able to demonstrate, by adducing Egyptian parallels, that it constituted a schematic

[18] On this type of noun formation see Poebel, *ZA* 37 (1927) 248.

[19] *KAV* 64 ovb i. 18-19 (correct to rev. ii. 18-19) cf. the related entries *CT* XXIV pl. 26 obv. ii 135, *CT* XXV pl. 30 K 2109 i. 12 and *MSL* IV, p. 6. 35.

[20] Thus emend with Landsberger *MSL* IV, p. 6 note to l. 35, and restore with him *CT* XXIV pl. 26.25 as dNin!-tùr:Ša-su-ru [dŠà-tur: MIN MIN].

[21] See W. G. Lambert and A. R. Millard, *Atra-hasīs* (Oxford 1969), p. 56 K 6634 (V) Obv. 1-2 and lines 189-190 of the OB version, also op. cit. p. 64 line 295, p. 102 line 43 and 46 etc.

rendering of the uterus of a cow [22]. Since the divine emblems of Ancient Mesopotamia may generally be considered survivals of older, nonanthropomorph forms of the deities they belong to [23] Nintur was probably at one time envisaged under the form of a cow's uterus, the organ in and of which she was the power.

The goddess then, would seem to be in essence numinous power in and behind the reproductive process of the cow. That power had its locus in the birth-hut of the cowpen and so became symbolized by it. More specifically yet, it resided in the reproductive organ, the uterus, of the cow for which, metaphorically, the birth-hut came to serve, as "birth-hut of the insides".

It was, as the herdsman well knew, a power for good, a power vitally important to him and his survival. But it was not only and always that, occasionally instead of a live well-made calf it might decide to produce a monstrous stillborn young. As the great hymn to Enlil says [24]:

kur-gal ^{d}En-líl-da nu-me-a
^{d}Nin-tur$_5$ nu-ug$_5$-ge sağ-ğiš nu-ra-ra
áb-e é-tùr-ra amar-bi nu-šub-bé
u$_8$ amaš-bi-a sila$_4$-ğá-gig nu-è

Without (warrant of) the great mountain, Enlil,
Nintur could not let die, could not slay,
The cow would not lose its calf in the cattlepen,
The ewe not bring forth a premature lamb in its sheepfold.

It was a power not to be taken for granted.

Having thus tentatively located the concrete locus in the external world "the hut for calving in the cowpen", in which a feeling of being in the presence of divine power such as is characteristic of Nintur may naturally have arisen, we may consider next the other name used for her by Gudea, that of Ninhursağa.

The Name Ninhursağa

The name Ninhursağa is etymologically transparent; it means "The Lady of the Foothills". hursağ, the term which we have translated "Foothills", has actually a somewhat wider application. It can denote the foothills and near mountain ranges of the Iranian highlands in the East, but also the stony Arabian desert bordering the alluvium of Southern Mesopotamia in the west. Basically, therefore, it would seem to denote "stony ground", "rock" [25].

[22] H. Frankfort, "A Note on the Lady of Birth", *JNES* 3 (1944) 198 ff. cf. Ursula Seidl, "Die babylonischen Kudurru-Reliefs", *Baghdader Mitteilungen* 4 (Berlin 1968), p. 202.

[23] See my remarks in "Formative Tendencies in Sumerian Religion", p. 269 = *TIT*, p. 3.

[24] A. Falkenstein, *Sumerische Götterlieder I* (Heidelberg 1959), p. 17: lines 123-126.

[25] This is suggested by the fact that Abiak on the Áb-gal (*apkallatu*) canal in the extreme west of the Mesopotamian alluvial bordered on the hur-sağ according to the text giving borders fixed by Ur-Nammu published by F. R. Kraus, *ZA* 51 (1955) 45-75. See p. 46 obv. ii 15-19 describing the west side (Kraus, p. 46 [1]) uru-ambar-ta hur-sağ-šè hur-sağ-ta

As "the Lady of the Foothills" Ninhursağa may be assumed to be the numen of rocky or hilly land, particularly perhaps of the eastern near ranges, and this is confirmed by the passage Gudea Cyl. B xxii (Frgm. S ii 3-5) [26] in which the parentage of her son, the power in the yearly floods Ninurta/Ningirsu, is given thus:

^{d}Nin-⌜ĝír⌝-[su]e_4-zi-^{d}En-l[íl-lá]
Hur-saĝ-e dú-da
maš-lulim-e ga-zi-gu_7-a

Ningirsu, rightful semen of Enlil,
born by Hursag (the foothills) [27]
suckled right with milk by deer

Here Hursag, the personified "Foothills" serves as name for the numinous power in them, without any anthropomorph honorary epithet nin- "Lady" [28], just as the early designation of Nin-tur_5 was simply Tur_5 "birth-hut".

As numen of the rocky wilds the wildlife belonged to her. In the passage just cited her son is suckled by deer and correspondingly Gudea speaks of deer as belonging to "the mother of Ningirsu" when he appoints a divine "shepherd of deer" called "Lord deer" to care for them in the Temple he built for Ningirsu (Cyl. B v 2-6) [29].

á-hur-saĝ-ĝá a-ba-a-dab$_5$ AN-ZA-GÀR-^{d}Nu-muš-da-ka-šè "from Marsh-town to the hursaĝ, from the hursaĝ, after you have passed along the side of the hursaĝ, to the Watchtower of Numushda". Note also the locality AN-ZA-GÀR-hur-saĝ-ĝá "The watchtower of the hursaĝ", ibid obv. i 20-21 on Abiak's northern boundary. In the region where we must locate Abiak no mountains, only the stony Arabian plateau, can come into consideration.

[26] *TCL* VIII pl. LIV, see André Baer in *RA* 65 (1971) 1-14 especially the reconstruction on p. 4 f.

[27] With this should be compared the description of Ningirsu in Cyl. A VIII. 15-16 lugal-$ĝu_{10}$ ^{d}Nin-ĝír-su en e_4-huš-gi_4-a en-zi e_4-kur-gal-e-ri-a "O my master Ningirsu, lord, seminal water, reddened in the deflowering. True lord, seminal water emitted by the 'Great Mountain' (Enlil)". The reference is to the red waters of the flood as they come down the mountain tributaries.

[28] Similarly the "Disputation Between Winter and Summer" 11. 11-16 tells how Enlil in the shape of a bull copulated with Hur-saĝ engendering Summer and Winter to whom she subsequently gave birth. Here too there is no nin and no divine determinative before her name, she is just Hur-saĝ. We owe knowledge of this episode to M. Civil who generously placed his MS. edition of the Disputation at our disposal.

[29] Note also that according to Cyl B vi 4-7 deer's milk formed part of the breakfast with which Gudea woke up Ningirsu on his first morning in the new built temple. Note also the panel from the Ninhursağa Temple at Al Obeid showing Ningirsu in his older form as Imdugud with stags. Frankfort finely comments on it in his *The Art and Architecture of the Ancient Orient* (Harmondsworth, Middlesex 1956), p. 30: "It shows in the centre the lion-headed eagle Imdugud, gripping a stag with either claw. The gesture does not represent aggression but affinity: the same deity is symbolized by bird and deer". Whether in the light of these connections the stereotype phrase in royal titles ga-zi-gu_7-a ^{d}Nin-hur-saĝ-ĝa-ka "suckled right with milk by Ninhursağa" should be understood as referring to deer's milk is possible, but hardly provable.

Also other wild animals appear as Ninhursağa's children. In a dying-god lament of her cult she bewails her sons, wild asses perished in the inhospitable wilds or lost to human captors [30]:

I am a mother who has given birth, I was mated in vain, was kissed
in vain was delivered of a sound young in vain

and she sobs [31]

I gave birth, I gave birth, I gave birth to free youngs
— for what did I give birth?
I became pregnant, I became pregnant, I became
pregnant with free youngs — for what did I become pregnant?
I the mother who gave (them) birth, what have I
for my giving birth, (for) my becoming pregnant?
I gave birth to a ..., a strong one killed it,
what have I for it?
I gave birth to ..., a strong one killed it,
what have I for it?
I gave birth to a choice donkey-steed, a lord mounted it
— what have I for it?
I gave birth to a strong mule, a lord hitched it up ...
— what have I for it?

And as she is the mother of the free wild animals so correspondingly is her husband in later tradition, Shulpae, likewise god of wildlife [32].

níğ-úr-lim mú-an-edin-dağal-la
máš-anše-níğ-zi-ğál-edin-na
ur-sag-^{d}Šul-pa-è za-e lugal-bi-me-en
An-lugal-diğir-re-e-ne-ke$_4$ šu-zu im-ma-an-sum

the quadrupeds of the broad high-desert
the bucks and the asses, the (wild)-life of the desert,
O warrior Šulpae, you are their owner,
An, king of the gods, gave them into your (hands).

We mentioned in passing the inhospitable side of the mountain and desert wilderness [33]. This, though she bewails it in the lament just quoted, is just as much part of her nature as giving birth to wildlife. In a lament from the cult

[30] *SK* 198 obv. 18-19.
[31] Id. 21-28.
[32] Falkenstein, *ZA* 55 (1953) 37 lines 35-38.
[33] Cf. the warning given Lugalbanda by his brothers and comrades: hur-sağ-gal lú-dili nu-du-ù-dam lú-bi lú-ra nu-gi$_4$-gi$_4$-dam ba-ra-gi$_4$-gi$_4$-nam "Since a single man is not to travel the great hursag, since such a man would not be returning to men, so you will never return", Claus Wilcke, *Das Lugalbandaepos* (Wiesbaden 1969) p. 126. 11. 335-336.

of her daughter, the goddess Lisin, the latter bewails a son of hers, a young donkey stallion that has perished, straying from the herd into the wilderness. She hopes against hope that it will be found or that it has reached cultivated and inhabited areas [34]:

He will bring it back to me! He will bring
it back to me!
My strong deputy will bring my donkey stallion
back to me!
Will bring my herd, which got cut off from me,
back to me!
Will bring my foal back to me from its
destroyed lair!
Will bring my wild ass, which got cut off in
the woods, back to me!
[The canal inspector] floating down [ri]ver (in his
boat) will bring it back to me
[The farmer] will bring it back to me from its
flooded field (i. e. where it may be mired down).

But she knows very well that it is in vain, her donkey-stallion has perished in the wilds and she bitterly blames the numen of the wilderness, her mother Ninhursaĝa [35]:

To whom should I compare her? To whom
should I compare her?
I, to whom should I compare her?
My mother let my (only) one die! —
I, to whom should I compare her?
My mother who bore me, Ninhursaĝa –
My mother let it die! –
I, to whom should I compare her?
To the bitch, that has no motherly compassion,
let me compare her!
Lisina, out of her grief sits alone.

As to the earliest external form or forms under which Ninhursaĝa was envisaged we are as yet largely in the dark. The form of the deer might be a possibility, that of the cow likewise, the latter was perhaps assumed for the H u r s a ĝ in the introduction to the Dispute between Summer and Winter [36]. In Ninhursaĝa, then, the character of the wilderness of foothills and stony desert is reflected: the power in it to produce wildlife in all its many-fold abundance, but also its power to starve and kill, imperiling animals and humans alike if they stray into its barren pathless regions.

[34] *UET* VI_2 144 obv. 8-14 cf. for restorations ibid. 25-27.
[35] *UET* VI_2 144 obv. 28-33.
[36] See above note 28.

All of this is very different from what we found in the case of the name Nintur. There the locus in the external world where a feeling of being in the presence of divine power of that particular kind appeared to be the birth-hut in the cowpen of the domestic herd; here the locus of confrontation would seem to be the wild of foothills and stony desert. We seem, thus, to be dealing with two quite differently conceptualized responses; which is of course but another way of saying that Nintur and Ninhursağa appear to be in origin two distinct and different deities, not one.

Such a conclusion is, in fact very clearly borne out by our older evidence as pointed out in detail by Sjöberg [37]. In a godlist from Fara (Deimel, *Schultexte*, No. 1 col. i) Nintur is listed as ^{d}Tur$_5$. In col. vi of the same text, as a separate deity, occurs ^{d}Nin-hur-sağ. From the period of Agade, some centuries later, we have an inscription of Narām-Sîn which likewise treats the two as distinct. It lists ^{d}*Nin-hur-sağ* *ù* ^{d}*Nin-tur*$_5$ "Ninhursağa and Nintur". Even as late as Ur III it seems possible that the distinction between them maintained itself here and there. At least the list of offerings *TCL* V No. 60.53 lists items for ^{d}Nin-tur$_5$ in ii 29 and such for ^{d}Nin-hur-sağ separately in rev. ii. 3. In the case of a list of offerings like this, however, one cannot really be sure that the different listing implies different deities. They could very well stand for offerings to what was considered the same goddess worshiped under different names at different sanctuaries [38].

These clear indications that Nintur and Ninhursağa originally were separate, different, deities naturally raise the question of how and why they could come to be identified and to blend so thoroughly as the data from the time of Gudea and onwards indicate that they actually did [39].

We may here point first to the fact that in spite of differences the two deities had very essential features in common: both were goddesses, both were powers for animal fecundity and reproduction visualized as mother figures,

[37] Sjöberg and Bergman, *TH* p. 72-73. It does not, though, look to us as if the Temple-Hymns treat Nintur and Ninhursağa as different.

[38] The use of the Sumerian Floodstory (*PBS* V no. 1) — Nintur, when her relations to her offspring are presented (col. i. 3′, iii. 15′), Ninhursağa when she makes decisions with An, Enlil, and Enki (col. i. 13′, iii. 18′) — is probably to be explained as "role-true" designations only. See below p. 295 f.

[39] See below note 84 for the Gudea evidence. In Old Babylonian times and later this blending may also have affected the pictorial renderings. In the well known description of ^{d}*Nin-tur*$_5$ *šu-ut Diğir-mah* "the Ninturs of Diğirmah" (F. Köcher, "Der Babylonische Gottertypentext", *MIO* I [Berlin 1953] 57-107 obv. iii 38′-51′) the puzzling *iš-tu me-sir-ri-šá ana kan-tap-pi-šá qú-li-ip-tu kîma* (GIM) *ṣiri* (MUŠ) *a-ta-at* "from her girdle to her footsoles is seen snakeskin as of a snake", which does not fit with anything else we know, may be due to misinterpretation of the traditional mountain pattern, suitable for Ninhursağa, as serpent scales. Further blending of entities and features may be involved if, as the plural form *šūt* might suggest the description has reference to the wombgoddesses or *šassurāti* identified with ^{d}Nin!-tur$_5$: ^{d}*Šassurum* (ŠÀ-TÙR) of the emended *KAV* 64 obv. i. 18 (See above p. 280 with notes 19 and 20). Another possible case of blending of attributes is in the much discussed clay plaque showing the goddess with symbols hanging on the wall behind her and two emaciated figures squatting on either side of her. Edith Porada who has repeatedly treated these and parallelfigures, has identified them, undoubt-

and both had in their character the polarity of being not only powers for new life but at the same time also powers for killing and death. Strikingly different is actually mainly that one, Nintur, is a power for reproduction in domestic animals, the other, Ninhursağa, for reproduction in wild animals. However, since calving and lambing take place in the spring when herds and flocks are away from home seeking pasture in foothills and desert, there is an actual coalescence of "locus" for the two powers. The birth-hut of Nintur actually stands in the domain of Ninhursağa, and it is understandable, to say the least, that herdsmen seeing how the power for animal reproduction was not limited to their flocks but manifested itself all around them in the increase of wildlife in spring, may have felt a single power to be active whether its name be Nintur or Ninhursağa.

The Epithet A m a *"Mother"* [40]

In the inscription on Gudea's Statue A the goddess to whom the statue is dedicated is given the epithet a m a – d u m u – d u m u – n e "mother of all little ones" following her name Ninhursağa, and a m a d i ĝ i r – r e – n e – k e$_4$ "mother of the gods" following her name Nintur. The first of these epithets, "mother of all little ones" indicates that the goddess by Gudea's time had transcended her original limitations as a power in animal procreation, wild or tame (bovine), and had become a goddess of birth generally. That this development began early, long before Gudea, is suggested by the fact that already Mesalim calls himself son of Ninhursağa, presumably indicating that he considered her the power in, and to, his own human birth [41].

In itself, of course, the term a m a , mother, common to both of the epithets of Statue A has both physical and social connotations. However, what we have already learned about the locus of manifestation of the goddess designated by it, certainly suggests that the former, the physical, implications of the word here are meant to predominate. A glance at the long list of names for the goddess given in the Great Godlist and A n :*Anum* bears this out, for while they contain few names, if any, that can be interpreted as expressing aspects of social motherhood, they abound in names relating to aspects of physical motherhood.

Of such aspects the most prominent, perhaps, is that of the gradual development of the embryo which is seen as a power in the wombgoddess to shape the formless early embryo into a distinctive, recognizable animal or human

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edly rightly, as symbols of death, and following a suggestion of Edzard's she has tentatively considered seeing in them representations of the demon Kubu, the power in the premature, stillborn, child. Difficult here, however, is the emaciated, clearly articulated, and rather mature bodies of the figures which do not suggest premature, stillborn, children but adults starving to death. It may therefore be worth considering whether we have here rather victims of the darker side of Ninhursağa's character which lets animals and men die from hunger when they lose their way in her wilderness domain.

[40] With this section cf. Poebel, *PBS* IV, p. 31-34.

[41] The argument is not conclusive for a goddess was apparently capable of taking appropriate shape for such occasions, see Gudea Cyl A xxiii 21 where Gudea, whose personal goddess was the cowgoddess Ninsuna, is told: "You are one born by a flawless cow in its (manifestation as) woman".

shape. This particular power in the goddess is often referred to in the texts. Thus a hymn to the Temple É-HI-ZA^{ki} states [42].

a m a ^{d}N i n - t u r$_5$ - n i n - u l u t i m - m a
š a g$_4$ - k i - k u k k ú - g a - k i n - a k - e
l u g a l - ù - d ú m u š - z i k é s - d i
e n - ù - d ú s a ğ - m e n - ğ á - ğ á š u - n a ì - ğ á l

Mother Nintur, mistress of shaping,
Working in the innards ("heart") where darkness (reigns), —
to give birth to kings — to tie on the rightful tiara
to give birth to *enu's*, to place crowns on (their) heads, is in her hand.

A self-praise of Ur-Nammu's [43] proudly states that

[^{d}N] i n - t u r$_5$ - r e ğ á - e m u - u n - d í m - d í m - e n g a - r i - m u n u - t u k

Nintur fashioned me bit for bit, I have none who oppose me

Hammurabi calls Nintur *um-mu ba-ni-ti* "my mother who shaped me" [44] and Sennacherib, using her Akkadian name, tells us that ^{d}*Be-lit-ili*$^{pl.}$ *be-lit nab-ni-ti i-na šasurri a-ga-ri-in-ni a-lit-ti-ia ki-niš ippalsa-an-ni-ma ú-ṣab-ba-a nab-ni-ti* "Belit-ili, mistress of shaping, looked purposively at me in the womb of the mother who gave birth to me, devising my shape" [45] — to cite only a few such passages.

In the list of names of the goddess in A n :*Anum* this function of hers would appear to underlie such names as ^{d}E n - a r h u š - $^{d\,i\,-\,i\,m}$d í m "High priestess 'Fashioning Womb' ", ^{d}N i n - $^{d\,i\,-\,i\,m}$d í m "Lady Fashioner", ^{d}N í ğ - z i - ğ á l - d í m - d í m - m e "Fashioner (of) all things in which is breath of life", and ^{d}S a$_7$ $^{s\,a}$ - l ú - u$_x$ "Creatress of man" [46].

In these names, it will be seen, the powers in the womb to give form have been personified in some measure though not necessarily more than is implied in the use of the honorifics e n "Highpriestess" and n i n "lady", "mistress", we are still dealing with powers in a human or animal organ. Rather further on the road to anthropomorphism and to the borrowing of forms from human society is a group of names in which the form-giving process in the womb is seen as analogous to the kind of shaping and fashioning that artisans do: potters, bronze-casters, and carpenters. Here the goddess, as developer of the embryo, takes form as artisan. She is ^{d}N i n - b a h a r $^{b\,a\,-\,h\,a\,-\,a\,r}$ "Lady potter", ^{d}T i b i r a - k a l a m - m a "Bronze-caster, (caster) of the Nation",

[42] Sjöberg and Bergmann, *TH* p. 46 lines 500-503.
[43] *TRS* no. 12 line 25.
[44] *CH* XLIV 73.
[45] *OIP* I p. 117 lines 3-4.
[46] The power to give shape also has its negative side, the goddess may let the young of an animal or a child be born malformed, a monster or freak, perhaps stillborn (see above page 281). This rather cruelly capricious side to the goddess is told about in the myth of Enki and Ninmah, — Ninmah is another of her many names — where she boasts of her powers to give form, she can make the shape of man good or bad at will, and she proceeds to prove her point by making human freaks.

^{d}Tibira-diĝir-re-e-ne "Bronze-caster, (caster) of the Gods", ^{d}Nagar-nam-lú-u$_x$-lu "Carpenter of Mankind", ^{d}Nagar-šà-ga "Carpenter of (i. e. "in") the innards (Heart/Womb)".

When the process of giving shape to the embryo is complete the time for its birth is at hand. This too was a matter for the goddess. It is she who "sets birthgiving going". Already in a hymn from Abu Ṣalabikh the Temple of Nintur in Kesh, sharing in her essence, is praised for such powers [47]:

Kèš sig$_4$-dú-dú
é-tu^{mušen}-tur-gùn
ama ^{d}Nin-tur$_5$-zag-mí

Kesh! brick (structure) causing birthgiving,
house (which is) a productive young dove —
Praise be Mother Nintur!

and in the great hymn in praise of this Temple we hear, in the old version from Abu Ṣalabikh [48], that

^{d}Dur$_9$ ama (!)-gal dú-dú- al-ĝá-ĝá
(Nin)tur, the great mother, sets birth-giving going.

The corresponding passage in the later version reads [49]:

^{d}Nin-hur-saĝ-ĝá ušum-gal-àm šag$_4$ im-mi-in-dab$_5$-(var. -ús) [50]
^{d}Nin-tu-ra-ama-gal-la (var. -àm) dú-dú-mu-un-ĝá-ĝá

None but Ninhursaĝa, uniquely great, makes the innards (heart) contract (var. "pushes the innards"),
None but Nintur, the great mother, sets birth-giving going.

The same phrase is also used of the goddess, under her name Ninmenna, in the Myth of the Creation of the Hoe [51]

^{d}Nin-men-na-ke$_4$ dú-dú al-ĝá-ĝá
Ninmenna sets birth-giving going.

The name which specifically designates the goddess as setting birth-giving going would seem to be ^{d}*A-ru-ru*, a name which may be interpreted as "The germ loosener" [52].

The goddess, though, is not only the power to set birth-giving going, she is the power in and to all facets of the process of birth. The form in which she

[47] Biggs, *ZA* 61 (1971) 195.
[48] Biggs, *ZA* 61 (1971) 202 line 78.
[49] Gragg, "The Keš Temple Hymn", in Sjöberg and Bergmann, *TH* p. 172: lines 77-78.
[50] With šag$_4$ ... dab$_5$ cf. šag$_4$ ... šu ... dib-dib: *ṣepēru* "pinch", "contract" *CAD* Ṣ s. v. *ṣepēru* p. 132 bilingual section quoting *CT* XVII 25:34f and *KAR* 368:7f and other occurrences.
[51] *PBS* X$_2$ no. 16 i.27.
[52] Cf. [ru]:[RU]: *ra-mu-ú* "to loosen", "to free" á-A-*nāqu* VI$_4$ 168.

manifests herself is that of the body of the woman in labor and so she is ^{d}A m a – d ù g $^{d u}$ – $^{b a}$ – $^{a d}$ b a d "The mother spreading her legs" and finally ^{d}A m a – d ú – u d – d a (var. ^{d}A m a – ù – d ú – d a) "The mother who has given birth". Here belongs probably also the name ^{d}M u d $^{k i}$ – $^{e š}$ k e š – d a "Bloodstancher" which may refer to the ceasing of bleeding after birth [53].

