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The Mesopotamian Theory of Music and the Ugarit Notation – A Reexamination

Introduction: Discoveries and Problems

At the present writing,¹ research on the Mesopotamian theory of music has already been going on for more than fifteen years. In 1960 Anne Kilmer published two lists of so-called key-numbers or coefficients for various computations — similar to today’s collections of “useful tables.” In one of these, the tablet known by the *siglum* CBS 10996, a section appeared that had not been known previously from similar mathematical lists; it presented pairs of numbered entities, each apposed to an entity of another class. Benno Landsberger who had suggested the publication of CBS 10996, noted that these paired entities appear singly in the lexical text U.3011 (still unpublished at that time), where they represented a paradigmatic sequence of strings. In the Key-Number Table, therefore, each pairing of strings denotes “something,” but it was not yet clear what these were (for this first presentation and discussion of CBS 10996, see Kilmer 1960: 274–275, 278, 281, 289–300). It should be mentioned, in parenthesis, that shortly before this time (1959) it had been proved that the “Babylonian notation” presented by Curt Sachs in 1923 had not been a notation at all (see here Appendix A, Excursus 1).

The first musicological study of the two new texts was undertaken by Marcelle Duchesne-Guillemin (1963). In 1965, Kilmer and Duchesne-Guillemin published adjoint studies on the same texts (Kilmer 1965; Duchesne-Guillemin 1965). Kilmer introduced a third text, which had already been known for more than forty-five years, but misunderstood; she explained how it related to the Key-Number Table and to the String List. This is a section of the large Song Catalogue from Assur (KAR 158, published in 1919; see Ebeling 1919) that sums up the number of songs in each of the seven categories. Stephen Langdon had interpreted these cat-

¹ This monograph was written by Bayer over a long period of time. The present manuscript dates from 1978 when it was intended for printing as volume II of *Yuval – Monograph Series* of the JMRC. Few additions and corrections were made by the author from 1978 until her untimely death in 1995. The manuscript was recovered from her estate when it was brought to the National Library of Israel long after 1995. Bibliographical updates and a critique by Ann Kilmer appear at the end of this article. For an update see: J. Rahn, *The Hurrian Pieces*, ca. 1350 BCE: Part One — Notation and Analysis, *Analytical Approaches to World Music Journal*, vol. 1, no. 1 (2011) http://www.aawmjournals.com/articles/2011a/Rahn_AAWM_Vol_1_1.htm (accessed November 15, 2012).

egory terms as instruments (Langdon 1921: 173, 183, 186ff.). In this case, Langdon cannot be blamed: what he did not see was, after all, not visible at that time. Neither was it visible a generation later, when we find Langdon's interpretations adopted by Farmer in his survey of Mesopotamian music in *NOHM* 1 (Farmer 1957; note that Galpin 1937 is now totally outdated and of value only to the history of the research). The seven category terms in KAR 158 were now recognized as identical with seven of the fourteen terms that are apposed to the string pairs in the Key-Number Table.

A fourth text became available soon afterward, in 1968: U.7/80 (known in literature as the "Tuning Text"), discovered in the British Museum by Edmond Sollberger and published by Oliver R. Gurney with an adjoint musical analysis by David Wulstan (Gurney 1968; Wulstan 1968). Here, the string terms and the seven song-categories are related by the description of a procedure: how to change the "instrument" from one state to another, by doing something to one string (in certain cases to two strings). By that time it had become clear that the categories represent modes, in the sense of scalar constructs. The Key-Number Table, however, seemed to imply that the categories were intervals; these two implications were reconciled and correlated by various explanations — today already in controversy. Further studies, until 1969–1970, were undertaken especially by Duchesne-Guillemin, and also by Wilhelm Stauder (1967, 1970) and Hans Martin Kümmel (1970). These publications mark the end of a period, for reasons that I shall explain presently. Meanwhile, the readings of the texts as such were also improved: the process can be observed most instructively through Kilmer's survey of 1971.