Her holy (copper) waterpail

One of the things Gudea tells us he had made for Ninhursağa/Nintur was "her holy (copper) waterpail". What this waterpail was for, and what its relation to the goddess was, may become clearer as we consider further her various aspects.

A characteristic feature of the growing dominance of anthropomorphism in Ancient Mesopotamian religion was a trend to disassociate numinous power from its original locus, from the phenomenon in which it was originally seen as immanent, and to view it instead as a professional or an official in charge of it. In the case of Ninhursağa/Nintur this tendency led, as we have seen earlier, to the use of craftsman metaphors to designate her. A far more constant such image, however, is that of the midwife; she is ^{d}Š a g$_4$ – z u – d i ĝ i r – e – n e "The midwife of the gods" [54] and she is Š a g$_4$ – z u – k a l a m – m a "midwife of the nation" [55]. We possess a description of her office and insignia given by Inanna in the myth of Enki and World Order [56]. It reads.

^{d}A – r u – r u n[i n$_9$ – ^{d}En – l í l – l a – k] e$_4$
^{d}N i n – t u r$_5$ n i n – h[u r – s a ĝ – ĝ á – k e$_4$]
[S i g$_4$] – d ú – d ú – k ù g n a m – e n – n a – n i
š[u h é – i m – m a – a n – t i]
... – n i[h] é – i m – m a – a n ... ZUBI + SÌG – a – n i
h é – i m – m a – d a – a n – r i
s i l a$_3$ – ĝ a r – r a – $^{n a}$$_4$ z a – g ì n – a – n i
š u h é – i m – m a – a n – t i
a – l a l – k u g – n a – d i$_5$ – g a – n i
h é – i m – m a – d a – a n – r i
š a g$_4$ – z u k a l a m – m a h é – e m

Aruru, the sister of Enlil,
Nintur, the mistress of the foothills,
verily holds in (her) hand the sacred brick causing
birthgiving (emblem of) her office
of Highpriestess [57]
verily ...s her ..., verily

[53] For these names see A n :*Anum CT* XXIV pl. 13 i 33-35, cf. the similar list pl. 25 ii. 92-93.
[54] *CT* XXIV. 25 ii. 88.
[55] Enki and World Order line 399, see quotation below.
[56] lines 393-399.
[57] For this restoration see the old Kesh hymn quoted above p. 288. Biggs *ZA* 61 p. 195. The reference is to the birthbrick which is her symbol (see e. g., Lambert and Millard, *Atra-hasīs*, p. 60 lines 259, and 6, p. 61 lines 15-16 and 288 etc. The great godlist lists it as a manifestation of Diĝirmah/Bêlitilī (= Nintur, Ninhursağa etc.) under the name [^{d}] S i g$_4$ – z a – g ĩ n – n a "the (divine)

carries on her arm her birth-chair (?) [58]
verily holds in (her) hand her lapis lazuli
(vessel) in which the afterbirth is placed [59]
verily carries on her arm her holy consecrated [60] waterpail
verily, she is the midwife of the nation

As will be seen this listing of Nintur's professional equipment includes the very kind of pail (a l a l) that Gudea speaks of. Presumably it was used by midwives to bring, and to heat, water for rinsing blood off the mother and newborn child. Gudea is thus seeing to it that the goddess is properly equipped for her profession.

The function of the midwife in Ancient Mesopotamia, and so the potentialities of the midwife metaphor, is rather sketchily known to us. Yet a few points stand out.

Before beginning her task the midwife covered her hair with a headdress [61] which may be reflected in the name ^{d}N i n – m e n – n a "Lady of the Headdress" for the birthgoddess. The prospective mother seems to have been placed in a birth-chair [62], but our data here are vague and unclear. It is possible that, as in modern practice, the membranes, if intact, were ruptured with a slender rod [63]. The term for the actual delivery would seem to be d u m u – z i (g)

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pure brick" *CT* XXIV pl. 22. obv. i. 114. A restoration to [n a $_4$] – TU–TU "birthstones" suggested by Falkenstein is difficult because, as he himself noted, the term for "birthstones" is n a $_4$ – ù – d ú, not n a $_4$ – d ú – d ú.

[58] Reading and meaning of ZUBI–SÌG is uncertain. It is mentioned in the myth of Enki and Ninmah where Enki tells his mother Nammu to have it put together (k é s – d a – n i) when she is to give birth to man.

[59] With the term s i l a $_3$ – ĝ a r – r a – ^{na} $_4$ z a – g i n – a – n i "her lapis-lazuli ..." cf. [s] i – l a SÌL.A *si–[l]i–tum, i–pu* "uterine membranes" "afterbirth" á–A–*nāqu* I 16:27f. and u z u – e $_4$ – s i l a – ĝ a r – r a i. e. part of the body in which (birth) water and afterbirth are situated, explained as *re–e–mu* "womb" and *ru–ub–şu* "repository" as well as *i–pu* "membrane", "afterbirth" and *i–bahu* "womb", "uterine membrane", see *CAD* I s. v. *ipu* (p. 173) and *ibahu* (p. 1). The term would thus seem to denote "(Vessel) in which the afterbirth is placed". It is presumably a shortened form for d u k – s i l a $_3$ – ĝ a r – r a "pot in which the afterbirth is placed" which occurs in the orthography d u k – s i – l a $_3$ – ĝ a r in *SLT* 32 rev. iii. 12, a precursor to Hh X (see *MSL* VII, p. 204, 102). The term also occurs in the hymn to Nininsina *SRT* 6 iii 1-2 7 obv. 11-12 d ù g k i – s i k i l – l i – l i ĝ á – ĝ á – d è d u k – s i l a $_3$ – ĝ a r – r a – k e $_4$ s i – s á – e – d è g i – d u r k u $_5$ – d è n a m – t a r – r e – d è "to bring the Ardatlili demon to her knees, to deal rightly with the pot of the deposited afterbirth, to cut the navel cord and determine fate". For all these references see also Falkenstein, *ZA* 56 (1964) 90.

[60] n a – d i $_5$ (g) means "to instruct Since water etc. that has been "instructed" by the orders of a god, a spell, are magically pure, holy, n a – d i $_5$ – g a can in such context be translated as *ellu* "pure", "holy" in Akkadian. Here the reference is to the goddess' waterpail over which the appropriate spells have been said.

[61] Lambert and Millard, *Atra–hasīs*, p. 62. 284 (read with von Soden *Or* 38 [1969] 425f. note 2 *i'–pu–ur*).

[62] See above note 58.

[63] Lambert and Millard, op. cit., p. 62 line 282. We restore ⌜*hu–lu*⌝ *–up pa–le–e* "the insertion of the *palû*". For the word *palû*, a loan from Sumerian b a l a, we have in mind the value "spindle" of the latter and assume *palû* refers to a pointed implement suitable for its purpose.

"extracting the child" [64] after which the umbilical cord was cut [65]. The knife for cutting the umbilical cord is represented on boundary stones of the First Millennium as an emblem of Ninhursağa/Nintur together with the Ω symbol [66] but in the texts it is so far only referred to in the case of another birthgoddess, ^{d}Nin-mug "Lady Vulva" [67] who seems to have been kept apart from Ninhursağa/Nintur [68].

The time of birth, and particularly the time of cutting of the umbilical cord, would seem to have been considered a formative period in which any rash or angry or illomened word might have lasting consequences for the child [69]. It was necessary for the midwife and her helpers to speak only in the most pleasant mood. The goddess Manungal, in boasting of her skill as birthhelper, expresses it succinctly [70]:

^{d}Nin-tur$_5$-re (var. -e) ki-nam-dumu-zi-ka
(var. nam-dumu-zu-a-ka and nam-^{d}Dumu-zi-ka) mu-da-an-gub-bé
gi-dur-ku$_5$-da nam-tar-re-da KA-sa$_6$-ge-bi mu-zu

[64] Cf. the Manungal hymn *PBS* I$_2$ no. 104 rev. 4-5 and duplicates cited below.

[65] See *SRT* 6 iii.1-2 = 7 obv. 11-12 quoted above in note 57 and *PBS* I$_2$ no. 104 rev. 4-5 and duplicates quoted below.

[66] See Seidl, *Baghdader Mitteilungen* IV (Berlin 1968), p. 131f. and literature there cited.

[67] I. Bernhard and S. N. Kramer, *Enki und die Weltordnung* (Jaen 1959), p. 239 line 406 ^{na}$_4$ğír-zú-gal-⌜x⌝-⌜sur⌝-ra-ni hé-em-⌜ma⌝-da-an-ri "she verily carries her great ... flint-flake knife". For ^{na}$_4$ğír-zú : *ṣurru* "flint" etc. see *CAD* Ṣ, p. 257 s. v. *ṣurru*. Another translation is *luṭûm* (also *leṭû*, *ludû*) "flake", "blade", see *AHw* p. 565 s. v. *luṭû(m)*. This latter was presumably also originally of flint, but the word was also used of a bronze blade (cf. *MSL* VII, p. 162 line 52 [ğír-z]ú-zabar : *lu-ṭu-ú* and other passages cited by von Soden, *AHw*) and apparently of a sharp flake or sliver of reed used for cutting (cf. *MSL* VII, p. 44.177-179 gi-zú : *le-ṭu-ú* "reed sliver" (lit. "tooth"), gi-zú-ra-ah : MIN "biting/gashing reed" and gi-zú-níğ-zú-ra-ah : MIN "reed sliver (for) biting/gashing things" and other passages cited *AHw* p. 565 *liṭû(m)* (1). The Akkadian *luṭû* in the meaning "sliver of reed for cutting" seems to occur also in Atra-hasīs MS S obv. iii. 5-7 (see Lambert and Millard, *Atra-hasīs*, p. 60 and addendum p. xi-xii) where we would read *14 ki-[ir]-ṣi tàk-ri-iṣ 7 ki-ir-ṣi ana imitti* (ZAG) *taš-ku-un 7 ki-ir-ṣi ana šumēli* (GÙB) *taš-ku-un ina be-ru-šu-nu i-ta-di libitta* (SIG$_4$) *lu-ṭa-a ap-pa-ri ba-ti!-iq a-bu-un-na-te tep-te-ši* "Fourteen piches (of clay) she pinched off, seven pinches she placed to the right, seven pinches she placed to the left, between them she laid the brick, the reed-sliver of the cane brake, the cutter of the umbilical cord, she unsheathed for it (i. e. for the brick, which would presumably serve as cutting board)".

For interpreting the name ^{d}Nin-mug as "Lady Vulva" see á-A-*nāqu* VIII/2 105 mu-ug : MUG: *bi-iṣ-ṣu-[rum]* quoted *CAD* B p. 268 s. v. *biṣṣūru*.

[68] An exception forms perhaps SK 198 obv. 3-4 where the litany seems to equate Ga-ša-an-⌜mug⌝-ga and Ga-ša-an-hur-sa$_{12}$-mà.

[69] See our remarks in E. I. Gordon, *Sumerian Proverbs* (Philadelphia 1959), p. 476f. and note the Gilgamesh passage *PBS* X$_3$ pl. LXVI 35-37 *i-na mi-il-ki ša ilim qá-bi-ma i-na bi-ti-iq a-bu-un-na-ti-šu ši-ma-as-súm* "it was said at a god's suggestion, was determined for him at the cutting of his umbilical cord".

[70] PBS I$_2$ no. 104 rev. 4-5 and duplicates SEM 53 obv. 5, rev. 1, SLTN 70 ii, 3N-T.409, 3N-T.453, and 3N-T.675.

I assist Nintur at the place of delivery (lit. "extraction of the child")
I know the propitious words pertaining to cutting the umbilical cord and determining fate.

After the newborn child had been washed and anointed and the afterbirth had been disposed of [71] the midwife could put on her belt again [72], apparently no knots and restrictions were allowed during birth for their possible magical effects. She then placed a brick symbolic of Nintur on the ground and surrounded it with a ring of flour before she joined in the general merrymaking of the house to celebrate the birth [73].

Names for Nintur/Ninhursağa presenting her as the power active in these various task of the midwife are probably ^{d}Šu-gal-an-zu "The expert hand" [74], referring to the actual delivery, and ^{d}Nin-sağ-sakar-sakar "Lady bather", referring perhaps to washing the blood off the mother and child [75], while the name ^{d}Làl-hur-gal-zu "Expert knower of the 'honeycomb'" (?) [76] would appear to refer to the delivery of the afterbirth, the term "honeycomb" (?) suggested by the appearence of the placenta as a mosaic of smooth tiles. The vessel for carrying the afterbirth to where it was ultimately disposed of was mentioned in the passage from Enki and World Order quoted above [77]. The silence which the goddess imposes upon the birth helpers so that no incautious word may have effects on the fate of the child conceivably underlies her name ^{d}Nin-sig$_5$-sig$_5$ "Lady silencer" [78]. The midwife's task of speaking propitious words while cutting the umbilical cord and thus determining a good fate for the child was referred to in the line from the Manungal hymn quoted above where Manungal says "I know the propitious words pertaining to cutting the umbilical cord and determining fate". It seems likely that it underlies names such as ^{d}Nin-nam-tar-tar-re "Lady (entitled) to determine fates" [79], ^{d}Nin-ka-aš-bar-ra "Lady of Decision-making" [80] and ^{d}Nin-ka-aš-bar-an-ki "Decision-making

[71] See note 58 above.
[72] Lambert and Millard, *Atra-hasīs*, p. 62 line 286.
[73] Op. cit. p. 62 lines 288-290 *bi-it qa-di-iš-ti* in line 290 is the house of the "woman in confinement". It corresponds to *bīt ha-riš-ti* in S iii. 17 (op. cit. p. 62) which has that meaning (cf. also S III.15 *bīt a-li-te ha-riš-ti* "the house of the mother in confinement"). The use of *qadištum* here has clearly overtones of "sacred", "set apart", "taboo", and it must seem likely that the woman after birth was considered surrounded by dangerous powers, a belief well attested to in folklore elsewhere.
[74] An: *Anum*, *CT* XXIV pl. 12.30; Great Godlist, *CT* XXIV pl. 25.90.
[75] Great Godlist, *CT* XXIV pl. 25 obv.ii.79.
[76] An: *Anum*, *CT* XXIV pl. 13 obv. i.36, and Great Godlist, *CT* XXIV pl. 25 obv. ii.94.
[77] See above p. 289 f.
[78] An: *Anum*, *CT* XXIV pl. 12 obv. i.20; Great Godlist, *CT* XXIV pl. 25 obv. ii.84. For the reading of the name see Sjöberg in Sjöberg and Bergmann, *TH* p. 73, comment to line 96.
[79] An: *Anum*, *CT* XXIV pl. 12 obv. i.8; Great Godlist, *CT* XXIV pl. 25 obv. ii.78.
[80] An: *Anum*, *CT* XXIV pl. 12 obv. i.9; Great Godlist, *CT* XXIV pl. 25 obv. ii.78. (Read ^{d}Nin-ka-aš(!)-bar re).

lady of (i. e., "for") heaven and earth" [81], even though they were later on understood more generally as referring to her role in making the great cosmic decisions with An and Enlil.

"Her august queenly seat"

The second votive offering mentioned by Gudea, "her august queenly seat" [82], clearly relates to the social prominence of the goddess, she is a n i n i. e. ạ "lady" or even "q u e e n", and the social prominence implied is pointed up by the adjective m a h "august" in her name ^{d}N i n – m a h "August Lady" [83]. Her rank is further stressed in the epithets Gudea uses in stating how she lengthened his life: "mother of the gods" [84] pointing up her maternal authority and "The Queen (entitled) to make decisions in (all) heaven and earth" [85]. His statement, which serves as name of the statue, is clearly phrased so as to emphasize the high rank of the goddess and therewith the authoritative and incontrovertible character of her decision on Gudea's behalf [86].

The prominence of the goddess which here finds expression is no new thing. Rather, as Poebel was first to point out [87] Southern texts from Eannatum to Gudea consistently include her in the trio of highest gods: An, Enlil and Ninhursağa. The reason for this high position are not altogether clear. Poebel assumes that her city must once have held sway over Babylonia and all the surrounding countries, but no evidence of such political domination of Kesh has come to light, just as no such domination by a city state of An that would have been the reason for his prominence is known. A different approach might seek the reason for her prominence in the fact that as "The Lady of the Foothills" she represents the power in a major element of the visible cosmos [88] as do An "Heaven" and Enlil "Lord Wind" and it is perhaps significant that in her role of associate of An and Enlil in the trio of highest gods she is consistently Ninhursağa, hardly ever Nintur. Still a third viewpoint might suggest that the economic prominence of calving and milkgiving in the cowherder's world, as

[81] A n : *Anum*, *CT* XXIV pl. 12 obv. i.10.
[82] St. A ii.3.
[83] Great Godlist, *CT* XXIV pl. 25 ibv. ii.75.
[84] St. A iii.6.
[85] St. A iii.4.
[86] The epithet a m a – d i ĝ i r – r e – e – n e "mother of the gods", seems actually to belong to Ninhursağa rather than to Nintur; at any rate it is given to the former by both Ur–Bau, Statue iii.8 and Lú–Utu, Claynail 1-2.
[87] *PBS* IV_1, p. 24-31.
[88] See our discussion of the term h u r – s a ĝ – a n – k i – b i – d a – k e$_4$ "the (near) mountainrange of (i. e., belonging to) both heaven and earth", in *JNES* 5 p. 141 = *TIT*, p. 117-118 and note phrases like Gudea Cyl. B xx.10 (= Fragm. 3 See André Baer, "Goudéa Cylindre B, colonnes XVII a XXIV", *RA* 65 [1971] 1-14 with reconstruction on p. 12) h u r – s a ĝ – g a – l a m – m a – g i m a n – k i – t a b a d – b a d – e "(Eninnu) like a towering mountainrange separating heaven from earth" and cf. Cyl. A. xx.7-11 G ù – d è – a s a ĝ – š [è] – n á mu – n á i n i m m u – n a – t a – è é – l u g a l – n a – k a d ù – b i É – n i n n u a n – k i – t a – b a d – b i [m] a š – a m u – n a – a – ĝ á l "Gudea lay down seeking a dream-oracle, a message (lit. "word") came forth for him: his master's house (in) its completion, Eninnu (in) its separating heaven from earth, was there for him in the vision".

of lambing and milkgiving in the shepherds' world, might justify her prominent position also as Nintur. Still, when all is said and done no one clear and convincing answer stands out. What does strike one is rather that the unquestioned traditional prominence of the goddess appears to have created difficulties for the trend toward anthropomorphism in patriarchal political terms. A goddess as supreme ruler, rather than a god, a midwife rather than a warrior, was difficult to fit into the pattern. These difficulties would seem to underlie what looks like determined attempts to reinterpret older symbols in terms of the values and symbols of what might be called politico-morphism. The function of birthgoddess, extended from animal birth to birth generally, was seen as reaching its summit and ultimate raison d'être in the birth of highpriests and rulers, while the headdress with which the midwife traditionally covered her hair became reinterpreted as an emblem of the priestly and royal crowns.

lugal ù-dú en ù-dú šu(!)-ni-a hé-en-ĝál

To give birth to kings, to give birth to high priests is verily in her hand,

says Inanna in Enki and World Order of Nintur, the "midwife of the nation" [89].

nin en-ù-dú-da lugal-ù-dú-da
^{d}Nin-men-na-ke$_4$ dú-dú al-ĝá-ĝá

The mistress of giving birth to high priests, of giving birth to kings, Ninmenna ("The lady of the Headdress") was setting birthgiving going,

we are told in the myth of the creation of the Hoe [90], and

lugal ù-dú mùš-zi kéš-di
en ù-dú sağ men-ĝá-ĝá šu-na i-ĝál

To give birth to kings, tie on the flawles tiara
to give birth to highpriests, place the headdress on (their) head, is in her hand

is said of her in the hymn to her Temple in HI.ZA^{KI} [91]. A ritual of Isin–Larsa times [92], apparently dealing with the investiture of a king, has her — under her name Ninmenna — place the crown on his head, and Rim–Sin II derives his royal authority from her, he calls her Ninmah [93].

In the long run, however, the position of the goddess in the cosmic hierarchy proved untenable, and slowly she had to yield before a male god who, as she herself, represented numinous power in giving form and giving birth, the god of the fresh waters, Enki/Ea. In the latter half of the Isin–Larsa period

[89] Kramer and Bernhard, *Enki und die Weltordnung*, p. 238 line 400.
[90] We quote it from *UET* VI no. 26 i.26-27.
[91] Sjöberg and Bergmann, *TH* p. 46 f. lines 500-503.
[92] *PBS* V no. 76 rev. vi.10-14 barag ^{d}Nin-men-na-šè mu-na-te aga-guškin sağ-ĝá-na mi-ni-in-ge-en "he drew near to Ninmenna's dais for her and she placed the golden crown firmly on his head".
[93] Ungnad, "Datenlisten", II p. 164 no. 283.

his name begins to precede that of Ninhursaĝa or other name of the birth goddess in the ranking of the highest gods An, Enlil, Enki, Ninhursaĝa etc. and eventually her name was completely replaced by that of Enki.

Unities of Person

As we mentioned at the beginning of these notes, our purpose with them was not to furnish a rounded study of the goddess Nintur, but merely to present a few grouped data to exemplify the approach we outlined and the kind of problems one encounters.

As will be seen the unity under which the approach attempts to subsume the varied materials bearing on an ancient deity is basically one of function historically viewed. The conceptual apparatus is that of the ancient texts themselves, whether the characteristic functioning of the deity is there seen as flowing directly from his or her inner nature (intransitive), from a social, usually family, role, or, as later, from occupation or political office. Since the conceptual apparatus is that of the ancient texts it shares with them a distinctly anthropomorphic bias. Even in cases where older, non-anthropomorph, forms survive it is already most likely that the inner form of the powers in question were seen as human, that they had been personified [94].

The degree of unity achievable in these terms is not without its limits, however, and the picture of divine figures it is possible to draw are not always as sharply outlined as one might have wished. Over and over again the materials, if pressed, prove to be in subtle ways instable and in flux, with shift of emphasis even to a point where identity itself may become moot. Names turn into mere epithets, epithets into names; a name may be but one of many designating a given deity and yet may prove also to be that of a separate, different, minor deity in his or her entourage. Deities separate and different originally may merge and exchange characteristics.

Least difficult here is probably the shift from name to epithet or vice versa, implying ideally shifts between more total denotation, name, and more partial description, epithet, for the line between these two modes of presentation is a fine one indeed. All Mesopotamian divine names are, as far as one can see, descriptive in origin, and their original descriptive content is rarely lost, rather, it continues to be seen as expressive of a specific power and ability in the bearer of the name. Great gods, therefore, with great variety and scope of powers have many corresponding names.

A clear example of use of proper names with such full awareness of their descriptive content offers the myth of Enki and Ninhursaĝa, in which the goddess is variously referred to as ^{d}Nin-sikilla "The pure (i. e. virgin) Lady" before Enki unites with her, as ^{d}Nin-tur$_{5}$ ama kalam-ma "Lady, womb (?), mother of the Nation", when his advances to her are told, and as ^{d}Dam-gal-nun-na "The Great Spouse of Nun (or 'the prince' i. e., Enki)" etc. [95]. Very similarly in the Sumerian Flood Story she is called ^{d}Nin-

[94] See above p. 282 and note the picture of Imdugud given in the Lugalbanda Epic.

[95] See our comment in *JNES* 5 (1946) 149-150 = *TIT* p. 128.

hur-saĝ-ĝa as the acts with An, Enlil, and Enki creating the animals or adjures the gods to silence about the decision to bring on the Flood [96] but ^{d}Nin-tur$_5$ as she laments the destruction of her children to whom she has given birth [97]. One might term such selective use of divine names "role-true", it lies halfway between denotation and description.

Fully descriptive use, turning a name completely into an epithet is also found. One may quote as an example Lugal-e 394-395 [98]:

> nunuz-zi me-ni me-a diri-ga nin nagar-šà-ga
> ^{d}A-ru-ru nin$_9$-gal ^{d}En-lil-lá ...
>
> Fair woman, whose offices exceed (other) offices, lady, carpenter of (i. e. in) the insides,
> Aruru, older sister of Enlil ...

where absence of divine determinative shows that nagar-šà-ga is to be understood as a descriptive phrase, "carpenter of the inside" rather than, as elsewhere, as a divine proper name. A similar case is the hymn *BL* 75 and 97 with duplicate *TuM* IV no. 86:

> [am]a ^{d}Nin-tur$_5$ nin-mah Kèš^{ki}-a
>
> mother Nintur, august queen of Kesh

the absence of divine determinative before nin-maḫ suggests that it is to be read as a descriptive phrase "august queen", rather than as the proper name Ninmah.

As long as names and epithets can be seen as referring to one and the same divine figure, shifts from one to the other are not disturbing. Otherwise, however, when one and the same proper name occurs both as name for the birth goddess herself and for a minor figure in her entourage. This is the case with ^{d}Nagar-šà-ga which we just saw used as a descriptive phrase. It is, however, also given as the twentyseventh name of Digirmah, Ninhursaga, Nintur etc., in An:*Anum* [99] and the Great Godlist [100] but An:*Anum* also lists it as designating not the goddess herself but a child of hers who is the "giver of girls" [101]. Similarly the name [^{d}]Nin-^{tur}túr occurring as the fifteenth name of Diĝirmah, Ninhursaĝa, Nintur etc. in An:*Anum* [102] appears to designate a separate minor goddess of her entourage in the Great Godlist [103] ^{d}Nin-tùr

[96] *PBS* V no. 1 i.13′ and iii.18′. We read the latter passage An ^{d}En-lil ^{d}En-ki ^{d}Nin-hur-saĝ-ĝá-[ke$_4$] diĝir-an-ki-ke$_4$ mu-An-^{d}En-líl mu-n[i-pàd-eš] "An, Enlil, Enki, and Ninhursaĝa adjured the gods of heaven and earth by the names of An and Enlil". Cf. Civil in Lambert and Millard, *Atra-ḫasīs*, p. 143.

[97] *Ibid.* iii.15′. We restore ud-bi-a ^{d}Nin-t[ur$_5$ nig-dím]-dím-a-ni[-šè ì-šéš] "On that day Nintur wept for her creatures".

[98] The line numbers are from a MS. edition by the regretted Father Bergmann which he kindly placed at our disposal. We quote from *UET* VI$_1$ no. 4 obv. 7-8.

[99] *CT* XXIV pl. 12. 27.

[100] *CT* XXIV pl. 25. 89.

[101] *CT* XXIV pl. 13 obv. ii.5.

[102] *CT* XXIV pl. 12. 15.

[103] *CT* XXIV pl. 26. 135.

Š a – s u – r u "Nintur, the womb". Correspondingly in the Akkadian Atrahasis Myth *Šassūru* "The Womb" is both a name or epithet of Nintur and also serves to designate fourteen minor goddesses, "wombs" in her entourage [104].

As in the these cases deities with the same name and function turn out to be different, so conversely, in other cases, deities with different names and functions appear to have been considered one and the same. We have met with one such case in Gudea Statue A, where Ninhursağa and Nintur are considered one and the same goddess even though there is every reason to think that they were originally separate deities with separate characters and functions. Yet the fusion is so complete that the scribe who wrote the inscription could use an epithet traditionally belonging to Ninhursağa for Nintur when he wished [105].

A similar example of merging of two originally separate deities is furnished by the list of gods K 2109 etc. [106] where the goddess known variously as ^{d}A m a – d ù g – b a d , ^{d}N a g a r – š à – g a , and ^{d}Š a g$_4$ – $^{š a}$ – s u – r u m t ù r is not, as usual, identified as Bēlit–ilī, the Akkadian name for Diğirmah, Ninhursağa, Nintur etc. but — with turning of the name Bēlit–ilī into a descriptive phrase — with *be–lit ilī* $^{pl.}$ ^{d}*Iš-tar* "the queen of the gods, Ishtar".

In discussing the merging of Ninhursağa and Nintur above we suggested that unity of the situations in which these two deities characteristically revealed themselves might be a significant factor: the birth-hut of Nintur in the natural order of things standing in the wilderness of Ninhursağa. Similarly we may here note the close connection with the situation of childbirth in the names involved: "Mother spreading the knees", "Carpenter of the insides (i. e. in the womb)" and Šag$_4$–tùr "Womb", and the fact that both Nintur and I n a n n a /*Ištar* were seen as tutelary deities par excellance, for tutelary goddesses were traditionally mothers of their human children. Nintur is also called D i ğ i r – l ú – g u – l a "The great personal deity of the man" [107] and I n a n n a / *Ištar* figures in the usual term for tutelary goddess a m a – ^{d} I n a n n a . Thus also here the merging of the two deities may have proceeded from a single situation of numinous character, childbirth, in which they were both seen, at one time or other, as the active principle.

Referring back to the characteristic situation of revelation may be helpful not only where two different deities are seen as one, but also where, as we saw before with the name Nagaršaga and with the name Nintur, one name is used to designate a major goddess as well as to denote separate minor figures around her. What seems to have happened here is that in the total experience of birth sometimes a single feature only, the womb, or even narrower, its ability to give form to the embryo, has been felt as the true locus of the numinous and has become personified as a deity in its own rights. At other times the total situation with its many features has been experienced as numinous and given form as one deity with varied aspects and powers.

A similar view may be taken also of the fluctuations between proper name and epithet mentioned at the beginning of this section. It would seem to depend on the degree to which the numinous was presented at the moment: as

[104] Lambert and Millard, *Atra–hasīs*, p. 56 lines 1-2 and 189-190, 194, as against op. cit. p. 60 line 251 and p. 62 lines 8-9, 11, and 277.
[105] See above note 86.
[106] *CT* XXV pl. 30 rev. i.9-12.
[107] *CT* XXIV pl. 12 i.29, pl. 25 ii.90; *TRS* no. 10 line 121.

bearer of more aspects than the one expressed, if so the mention tended toward the epithet; as bearer of one aspect only, if so the mention tended toward the name.

In thus referring back to the underlying situation or situations of numinous confrontation with their various possibilities of human effort to connect up the experience of numinous power with the external situation as a whole, or with one or other prominent element in it, we underline again Otto's fundamental insight with which we began these notes, that the numinous is *sui generis*, "Wholly Other", outside of normal experience and indescribably in its terms. Human categories such as one and many, part and whole, personal and impersonal etc. do not thus apply and are not given with the experience. They are introduced arbitrarily, but necessarily, in the human attempt to give expression to the experience and may therefore vary with human psychological and cultural factors as well as with different individual such numinous events and different people undergoing them.

É.NUN in Mesopotamian Literature

R. CAPLICE – Rome

This brief inquisition into the theme É.NUN or "holy chamber" in Mesopotamian literature was prompted initially by a Sumerian line in a namburbi text: é n m u l É.NUN. t a . ⌜ è ⌝ . [a ...] a n . ú r . t a ⌜ è ⌝ . [a], "Incantation: star which has come forth from the É.NUN, [star] which has come forth from the horizon" [1]. The line cited is the beginning of an incantation to be recited by one on whose property a "star" has fallen [2] in order to avert the malign effects of such a prodigy. The "star" was in all probability a meteorite, to judge from the fact that in other texts of this type the prodigies or "signs" are always things or events of real life, rather than sheerly literary conceptions. The problematic portion of the line is however in the word É.NUN; it is well known that É.NUN serves as a logogram for *kummu*, "sanctuary", but what should it mean that a star comes forth from a sanctuary, and in what relation does the sanctuary stand with the horizon, a n . ú r = *išid šamê*? Clearly, the star is here set in relation with a mythological conception, and indeed a glance at the textual attestation to be discussed below suffices to tell us that É.NUN has different senses. In the first place, it is equivalent to *kummu*, "sanctuary", that is, a cella in the temple, dedicated to a particular god or goddess and (to judge from the occurrence of the word in contexts dealing with night and resting [3]) conceived as his or her bedchamber; in this general sense one may speak of the cella of any god, male or female. In a more specific sense, É.NUN is also a proper noun signifying a single concrete sanctuary, that of Ningal, spouse of the moon god, in Ur [4]. Further, É.NUN is found in a spectrum of mythological contexts that center on Enki, god of Eridu, and on Dumuzi, also in contexts dealing with Eridu; in the latter case, as we shall see, the references, though in a mythological context, are probably to a sanctuary É.NUN in the city of Eridu. This multiplicity of senses may be compared to that of the word é . k u r , which signifies "temple" in general, but also the specific temple of

[1] *Or* 39 (1970) 113:1. A further reference to the incantation has been pointed out to me by E. Reiner; K. 13378, an unpublished fragment containing astral omens, concludes (rev. 4f.) with the incipit: é n m u l É.NUN. t a ⌜ è ⌝ . [...], s i p a . a n . n a d u$_{11}$. g a . a [...].

[2] *Or* 39 113:9: MUL *ša ina bītika imqutu*; note also ibid. 8: KA. i n i m . m a m u l é . t a k á . n a è . d a . k e$_4$, "Incantation-formula for a star which 'comes out' from the 'house' at a man's gate". Compare also *A Ch* Suppl. LIV 21, kindly pointed out to me by Mrs. Judith K. Bjorkman: É MUL *i–ma–qut–u–ni* GIŠKIM *ana* KUR *id–dan*, "a house on which a 'star' falls will give a sign for the land".

[3] For refs. see *AHw kummu*, *CAD kummu* A.

[4] É.NUN = É ^{d}*Nin–gal*, *HGT* 106 IV 19.

Enlil in Nippur, and finally the underworld. In both cases it is plausible to suppose that a primary concrete sense developed secondary and mythological overtones. The remainder of the present study will be devoted to an examination of the various senses of É.NUN in Mesopotamian texts in order to elucidate its occurrence in the incantation which was our starting point; the study is presented as a small tribute to Prof. Gelb, and an acknowledgement of the writer's indebtedness to the classes and writings of his former teacher.

Of the three spheres of reference of É.NUN — general, concrete, and mythological — the first evidently has no relevance to the cited incantation. Had the star come out of a sanctuary in the normal sense, the sanctuary would surely have been specified by the name of its god; further, what should "come forth from a sanctuary" mean? Nor can our incantation be directly concerned with the specific series of sanctuaries attested from the middle of the third millennium in Ur under the name É.NUN [5], since allusion to a specific geographical site would not fit the mythic tone of the incantation. Our inquisition, then, will be concerned with the mythological É.NUN of the Eridu tradition.

Before examining the relevant texts, it would be well to devote preliminary attention to the Sumerian reading of the signs É.NUN. Three readings have been proposed or supposed: a g r u n , é . n u n , and é . g a r$_6$. (a) the first of these (a g – r u – u n , also a g – [r u – n] a , [e – g a – r a] – a n [6]) is well attested for É.NUN as lexical equivalent to Akk. *kummu*, and is supported by Akk. translation of É.NUN with the loan word *ag(a)runnu* in two bilingual passages; the first [7], in a context dealing with gods of the Ur circle, gives to Ninnigara, spouse of Ningubla and daughter-in-law of Sîn, the epithet "lady of the pure É.NUN" (n i n . É.NUN. k ù . g a : *bēlet* ⌜*a–ga*⌝*–ru–nu/ag–ru–un–ni*

[5] On the varying location of the Ningal sanctuary see L. Woolley, *AJ* 5 (1925) 366-376 and P. N. Weadock, *The Giparu at Ur: A Study of the Archaeological Remains and Related Textual Material* (University of Chicago dissertation, 1958), esp. p. 32ff. Textual references to the É.NUN in Ur may be summarized here. *Ur archaic texts*: offerings to Lugal–É.NUN–si and Ama–É.NUN–si, *UET* 2 passim, see index p. 28 no. 97, p. 34 no. 476. *Ur III period*: reeds for offerings in E. (É.NUN. n a), *UET* 3 64:2; foodstuffs for E. (É.NUN. š è), ibid. 71:5; personnel of E. (g e m é . a r à É. NUN [. . .]), ibid. 1066 r. i 18′; foodstuffs (for offerings at the) é . d a . s á . a É. NUN. n a , s i . g a r É. NUN. n a , ibid. 270 r. i 15 and 28; date-formula "year when Nanna of Karzida (came) to the E. (É. NUN. n a . š è), ibid. 1302:2, cf. *RLA* 2 140, 26 and 142, 55; É.NUN. k ù, temple of Ningal and "house of her ladyship", *AS* 12 50.113. *Isin-Larsa*: dedication to Ningal of a statue brought to the E. (É.NUN. n a . š è) by Enanatumma, daughter of Išme-Dagan, *UET* 1 103:14; Nūr-Adad of Larsa built the pure E. (É.NUN. k ù) for Ningal and restored the "E., the ancient structure" (É.NUN n í g . u l . e) , ibid. 111:33 and 39. *Old Babylonian*: hymn of Enanedu, daughter of Kudur-Mabug, en-priestess of Nanna in Ur and "ornament to E." (ḫ é . d u$_7$ É.NUN. n a), *Iraq* 13 (1951) 27 i 6; receipt for foodstuffs received by the gudu-priests of E. (g u d ú . É. NUN . n a) from the large storehouse of Ningal (g á . n u n . m a ḫ . ^{d} N i n . g a l . t a), *UET* 5 568 i 6. *Neo-Assyrian*: Sîn-balāssu-iqbi, governor of Ur under Aššurbanipal, built anew for Ningal the *giparu* and drove the peg in E. (É. NUN. t a), the dwelling of her entuship, *UET* 1 171:15, cf. L. Woolley, *UE* 5 p. 64. Note also è š . NUN, Å. Sjöberg, *Der Mondgott Nanna-Suen in der sumerischen Überlieferung*, I. Teil: Texte (Stockholm, 1960), p. 123:9.

[6] See *CAD kummu* A, lex. section, also ^{d}Lugal–É.NUN.na$^{ag\text{-}ru\text{-}\lceil nu \rceil}$ *CT* 25 39 K. 2098:1.

[7] See *ArOr* 21 376:45.

elli); the second [8] represents Šala, spouse of Adad, as "lady of the É.NUN" (g a š a n . É.NUN. n a . k e$_4$: *bēlet ag–ru–un–ni anāku*). (b) A reading ending in -n is indicated by the writing É.NUN. n a, both in reference to the Ur temple and, as we shall see, in mythological contexts. While this allows for a reading either as a g r u n or as é . n u n, it is not consonant with a reading é . g a r$_6$ which has been suggested [9] on the basis of a lexical passage giving g a – a r as the reading of NUN in the writing É.NUN = *kummu* [10]. It seems clear, therefore, that g a – a r in this text is an incomplete rendition of a g r u n or *a g a r u n [11], and that the reading é . g a r$_6$ should be discarded. (c) In mythological contexts where É.NUN is rendered by Akk. *kummu*, the reading a g r u n is assured by the evidence presented under (a) and (b). (d) In the specific mythological context involved in the laments for Dumuzi, no Akk. rendering is known to the writer; the syllabic, writing e . n u . u n in one of these texts [12] indicates the reading é . n u n, referring probably to a specific sanctuary in Eridu. (e) Both é . n u n and a g r u ḫ remain as possibilities for the reading of the Ningal temple É.NUN in Ur; though the above cited reference to the goddess of the Sîn circle in Ur, Ninnigara, as "lady of the agrun" may suggest a specific sanctuary of this name in Ur, there is no evidence that proves its identity with the Ningal sanctuary, and the reading of the latter remains uncertain at present.

In mythological contexts, a particular relation between a g r u n / *kummu* and the god Enki-Ea has long been recognized by Assyriologists [13]. As is known, Enki is the patron of "wisdom" par excellence, and in the universe of Mesopotamian thought, wisdom (*nēmequ*) refers to magical power, the power of controlling by word and rite every kind of destructive and malign influence. This special position, in a world very conscious of the malign power of demon, sickness and chance, assured Enki a particular importance among the gods of the pantheon, an importance enhanced by the secondary identification between Marduk and a minor god of the Enki circle, Asalluḫi, which gave that circle all the prestige and authority of Babylon. It is not unexpected, then, that magical texts from all the literary centers of Mesopotamia turn in the first place to the gods of Eridu, Enki and Asalluḫi. It is no surprise that the agrun occurs in such texts, not only as the abode of Enki-Ea, but also as the abode of other gods of his circle.

The classical text on the "chamber" of Ea himself is to be found in Enūma eliš, and it offers an outright identification of this term with the subterranean

[8] Langdon, *BL* pl. XIV ii 8f., cf. Frank *ZA* 41 (1933) 198.

[9] Falkenstein, *ZA* 49 (1949) 321 ad 50, discussing the É.NUN. k ù of Ur; Å. Sjöberg, *Sumerian Temple Hymns* (*TCS* 3; Locust Valley, N. Y., 1969) 85 ad 158 takes the reading of É.NUN (in the epithet É.NUN. g a l of the Nanna temple in Gaeš) as uncertain. See already P. Jensen, *Die Kosmologie der Babylonier* (Strassburg 1890) 490.

[10] MIN (= [g a – a r]) = NUN = *šá* É.NUN *ku–um–mu*, *AJSL* 36 (1919-20) 158 ii 7 (Recip. Ea).

[11] See also g a – r a (var. g a – a r) NUN, Proto-Ea 394 (*MSL* 2 62), also a – g a r – g a – r a [NUN–*tenû*], ibid. 396 and dictionaries s. v. *agargarû*. Though not attested as such, a variant Sum. form * a g a r u n is supported by the Akk. *agarunnu* (note 7 above).

[12] See below, note 31.

[13] See, for example, R. Labat, *Le poème babylonien de la création* (Paris, 1935) 84 ad 75.

watery region of the *apsû*: when Ea had slain the god Apsû and fixed his abode on him, "he rested in peace in his ' chamber'. which he called *apsû*" [14]. The same identification of a g r u n / *kummu* with the watery realm of Enki is found in Sumerian texts which place there Enki and the gods of the Enki group. Thus the efficacy of an incantation is assured by ascribing it to Asari and Enki: "the exorcist Asari has pronounced the spell in the *apsû*, Enki has given liberation in the agrun" [15]. Similarly, it is "in his passage from the a g r u n / *kummu*" that Asalluḫi, according to another incantation, sees and remedies the evil done in the sheepfold by the demons utukku, alû and asakku [16]. Further, Nammu, the mother of Enki, can be described as "child of the agrun". In an Old Babylonian collection of incantations against scorpions, the incantation-priest identifies himself as her representative: "I belong to Nammu, I belong to Nammu, I belong to the child of the agrun. Who will go with me to the house of darkness? I myself will go to the *apsû* ... I belong to Nammu, I belong to Nammu, I belong to the child of the agrun. Who will go with me to the house of darkness? The Prince [17] will go with me from the 'storehouse'. When you call at the portals of the agrun, the *viper* of field *and ditch* is subdued" [18]. This description of Nammu as "child of the agrun" is not in full accord with the theology of Enūma eliš, where the existence of the agrun does not precede Ea's defeat of Apsû, but it represents the agrun as the same subterranean watery realm, the "house of darkness". The conception is comparable to that of the legend of the *kiškanû*-tree growing in Eridu, whose "abode was in the nether world" and whose "sleeping chamber was the bedroom of Nammu" [19].

A further group of texts represents a wider conception of the agrun as the underworld in general, without restriction to the Enki circle. Into this

[14] *qirbiš kummišu šupšuḫiš inūḫma, imbīšumma apsâ*, En. el. I 75, cf. V 124, VI 52.

[15] m u$_7$.mu$_7$ ^{d}A s a r . r i a b z u . a n a m . š u b b a . a n . s ì , ^{d}E n . k i . k e$_4$ É.NUN. n a . k e$_4$ ḫ é . e m . m a . a n . d u$_8$. d u$_8$, *ASKT* No. 12 23f. See also the incantation edited by B. Alster, *Or* 41 (1972) 350:13, ^{d}E n . k i d a g . É. NUN. n a . k a ḫ é . e m . m a . a n . d u$_8$. d u$_8$. e, "May Enki undo (the evil) in the dwelling of the agrun", with var. è š . é . a n . n a . k e$_4$ (*ibid.* 352 text C:7). Occurrences of É. NUN cited in the present paper, like the var. d a g . g a l . n u n . n a to d a g . É.NUN. n a in "Enki and the World Order" (cited Alster, *ibid.* 354), indicate that É.NUN is the watery dwelling of Enki, not "the bedroom where the sick man is lying" (Alster, *ibid.*). The var. é . a n . n a remains unexplained; for an alternate interpretation of d u$_8$. d u$_8$ see J. van Dijk *apud* Alster, 354 *ad* 14.

[16] ^{d}A s a l . l ú . ḫ i d u m u E r i d u^{ki} . g a . k e$_4$, [š à ?] ⌜É⌝.NUN. n a . k e$_4$ g ì r i g i n . n a . k e$_4$ t ù r . b i i g i i m . m a . a n . s í : ^{d}*Marduk mār Eridu, [ina li]b²-bi kummi ina ītallukišu tarbaṣu šuātu ippalisma*, 4*R* 18* No. 6 r. 11f.

[17] Presumably Enki.

[18] ^{d}N a m m u . m e . e n ^{d}N a m m u . m e . e n d u m u . É.NUN. k a . m e . e n , é . k u$_{10}$. k u$_{10}$. š è a . b a . a ḫ u . m u . d a . d u , g á . e a b z u . š è ḫ é . d a . d u ... ^{d}N a m m u . m e . e n ^{d}N a m m u . [m e] . e n , [d u - m u . É] . ⌜NUN. k a ⌝ . m e . e n , ⌜ é ⌝ . k u$_{10}$. k u$_{10}$. g a a . b a ḫ u . m u . d a . a n . d u , n u n . e g á . n u n . t a ḫ é . d a . d u , ^{giš}i g . É.NUN. t a g ù . d é . d é . a . z u . d é , m u š . g í r . g á n . ⌜ e ⌝ . k e$_4$ g ú k i . š è . a ḫ é . b a . e . d a . g á . ⌜ g á ⌝ . d è , van Dijk, *VAS* 17 10:9-11 and 117-122. I am grateful to J. van Dijk for this reference.

[19] k i . t u š . a . n a k i . IGI.KUR. à m , k i . n á . a i t i m a . ^{d}N a m m u . à m : *šubassu ašar erṣetimma, kiṣṣušu majālu ša* ^{d}MIN, *CT* 16 46:189ff.

nether world the setting sun sinks, and from it it rises [20]. Underworld deities were also at home in the agrun. É.NUN is defined as "the house of Manungal", [21]. an underworld goddess [22] who is spouse of Birdu, a Nergal figure. Manungal is identified in the godlists with Nungal [23] and her epithets indicate her malevolent character: she is "the snatcher" [24] and "the violent one" [25]. The series Utukkū lemnūti also identifies a group of seven demonic figures as springing from the *apsû* and the agrun: "they are seven, they are seven; from the depths of the *apsû* they are seven; adorned (Akk. adds: in the sky) they are seven; from the depths of the *apsû*, from the agrun, they came forth (Akk.: in the depths of the *apsû*, in the *kummu*, they grew)" [26]. References to the mythological activity of these seven are sketchy, but they hint at a rôle of some importance. They are "enemies of Ea" [27] and "binders of Enki" who come from the *apsû* [28] or "approached the bounds of the *apsû* to trample on it" [29].

We have seen that É.NUN in texts dealing with Dumuzi hold a special place. In a composition studied by S. N. Kramer in relation to the sacred marriage rite, Inanna speaks of her journey to Eridu and its shrines: "When I, the queen, proceeded to the Abzu, When I, the queen of heaven, proceeded to the Abzu, When I proceeded to the Abzu, the É.nun, When I proceeded to Eridu, the goodly, When I proceeded to the E-Engurra" [30]. The é.nun, here

[20] The interceding god should approach the sun god "as Šamaš goes forth from the agrun" (dUtu É.NUN.na.ta è.a.na: *dŠamaš ištu kumme ina aṣîšu*), *CT* 16 36:36f., dupl. *BIN* 2 22:179f., cf. "when Utu enters the agrun" in a hymn to Inanna (dUtu É.NUN.na.šè ku$_4$.ra.a.ba), *BE* 31 12 r. 24; during the night Šamaš "entered his chamber" (*īterub ana kummīšu*), *RA* 32 (1935) 180:13, dupl. ibid. 181:15 (OB); S. N. Kramer, *Enmerkar and the Lord of Aratta* (Philadelphia, 1952) 14:132 and 134 places this conception in relation to the "chambers" of the Nudimmud temple: "may (the temple) be bright like Utu coming forth from the agrun (dUtu É.NUN.ta.è.a.gim) ... in its chambers (É.NUN. É.NUN.ba) ... pronounce the spell of Nudimmud" (for the reading É.NUN, rather than gán.nun, I am grateful to S. Cohen for collation; see also pl. XX, photo of exemplar C, line 134).