These four texts are all that we have until now from Mesopotamia itself. More precisely: four texts that have been recognized as "theory texts" (see below), have been brought to the attention of musicologists, and are available through publications that included a transcription as well as a hand-drawn facsimile ("autograph") and sometimes a photograph of the tablet. Since the vocabulary of the theory has been identified, at least in part, more texts of this kind can surely be expected. A fifth text is already being prepared for publication by Kilmer. But the discoveries will continue to come singly and slowly. The theory of music was a part of higher education in Mesopotamia. Yet, as in all other cultures, it was not a core subject in the curriculum: not every scribe would — or indeed could — be trained as a *musicus*. An avalanche of texts cannot be expected even under the best of circumstances. However, the circumstances themselves have at least improved. The incessant sifting of the huge museum tablet collections, which now come to several hundreds of thousands of specimens (many of them fragmentary), has always had to be governed by known research priorities. Nowadays, a text about music turns on a "red light"; this would not have happened

prior to approximately 1965. Indeed, I have been told that the Key-Number Table CBS 10996 had already been examined and rejected during the preparation of Neugebauer and Sachs' *Mathematical Cuneiform Texts* (1945; note: Abraham Joseph Sachs, not Curt). What still lies below the ground cannot be estimated — only hoped for.

At this point it becomes necessary to define what kind of document should be considered as a theory text, but before that, we must agree on a minimal definition of a “theory of music” (the regress stops here — without a definition of “theory” and “music”), I would say that in all cases there must be a highly systemic concept in which (a) abstracted pitch-values are the nuclear entities; (b) further entities, and relationships between them, are postulated at and between several levels, the cardinal relationships being pitch: pitch, scale: pitch and scale: scale; and (c) in at least one domain of musical performance, the performance constructs (“the music”) are being related to (a) and (b), and thus also to each other with respect to this system. The definition thus excludes the two other systems that constrain performance — the technological and the ideological. These two can be seen, each in its own way, as a “science of doing.” A theory of music, as defined here, is no doubt a “doing of science.”

A theory text, then, would have to contain terms that are used in the theory. But this is not enough. The statement must also be in itself systemic: it must present at least two entities and one relationship between them, as conceived by the theory. The Key-Number Table and the Procedure Text do so very obviously. In the Song Catalogue (KAR 158), the systemic sequence of the classification is not obvious by itself, but is known to be so once we have the two other texts. The listing of the names of nine strings in their ordinal sequence in the lexical fragment U.3011 is systemic because the sequence is ordinal, and (as we shall see) the scalar points of various modes are mapped on it. Kilmer assembled a rich assortment of Sumerian and Akkadian citations in her studies of 1965 and 1971, but these come from statements that are not theory texts (at least those that I have checked so far). Here it must be mentioned that the probability of finding texts of the treatise type is almost nil. At the most, a didactic-discursive or speculative-discursive text or passage could perhaps be expected in the Seleucid period, in some acculturative context. The Mesopotamian scribal tradition communicates even the “doing of science” only in the form of ready-made lists, tables and exercises (further on this, see below, p. 30). Musicologists must make an adjustment in their conceptions here, and this is not easy.

The nontheoretical texts are nevertheless of importance for our work on the theory and its texts. What lexical support they may give to the theory texts is a matter to be handled with caution: it is the theory texts that can explain what happens to the terms in other texts, not vice versa. But a nontheory text may bear

witness to the time and place of its composition, within a more-closely circumscribed range than the lexica and tables and exercises; this may help to throw some light on the historical development of the theory.

At present, the theory and its texts exist for us almost outside time and place. Such a condition is as intolerable here as it would be for a collection of artifacts. The texts are published with assignments to certain historical periods, mostly by graphic and linguistic criteria. But these date the specimen, i.e., the particular tablet, and not its content. The “scribal-religious complex” of Mesopotamian culture to which these texts belong is founded on continuous copying. If there is no evidence to the contrary, a tablet could be considered a copy. The archaeological data (often unsatisfactory when it comes to the older museum collections — another problem!) and the scribal and other characteristics of the tablet yield only the crudest *terminus ante quem*. In the present study, I shall not try to solve the chronological problem, but the little information that is available will be used.