[21] É.NUN = É dMa-nun-gal, *HGT* 106 iv 20; for Manungal (also Ma-nu-gal, Ma-nu-kal, OB Ma-nun-na) see Weidner, *AfK* 2 (1924-25) 73 iv 21 with notes 2f. and copy p. 4; *CT* 24 47 i 29 = *CT* 25 4 iii 5 (An = Anum).

[22] She is nin.é.kur.ra.ke$_4$: *bēlet ṣibitti*, "mistress of the underworld/prison", *BA* 10/1 (1913) 93 No. 15 r. 5f.; cf. dNun-g[al] = [dM]a-nun-gal *ša ṣibitte*, *CT* 24 43 xi 138 (An = Anu ša amēli).

[23] *CT* 25 4 iii 6; see also preceding note.

[24] *ṣabbutītu*, Šurpu III 77.

[25] d[...] = ([dM]a-nun-gal) *ša ḫabalāte*, *CT* 24 43:140 (see ibid. 139 and 141).

[26] én imin.na.meš imin.na.meš, idim.abzu.ta imin.na.meš, še.er.ka.an du$_{11}$.ga.na imin.na.meš, idim.abzu.ta É.NUN.ta è.a.meš: *sibitti šunu sibitti šunu, ina nagab apsî sibitti šunu, zu'unūti ina šamê sibitti šunu, ina nagab apsî ina kumme irbû šunu*, *CT* 16 15 v 28ff. On the heptade of demonic figures, see Jean, *RA* 21 (1924) 102f.

[27] dEn.ki.ke$_4$ šeš.si.meš: *ša dEa lemnūti šunu*, *CT* 16 15 49f.

[28] šu.lá.dEn.ki.ke$_4$: *mukassû dEa*, *CT* 17 13:13f.

[29] *idāt apsî ana kabāsu iṭḫûni*, *CT* 16 45:137.

[30] ga.ša.an.mèn abzu.šè di.da.mu.dè, ga.ša.an.an.na.mèn abzu.šè di.da.mu.dè, abzu é.nun.šè di.da.mu.dè, uru.zé.eb^{ki}.zé.eb.šé di.da.mu.dè, é.engur.ra.šè di.da.mu.dè, *CT* 42 No. 13:3-7, cf. S. N. Kramer, *PAPS* 107 (1963) 503f.

in apposition to a b z u , is probably to be taken as the name of a real temple in Eridu, like the É . e n g u r . r a and the É . a n . n a temple of Mullil mentioned in the same passage. Its reading as é . n u n is assured by a syllabically written lament for Dumuzi, parallel to the lines cited above though not a duplicate, in which Inanna speaks of going "to the A b z u , the e . n u . u n" [31].

The same sanctuary or temple seems to be referred to in a further lament for the dead Dumuzi, who is mourned because his body has not received the funeral rites, "because he is not washed with water in (var.: washed in the temple of) Eridu, he is not scoured with alkali in the É.NUN" [32]. Despite parallelism with "the temple of Eridu" in one variant, however, it may be that the reference here is to the agrun, conceived as an underground realm to which the dead Dumuzi has gone. Such an interpretation is possible in the light of Gudea's mention of "alkali of the É.NUN" in a temple-purification context which can hardly be connected with the Eridu sanctuary, and seems rather to refer to the mythically conceived underground depths from which the alkali plant was considered to have sprung: upon completion of the Eninnu-temple in Girsu, Gudea had the god Kindazi exercise his office for Ningirsu "in order to purify (the temple) with water, to cleanse it with alkali, with sparkling *pūru*-oil and alkali of the É.NUN" [33].

Whatever the interpretation of these last texts, it is clear that the agrun or *kummu* is a kenning or alternate name in Mesopotamian mythological texts for the subterranean *apsû*, and that it was conceived as the habitat of Enki and the gods associated with him as well as of demons who do harm to men, and the region traversed by the sun in its nighttime passage from setting point to rising point [34]. This background enables us to understand the relation between the agrun and the ominous "star" which came from it in the text which was our starting point. The stars, like the sun, rose from the horizon; for Mesopotamian mythopoeic thought this was represented as passage from the underworld of the agrun to the sky, and the same origin could be ascribed to "shooting stars" or meteors.

This conception of the agrun as the daytime "home" of astral bodies may also underlie the relationship between demons and stars that is sometimes

[31] k a . š a . n a . n a ab . z u . š i du_7 . m u . d a . d è . n a , ab . z u e . n u . u n . š i du_7 . m u . d a . d è . n a , T. G. Pinches, *Memoirs and Proceedings of the Manchester Literary and Philosophical Society* 48 (1903-04) pl. IV iv 5f.

[32] a (var. é) U r ú . z é . b a^{ki} n u tu_5 (var. t u) . a . n a , n a . m a É.NUN. n a (v a r . + n a) n u . s u . u b (. b a) . a . n a , *RA* 12 (1915) 34:12f.

[33] a k ù . g e . d a n a g a . s i k i l . e . d a , ì . b u r . bìr . bìr . r a n a g a .É.NUN. n a . d a , Gudea Cyl. B ix 7. Falkenstein's translation "Dass mit Wasser gereinigt, mit Soda gesäubert werde, dass mit Öl aus leuchtender Schale mit Soda das Haus geläutert werde" (*SAHG* 173) takes NUN. n a . d a as a verbal phrase parallel to k ù . g e . d a and s i k i l . e . d a , but a verb NUN is not otherwise attested in this sense, and a postfix – a d a would be unusual; for these reasons, interpretation of n a g a .É.NUN. n a as a nominal phrase parallel to ì . b u r . bìr . bìr . r a seems preferable.

[34] Other occurrences of a writing É. NUN are not pertinent here. For É.NUN (.NA) and É.DÙ. NU(N) as technical descriptions of a type of land see L. Matouš, *ArOr* 18/4 (1950) 21; note also dUr–é–nuna–ta–è–a, whose name is derived by Falkenstein (*AnOr* 30/1 112f.) from an original dUr–nuna–ta–è–a.

expressed in the Akkadian literature of demonology. In some instances this relation is expressed by a simile; thus the demon " 'Headache' is cast unrecognized (Akk.: unfittingly) on the plain like a star of heaven" [35] and "flashes like the stars of heaven, moves like water in the night" [36]. In other instances, demons seem to be even more closely associated with the stars. Thus disease demons "came down from the stars in the heavens" [37], and the seven evil demons, having spread destruction, "went off to the heavens on high, departed to the unapproachable heavens; they cannot be recognized among the stars of heaven (Sum.: the stars of heaven do not reveal their sign) in their three watches" [38]. The identification between stars and demons that is suggested by this text may rest on the fact that the stars, sharing their conceptual place of origin with demonic forces, could in part be conceived as identified with these forces. Further, the conception of an underworld realm into which the astral bodies descend during the daylight hours may have influenced such tales as the descent of Inanna-Ištar, to the extent that the goddess was conceived as identified with the "descending" Venus-star.

In summary, the agrun is primarily the underworld, conceived as the chamber of Enki, but also the locus of demonic forces and of the heavenly bodies, when these are not visible in the sky. Its written form, with NUN, is of course already an indication of a connection with Enki and the Enki circle. Secondly, it is used as a logogram for *kummu* in the sense "cella" (the chamber of a god or goddess). A further proposal on the derivation and meaning of the Sum. word a g r u n will be presented by S. Cohen of the University of Pennsylvania in a forthcoming study of "Enmerkar and the Lord of Aratta". The present study, it is hoped, has provided an explanation of the hitherto enigmatic text with which our inquiry started, the star which "has come forth from the É.NUN". In relation to the scope of the present volume, the depth and variety of the connotations which have been seen to underlie a single word are impressive, and a reminder that lexicographic inquiry — an area in which we are indebted to Dr. Gelb — must go hand in hand with the delineation of Mesopotamian mythology or religion. The Mesopotamian conception of the agrun remains in some respects problematic, as we have pointed out. It is clear, however, that each of the widely varying contexts in which the term is found contributes to our total understanding.

[35] é n . s a g . g i g m u l . a n . g i m a n . e d i n . n a g u r u d . d a n u . u b . z u : *muruṣ qaqqadi kīma kakkab šamāmi ina ṣēri nadīma ul naṭi, CT* 17 14 K. 8386:1f.

[36] m u l . a n . g i m s u r . r a a . g i m g e$_6$. a a l . d u . d u : *kīma kakkab šamāme iṣarrur kīma mê mūši illak, CT* 17 19:11f.; note also *CT* 16 19: 44f., where the seven destructive demons, messengers of Anu their king, flash like lightning on the horizon (*ina išid šamê*).

[37] *ištu kakkab šamê urdūnim, JCS* 9 (1955) 11 C 5 (OB, for SB parallels see *CAD* s. v. *kakkabu* 1b).

[38] m u l . a n . n a g i s k i m . b i l a . b a , r a , a n . d u$_8$. d u$_8$. e š e n . n u n e š . š á . b i . t a : *ina kakkab šamāmi ul utaddû ina maṣṣarāti šelaltišina, CT* 16 43:70f.

Comments on the Nassouhi Kinglist and the Assyrian Kinglist Tradition [1]

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In 1954, I. J. Gelb's publication of two almost complete eighth-century copies of the Assyrian Kinglist (KhKL and SDAS) put an end to more than two decades of dilatory irresponsibility which had withheld this text, save in measured doses, from the scholarly world. Since that time, historians of ancient Mesopotamia have had the advantage of basing their chronologies of Assyria directly on a carefully preserved tradition of the Assyrians themselves; and most Middle Assyrian or Neo-Assyrian chronological problems discussed since 1954 have centered around conflicts or lacunae within the Assyrian tradition.

The present meager offering in honor of Professor Gelb consists of two parts: (A) a contribution to the textual accuracy of the kinglist tradition, comprising some collations made of the Nassouhi Kinglist, the oldest extensive copy of the Assyrian Kinglist presently known [2]; and (B) a few remarks on the Assyrian Kinglist tradition, including its relevance and factual accuracy, from the point of view of an historian.

A

The first lengthy version of the Assyrian Kinglist to be published in modern times was the tenth-century text edited by Essad Nassouhi in 1927. In view of the very damaged condition of the tablet and the fact that only one tiny duplicate (containing parts of twelve lines) [3] was then known, Nassouhi's edition was commendable for its accuracy and usefulness. Some twenty years later, Weidner published comments on various lines of the text, basing his readings on photos [4]. In the summer of 1971, I had a brief opportunity to collate the tablet in Istanbul; my comments and some of the more significant

[1] Abbreviations used in this article generally follow *CAD* K (1971), pp. vi-xix, with the following additions: AsKL = VAT 11554 (published as *KAV* 15); KhKL = Khorsabad Kinglist (*JNES* 13 [1954] 209-230); NaKL = Nassouhi Kinglist (*AfO* 4 [1927] 1-11); NiKL = kinglist fragment from Nineveh (BM 128059, published by Millard, *Iraq* 32 [1970] 174-176); SDAS = Seventh Day Adventist Seminary Kinglist (*JNES* 13 [1954] 209-230). Throughout this article, the term "Assyrian Kinglist" will be used to refer to the tradition embodied in the five documents above (to the exclusion of other types of Assyrian kinglists, such as the synchronistic kinglists).

[2] *AfO* 4 (1927) 1-11.

[3] AsKL.

[4] *AfO* 15 (1945-51) 88, especially note 16.

collations are published below. Eventually, a re-edition of the text is desirable; but, because of the condition of the tablet, the editor should be able to study it carefully over an extended period and to compare the tablet sign for sign with the excavation photos [5].

Since 1954, commentators have had the benefit of being able to compare the Nassouhi list with the longer and better-preserved eighth-century versions (KhKL and SDAS). Needless to say, these duplicates frequently furnish insights—denied to Nassouhi—into enigmatic sign traces.

Column I

It is sometimes difficult to see how Nassouhi determined the line numbering for this column. It is uncertain how many lines are missing before the first visible traces [6]. In Nassouhi's copy, the line number "20" to the left of this column is so placed as to overlap both with the line ending in *-⌜li-e⌝* and with the apparently blank next line. It is only by counting from line "18", which ends in *-⌜a?-ni⌝* (according to Nassouhi's copy), that one can see that the line terminating in *-⌜li-e⌝* was presumably reckoned as line 19. But then, between line 16 (ending in *-si*) and line 19 (ending in *-⌜li-e⌝*), there are apparently traces of two lines in the copy, while the tablet clearly has three lines here [7].

Since this is an eclectic commentary on a published text rather than a re-publication, I have retained Nassouhi's line numbering for easy reference. A thorough re-edition would undoubtedly renumber the lines.

8–11 There are no readable traces on the tablet.

12 *-u]l-ta-ri* is clear (cf. KhKL i 10, SDAS i 9).

13–21 There are ten lines to this section in each of the three preserved kinglists—NaKL, KhKL, and SDAS. As noted above, the Nassouhi copy seems to omit one line [8].

13]-*bu* (The name of the father of Šamši-Adad I ends in *-bu* also in i 37; KhKL and SDAS have *-bi*.)

14]-⌜x⌝-DINGIR (⌜x⌝ could be the end of *kur*; cf. KhKL i 13, SDAS i 12)

15]-⌜*me-ni*⌝

16]-⌜x⌝-*me-si*

16a] ... (no readable traces, but clearly a line of text here)

[5] In contrast with the synchronistic kinglist Assur 14616c, also in Istanbul, which has markedly deteriorated since its excavation photo was taken, NaKL seems to be in much the same condition now as it was when discovered.

[6] KhKL and SDAS have 9 and 8 lines respectively before the summary at the end of the first section. Nassouhi estimates room for 11 lines here (probably based on spatial considerations and a comparison with the bottom of the third column, which is relatively well preserved); but it is difficult to tell whether the original count may not have been more in line with the other lists.

[7] As do KhKL and SDAS.

[8] Here inserted as line 16a, though from the copy one could suspect that line 17 was omitted (and hence the numbering 17, 17a, 18 might be more accurate).

17] ⌜x–(x)–*ni*⌝
18] ⌜*Sa–ma–a–ni*⌝
19]–⌜*li–e*⌝
20]–⌜x⌝
21]–⌜x⌝

A horizontal dividing line (not in the copy) follows immediately after line 21.

22–30 The only sign which I can positively verify in these lines is the *–ni–* in line 30 [9]. It is difficult to match the traces here in NaKL with KhKL and SDAS [10]. In general, in NaKL i 22-34, Nassouhi's line numbering is suspect; from line 28 on, the numbers may be at least two lines too low for the available space [11].

34 It should read: [ ]⌜x⌝[ ]⌜4(+?) MU.MEŠ⌝ LUGAL–*ta* ⌜DÙ–*uš*⌝

35 can be read *–a*]*m–*^{d}30

39]–⌜*áš*⌝ *il–*⌜*lik*⌝

41 *K*]*ar–du–*⌜*ni*⌝–[x]

42]⌜x⌝.MEŠ

Column II

1–14 The traces here are indefinite, but might repay further study.

15–24 Because of his relatively incomplete knowledge of the sequence of the Assyrian kings, Nassouhi restored incorrect names at the beginning of lines 15, 17, 19, 21, and 23. Bazaju, Lullaju, Šú–Ninua, Šarma–Adad, and Erišu should be restored in these lines [12].

16 The number at the beginning of the line is 20[(+x)].

17 The RN clearly ends in –⌜*ia*⌝ as opposed to *–a–a* in KhKL ii 22 and SDAS ii ⌜19⌝.

21 There may be a trace of ⌜šú⌝ at the beginning of the patronymic here.

[9] The usual restoration of KhKL i 28 and SDAS i 27 (*JNES* 13 [1954] 212-213, following Poebel in *JNES* 1 [1942] 283), was probably ruled out in any case because NaKL i 29-30 should refer to Ikūnum, not Erišum.

[10] In *JCS* 8 (1954) 108 note 198, Landsberger transliterates lines 27-28 on the basis of Nassouhi's copy and photo (?), but notes that in 1954 only unreadable traces remained. After a comparison of the present-day (1971) condition of the tablet with the excavation photos, I am unable to establish any significant deterioration of the tablet here or elsewhere and hence believe that the basis for Landsberger's reading is insecure. Similarly with his reading for i 33 proposed in *JCS* 8 (1954) 110 note 208 (where Narām–Sîn, not Puzur–Aššur is expected).

[11] As may be seen from measuring the average size of a line elsewhere and comparing the damaged area here. Furthermore, if one accepts Nassouhi's numbering, one would obtain the following correlation:

NaKL i 22 = KhKL i 21 = SDAS i 20
NaKL i 35 = KhKL i 37 = SDAS i 36

which would mean that NaKL covered the same material in fewer lines than the other texts—the exact reverse of the situation in the other columns, where NaKL eschews the abbreviation of formulae.

[12] For the reading Šu–Ninua vs. Kidin–Ninua, see the Excursus below.

21–26 My collation was able to verify very few of the traces indicated in Nassouhi's transliteration, *AfO* 4 (1927) 3.

29 ^{m}*Šam–*⌜*ši–*^{d}⌝[ ] ⌜*Iš–me–*^{d}⌝[

31 The number at the beginning of the line is 15[(+x)]; the digits are hopelessly damaged here, but the traces do not necessarily disagree with the figure 16 given in both KhKL and SDAS.

32 ^{m}*Aš+šur–*⌜ERÍN.⌝[x] ⌜DUMU *Iš–me–*^{d}*Da–gan*⌝

35 NaKL clearly reads 14 for the regnal years of Puzur–Aššur III. SDAS has 24; KhKL is broken [13].

40 ^{m}*Aš+š*[*ur*]–KUR–⌜*ni*⌝ DU[MU Z]AL.ÁG–DINGIR

41 ⌜1 ITI UD.ME?–*t*⌝[*e*] LUGAL–*ta* DÙ–*u*[*š*]

43 –*b*]*i* ⌜GIŠ.GU.ZA⌝ [

Column III

11 U]N.ME–*šú* DUMU *Aš+šur–*⌜EN–UN.ME⌝–[

12 The number is too badly damaged to be read with any degree of probability. This is true also on the excavation photos, and it is difficult to see any basis for Nassouhi's transliteration "6".

13]–*a–*⌜*ḫi* DUMU *Aš+šur–*ÁG–UN.ME–*šú*⌝

20 –*t*]⌜*a* DÙ–*u*⌝[*š*] is visible.

21 Traces of one uncertain sign are visible.

23 [ ].⌜GABA⌝ DUMU GÍD–⌜*di–en*⌝–[

24 The number at the beginning of the line is 10[(+x)], as copied.

28 The number is 30[(+x)].

29 ⌜x?⌝ [ ]–*ti–*^{d}MAŠ *da–a–*⌜*ri*⌝

30 [^{m}*Aš+šu*]⌜*r–*SUM–IBILA⌝ DUMU–*šu*

31 ⌜GIŠ.GU.ZA *lu iṣ–*⌝*bat* 4 MU.MEŠ[[14]

36 end: DUMU DINGIR–*ḫad–*⌜*da*⌝

40 There is no doubt about the reading "13" for the regnal years of Ninurta-apil-Ekur, while KhKL and SDAS both have "3".

42 The number is 26[(+x)] [15].

Column IV

1] ⌜*i*⌝–[*du*]*k a–n*[*a*

2 –*tà*]*k–ḫil–*^{d}[ ] x [

3 –*i*]*l* ⌜KUR⌝–[*a*] ⌜*e*⌝–[ ] (–*ta* is totally gone both on the tablet and on the excavation photo).

7 The number for the regnal years of Tiglath-pileser I cannot be verified [16].

[13] This is the only case in which NaKL preserves a lower figure for a reign than either KhKL or SDAS.

[14] The number is clearly 4, as opposed to 3 in both KhKL and SDAS.

[15] Weidner's reading of 46! (based on a photo) in *AfO* 15 (1945-51) 88 note 16 seems to me excessively optimistic, considering both the tablet and the old excavation photos.

[16] The digits are badly mangled; but they could have been "9" if written in two horizontal rows (5+4). Cf. the number in iv 11, where the "8" is written 4+4.

14 [ ] ⌜*Šam–ši–*^{d}IM DUMU GIŠ.TUKUL–A–É.Š⌝[ÁR]

15 T]A KUR ⌜*Kar–du*⌝–[ ] *e–*⌜*la*⌝–[

21 at end: ⌜^{d}*Šùl–ma–nu–*SAG⌝

23 The damaged number shows at least two *Winkelhaken*; readings of 40 or 41 cannot be ruled out [17].

27] TUKUL–*t*[*i–*A]–⌜É.ŠÁR.RA⌝ A *Aš+šur–*⌜SAG–*i*⌝–[*ši*]

28 Nassouhi's copy of the number here as [(x+)]⌜3⌝ is not exact. Only one final vertical is visible on the tablet (and also on the excavation photo) [18]. Hence the supposed NaKL "33" for the "32" of KhKL (SDAS is broken here) is non-existent [19]. Thus the balance of evidence favors a thirty-two-year reign for Tiglath-pileser II; and, with his accession date set at 967 (rather than 968 as in *CAH*, 3rd ed., and my chronological tables in Oppenheim's *Ancient Mesopotamia*), all dates in Assyrian chronology before this time which are calculated by dead reckoning by means of the Assyrian Kinglist figures should be lowered by one year. Thus Shalmaneser I should be set at 1273-1244 instead of 1274-1245, etc. Likewise all chronologies based on the Assyrian, e.g., the Babylonian, should be dropped by one year [20].

B

Chronology is the backbone of history and especially of political history; and, for Mesopotamian chronology between 1500 and 600 B.C., there is no body of evidence more important or more widely used than the Assyrian Kinglist tradition. Practically all dates in Mesopotamian history calculated over this time span are based directly or indirectly on the data contained in this tradition. Because the Kinglist preserves a detailed list of Assyrian rulers, their genealogies, and their lengths of reign which is supposed to be complete for more than a millennium preceding 722 B.C. and because it is the only text which provides such a skeleton essential to all historical work, there has been an understandable tendency on the part of historians to utilize this evidence gratefully, sometimes with little critical examination.

This is not to say that there has not been significant historical criticism concerned with the Assyrian Kinglist. F. R. Kraus [21] and B. Landsberger [22] have contributed greatly to our understanding of the text, especially the origins

[17] Contrary to Weidner, *AfO* 15 (1945-51) 88 note 16.

[18] Weidner also believed, on the basis of the photo, that a reading of [30] +2(!) was possible.

[19] Nassouhi may have been influenced, consciously or unconsciously, by the number of years ("33") listed for the eponym period of Tiglath–pileser II in *KAV* 22 v 9".

[20] This holds for all dates of the Kassite dynasty after 1500 and for all dates of the Post-Kassites before 935 (the latter are calculated on Assyrian-based dates for the Isin II dynasty).

[21] *WZKM* 52 (1953-55) 238-243 and *Könige, die in Zelten wohnten* (*Mededelingen der Koninklijke Nederlandse Akademie van Wetenschappen*, Afd. Letterkunde, Nieuwe Reeks, Deel 28, no. 2; Amsterdam, 1965).

[22] *JCS* 8 (1954) 31-45, 47-73, 106-133.

and completeness of its earlier portions [23]. Röllig has recently presented a short theoretical treatment of the typology and sources for the Kinglist [24]; and his assessment of the role of chronicles or chronicle-like documents in the evolution of the Kinglist follows in the tradition of Poebel and Landsberger, who either classified the document among the chronicles [25] or said that it was written in chronicle style [26].

My present purpose is more modest. I wish simply to raise certain questions about isolated points concerning the Assyrian Kinglist tradition and then, in conclusion, to propose further desiderata for a critical approach to the tradition.

The first topic I would like to broach is the reliability of the latter two-thirds of the list, from Bēlu-bāni (king no. 48) on. Landsberger's perceptive analysis of the deficiencies of the Assyrian Kinglist (*JCS* 8) concentrated on the completeness and accuracy of its earlier portions. His study of the Kinglist's conflict with other traditions [27] and its occasional internal chronological improbabilities [28] showed that these early sections have to be interpreted critically, not literally. But what of the later sections of the list? Are they as reliable as generally assumed? Here too, one can easily unearth inconsistencies within the tradition as well as conflicts with other traditions. It may be instructive to list some of the more obvious discrepancies.

A. Length of reign [29]

- A.1 Puzur-Aššur III (no. 61): 24 years (SDAS ii 29), 14 years (NaKL ii 35)
- A.2 Aššur-nādin-apli (no. 79): 4 years (NaKL iii 31), 3 years (KhKL iii 22, SDAS iii 12)
- A.3 Ninurta-apil-Ekur (no. 82): 13 years (NaKL iii 40), 3 years (KhKL iii 30, SDAS iii 17)
- A.4 *Tiglath-pileser II (no. 97): 33 years (*KAV* 22 v 9'', referring to the eponym period), 32 years (KhKL iv 13) [30]

[23] An earlier contribution in the same vein was made by Weidner in *AfO* 15 (1945-51) 96-97. See also Finkelstein, *JCS* 20 (1966) 95-118, especially pp. 112-113, for an assessment of the initial "list of ancestors".

[24] *AOAT* 1 265-277.

[25] *JNES* 1 (1942) 281, covering all but the earliest sections of the list.

[26] *JCS* 8 (1954) 34 note 23.

[27] Notably with royal inscriptions and with the kinglist *KAV* 14, which showed an alternate line of rulers continuing the house of Šamšī-Adad I after Išme-Dagan I.

[28] Especially in the matter of assigning too many generations to a relatively short period of time.

[29] One can add two further discrepancies to this list, if one interprets *KAV* 9 in the usual fashion (see Rowton, *Iraq* 8 [1946] 99 and Grayson, *AOAT* 1 112-114). *KAV* 9 apparently assigns 35 years to [Aššur-uballiṭ (I)], as opposed to KhKL's 36, and 33 years to [Adad-nīrārī (I)], as opposed to KhKL's 32. It should be noted, however, that these variants balance out in the totals; and it would probably be preferable for future chronologies to accept a total of 68 years for these two reigns (which is agreed upon by both sources) rather than to pick the higher figure in each case and arrive at a sum of 69 (supported by neither source).

[30] For other discrepancies between regnal years and eponym periods, see Poebel's table in *JNES* 2 (1943) 88. Note too that the (new?) king reign-

B. Genealogy [31]

B.1 *Aššur–nīrārī II (no. 68): son of Enlil–nāṣir (I/II; KhKL iii 3), son of Aššur–rabi I according to an inscription of his own son [32], Aššur–rā'im–nišēšu (*KAH* 1 63:3-4, cf. *KAH* 2 25:8-9)

B.2 Aššur–rā'im(rē'im)–nišēšu (no. 70): son of Aššur–bēl–nišēšu (KhKL iii 7, cf. NaKL iii 11), son of Aššur–nīrārı II according to his own text (*KAH* 1 63:3)

B.3 Erība–Adad I (no. 72): son of Aššur–bēl–nišēšu (KhKL iii 11, SDAS iii 4, possibly once supported by *KAH* 2 25), son of Aššur–rā'im–nišēšu (NaKL iii 15)

B.4 Adad–nīrārī I (no. 76): son of Arīk–dēn–ili (NaKL iii 23 and his own royal inscriptions, *KAH* 1 7:2, 8:2, 9:2, etc.), brother of Arik–dēn–ili (KhKL iii 17, SDAS iii 8)

B.5 Aššur–nīrārī III (no. 80): son of Aššur–nāṣir–apli (KhKL iii 23, SDAS iii 13), son of Aššur–nādin–apli (NaKL iii 32)

B.6 Erība–Adad II (no. 90): son of Aššur–bēl–kala (NaKL iv 12.16, SDAS iii 31.35, KhKL iii 45), son of [Ilu–kabk]abī (KhKL iv 2-3)

B.7 Tiglath–pileser III (no. 108): son of Adad–nīrārī III (*KAH* 1 21:2), son of Aššur–nīrārī V (SDAS iv 24)

C. Omission [33]

C.1 Shalmaneser II (no. 93) is omitted in NaKL.

D. Variation in royal names [34]

D.1 King no. 79 is Aššur–nādin–apli in NaKL iii 30 and KhKL iii 21, but Aššur–nāṣir–apli in SDAS iii 11 [35].

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ing 828-824 B.C. supposedly inserted in the eponym list *STT* 46+348 (according to Parpola, *AOAT* 6 xvii note 1) is non-existent; the separation of the eponyms for the years 827-823 in some lists simply marks off the second eponym period of Shalmaneser III (cf. Cal ii 36-40, *STT* 47 ii).

[31] Landsberger, *JCS* 8 (1954) 42-44, argues on the chronological grounds of figures (for various "generations") given in the Assyrian Kinglist tradition that between Bēlu–bāni (no. 48) and Aššur–nādin–aḫḫē II (no. 71) the tradition incorrectly designates no fewer than eleven kings as "son" of their immediate predecessor, when each should have been designated "brother" (plus one instance in which "his brother" should read "son of his brother"). Since only one of these cases can be supported by outside textual evidence, the rest are omitted from the list here (without in any sense implying the present writer's dismissal of the cumulative weight of Landsberger's evidence).

[32] *māru* can also be translated "descendant"; but, in context, this is less likely in the royal inscriptions cited in section B.

[33] *KAV* 14 also apparently omits kings 41-53, the latter of which fall after Bēlu–bāni; but this has been amply discussed by Landsberger, *JCS* 8 (1954) 31-33, etc.

[34] This excludes possibly orthographic variants as –PAB.MEŠ (KhKL iii 9, SDAS iii 3) vs. *–a–ḫi* (NaKL iii 13) for king no. 71 and *–ḫad–da* (NaKL iii 36) vs. *–i–ḫad–da* (KhKL iii 27, SDAS iii 15) in the patronymic of king no. 82. — Some apparent variants between col. i of the synchronistic kinglist Assur 14616c (copied most recently from a photo by Weidner, *AfO* 3 [1926] 70-71) and the Assyrian Kinglist do not exist. My collation of Assur 14616c (summer 1971) showed the following readings: (3′) m⌜*Li–ba–a–a*⌝, (5′) m⌜IB–TAR⌝–d30, (8′) ⌜mŠÚ–*Ni*⌝–*nu–a*.

[35] Tukulti–Ninurta's son and successor is called Aššur–nādin–apli in NaKL and KhKL, but Aššur–nāṣir–apli in SDAS (cf. Chronicle P iv 10). The matter

Using the above data, the following observations may be made.

1. Among the various versions of the Assyrian Kinglist, the most nearly contemporary source is not necessarily the most accurate. NaKL omits Shalmaneser II, who ruled about a century before the list was written (C.1). SDAS apparently gives erroneous information on the parentage of Tiglath-pileser III, who died within a few years of the writing of the text (B.7).

2. Most chronological discrepancies involve only one year (A.2, A.4, and the two listed on page 311 note 29) and are relatively insignificant for approximate calculation. More troublesome are the two conflicts of ten years each: A.1 and A.3. Though these two variations—between NaKL and KhKL/SDAS—could conceivably cancel each other out for dates before Puzur-Aššur III (no. 61), A.3 affects the reckoning of dates for most of the latter half of the second millennium. Despite the current historical fashion which prefers "13" rather than "3" years for the length of the reign of Ninurta-apil-Ekur, it should be pointed out that there is not a single shred of positive evidence in favor of either alternative [36]. For several hundred years before 1181 B.C. we are faced with a Mesopotamian chronology that may be inaccurate by as much as ten years (plus the usual minimal margin of error reckoned for dates in the early first millennium B.C.). When, in addition, one considers that we are still not sure of the meaning of the phrase "he ruled/held the throne *ṭuppišu*" applied to kings 84-85 and that many of the uncontested dates in the latter part of the Kinglist (principally before 910 B.C.) are uncontested simply because there is no additional evidence with which to compare them, one is apt to temper one's enthusiasm for the presently unverified later portions of the Kinglist and to wish for further comparative chronological data, preferably contemporary [37].

3. Because of the relatively high number of variations, the genealogical tradition of the Assyrian Kinglist is quite faulty. This conclusion is bolstered by Landsberger's analysis of generations for kings 48-71, which shows that half the genealogical attributions of this section of the Assyrian Kinglist are likely to be erroneous [38]. For these reasons, it is probably unsafe to accept genealogical statements of the Assyrian Kinglist as true without supporting evidence [39].

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is further complicated by the fact that Tukulti-Ninurta's second successor, Aššur-nīrārī III, is described as the son of Aššur-nādin-apli only in NaKL, but as the son of Aššur-nāṣir-apli in both KhKL and SDAS. Contemporary inscriptions survive only from the reign of Aššur-nādin-apli, son of Tukulti-Ninurta (Weidner, *Tn. I*, nos. 40-41). Poebel, *JNES* 1 (1942) 484-490, suggested that Aššur-nādin-apli and Aššur-nāṣir-apli were two quite different sons of Tukulti-Ninurta—the latter leading the revolt against his father and the former succeeding his father on the throne; and a similar position was advanced by Weidner, *Tn. I*, no. 37:10/11, Kommentar. With the present evidence, it seems uncertain whether one or two princes lie behind the conflicting scribal traditions.

[36] To those who would point out that a damaged "13" is apt to be copied as "3" in later lists, one may refer to A.1 as an example of the reverse phenomenon.

[37] Such data are now becoming available for the Middle Babylonian period and are throwing interesting light on the figures given for the Kassite dynasty in the Babylonian Kinglist A.

[38] See note 31 above.

[39] Especially for kings in the second millennium. Babylonian Kinglist A has a tendency to make the same type of genealogical errors; see J. A. Brink-

One does not wish to be overly sceptical about the data of the Assyrian Kinglist tradition. But there is a tendency when dealing with such a unique and—at least in its later portions—seemingly scientific document to forget that all of its data may not be equally reliable [40].

The second topic I would like to touch on is the relative age of the five currently published examples of the Assyrian Kinglist. One, KhKL, is dated exactly by its colophon to 738 B.C. Two more, NaKL and SDAS, may be dated with reasonable accuracy by their concluding portions: NaKL ends with the reign of Tiglath-pileser II (died 935 B.C.) and SDAS ends with the reign of Shalmaneser V (died 722 B.C.); and each should presumably be dated within a few years after these events. Most difficult to date are the two fragmentary exemplars of the list: AsKL and NiKL. Poebel [41] argued that AsKL is the oldest known fragment of the list; and his conclusion was accepted, among others, by Landsberger [42] and Grayson [43]. The date proposed by Poebel would probably lie around the middle of the eleventh century B.C. [44]. Millard tentatively dated NiKL to sometime in the tenth century [45], roughly around the time of NaKL.

Though Poebel himself expressed doubts about the certainty of his argument for the date of AsKL [46], recent commentators have tended to become less reserved in their statements until AsKL has come to be designated simply as the oldest text of the Kinglist. This is not so sure. Only a small portion —approximately half a dozen lines on each side—of the left column on the obverse and the reverse of the tablet survive. These lines occur towards the bottom of column i on the obverse and towards the top of column iv on the reverse [47]. If one estimates the amount of space remaining at the bottom of column iv, one can see that there may have been room for AsKL to end with Tiglath-pileser II or Aššur-dan II [48]; and, conceivably, AsKL could be roughly the same age as NaKL or even slightly younger than it [49]. Unfor-

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man, *A Political History of Post-Kassite Babylonia* (AnOr 43 [Rome, 1968], p. 27.

[40] Especially when its material on a given ruler may be the only historical information available.

[41] *JNES* 1 (1942) 251.

[42] *JCS* 8 (1954) 39 note 48.

[43] *AOAT* 1 109.

[44] Since he infers that the list "closed with a king six or seven more reigns before Tukulti-apil-Ešarra II" (*JNES* 1 [1942] 251).

[45] *Iraq* 32 (1970) 176.

[46] *JNES* 1 (1942) 251 note 5.

[47] Not, as Landsberger stated, in the middle of the column (*JCS* 8 [1954] 39 note 48). Obv. 1′ deals with king no. 35 (= KhKL i 32, SDAS i 31), thereby affording some basis for believing that approximately thirty lines may be missing from the beginning of col. i. All the reasonably well preserved versions of the Kinglist (NaKL, KhKL, SDAS) end their first column with the episode of Šamši-Adad I.

[48] Rev. 6′ = KhKL iii 28, SDAS iii 15. If one calculates that the bottom of col. iv in AsKL is identical in size to the missing top of col. i (which corresponds to thirty-one lines in KhKL and thirty in SDAS), one can form an approximate estimate of how much may be lacking in col. iv. (Thirty-one additional lines in KhKL lead to the Aššur-dan II entry, and thirty lines in SDAS to the second line of the Tiglath-pileser II entry).

[49] The latter alternative is highly unlikely. It must be stressed that these are maximal estimates in that one cannot be sure that the whole of column iv in AsKL was inscribed and that there was no colophon (as in the tenth-century NaKL).

tunately, the sections marked *ḫepi* in NaKL i 33 ([*ḫe–p*]*i*, Sargon I entry) and in AsKL 3′ ([*ḫ*]*e–pi*, Puzur–Aššur II entry) do not correspond; so we cannot tell which scribe is likely to have had a more complete original to copy from [50]. One may also note that the vertical dividing line within the columns, present in both eighth-century copies (KhKL and SDAS) and lacking in the tenth-century copy (NaKL), is also present in AsKL [51].

NiKL also has the vertical dividing line, but the top of the final column on the reverse is uninscribed. Whether this latter feature is to be interpreted as evidence for an early date or whether it should simply be suggested that the inscription was not completed is uncertain. Millard's resort to paleographic evidence is useful, if inconclusive [52]. If the AsKL and NiKL fragments were larger or if we had a detailed study of the development of the Middle Assyrian and early Neo-Assyrian script, this could be of real value. In the present state of affairs, it seems safer to assert only that NaKL, AsKL, and NiKL represent older versions of the Assyrian Kinglist tradition than do KhKL and SDAS. It is difficult to assign absolute chronological priority—in any meaningful sense—to any one of these tablets.

It is also worth stating that the latest significant redaction of the Assyrian Kinglist probably took place in Middle Assyrian times or very early in the Neo-Assyrian period (the latter part of the tenth century being the latest possible date) [53]. Regardless of how one speculates on the sources and origins of the early portions of the list or of various preliminary compilations, the consistent use in the final version of the term *Karduniaš* for Babylonia (in the various episodes concerned with exile of princes or kings) [54] rules out a

[50] Landsberger in *JCS* 8 (1954) 110 note 208, followed by Grayson, *AOAT* 1 110, miscalculated the reference in NaKL i 33 and assigned it to Puzur–Aššur II. Landsberger failed to note that NaKL in the body of the text (i.e., beginning with king no. 33)—in contrast with the practice in both KhKL and SDAS—does not abbreviate the repetitious formulae, and so there are never less than two lines to an individual entry. Since it is quite clear that both lines 32 and 34 end in *šarrūta ēpuš*, the end of column i should be assigned as follows:

i 37-43	Šamši–Adad I	(no. 39)
i 35-36	Erišum II	(no. 38)
i 33-34	Narām–Sîn	(no. 37)
i 31-32	Puzur–Aššur II	(no. 36)

Though it is interesting that the only *ḫepi*'s in the Kinglist tradition occur so close together in this section (raising the possibility that AsKL and NaKL may descend from a single original with damage to more than one entry in this area), one should note that the Nassouhi lacuna—apparently in the father's name of Narām–Sîn—does not seem to affect the later KhKL i 35.

[51] It is difficult to tell whether such an argument, based on a superficial analysis of physical style, has any significance. Only the recovery of further exemplars can decide.

[52] The form of SU in NiKL is not quite so distinctively Middle Assyrian as Millard states (*Iraq* 32 [1970] 176). A similar form, slightly abbreviated, occurs in the tenth-century NaKL iii 17, 37, and iv 12; and a virtually identical form can be found in the tenth-century annals of Aššur–dan II (*AfO* 3 [1926] 155 no. 1 rev. 11), in the early-ninth-century annals of Adad–nīrārī II (*KAH* II 84:33), etc.

[53] Obviously at the time of or before NaKL, our oldest reasonably dated copy.

[54] Šamši–Adad I, Ninurta–apil–Ekur, Ninurta–tukulti–Aššur (Mutakkil–Nusku), and Šamši–Adad IV. It is noteworthy that most of the longer entries in the later portion of the Assyrian Kinglist mention Babylonia.

time before the Middle Assyrian period for the final redaction [55]. As is evident from KhKL and SDAS, once the last significant redaction had been made, references to further reigns were added on in stereotyped formulae [56].

Another interesting feature is the connection of most of the Kinglist tablets with the city of Assur. Two of them were excavated there (AsKL, NaKL). KhKL, although excavated at Khorsabad, was copied from an original from Assur (iv 33); and SDAS, according to its colophon, belonged to Bēl-šuma-iddin, a *mašmāšu* of Assur (iv 29). Only the fragmentary NiKL, the colophon of which is missing, cannot be connected in any way with Assur [57].

One minor topic or textual query which, to my knowledge, has not yet been raised concerns the place where Erišum II, son of Narām-Sîn, ruled. According to the wording of the section of the Kinglist dealing with Šamšī-Adad I, Šamšī-Adad, after proceeding north from Babylonia to Ekallāte, then "came up" (*ēlâ*) [58] further north to depose Erišum. This, taken literally, would imply that Erišum was ruling north—or upstream—of Ekallāte [59] and not, as Landsberger and David Oates have implied, at Assur [60]. Was the capital north of Ekallāte at this time, perhaps removed thither temporarily because of Šamšī-Adad's invasion? Or should *ēlâ* be taken simply as a repetition of the customarily used verb [61] without intended geographical precision [62]? In any case, it is worth observing that the normal sense of the verb seems to require that Erišum was deposed from a seat north or upstream of the now agreed on location of Ekallāte [63].

In conclusion, if one may look forward to the future, it would be a great service to students of Mesopotamian history if all the various Assyrian and Babylonian kinglists [64] were available in more accessible and reliable form.

[55] Landsberger in *JCS* 8 (1954) 35 referred to the *māt Akkad* of the hypothetical forerunner(s). He also speculated on the compilation of the "Grundstock" of the Assyrian Kinglist at some time during the dynasty of Šamšī-Adad I (*ibid.*, p. 109).

[56] One should note that the last entry with additional information (other than royal name, patronymic, and length of reign) pertains to Šamšī-Adad IV in the middle of the eleventh century. Note also that Rowton, *CAH* I/1, 3rd ed., p. 195, would date the compilation of the "original copy" of the Assyrian Kinglist to the eleventh century for reasons he is yet to publish.

[57] Oppenheim has pointed out privately the similarity in the distinctive shape of KhKL/SDAS and the ruled-off inscriptional sections of some *līmu* steles found at Assur (*WVDOG* 24 nos. 15, 28, etc.) and has suggested that certain copies of the Assyrian Kinglist may have been intended for funerary or ceremonial purposes (rather than for strictly chronological ends). See also note 70 below.

[58] I.e., in the sense of going upstream.

[59] Located north of Assur. See Finkelstein, *JCS* 7 (1953) 119; Hallo, *JCS* 18 (1964) 72; Oates, *Studies in the Ancient History of Northern Iraq*, p. 38.

[60] Note the capture of Assur mentioned in Landsberger, *JCS* 8 (1954) 35; Oates, *Studies*, p. 38.

[61] Besides the Kinglist passage referring to Šamšī-Adad I, *ēlâ* is used also of Ninurta-apil-Ekur and Šamšī-Adad IV (in connection with their journeys from Babylonia to Assyria).

[62] This seems less likely, though one might speculate whether Erišum operated along an Ešnunna-Assur axis which was not totally disrupted by Šamšī-Adad's presence at Ekallāte.

[63] Nineveh and Šubat-Enlil come to mind as possibilities, though the latter is probably not directly upstream from Ekallāte and is suggested principally because of Šamšī-Adad's later connection with the town.

[64] And the eponym lists and date lists as well.

It is essential that these documents be carefully re-edited from the original tablets [65], since some of the present disagreements concern even the basic reading of the text [66]. In cases where a tablet has deteriorated over the years [67], it would also help to provide a list of variant readings offered by previous editors [68]. At this stage in the history of the discipline, we would all derive much more benefit from an adequate and reliable text edition than from further elaborate theorizing on an unevenly edited jumble.

When the text of the kinglists has been reasonably established, it would be desirable to have a detailed typological analysis of the various genres that would transcend a superficial classification of types relying on comparative phrase- or sentence-structure [69]. One may even look forward to a day when the determination of the *Sitz im Leben* and textual origins of the kinglists and other chronological documents may represent something other than well-intentioned speculations based on minimal or non-existing evidence [70]. Finally, when one has gained some appreciation of the textual tradition and what it stands for in terms of "literary truth" [71], one should also attempt to assess the tradition as "historical truth" preferably by means of reliable contemporary documents. Obviously, these various analyses and assessments cannot

[65] Editing from photographs or simple re-working of older editions is quite inadequate, as has been documented by the numerous disputes or explanations concerned with non-existent readings. In the case of new editions, one would hope that, in so far as possible, ambiguous signs or traces would be carefully described to minimize the scurrying after collations following the discovery of each new piece of evidence.

[66] While it is helpful to have collations such as those offered here for NaKL and those published by Grayson in *AOAT* 1, it would be much more advantageous to have a standard edition of all kinglists available in a single volume. Assyriologists and historians spend an inordinate percentage of their time threading their way through bibliographical mazes.

[67] As is true, e.g., with the synchronistic kinglist Assur 14616c and probably with Babylonian Kinglist A.

[68] For Babylonian Kinglist A alone, this would probably fill a rather large article.

[69] Lower criticism has its uses, if applied with restraint and common sense. But it would be pointless to attempt a reconstruction of the origin of the sundry types of kinglists and chronological documents based solely on the arbitrary distinction of certain documentary groups by reason of their choice of one or other sentence structure within the allowable variations of Sumerian or Akkadian syntax. Similarities of this type could well be fortuitous and, unless some sort of ulterior connection can be shown (such as origin within a very restricted time or place range), need not reflect anything other than the convenience of the classifier.

[70] The ceremonial function of the genealogical list of the Hammurapi dynasty (Finkelstein, *JCS* 20 [1966] 95-118), as indicated in the latter part of the text, is of particular relevance here, since this is the only document of this type for which we have direct evidence concerning its *Sitz im Leben*. It could be desired that the often-repeated theorizing on the derivation of the Assyrian Kinglists from eponym lists or chronicles/chronicle-like documents would be based on similar grounds rather than on questionable interpretations of the word *līmāni* (KhKL i 26, etc.) or vague similarities in sentence arrangement. We have at present no eponym lists or chronicles which are capable of supplying all the information contained in the terse formulae of the Assyrian Kinglist—royal name, immediate descent, length of reign.

[71] See the analysis by Landsberger, *JCS* 8 (1954) 109.

always proceed sequentially because of the random nature and very incomplete state of the evidence; but they are essential parts of the historical process and can be ignored only to the detriment of history.

Excursus: The Reading of the Royal Name šú-Ninua

In 1954, Gelb and Landsberger independently and almost simultaneously introduced the reading Kidin-Ninua for Assyrian king no. 54. Gelb in *JNES* 13 (1954) 225-226 translated the name written as ^{m}šú-URU.AB×ḪA (in both KhKL ii 24, 26, 28, 35 and SDAS ii 20, 21, 22, 27) as Kidin-Ninua. Though Gelb did not annotate his translation, he today (August 1972) believes that he made this translation on the basis of the use of the logogram šú for *kidinnu* in Middle Assyrian personal names, as analyzed by Ebeling, *MAOG* 13/1 (1939) 52-54. Landsberger in *JCS* 8 (1954) 42 transliterated *KAV* 14:6′ as [*Ki-d*]*in*-^{d}AB×ḪA. Since 1954, the reading Kidin-Ninua has generally remained unquestioned.

There is, however, some reason for doubting its accuracy. First of all, it is by no means certain that šú was ever used as a logogram for *kidinnu*. The Middle Assyrian sign in question varies from a form in which a horizontal wedge and a vertical wedge cross each other at right angles (akin to the common Neo-Assyrian BAR) to a form where the horizontal wedge is at such an oblique angle as to become like the Achaemenid Babylonian form of BAR (often difficult to distinguish from šú). This variation both major Akkadian dictionaries have rightly taken as indicating that the sign in question is BAR [72]. Though Saporetti retains both BAR and šú as transliterations in his *Onomastica medio-assira*, I, 279-290, it would have to be established that these forms are not variants of the same sign [73]. While there is no doubt that BAR can represent *kidinnu* in Middle Assyrian personal names [74], one might question whether some of the references currently interpreted as a logographic šú/BAR might not in fact represent a syllabic *Šú*, which was gradually coming into use in Babylonia and Assyria in the fourteenth and thirteenth centuries and was to win increasing favor as a short substitute for *Šu* in the later periods. Some names would, therefore, be read as *Šú*-DN and would parallel other Middle Assyrian names which are written *Šu*$_1$-DN [75].

On the other hand, there is also reason for doubting Landsberger's reading [*Ki-d*]*in*- in *KAV* 14:6′. The copies, by Schroeder in *KAV* and by Weidner in *MVAG* 26/2 (1921) pl. 5, show for the first preserved sign in this line only a vertical wedge followed by an oblique wedge; the oblique wedge of Schroeder

[72] *AHw*, p. 473 and *CAD*, vol. K, p. 342. The šú sign would very seldom have the oblique wedge crossing so high (and almost horizontally) across the vertical.

[73] As they clearly are in the Achaemenid and later Babylonian scripts and in Kassite Babylonian.

[74] The best evidence for this is the alternate spellings for the name of Kidin-Sîn, the *bēl pāḫete* and eponym official in *KAJ* 109:18 (*Ki-din*), *WVDOG* 24 no. 132:2 (BAR-, slanted), *JCS* 7 (1953) 148 no. 1:26 (BAR-, slightly slanted horizontal crossing almost at the top of the vertical).

[75] E.g., Šu-Adad, Šu-ilāni (Saporetti, *Onomastica*, I, 465-467). One cannot, of course, entirely disregard the possibility that šu was being read as *gimil*(*lu*) at this time.

is a *Winkelhaken*, while that of Weidner approaches the angle of a horizontal stroke and he interprets the sign traces as [š]I in *MVAG* 26/2 (1921) 6[76]. In *JNES* 1 (1942) 475, Poebel noted that he collated the passage and that it apparently read [š]Ú-U-^{d}*Ninua*[77]. Thus, four different authors have given four different interpretations of the sign traces before ^{d}AB × ḪA in this royal name: Schroeder—vertical wedge plus *Winkelhaken*; Weidner—[š]I; Poebel—[š]Ú-U; Landsberger—[*Ki-d*]*in*. Grayson in his collations of *KAV* 14 in *AOAT* 1 111 did not comment on this line.

With the exception of the disputed reading in *KAV* 14, the first element of the RN is always written šú (SDAS, KhKL; NaKL possibly in ii 21, 30). The synchronistic kinglist Assur 14616c i 8′, despite Weidner's copy in *AfO* 3 (1926) 70 (made from a photo), reads ⌜^{m}šú-*Ni*⌝-*nu-a*[78]. Since all these copies of kinglists date from Neo-Assyrian times and since there is no evidence that BAR (even similar to šú) was used as *kidinnu* after the thirteenth century, it would seem that Šu-Ninua is at present a more likely reading for the royal name than Kidin-Ninua; but more conclusive evidence is certainly to be desired.

[76] Presumably because of his reading of the same name as ^{m}šI-*Ni-nu-a* ("vielleicht *Pân-Ninua* zu lesen") in Assur 14616c (= Weidner's "Assur 4128") i 8′ in *MVAG* 26/2 (1921) 13. For the correct reading, see note 34 above and also the last paragraph of this Excursus.

[77] He stated that "of the first sign only the perpendicular wedge and apparently the lower end of its slanting wedge is preserved".

[78] Personal collation of both the tablet and the excavation photo.

The Persepolis Fortification Archive

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The Persepolis fortification tablets were not found in their original place of deposit, but in the fortification wall, where they had been used as fill. There is therefore a theoretical possibility that they do not all belong to a single archive. There is a more serious possibility that they do not include all the types of text present in the original archive, or that they misrepresent the proportional quantities of the various types of text.

The great majority of the tablets are inscribed in Elamite [1]. A small proportion of these bear Aramaic glosses, usually very short. There are also some hundreds of clay tablets inscribed in Aramaic. Nothing has been published about these. The only definite thing that can be said here is that the texts are all relatively short, and so there can be none that correspond to the longer of the Elamite texts. The Aramaic tablets come to us thoroughly mixed with the Elamite tablets. It does not seem likely that they belonged to a separate archive.

The Elamite texts are concerned almost exclusively with the administration of food commodities in the years 13 through 28 of Darius I (509-494 B.C.), covering an area from Susa in the northwest to beyond Niriz in the southeast. Most of them were written elsewhere and sent to Persepolis for accounting purposes. Nearly two-thirds of them record disbursements to the ultimate consumer, particularly regular monthly rations (chiefly barley and wine) to settled work groups, and daily rations (chiefly flour) to travelers. There are also, among others, rations for named persons, for itinerant workers, and for horses and other animals. The payments range from 180 quarts of flour per day to a high official [2] down to one-fiftieth quart of barley per day for a small fowl [3]. The remaining texts are of many kinds. There are relatively simple texts, recording the transportation, delivery or assignment of a given amount (usually large) of some commodity. There are accounting texts, sometimes very complex: balances of receipts and disbursements, inventories, records of increase and decrease of cattle herds, and so forth.

There does not seem to be any reason to doubt that all these various kinds of text belong to the same archive. Neither, in view of their great variety, is

[1] 2,087 texts (identified by *PF* in the following) are transliterated and translated in Hallock, *Persepolis Fortification Tablets* (Chicago, 1969). Nearly 2,500 additional tablets have been studied. An unknown, but relatively small, number of well-preserved tablets, and innumerable fragments, remain untouched.

[2] E. g., *PF* 668.

[3] *PF* 1943: 29 f.

there reason to suspect that there was any other contemporary archive devoted to food commodities.

But we do need to consider the relatively few texts which are not concerned with foods.

The most numerous such texts are those which record deliveries of hides of slaughtered sheep, goats and cattle to treasuries at various places (Shiraz, Pasargadae, Matezziš, etc.) [4]. The slaughter of the animals should be a responsibility of the food administration; it is not surprising that these texts, recording the transfer of a by-product to another jurisdiction, should appear in our archive. In view of the "scribes (writing) on hides (that is, parchment)", mentioned below, we can conclude that one use of the hides was to serve as a writing material.

One unique text seems to record the giving of tools to three persons [5]. One of the items given is *like*, "spike". Another is *basram*, perhaps representing Old Persian **baθra-*, "spade" [6]. There is no clue to the specific meanings of the other three items. Apparently the presence of this text must be accidental. Many kinds of workers, for example, "treasurers", received food from the food administration; likewise the recipients in our text, even if attached to the food administration, should obtain their tools from another agency.

The texts show that the food administration was charged with the care of camels, asses and horses [7], as well as edible animals. This fact is not surprising, since feeding would be the most important element in such care. It is surprising to find, in the record (unpublished) of a herd of asses, the statement: 3 were issued to the herdsman / 3 were brought in and slaughtered. Three asses constitute the herdsman's normal ten percent. Three were brought in, to avoid depleting the herd. But why were they slaughtered? Where such statements occur in texts relating to sheep [8] we assume the sheep were slaughtered for food. It is difficult to avoid the conclusion that the asses were slaughtered for the same purpose. The rare texts mentioning meat rations never mention the meat of asses. Perhaps the herdsman sold it for dog food.

A few letters have nothing directly to do with foods [9]. These employ unfamiliar phraseology and are difficult to interpret. One unpublished letter, more comprehensible than the others, tells of a document sent to Parnaka concerning the non-delivery of accounts; the carrier of the document took to flight; he is to be caught and quizzed; in future the name of the carrier must be written on documents sent to Parnaka. It is possible to assume that all these letters concern operational problems of the food administration (though they could have had a wider application, and been stored in the food archive for lack of a more suitable place).

It seems that all our texts, excepting the tool text mentioned above, can be accepted as belonging to the archive of the food administration. We now

[4] *PF* 58-76.
[5] *PF* 335. *PF* 727 may concern axes, but this is uncertain.
[6] See I. Gershevitch, "Iranian Nouns and Names", *Transactions of the Philological Society* 1969, p. 167.
[7] Camels in *PF* 331, asses in *PF* 289 and 290, horses in an unpublished text.
[8] See, e. g., *PF* 2008: 10 f.
[9] *PF* 1858-60, *PF* 2071, and four unpublished letters.

consider whether that archive originally contained kinds of texts not present, or inadequately represented, among the fortification tablets.

There is a very striking scarcity of texts recording regular rations of groups at Persepolis. We have only three such texts, which involve groups of five to seven workers [10]. In contrast there are many such texts for the next two most prominent cities in the Persepolis area, and some involve large groups. For Matezziš [11] there are twenty-one texts, dealing with groups as large as 712. For Shiraz there are thirty-six texts, with groups up to 490. On the other hand, there are nineteen letters ordering rations for work groups at Persepolis. Such letters are otherwise rare, and there are none for Matezziš or Shiraz. One such letter, to Mamnakka at Niriz, orders him to supply grain rations for four men, previously treasury workers at Niriz, who have been transferred to Persepolis to work as stonemasons [12]. This letter evidently deals with the first appearance of a group at Persepolis, before regular arrangements have been made for its rationing there. Subsequently the group should receive its rations from a source at Persepolis. The almost complete lack of texts recording such ration payments requires explanation. The only adequate explanation would seem to be that most of those texts were written in Aramaic on perishable material.

There is evidence that Aramaic scribes were more numerous than Elamite scribes. Groups of scribes are mentioned eleven times, seven times described as "Babylonian scribes on parchment" [13], four times simply as "Babylonian scribes". Ten times the scribes are said to be "assigned by Parnaka", who is shown by the fortification texts to be the chief officer of the food administration, and once "assigned by Ziššawiš", his assistant. That Aramaic scribes did write food documents is shown by statements in two fortification texts that certain information was conveyed on "a document (written) on parchment" [14].

If the Aramaic texts on clay belong to our archive, as assumed above, then the lost Aramaic texts on parchment also should have belonged to it. There would be no obvious reason for excluding them just because they were written on a different material.

There are other apparent gaps in the information provided by the fortification texts. For example, among the texts recording daily rations for exalted officials, there are fifty for Parnaka and twenty-three for Ziššawiš, but only a handful for other persons [15]. In this and other cases documents may be scarce because they were usually written in Aramaic. But there cannot be any assurance that this is the correct explanation.

The Persepolis treasury tablets [16], also written in Elamite on clay, comprise the only known body of material that is at all close to the Persepolis fortification tablets. They date to 492-458 B.C., thus beginning two years after the fortification tablets end. They are chiefly concerned with the payment of silver

[10] *PF* 872 and two unpublished texts.
[11] Old Persian *Uvādaicaya*. Exact location unknown.
[12] *PF* 1825.
[13] E. g., *PF* 1810.
[14] *PF* 1986: 31 f., also an unpublished letter.
[15] See *PF* 654-90. Gobryas, the father of Mardonius, appears in *PF* 688 and in two unpublished texts. Artazostra, not named, but described as wife of Mardonius and daughter of the king, appears in a unpublished text. Artavardiya appears in *PF* 689-90 and in two unpublished texts.
[16] G. Cameron, *Persepolis Treasury Tablets* (Chicago, 1948).

in lieu of rations in kind (in some cases for very large work groups at Persepolis). Since they come from a period in which Aramaic, one would assume, is increasingly dominant, the question arises how they came to be written in Elamite. A plausible guess is that the recording of a peripheral activity of the treasury was assigned to Elamites for the purpose of giving work to underemployed scribes.

It is possible to infer the existence of parallel archives: a treasury archive in the fortification period and a food archive in the treasury period. The workers had to be clothed; presumably records of clothing issuance formed all or part of another archive. The single misplaced tool text, mentioned above, suggests that there was also an archive concerned with tools. This last was at least partly in Elamite. The others may have been in Elamite, in Aramaic, or in both.

A New Subdivision of the Shekel in the Arsacide Period

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A new designation for a subdivision of the shekel occurs in a small group of tablets from the time of the Arsacide ruler Mithridates II (125-88 B.C.). It is written, in the singular, either as *ma-ḫi* (five times) or as *ma-ra* (three times), and, in the plural, as *ma-ḫat* (fifteen times).

Douglas A. Kennedy copied the fifteen tablets of this group for the British Museum, where they are now kept, and published them in the volume *CT* 49[1]. They are Nos. 150, 152-164, and 166. J. N. Strassmaier had previously copied *CT* 49 156 as No. 5 in *ZA* 3 (1889) 145f., *CT* 49 159 as No. 4 ibid. 144f., and *CT* 49 162 as No. 7 ibid. 146f.

The dates range from the year 217 to 219 of the Seleucid Era, i.e., from 95 to 93 B.C. Two of the texts have damaged dates (Nos. 164 and 166); the others extend from the 21st of the fifth month 217 SE (No. 159) to the 20th of the sixth month 219 SE. They clearly belong to an archive, the same name (written *Ra-ḫi-i-me-e-su* with several variants) occurring in 14 of the 15 tablets. With the exception of a legal document (*CT* 49 160), they all deal with administrative matters pertaining to a sanctuary of the Esagila complex in Babylon, Esagila being mentioned in Nos. 153 and 162, the city of Babylon, in Nos. 159 and 160. Although rather interesting in itself, the nature of the recorded administrative transactions is not the concern of this paper.

The word under discussion appears only in the following nine texts: Nos. 150, 152, 154, 156, 158, 159, 160, 162 and 166.

Following is a survey of the different spellings:

written *ma-ḫi*:	154:9	3	*ma-ḫi*
	154:12	2	GÍN 2 *ma-ḫi*[2]
	156:10*	1	*ma-ḫi*
	158:19	11	GÍN 2 *ma-ḫi*
	158:7**	5	GÍN 4 *ma-ḫi*
	*beside *ma-ḫat* in lines 1 and 13		
	**beside *ma-ḫat* in lines 1 and 12		
written *ma-ra*:	152:1	$1^1/_3$	MA.NA 8 GÍN 2 *ma-ra*
	152 r. 6'	$^2/_3$	MA.NA $4^1/_2$ GÍN 4 *ma-ra*
	159 r. 6'*	$1^1/_3$	MA.NA 8 GÍN 2 *ma-ra*
	*beside *ma-ḫat* in line 10		

[1] I have to thank Mr. Kennedy for placing at my disposal his transliterations of the texts of this group. They contain a number of collations of which I was able to make use (see below n. 2, n. 3 and n. 4).

[2] Read 2 (!) instead of the one on the copy (coll.).

written *ma–ḫat*:	150:4	18 GÍN 5 *ma–ḫat*
	150:7	$1^1/_3$ MA.NA 6 GÍN 3 *ma–ḫat*
	150:39	$5^1/_2$ GÍN 4 *ma–ḫat*
	150:41	$^1/_2$ GÍN 2 *ma–ḫat*
	150:59	4 GÍN 2 *ma–ḫat*
	150:60	4 *ma–ḫat*
	156:1*	$^2/_3$ MA.NA $6^1/_2$ GÍN 2 *ma–ḫat*
	156:13	$^1/_3$ MA.NA 9 GÍN 3 *ma–ḫat*[3]
	158:1**	[$^1/_3$ MA.N]A. 9 GÍN 2 *ma–ḫat*
	158:12	6 GÍN 2 *ma–ḫat*
	159:7***	$^1/_3$ MA.NA 1 *ma–ḫat*
	159:10	$6^1/_2$ GÍN 4 *ma–ḫat*
	160:13	$10^1/_2$ GÍN 5[4] *ma–ḫat*
	162:23	3(!)[5] GÍN 5 *ma–ḫat*
	166:6'	$1^1/_2$ GÍN 4 *ma–*[*ḫat*]

* beside *ma–ḫi* in line 10
** beside *ma–ḫi* in lines 7 and 19
*** beside *ma–ra* in line r. 6'

The cardinal numbers which appear before the subdivision *m.* are: 1, 2, 3, 4 and 5. This is the case whether the preceding unit is a full shekel or a half shekel. The absence of the numbers 1 and 3 in the latter instance is accidental.

A tabulation of the evidence follows:

after full shekels:	1 *m.* in 156:10, 159:10
	2 *m.* in 150:59, 152:1, 154:12, 158:1, 12, 19, 159 r. 6'
	3 *m.* in 150:7, 154:9, 156:13
	4 *m.* in 150:58, 60, 158:7
	5 *m.* in 150:4, 162:23
after half shekels:	1 *m.* (not attested)
	2 *m.* in 150:41, 156:1
	3 *m.* (not attested)
	4 *m.* in 150:39, 152 r. 6', 166:6'
	5 *m.* in 160:13

These figures clearly indicate that the shekel was, in the Arsacide period, subdivided into twelve units called *m.* Such fractions as $^1/_3$ and $^2/_3$ are conspicuously absent, although they are used to subdivide the mina in our group of texts (see 150:7 and 152 r. 5). The only fractions admitted are $^1/_2$ and $^1/_4$. The former appears before *m.* (see above), but the latter is never followed by *m.* Thus we have 4–*tú* "one fourth" in 156:11 and 162:7, and "three fourths" written $^1/_2$ GÍN 4–*tú* in 162:1. We observe two ways of rendering the fraction "one fourth", i.e., by 4–*tú* and by 3 *m.*, while there is no evidence for rendering "three fourths" as $^1/_2$ GÍN 3 *m.* in this small group of documents. Apparently we have here evidence for the use of two systems of subdividing the shekel: an older one, and a new one, using from one to five *m.* in combination with $^1/_2$.

[3] Read according to collation.
[4] Read 6 (!) $^1/_2$ GÍN (coll.).

Apart from the material which has just been presented and discussed, there is other evidence available within the texts of our group. Ten out of the fifteen documents deal with the accounting for expenditures made from a basic sum and list the remaining balance. Their use for the present investigation is restricted by two factors: texts which contain figures that do not express a subdivision of the shekel are as worthless for our purpose as are those which are partly broken and do not allow us to check their arithmetic. We are, therefore, left with but three documents (154, 156 and 158) which allow us to check on the figures of the scribes.

In *CT* 49 154 we read that from an amount of 12 shekels, the following expenditures were made:

Basic sum:	12 GÍN	
Expenditures:		
line 6:	$1^1/_2$ GÍN	
line 9:		3 *ma-ḫi*
line 12:	2 GÍN	2 *ma-ḫi*
Total:	$3^1/_2$ GÍN	5 *m.*
Balance:	8 GÍN	1 *m.*

In line 16 the scribe gives as a balance only eight shekels. If this document were the only evidence available, one would have to conclude from it that the shekel was subdivided into 10 *m.*, giving a balance of exactly eight shekels. Since 4 *m.* and 5 *m.* occur elsewhere, we have to assume either that there is an error or that the scribe has dropped one twelfth of a shekel.

A similar situation occurs in *CT* 49 156.

Basic sum:	$46^1/_2$ GÍN	2 *ma-ḫat*
Expenditures:		
line 7:	16 GÍN	
line 10:		1 *ma-ḫi*
line 11:	$1^1/_4$ GÍN	
Total:	$17^1/_4$ GÍN	1 *m.*
Balance:	$29^1/_4$ GÍN	1 *m.*

Instead of the correct amount of 29 shekels and 4 *m.*, line 13 gives us the figure $^1/_3$ MA.NA 9 GÍN 3 *ma-ḫat*, which is again one *m.* short of the correct result. Moreover, if we attempt to make the arithmetic accurate, we could do so only by assuming that the shekel was subdivided into eight *m.*-units. But this is excluded by the fact that 5 *m.* occur twice after a full shekel and once after a half shekel. Furthermore, the subdivisions of the shekel cannot be at variance in *CT* 49 154 and 156, since they both belong to the same archive. This compels us again to assume the same reasons for the discrepancy that was observed in the preceding document[5].

[5] The copy in *CT* 49 shows the figure four written by means of four vertical wedges, while the copy made by Strassmaier in *ZA* 3 (1889) 146f. (No. 7) seems to show only three such wedges. The figure four is never written in this way, however, and since the accounting requires a figure three, the latter was adopted in the transliteration.

Let us now turn to the last document. In *CT* 49 158, the figures of the basic sum are partly destroyed but their usefulness is not affected.

Basic sum:	[x MA.N]A	9	GÍN	2 *ma–ḫat*
Expenditures:				
line 7:		5	GÍN	4 *ma–ḫi*
line 10:		$2^1/_2$	GÍN	
line 12:		6	GÍN	2 *ma–ḫat*
line 16:		4	GÍN	
Total:		$17^1/_2$	GÍN	6 *m.*
Balance (l. 19):		11	GÍN	2 *m.*

The sum of the expenditures plus the balance at hand add up to $28^1/_2$ GÍN and 8 *m.*, i.e., 29 shekels and 2 *m.* Hence one has to restore the damaged figure of the basic sum as [$^1/_3$ MA.N]A 9 GÍN 2 *ma–ḫat*. This accounting makes sense when the *m.* is the twelfth part of a shekel.

The word *ma–ḫi* is obviously of Aramaic origin, as Prof. A. Sachs of Brown University has suggested. It occurs in Biblical and Rabbinic Hebrew as *mā'ā* with the meaning "grain, kernel", and as my colleague Prof. S. Kaufman informs me, is used in Rabbinic Hebrew and in Aramaic to denote a subdivision of the shekel [6]. The use of the word *maḫi* "grain" as a subdivision of the shekel corresponds to the use in earlier Neo-Babylonian texts of the words *ḫallūru* "chick pea" for the tenth of a shekel and *girû* "carob (seed)" for the twenty-fourth [7]. As a matter of fact, *mā'ā* appears in both Talmudic Aramaic and Syriac to render the Hebrew *gērāti* which designates respectively $^1/_{20}$ of the smaller, or $^1/_{24}$ of the larger, shekel.

With regard to the word *girû* in Neo-Babylonian texts, we are faced with a difficulty which has been duly pointed out in the *CAD*. Beside the meaning $^1/_{24}$, established by Ungnad (see note 4), there is also evidence, admittedly an isolated instance (*YOS* 6 112) [8], that *girû* denotes $^1/_{12}$ of a shekel. This would not argue against our interpretation of *m.* as a word for the twelfth of a shekel since half a millennium separates these two words. Nevertheless, the double use of the designation *girû* poses a problem. It is possible that there existed a *girû* amounting to $^1/_{24}$ and a "double" *girû* denoting $^1/_{12}$ of the shekel. This possibility is suggested by the small text *CBS* 11032, published in *JCS* 1 (1947) 67 f. by A. Sachs. This document enumerates the subdivisions of the Neo-Babylonian shekel beginning with *girû* and ending with 1 GÍN. However, in doing so, it allots two lines to *girû* and these run as follows: 2,30 (= $^1/_{24}$) *gir–ú*, and 5 (= $^1/_{12}$) *gír–e*, i.e., $^1/_{24}$ (of the shekel) is *girû*, $^1/_{12}$ (of the shekel) is (two) *girûs*, using the plural of *girû*. Perhaps the "double" *girû* of the earlier Neo-Babylonian period was replaced by the *maḫi* under Arsacide rule. At any rate, the relationship pointed out here between *mā'ā* and *girû* supports the interpretation of *maḫi* (*mara*) as one twelfth of a shekel.

The intricacies of late Mesopotamian metrology are, however, not the concern of this paper. My interest in the cited passages is primarily lexicographical.

[6] See also Charles F. Jean and Jacob Hoftijzer *Dictionnaire des Inscriptions sémitiques de l'Ouest* (Leiden, 1965) p. 161.

[7] A. Ungnad, "Zum babylonischen Geldwesen", *OLZ* Beiheft 2 (1908) No. 26-28.

[8] The addition is: 24 $^1/_2$ GÍN (line 4), 1 GÍN 4–*tú* (line 5), 52 GÍN *šul–lul–ti* 1 GÍN (line 6), 15 GÍN 3 IGI.4.GÁL.LA.ME (line 8) and 9 GÍN 4–*tú* (line 9); the sum is given in line 10 as 1 $^2/_3$ MA.NA 3 GÍN *gi–ru–ú*.

Ecclesiastes in Judeo-Persian

Herbert H. PAPER – Ann Arbor

The Jewish Theological Seminary in New York City has long had in its possession the unusually valuable manuscript collection of the late Elkan Nathan Adler, an English attorney-bibliophile who travelled and collected numerous important materials during his lifetime [1]. At the end of the nineteenth century, Adler made an extensive trip through Central Asia and Iran and acquired a number of Judeo-Persian manuscripts [2]. One text, No. 433, contains Judeo-Persian translations of Proverbs (beginning at 7:6), Song of Songs, Ecclesiastes, and a rabbinic text named Kinyan Torah. The ms was originally labelled B 46 (B = Bokhara) in Bacher's catalogue of the Adler Judeo-Persian materials.

As part of my effort to make more JP materials more readily available, I am taking this occasion to publish the Ecclesiastes portion of this ms. My debt to my teacher Professor Ignace J. Gelb is happily only partly repaid by this means of showing him honor. This edition illustrates two of the important lessons I learned from him: respect for primary sources and deep interest in writing systems. Expanded knowledge of the generally neglected area of JP — minimally defined as Persian language materials written in the Hebrew alphabet — will go a long way toward providing important documentation for many aspects of Persian linguistic history.

In recent years there has been a notable increase in scholarly interest in the JP area of Iranian studies [3]. While the time is not yet ripe for overall studies, much has been done and continues to be done in this important field and in the prerequisite work of unearthing texts from their respected interment in various libraries, and of preparing them for publication. Only when a significant number of such texts are published and thereby made available to all

[1] On E. N. Adler (1861-1946), cf. *Encyclopedia Judaica* (Jerusalem 1972), Vol. II, pp. 275-6.

[2] E. N. Adler, *About Hebrew Manuscripts* (1905); *Jews in Many Lands* (1905).

[3] Jes P. Asmussen, *Studier i jødisk-persisk litteratur* (Copenhagen 1970); Walter J. Fischel, "Israel in Iran (A Survey of Judeo-Persian Literature)", in L. Finkelstein, editor, *The Jews: Their History, Culture, and Religion*, 2nd ed. (New York 1960), pp. 1149-90; Gilbert Lazard, *La langue des plus anciens monuments de la prose persane* (Paris 1963), pp. 128-34; Herbert H. Paper, "Judeo-Persian Bible Translations: Some Sample Texts", *Studies in Bibliography and Booklore*, Vol. VIII (Spring 1968), Nos. 2-4, pp. 99-113; now also, cf. Herbert H. Paper, *A Judeo-Persian Pentateuch* (Jerusalem: Ben-Zvi Institute, 1972), an edition of the earliest dated Judeo-Persian Pentateuch translation (1319 A.D.) from a manuscript in the British Museum; in general, the entire Spring 1968 number of *Studies in Bibliography and Booklore* is devoted to "Judeo-Persian Studies" and contains much bibliographical information.

students of modern Persian can the next steps be undertaken. This one text is thus to be viewed as part of this continuing effort.

The ms [4] is written in one of the typical translation forms of JP texts: each Hebrew verse fully vocalized (Tiberian) is followed by its JP translation. The following salient points in this text may be noted:

1. -št, -šn

's'yšt	2:18; 4:6; 5:11; 6:5
bxššt	3:13; 5:18
yvs'ršt	2:1
fr'z rsšt	3:19 [3]; 9:2, 3, 11; 10:18
nvm'yšt	6:9; 11:9
st'yšt	2:2; but, *st'yš* 4:2; 8:15
	also, *xvvšmnš* 4:1
	xvvšmnyš 4:1
čršn	1:14, 17; 2:11, 17, 26; 4:4, 6, 16; 6:9

2. hmy

hmy	8:4, 10.

3. Orthographic variants

'nč	:	*'nčy*	passim
dl	:	*dyl*	1:16; 2:3
d'r'y	:	*d'r'hy*	1:4
xvršyd	:	*xvvršyd*	2:22; 1:3, 5 [2], 9 et passim
xvhlh	:	*xvvhlh*	1:15; 7:13
ḥkmt	:	*ḥykmt*	1:16, 18

4. Hebrew words

bny 'dm	2:8; 3:21; 8:1 — but pvsr'n mrdvm 3:18
ml'k	5:5
ndr	5:3, 4
drvm	1:6; 11:3
ṣfvn	1:6; 11:3
'vlm	3:11
ṣdyq	7:15; 9:2
rš'	8:13; 9:2
ḥkm	2:16, 19; 4:13; 6:8; 7:5, 7, 19; 8:1
ḥkmym	12:11 [2]

5. Causatives

by zyh'nd	7:12
rnj'nydn	1:13
'ngyz'nydn	4:10
r'y'n'	2:3
z'y'nd	5:13; 6:3
ryz'nd	11:3
bygvdr'n	11:10
vr xyz'nd	4:10

[4] I wish to record my thanks to Dr. Menahem Schmelzer, Director of the Jewish Theological Seminary Library, for permission to publish this text.

6. Special Persian forms

'b'z	
'br	
'ndr	
'ym'	
hnvz	
p'dy'vnd	4:12
tnǰydn	2:6; 10:17
mr	3:17
dvdvmyn	4:8, 10, 12, 15
mnm	1:12
hm mn	7:26
hnd	4:9
xv'b	5:11
dry'h	1:7 [2]
bnǰšk	12:4
bnǰšk'n	9:12
švb	2:23; 8:16
fvrm'n	8:2, 5
pvsr	passim
zr	2:8; 6:9; 7:12
sym	
svl'xh'	12:3
sxvn'n	1:1; 5:1; 6:11; 9:17; 10:12, 13
sxvn'nh'	1:8; 5:2, 6; 7:12; 10:14
lby'nh'	10:12

1·1 סכונאן קהלת פוסר דוד פאדשאה דר ירושלם: 1·2 הרזה הרזהא גופת קהלת הרזה הרזהא המה צ̇יזי הרזה: 1·3 צ̇י פאידה דהד במרדום בהמה רנג אוי כי רנג ברד זיר כוורשיד: 1·4 דאראהי אייא ודאראי רווא וזמין בגאוידאן פאדיא פאינדהסת: 1·5 ובידרפשד כוורשיד ואנדר שוד אן כוורשיד בגאיגאה אוי אושתאב כונא ודרפשא אוי אנגאי: 1·6 רווא בדרום וגרד אנדר גרדא בצפון גרד אנדר גרדא גרד אנדר גרדא רווא אן באד ואבר גרדאן גרדהא אוי באז גרדד אן באד: 1·7 המה אן רודהא רוואאן בדריאה ודריאה ניסת אוי פור שווא בגאיגאה כי אן רודהא רוואאן אנגאי אישאן באז גרדאאן ברפתן: 1·8 המה אן סכונאנהא רנג בראאן ני תואנד מרד בסכון גופתן ני סיר באשד צ̇שם בדידן וני פור שודה אייד גוש בשנידן: 1·9 צ̇י כי בוד אוי כי באשד וצ̇י כי כרדה אמד אוי כי כרדה אייד וניסת היץ צ̇יז נוו זיר אן כוורשיד: 1·10 הסת צ̇יזי כי בי גויד ביבין אין נוו הסת אוי פישתר בוד בגאוידאן אנצ̇י בוד דר פיש אימא: 1·11 ניסת יאד כרד באוולינאן ואניז באכרינאן כי באשנד ני באשד באישאן יאד כרד אבאז כי באשד באכרין: 1·12 מגם קהלת בודם פאדשאה אבר ישראל דר ירושלם: 1·13 ובי דאדם מר דל מן בטלב כרדן בגוסתן בחיכמת אבר המה אנץ כרדה אמד זיר אן כוורשיד אוי מעני בד הסת דאד כודאי בפוסראן מרדום ברנגאנידן באוי: 1·14 ודידם מר המה אן עמלהא כי כרדה אמדנד זיר אן כוורשיד ואינך המה הרזה וצ̇רשן באד: 1·15 כוהלה ניתואנד בארסתן ונוקצאני ני תואנד בשומורדה אמדן: 1·16 סכון גופתם מן אבאז דיל מן בגופתן מן אינך בוזורג כרדם ואבזודם חכמת אבר המה

אנצי בוד דר פיש מן אבר ירושלם ודיל מן דיד בסיאר חכמת ועקל : 1·17 ובדאדם דיל מן
בשנאכתן חיכמת ועקל אפסוס ונאדאני שנאכתם כי אניז אין אוי צרשן באד : 1·18 כי בבסיארי
חיכמת בסיארי כשם ובי אבזאיד עקל ובי אבזאיד דרד : 2·1 גופתם מן בדיל מן בי איי נון גוסארשת
כונם בשאדי וביבין בניכי ואינך אניז אוי הרזה : 2·2 בכנדה גופתם סתאישת בודה ובשאדי צי אין
כונא : 2·3 גוסתם בדל מן בכשידן במיי מר תן מן ודיל מן ראיאנא בחיכמת ובגרפתן בנאדאני תא
אנץ בי ביגם כדאם ניכוי בפוסראן מרדום אנץ בי כוננד זיר אן אסמאן שומאר רוזגאראן זנדגאני
אישאן : 2·4 בוזורג כרדם עמלהא מן אבדאן כרדם במן כאנהא נשאנדם במן ריזהא : 2·5 כרדם
במן בוסתאנהא ובאגהא ונשאנדם באישאן דרכת המה ברי : 2·6 כרדם במן חווטהא אב בתנגדן אז
אישאן בישה רויא היזומהא : 2·7 כרידם בנדיגאן ופר סתאראן ואהל כאנה בוד במן אניז רמה גאו
וגוספנד בסיאר בוד במן אז המה צייזי כי בודנד דר פיש מן דר ירושלם : 2·8 גמע כרדם במן אניז
זר וסים ודוסתי פאדשאהאן ואן שהרסתאנהא כרדם במן סרהנגאן וכאתונאן ותנעומהא בני אדם
סרוד וסרודהא : 2·9 ובוזורג כרדם ואבזודם אז המה צייזי כי בוד דר פיש מן דר ירושלם אניז
חיכמת מן איסתיד במן : 2·10 והמה אנץ טלב כרדנד צשמאן מן ני רהא כרדם אז אישאן ני מנע
כרדם מר דיל מן אז המה שאדיאי כי דיל מן שאד באשא אז המה רנג מן ואין בוד בכש מן אז המה
רנג מן : 2·11 ורוי כרדם מן בהמה עמלהא מן כי כרדנד דסת מן ברנג כי רנג בורדם בכרדן ואינך
המה הרזה וצרשן באד וניסת פאידה זיר אן כוורשיד : 2·12 ורוי כרדם מן בדידן חיכמת ואפסוס
ונאדאני כי ציסת אן מרדום כי בי שווד פס אן פאדשאה מר אנצי פישתר כרד אוירא : 2·13
ודידם מן כי הסת פאידה בחיכמת אז נאדאני צון פאידה אן רושנאיי אז תאריכי : 2·14 אן חכם
נטר אוי בסר אוי ואן נאדאן בתאריכי רווא ושנאכתם אניז מן כי פראז רססת יכי פראז רסד מר
גומלה אישאן : 2·15 וגופתם מן בדיל מן צון פראז רססת אן נאדאן אניז מן פראז רסד מרא וצירא
כרדמנד בודם מן אן הנגאם זיאדה וסכון גופתם בדיל מן כי אניז אין הרזה : 2·16 כי ניסת יאד כרד
בחכם אבאז אן נאדאן בגאוידאן בצי פישתרין רוזגאראן אן אייאאן המה צייזי פראמוש שודה אמד
וצירא בי מירד אן חכם אבאז אן נאדאן : 2·17 ודושמן מן דאשתם מר אן זנדגאני כי בד בוד אבר
מן אן עמל כי כרדה אמד זיר אן כוורשיד כי המה צייזי הרזה וצרשן באד : 2·18 ודושמן דאשתם
מן מר המה רנג מן כי מן רנג ברא זיר אן כוורשיד כי אסאישת באשד במרדום כי באשד פס מן :
2·19 וכיסת שנאסא אן חכם באשד יא נאדאן ופאדשאהי ראנד בהמה רנג מן כי רנג בורדם וכי
כרדמנד בודם זיר אן כוורשיד אניז אין הרזה : 2·20 וגרד אנדר גשתם מן בנאומיד כרדן מר דיל
מן אבר המה אן רנג כי רנג בורדם זיר אן כוורשיד : 2·21 כי הסת מרדום כי רנג אוי בחיכמת ובעקל
ובכובי ובמרדום כי ני רנג בורד באוי בי דהד אוירא בכש אוי אניז אין הרזה ובדי בסיאר : 2·22
כי צי באשד במרדום בהמה רנג אוי ובצרשן דל אוי כי אוי רנג ברא זיר אן כוורשיד : 2·23 כי המה
רוזגאראן אוי דרדגין שוואאן וכשם מעני אוי אניז בשוב ני כופסיד דיל אוי אניז אץ הרזה הסת אוי :
2·24 ניסת ניכי במרדום כי בי כוורד ובי תנגד ובי בינד מר נאן אוי ניכוי הסת ברנג אוי אניז אין
דידם מן כי אז קודרת כודאי הסת אן : 2·25 פי כה בי כוורד וכה אושתאב כונד גייר אז מן : 2·26
כי במרדום כי ניכוי באשד דר פיש אוי דאד חיכמת ועקל ושאדי בכטא כונא דאד מעני בגרד כרדן
ובגמע כרדן בדאדן בניכי דר פיש כודאי אניז אין הרזה וצרשן באד : 3·1 בהמה זמאן וקת בהמה
מוראד זיר אן אסמאן : 3·2 וקת בזאדן וקת במורדן וקת בנישאנדן וקת בכנדן נשאנדה : 3·3 וקת

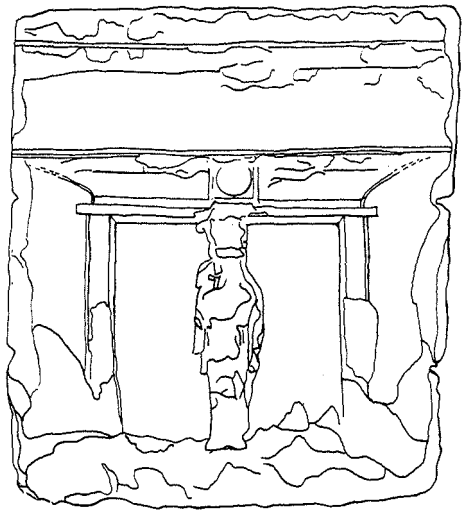
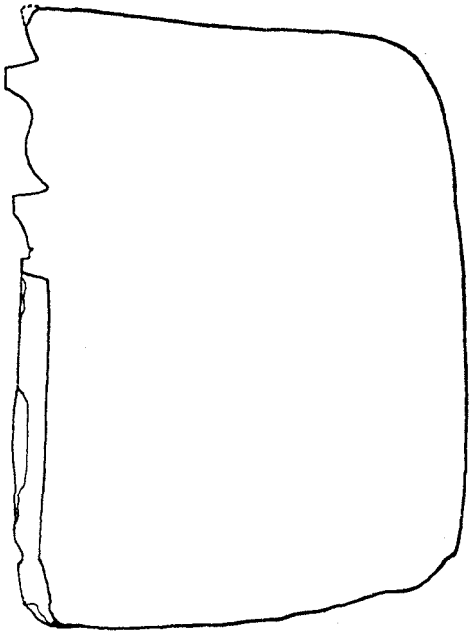
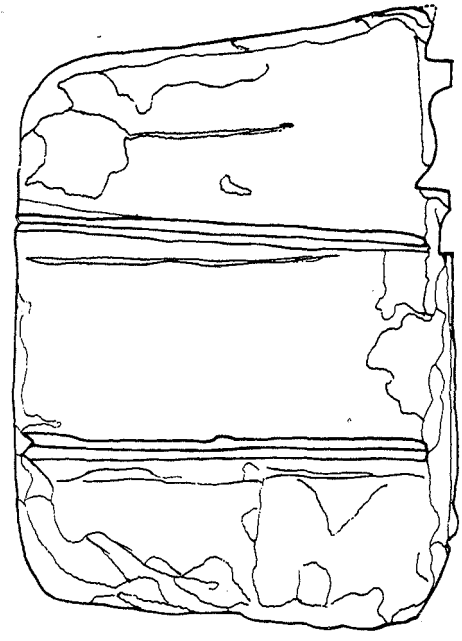
בכושתן וקת בדרמאן כרדן וקתי ברכנה כרדן וקתי באב<ד>אן כרדן : 3·4 וקת בגריסתן וקת בכנדה כרדן וקת שׂון כרדן וקת רקץ כרדן : 3·5 וקת באבגנדן סנגהא וקת גמע כרדן סנגהא וקת באגוש גרפתן וקת בדור כרדן אז אגוש גרפתן : 3·6 וקת בטלב כרדן וקת בגום כרדן וקת בניגה דאשתן וקת באבגנדן : 3·7 וקת בדרידן וקת בדוכתן וקת בכאמוש בודן וקת בסכון גופתן : 3·8 וקת בדוסת דאשתן וקת בדושמן דאשתן וקת כארזאר וקת סלאמתי : 3·9 צׂיסת מנפאעת אן כונא באנץ אוי רנג ברא : 3·10 דידם מר אן שוגל אנץ דאד כודאי בפוסראן מרדום ברנג בורדן באוי : 3·11 מר המה צׂיז כרד כוב בוקת אוי אניז מר אן עולם דאד בדיל אישאן אז אנץ ני יאבד אן מרדום מר אן עמל אנצׂי אפריד כודאי אז אוול ותא אכיר : 3·12 שנאכתם כי ניסת ניכי באישאן אלא בשאדי כרדן ובכרדן ניכי בזנדגאני אוי : 3·13 ואניז המה אן מרדום כי בי כוורד ובי תנגד ובי בינד ניכי בהמה רנג אוי בכששת כודאיי הסת אן : 3·14 שנאכתם כי המה אנץ בי כונד כודאי אוי באשד בגאוידאן אבר אוי ניסת באבזודן ואז אוי ניסת בכם כרדן וכודאי כרד כי בי תרסנד אז פיש אוי : 3·15 צׂי כי בוד פישתר אוי ואנץ בבודן פישתר בוד וכודאי טלב כונד מר תאכתן ברא : 3·16 ואניז דידם זיר אן כוורשיד גאיגאה אן חוכם אנגאי אירכתה וגאיגאה אן ראסתיגר אנגאי אן אירכתה : 3·17 גופתם מן בדל מן מר אן צדיק ומר אן רשע דאורי כונד כודאי כי וקת בהמה מוראד ואבר המה אן עמל אנגאי : 3·18 גופתם מן בדיל מן אבר סבב פוסראן מרדום בפאכיזה כרדן אישאן אן כודאי ובדידן כי אישאן צׂהאר פאי אישאן באישאן : 3·19 כי פראז רסשת בני אדם ופראז רסשת צׂהאר פאי ופראז רסשת יכי הסת באישאן צׂון מורדן אין המידון מורדן אין ובאד יכי ובהמה ופרק בני אדם אז צׂהאר פאי ניסת כי המה צׂיזי הרזה : 3·20 המה צׂיזי רווא בגאיגאה יכי המה צׂיזי בוד אז כאך והמה צׂיזי באז גרדד באן כאך : 3·21 כיסת שנאסא רוח בני אדם אן ור שווא הסת אן בבאלא ורוח אן צׂהאר <פאי> פרוד שווא הסת אן בזיר בזמין : 3·22 ודידם כי ניסת בהתר אז אנץ שאדי כונד אן מרדום בעמלהא אוי כי אוי בכש אוי כי כה בי אוורד אוירא בדידן בצׂי כי באשד פס אוי : 4·1 ובאז גשתם מן ובי דידם מר המה טולמהא אנץ כרדה אמדגאן זיר אן כוורשיד ואינך ארס אן טולמהא וניסת באישאן כושמנש דהאיי ואז דסת טולם כונאאן אישאן זור וניסת באישאן כושמניש דהאיי : 4·2 סתאיש גויא הם מן מר אן מורדיגאן כי פישתר מורדנד אז זנדגאן אנץ אישאן זנדגאן הנוז : 4·3 בהתר הסת אז הר דואן אישאן מר אנץ הנוז ני בוד ואנץ ני דיד מר אן עמל אן בד אנץ כרדה אמד זיר אן כורשיד : 4·4 ודידם מן מר המה רנג ומר המה כובי אן עמל כי אן רשך מרד אז רפיק אוי אניז אין הרזה וצׂרשן באד : 4·5 אן נאדאן דר אגוש גירא מר דסתהא אוי וכוורא מר גושת אוי : 4·6 בהתר הסת פור דסתהא אסאישת אז פור מושתהא רנג וצׂרשן באד : 4·7 ובאז גשתם מן ובי דידם הרזה זיר אן כוורשיד : 4·8 הסת יכי וניסת דודומין אניז פוסר ובראדר ניסת באוי וניסת פרגאם בהמה רנג אוי אניז צׂשמאן אוי ני סיר באשד תואנגרי ובכיסת מן רנג ברא ונוקצאן כונא מר גאן מן אז ניכי אניז אין הרזה ומעני בד הסת אוי : 4·9 ניכויתראן הנד אן דו אז יכי אנץ הסת באישאן מזד ניכוי ברנג אישאן : 4·10 כי אגר בי אופתנד אן יכי ור כיזאנד מר רפיק אוי ואגר אן יכי כי בי אופתד וניסת דודומין באנגיזאנידן אוירא : 4·11 אניז אגר בי כופסנד דו וגרם באשד באישאן וביכי צׂי גונה גרם באשד : 4·12 ואגר פאדיאונד באשנד אן יכי אן דודומין בי איסתנד מוקאבל אוי ואן רשתה סה גאנה בודה ני בזודי גוססתה אייד : 4·13 בהתר הסת כודך מסכין וחכם אז פאדשאה פיר ונאדאן אנץ ני שנאכת באהתריז כרדן אניז : 4·14 כי

4·15 אז כאנה אן זנדגאן בירון אמד בפאדשאהי כרדן כי אניז בפאדשאהי אוי זאדה אמד דרויש : דידם מר המה אן זנדגאן רוואאן זיר אן כוורשיד אבאז אן כודך אן דודומין אנצי בי איסתד עיוד אוי : 4·16 ניסת פרגאם בהמה קוום בהמה אנץ בוד דר פיש אישאן אניז אן אכרינאן ני שאדי כוונד באוי כי אניז אין הרזה וצרשן באד : 4·17 ניגה דאר פאי תו צונאנצי בי רוי בכאנה כודאי וגזדיך באשנידן אז דאדן אן נאדאנאן דביחת כי ניסת אישאן שנאסאאן בכרדן בדי : 5·1 מה נהיב זני אבר דהאן תו ודיל תו ני אושתאב כונד בבירון אבורדן סכון דר פיש כודאי כי כודאי דר אסמאן ותו אבר זמין אבר אין סבב באשנד סכונאן תו אנדכאן : 5·2 כי אמד בושאסף בבסיארי שוגל ואבאז נאדאן בבסיארי סכונאנהא : 5·3 צון אנץ נדר כרדי נדר דר פיש כודאי ני דרגג כוני בבאז תוכתן אוירא כי ניסת מוראד בנאדאנאן מר אנץ נדר כוני באז תוזי : 5·4 בהתר הסת אנצי ני נדר כוני אז כי נדר כוני וני באז תוזי : 5·5 ני דהי מר דהאן תו בכטא כרדן מר תן תו וני גויי דר פיש מלאך כי גלט הסת אן צירא כשם גירד כודאי אבר אבאז תו ותבאה כונד מר עמל דסת תו : 5·6 כי בבסיארי בושאספהא והרזהא וסכונאנהא בסיאר מר כודאי בי תרס : 5·7 אגר טולם כונא דרויש ורובודה כונא חוכם וראסתי בי ביני דר שהרסתאן ני עגב מאני אבר אן מוראד כי בולנד אז ור בולנד ניגה דארא ובולנדאן אבר אישאן : 5·8 ואבזוני זמין בהמה צייזי אוי מליך בדשת וורז כונים : 5·9 דוסת דארא סים ני סיר באשד סים וכיסת דוסת דארא באמבוה נה דכל אניז אין הרזה : 5·10 בבסיאר בודן ניכי בסיאר באשנד כווראאן אן וציסת כובי בכודאונדאן אן אלא דידן צשמאן אוי : 5·11 שירין הסת כואב אן וורז כונא אגר אנדך ואגר בסיאר ובי כוורד וסיר באשד בתואנגרי ניסת אוי אסאישת ב[אוי] אנדר כואב שודן : 5·12 הסת בדי רנגור דידם זיר אן כוורשיד תואנגרי ניגה דאשתה בכודאונדאן אוי בבדי אוי : 5·13 וגום באשד אן תואנגרי אוי במעני בד ובי זאיאנד פוסרי וניסת בדסת אוי צייזי : 5·14 צון אנץ בירון אמד אז אשכם מאדר אוי וברהנה באז גרדד ברפתן צון כי אמד וציזי ני ור דארד ברנג אוי כי בי ברד בדסת אוי : 5·15 ואניז אין בדי רנגור המה מוקאבל כי אמד המידון בי רווד וצי מנפאעת יאבד באוי כי רנג ברד בבאד : 5·16 אניז המה רוזגארן אוי בתאריכי בי רווד וכשם בסיאר ורנגורי אוי וכשם : 5·17 יאינך אנץ דידם מן ניכי אנץ כוב הסת בכוורדן ובתנגידן ובדידן ניכי בהמה רנג אוי כי רנג ברד זיר אן כוורשיד שומאר רוזגארן זנדגאני אוי אנץ דאד באוי כודאי כי אוי בכש אוי : 5·18 אניז המה אן מרדום אנץ דאד באוי כודאי תואנגרי ומאלהא ומוסלט כרד אוירא בכוורדן אז אוי ובוור דאשתן מר בכש אוי ובשאדי כרדן מר רנג אוי אין בכששת כודאיי הסת אן : 5·19 כי ני בסיאר יאד כונד מר רוזגארן זנדגאני אוי כי כודאי גואב דהא בשאדי דל אוי : 6·1 הסת בדי אנצי דידם זיר אן כוורשיד ובסיאר הסת אן אבר אן מרדום : 6·2 מרדי אנץ בי דהד באוי כודאי תואנגרי ומאלהא ועזיזי וניסת אוי נוקצאן בגאן אוי אז המה אנץ ארזוי כונד וני מוסלט כונד אוירא כודאי בכוורדן אז אוי כי מרדי בי גאנה בי כוורד אוירא אניז אין הרזה ובימארי בד הסת אוי : 6·3 אגר בי זאיאנד מרד צד וסאלהא בסיארן בי זיהד ובסיאר כי באשנד רוזגארן סאלהא אוי וגאן אוי ני סיר באשד אז ניכי ואניז קברה ני בוד באוי גופתם בהתר הסת אז אוי אן סקט בודה : 6·4 כי בהרזה אמד ובהרזה נאם אוי פושידה אייד : 6·5 אניז כוורשיד ני דיד וני שנאכת אסאישת באין אז אין : 6·6 ואגר בי זיהד הזאר סאלהא ובארהא וניכי ני דיד הא ני בגאיגאה יכי אן המה צייזי אן רווא : 6·7 המה רנג אן מרדום בדהאן אוי ואניז אן גאן ני פור שודה אייד : 6·8 כי ציסת אבזוני בחכם אזאן נאדאן צי בדרויש

שנאסא ברפַתן מוקאבל אן זנדגאני: 6·9 בהתר הסת נומאישת צשמאן אז רפַתן גאן אניז אין הרזה וצרשן באד: 6·10 צי כי בוד פישתר כאנדה אמד נאם אוי ושנאכתה אמד אנץ אוי מרדום וני תואנד בחוכם כרדן אבאז כי פאדיאונדתר אזוי: 6·11 כי הסת סכונאן בסיאר כוניאן הרזה צי פאידה הסת במרדום: 6·12 כי כיסת שנאסא צי בהתר במרדום בזנדנאני שומאר רוגאראן זנדנאני הרזה מן ובי כונד אישאן רא צון סאיה אנץ כיסת אנאה כונד במרדום צי באשד פס אוי זיר אן כוורשיד: 7·1 בהתר הסת נאם אז רונן ניכוי ורז מורדן אז רוז זאדן אוי: 7·2 בהתר הסת ברפַתן בכאנה מוציבת אז רפַתן בכאנה דעות באנץ אוי אכר המה מרדום ואן זנדה בידהד בדיל אוי: 7·3 כהתר הסת כשם אז כנדה כי בבדי רויהא כוש נרדאנד דל: 7·4 דל כרדמנדאן בכאנה מוציבת ודיל נאדאנאן בכאנה שאדי: 7·5 בהתר הסת באשנידן זגר חכם אז מרד אשנווא סרוד נאדאנאן: 7·6 כי צון אואז אן כארהא זיר אן דינ המידון כנדה נאדאן אניז אין הרזה: 7·7 כי אן טולם כונא אפסוס כונד חכם וגום כונד מר דיל בכשנדה: 7·8 בהתר אכרת סכון אז אוולין אוי בהתר הסת דראז באד אז בולנד באד: 7·9 ני נהיב זני בבאד תו בכשם נרפַתן כי כשם באגוש נאדאנאן בי נשינד: 7·10 ני נויי צי בוד כי אן רוגאראן אן אוולינאן בודנד ניכויתראן אז אינאן כי ני אז חיכמת סואל כרדי אבר אין: 7·11 בהתר הסת חכמת אבאז אחסנתה ואבזוני בבינאאן אן כוורשיד: 7·12 כי בסאיה אן חכמת בסאיה אן סים ואבזוני עקל חיכמת בי זיהאנד כודאונדאן אן: 7·13 בי בין מר קודרת כודאי כי כה בתואנד בראסתה כרדן מר אנץ כווהלה כרד אוירא: 7·14 ברוז ניכי באש בניכי וברוז בדי ביבין אניז מר אין במוקאבל אין אפריד כודאי אבר סבב אנץ ני יאבד אן מרדום פס אוי ציזי: 7·15 מר המה ציזי דידם ברוגאראן הרזה מן הסת צדיק גום שווא בראסתיגרי אוי והסת רשע דראז כשא בבדי אוי: 7·16 מה באש צדיק בסיאר ומה כרדמנד שווי זיאדתי צירא פרומנין שווי: 7·17 ני אירכתה באשי בסיאר וני באשי נאדאן צירא בי מירי בבי וקת תו: 7·18 בהתר הסת אנץ בינירי באין ואניז אז איז מהיל דסת תו כי תרסידנאר כודאי בירון אייד מר גומלה אישאן: 7·19 אן חכמת פאדיאונד באשד בחכם אז דה סולטאנאן אנץ בודנד דר שהר: 7·20 כי מרדום ניסת ראסתינר דר זמין אנץ בי כונד ניכי וני כטאנאר שווד: 7·21 אניז בהמה אן סכונאנהא אנץ סכון נוינד ני דהי דל תו אנץ ני אשנווד מר בנדה תו נפַרין כונא תורא: 7·22 כי אניז בארהא בסיאראן שנאכת דיל תו אנץ אניז תו נפַרין כרדי דינראן: 7·23 המה אין אוזמאישת כרדם בחיכמת נופתם כרדמנד באשם ואן דור הסת אז מן: 7·24 דור צי כי בוד ווורפַי זורה כה בי יאבד אוירא: 7·25 נרד אנדר נשתם מן ודל מן בשנאכתן ובגוסתן ובטלב כרדן חיכמת וחסיב ובשנאכתן אירכתה נאדאן ואן נאדאני אפסוס: 7·26 ויאבא הם מן טלך תר אז מרגי מר אן זן אנץ אן דאמהא וכמינהא דיל מן בסתינאן דסתהא אן ניכוי דר פיש כודאי רסתה שודה אייד אזאן וכטא כונא גרפַתאר אייד באן: 7·27 בי בין אין יאפַתם נופת קהלת יכי ביכי ביאפַתן חסיב: 7·28 אנץ אניז טלב כרד גאן מן וני יאפַתם מרדום יכי אז הזאר יאפַתם וזן בהמה אינאן ני יאפַתם: 7·29 גוד ביבין אין יאפַתם אנצי אפריד כודאי מר אן מרדום כוב ואישאן טלב כרדנד חסיבהא בסיארן: 8·1 כיסת צון חכם וכיסת שנאסא תעביר סכון חיכמת בני אדם רוושן גרדאנד רוי אוי וצהרה אוי גודגון שודה אייד: 8·2 מן פורמאן מליך ניגה דאשתם ואבר סבב סוונגד כודאי: 8·3 ני נהיב זדה איי אז פיש אוי בירווי ני איסתי בציזי בד כי המה אנץ מוראד דארד בי כונד: 8·4 באנץ סכון פאדשאה סולטאן וכה בינויד באוי צי המי כוני: 8·5 ניגה דארא פורמאן ני שנאסד ציזי בד וקת

וחוכם בשנאסד דל חכם: 8·6 כי בהמה מוראד הסת וקת וחוכם כי בדי אן מרדום בסיאר הסת אבר אוי: 8·7 כי ניסת אוי שנאסא צי כי באשד כי צונאנצי באשד כה אנאה כונד באוי: 8·8 כיסת מרדום מוסלט בבאד במנע כרדן מר אן באד וניסת פאדשאה ברוז מרגי וניסת פרסתא בכארזאר וני רסתה כונד אירכתה מר כודאונד אוי: 8·9 מר המה אין דידם ודאדה שווא מר דיל מן בהמה עמל אנץ כרדה אמד זיר אן כוורשיד וקת אנץ מוסלט שוד אן מרדום במרדום בבדי באוי: 8·10 ובאין סבב דידם טאלימאן קאבר כרדיגאן ואמדנד אז גאיגאה כאין המי רוונד ופראמוש כרדנד דר שהר אנץ צונין כרדנד אניז אין הרזה: 8·11 אנץ ניסת כרדה אמד סכון עמל אן בדי זוד אברין סבב פור שודה אמד דיל פוסראן מרדום באישאן בכרדן בדי: 8·12 אנץ כטאגאר כונא בדי צד ודראז כשא באוי כי אניז שנאסא מן אנץ באשד ניכי בתרסידגאראן כודאי אנץ בי תרסנד אז פיש אוי: 8·13 וניך ני באשד ברשע וני דראז כשד רוזגאראן צון סאיה אנץ ניסת אוי תרסא אז פיש כודאי: 8·14 אסת הרזה אנץ כרדה אמד אבר אן זמין אנצי הסת צדיקאן אנץ רסא באישאן צון עמל אן טאלימאן והסת טאלימאן כי רסא באישאן צון עמל צדיקאן גופתם כי אניז אין הרזה: 8·15 וסתאיש גוים מן מר אן שאדי אנצי ניסת ניכוי במרדום זיר אן כוורשיד אלא בכוורדן ובתנגידן ובשאדי כרדן ואוי בדרקה כונד אוירא ברנג אוי רוזגאראן זנדגאני אוי צון דאד באוי כודאי זיר אן כוורשיד: 8·16 צונאנצי דאדם מן דל מן בשאנכתן חכמת ובדידן מר אן שוגל אנצי כרדה אמד אבר אן זמין כי אניז ברוז ובשוב כואב בצשמאן אוי ניסת אוי בינא: 8·17 ודידם מר המה קודרת כודאי כי נה תואנד אן מרדום ביאפתן מר אן עמל אנץ כרדה אמד זיר אן כוורשיד בסבב אנץ רנג ברד אן מרזום בטלב כרדן וני יאבד ואניז אגר בי גויד אן חכם בשנאכתן וני תואנד ביאפתן: 9·1 כי מר אין דאדם מר דיל מן ובפאכיזה כרדן מר המה אין אנץ אן ראסתיגראן ואן כרדמנדאן כרדארהא אישאן בקודרת כודאי אניז דוסתי אניז דושמני ניסת שנאסאאן מרדום המה צויזי דר פיש אישאן: 9·2 אן המה צונאנצי בהמה פראז רסשת יכי בצדיק ברשע בניך ובפאך ובפליד ובדביחת כונא ובאנץ ניסת אוי דביחת כונא צון ניך צון כטא כונא אן סוגנד כוורא צונאנצי סוגנד תרסא: 9·3 אין בד באשד בהמה אנץ כרדה אמד זיר אן כוורשיד כי פראז רסשת יכי בהמה ואניז דיל פוסראן מרדום פור בדי ואפסוס בדל אישאן בזנדגאני אישאן ופס אוי באן מורדגאן: 9·4 כי כיסת אנץ פיוסתה אייד בהמה אן זנדגאן הסת אעתמידיהא כי בסג זנדה אוי בהתר אז שיר מורדה: 9·5 כי אן זנדגאן שנאסאאן כי בי מירנד ואן מורדגאן ניסת אישאן שנאסאאן צויזי וניסת אניז באישאן מיזד כי פראמוש שודה אייד דכר אישאן: 9·6 אניז דוסתי אישאן אניז דושמני אישאן אניז רשך אישאן פישתר גום שוד ובכש ניסת באישאן אניז בגאוידאן בהמה אנץ כרדה אמד זיר אן כוורשיד: 9·7 בי רוו בי כור בשאדי נאן תו ובי תנג בדלי כוש מיי תו כי פישתר מוראד בוד כודאי מר עמלהא תו: 9·8 בהמה וקתי באשנד גאמהא תו סופידאן ורונן אבר סר תו ני נוקצאן באשד: 9·9 ביבין זנדגאני אבאז זן אנץ דוסת דאשתי המה רוזגאראן זנדגאני הרזה תו כי אוי הסת בכש תו בזנדגאני וברנג תו אנץ תו רנג ברא זיר אן כוורשיד: 9·10 המה אנץ בי רסד דסת תו בכרדן בקוות תו בי כון כי ניסת עמל וחסיב ועקל וחיכמת בגור אנץ תו רווא באנגאי: 9·11 באז גשתם ודידם זיר אן כוורשיד כי ני בסבוכאן אן דוונדגי וני בגבאראן אן כארזאר ואניז נה בכרדמנדאן נאן ואניז ני בפהימאן תואנגרי ואניז ני בשנאסאאן כובי כי וקתי ופראז רסשת פראז רסד מר גומלה אישאן: 9·12 כי אניז ני שנאסד אן מרדום מר וקת אוי צון מאהיאן כי גרפתה אמדגאן בדאם בדי

וצון בנגשכאן אן גרפתה אמדיגאן בדאם צון אישאן תלה זדגאן פוסראן מרדום בוקת בדי צון כי
בי אופתד אבר אישאן נאגאה : 9·13 אניז מן דידם חיכמת זיר אן כורשיד ובוזורג אסת אן במן :
9·14 שהר כוצך ומרדומאן באן אנדך ובי אייד באן פאדשאהי בוזורג וגרד אנדר גרדד אנרא
ואבאדאן כונד אבר אן קלעההא בוזורגאן : 9·15 ויאפת באן מרד מסכין וחכם רסתה כונד אוי
מר אן שהר בחיכמת אוי ומרדום ני יאד כרד מר אן מרד מסכין אוי : 9·16 וגופתם מן בהתר הסת
חיכמת אז גברוותי וחיכמת מסכין כואר בודה וסכונאן אוי ניסת אישאן אשנוואאן : 9·17 סכונאן
חכמים בסיכיני אשנידה שוואאן אז באנשאהי פאדשאהי ראנא בנאדאנאן : 9·18 בהתר הסת חיכמת
אז סליח כארזאר וכטא כונא יכי גום כונד ניכי בסיאר : 10·1 מגואן מורדה גנדה שוד כוש
גרדאנד רוגן עוטר אמיז עזיזתר אז חיכמת אז עזיזי נאדאנאן אנדכי : 10·2 דל חכם בראסת אוי
ודל נאדאן בצף אוי : 10·3 ואניז בראה צון כי נאדאן רווא דיל אוי נוקצאן שוא וגויד בהמה צייזי
נאדאן הסת אוי : 10·4 אגר באד אן פאדשאהי ראנא בר אייד אבר תו גאיגאה תו ני רהא כוני כי
דרמאן כונא בי הילד כטאגאראן בזורגאן : 10·5 הסת בדי דידם זיר אן כורשיד צון גלטי כי בירון
אייד אז פיש אן פאדשאה : 10·6 דאדה אמד אן נאדאן דר אבראזהא בסיאראן ותואנגראן באוביסתיגי
בנשינגד : 10·7 דידם בנדיגאן אבר אספאן וסרהנגאן רוואאן צון בנדגאן אבר זמין : 10·8 כנא
גבדאל באוי בי אופתד ורכנה כונא דיואר בי גזד אוירא מאר : 10·9 אבגנא סנגהא דרדגין שודה
אייד באישאן ושכאבא היזומהא גרם שודה אייד באישאן : 10·10 אגר כונד שוד אן אהין ואן ני
רווא תיר וספאהא גבאר שוד ואבזוני כובי חיכמת : 10·11 אגר בי גזד אן מאר בי אפסון וניסת
אבזוני בכודאונד אן זבאן : 10·12 סכונאן דהאן חכם כובי ולביאנהא נאדאן בי אובארד אוירא :
10·13 אוולין סכונאן דהאן אוי נאדאני ואכרת דהאן אוי אפסוסהא בדי : 10·14 ואן נאדאן בסיאר
כונד סכונאנהא ני שנאסד אן מרדום צי כי באשד ואנץ באשד אז פס אוי כה אגאה כונד באוי :
10·15 רנג אן נאדאנאן רנגה כונד אוירא אנץ ני שנאכת ברפתן בשהר : 10·16 ואי בתו זמין כי פאדשאה
תו בורנא וסרהנגאן תן בבאמדאד בי כוורנד : 10·17 כונוכי תו זמין כי פאדשאה תו פוסר אזאדאן
וסרהנגאן תו בוקת בי כוורנד בגברוותי וני בתנגידני : 10·18 בכאהילה מסכין שוד אן פראז
רסשת ובאובסתיגי דסתהא בי צכד כאנה : 10·19 בכנדה כונאאן נאן ומיי שאד באשד זנדגאני
ואן סים גואב דהד מר המה צייזי : 10·20 אניז בעקל תו פאדשאהאן ני נפרין כוני ובחוגרהא
בסתר תו ני נפרין כוני תואנגר כי מורגאן אסמאן בי ברד מר אן אואז וכודאונד פרהא אגאה כונד
סכון : 11·1 בפרסת נאן תו אבר רוי אן אב כי בבסיארי אן רוגאראן בי יאבד אוירא : 11·2 בי
דה בכש בהפת ואניז בהשת כי ני שנאסי צי באשד בדי אבר זמין : 11·3 אגר פור שודה איינד אן
אברהא באראן אבר זמין בי ריזאנד ואגר בי אופתד דרכת בגאניב דרום ואגר בצפון גאיגאה כי בי
אופתד אן דרכת אנגאי באשד : 11·4 ניגה דארא באד ני תוכם כארד ובינא באברהא ני דרובאן
כונד : 11·5 צונאנצי ניסת תו שנאסא ציסת ראה אן באד צון אסתוכאנאנהא באשכם פור צונין ני
שנאסי מר עמל כודאי אנץ בי כונד מר המה צייזי: 11·6 בבאמדאד תוכם כאר מר תוכם תו ובאיבאר
ני רהא כוני דסת תו כי ניסת תו שנאסא כדאם כוב באשד אין יא אין ואגר הר דואן אישאן צון
יכי ניכויאן : 11·7 ושירין הסת אן רושנאיי וניכוי הסת בצשמאן בדידן מר אן כוורשיד : 11·8 כי אגר
סאלהא בסיאר בי זיהד אן מרדום בגומלה אישאן ושאד באשד ויאד כונד מר רוזגאראן אן תאריכי
כי בסיאר באשנד המה אנץ אמד הרזה : 11·9 שאד באש בורנא בגואני תו וניכוי גרדאנד תורא דיל

תו ברוזגאראן בורנאיי תו ובי רוו בראהא דל תו ובנומאישת צשמאן תו ובשנאס כי אבר המה אינאן בי אוורד תורא כֹודאי דר חוכם׃ 11·10 ודור כון כֹישם אז דיל תו וביגודראן בדי אז תן תו כי אן כודכי ואן סיאהי הרזה׃ 12·1 ויאד כון מר אפֹרידגאר תו ברוזגאראן בֹורנאיי תו תא אנֹץ ני איינד רוזגאראן אן בדי ובי רסנד סאלהא אנֹץ בי גויי ניסת במן באישאש מוראד׃ 12·2 תא אנֹץ ני תאריך שוד אן כֹוורשיד וזוהרה ואן מהתאב ואן סתארגאן ובאז גרדנד אן אברהא פס אן באראן׃ 12·3 ברוז כי בי לרזנד ניגה דאראאן אן כֹאנה וכֹוהלה בודנד מרדומאן אן כֹונרי ובאטל שוונד אן אסיאב כי אנדך באשנד ותאריך שוונד אן בינאאן בסולאכֹהא׃ 12·4 ובסתה אמדנד דרהא דר באזאר באובסתה שודן אואז אן אסיאב ובר כֹיזד באואז אן בנגֹשך ודולאב שוונד המה אהל סרוד׃ 12·5 אניז אז בולנד גֹאי בי תרסנד ואן שכנהא בראה גול ור אוורד אן באדאם ובאר כשד אן מייג ובאטל כונד אן בר כי רווא מרדום בכֹאנה עולם אוי וגרד אנדר גרדנד דר באזאר אן שיון כונאאן׃ 12·6 תא אנֹץ ני גוססתה אייד בנד סימין ובי דווד צֹרך זרין ושכסתה אייד סבֹוד אבר אן צשמה ובי דווד אן צֹרך בצֹאה׃ 12·7 ובאז גרדד אן כֹאך אבר זמין צֹון כי בוד ואן באד באז גרדד בכֹודאי אנֹץ דאד אנרא׃ 12·8 הרזה הרזהא גופֹת קהלת המה צֹיזי הרזה׃ 12·9 ואבזוני כי בוד קהלת חכם אניז אמוכֹת עקל אז או קוום והאז גוש כרד ותפֹחוס כרד אראסתה כרד מתלהא בסיאר׃ 12·10 טלב כרד קהלת ביאפֹתן סכֹונאן מוראד ונבשתה כֹובי סכֹונאן ראסת׃ 12·11 סכֹונאן חכמים צֹון פושתובאנהא וצֹון מסמארהא נישאנדגאן כֹודאונדאן חכמים דאדה אמדנד אז רפֹיק יכי׃ 12·12 וויאדתר אז אישאן אי פוסר מן אהתריז כון כרדן סופֹרהא בסיאר ניסת פֹרגֹאם ואפֹסוס בסיאר רנג בורדן גושת׃ 12·13 אכֹר סכֹון המה צֹיזי אשנידה שווא אז כֹודאי בי תרס ומר פֹרמאנהא אוי ניגה דאד כי אין המה אן מרדום׃ 12·14 כי המה עמל כֹודאי בי אוורד דר חוכם אבר המה פושידה אגר ניך ואגר בד׃

רֹכֹב פסוֹ< קים >