The central problem has been, and remains the small amount of evidence on which all the reconstructions of the theory have hitherto been based. A pessimist might well conclude that the devoted efforts invested in the task by Assyriologists and musicologists have been in vain. The situation is partly analogous to the decipherment of an unknown language and/or script. And here the experience of the archaeological and military code breakers has yielded some cautionary insights, which may at least moderate an undue optimism. One quotation from a work on this subject will suffice here; a few others from the same author will help us later on:

For determining any particular [i.e., specific] linguistic information, of course, larger amounts of text give us more reliable statistics. Anyone who claims to have deciphered a script for which only 241 signs of non-alphabetic text are known must expect his genius to go unrecognized until more texts turn up. *Not only is there not enough statistical information for him to prove his claim, but by the same token there is not enough for anyone else to disprove it.* [emphasis mine B.B.] (Barber 1974: 19)

In our case, the situation is not quite as hopeless as a purely statistical assessment would imply. Because of the highly systemic character of the theoretical construct, and the formulaic style of the texts, we are able to carry out such consistency checks for every “deciphering” hypothesis as would not be feasible for a similarly limited corpus of texts of another kind. Certain reservations do remain however, and one of the purposes of this study is to define these more clearly.

Among those who have followed the publications — and it is to them that I here mainly address myself — the impression may prevail that the Mesopotamian theory of music is now satisfactorily understood. In fact, however, there is no

true consensus, similar to that which comes about when a certain decipherment hypothesis for a recently discovered script is perceived in current use (ongoing refinements notwithstanding). The situation at present is somewhat unusual, and this has happened not only because no more Mesopotamian texts were added to the corpus after 1968, but also because at that time there occurred what I can only term a “cursed blessing.”

1968/9 was an *annus mirabilis* for our subject, with events treading on each other’s heels: the publication of the fourth Mesopotamian theory text (Gurney 1968; Wulstan 1968); the full publication of the notations from Ugarit, not yet recognized as such (Laroche 1968); and Hans G. Güterbock’s recognition of the transmogrified Mesopotamian terms and of these documents as notations (first noticed by Kümmel 1970: 262–263, followed by Güterbock 1970). Some of the Ugarit notations had indeed already been published by Laroche in 1955, but at that time, and until 1965, a correct identification was simply not possible. The scholars who had been working on the Mesopotamian texts now rushed to the decipherment of these new and truly sensational finds. A new wave of publications soon arose, with about eight different musical transcriptions, published or communicated in scholarly meetings, vying for approval. Each of the proponents brought to the task his own current theory-of-Mesopotamian-music, now combined his own theory-of-the-Ugarit-notation. Some of the scholars published more than one attempt, with changed premises. In my opinion, at least, the problem has not yet been solved. In Part Two, I shall discuss the Ugarit notation, but only in order to suggest another approach, which may lead to a more probable solution. Whatever the outcome may be, one conclusion is patent from the literature: the lure of Ugarit became so overwhelming after 1970, that no one thought it necessary to go back and check whether all was indeed truly well in Mesopotamia. Crocker did take up Procedure Text U.7/80 again, together with the adjacent fragmentary listings of terms, but his study was published at the end of 1978 and only offered certain modification of the basic consensus.

In what follows, I shall try to carry out a renewed examination of the Mesopotamian texts, and then explore some related matters, including the Ugarit notations. Since a reexamination should consider the sources and not the commentaries, I shall not take issue at every point with what others have said about it. Moreover, a running discussion is only necessary, and possible at all, if one accepts the basic hypothesis but wishes to improve the deductive superstructure; this is not the case here. A few points will have to be discussed along the way, but these are relegated (with one exception) to Appendix A, as excursions. Our struggle is not with each other, but with the material and with a challenge that has no precedent in the history of musicology. I know that I stand indebted to all

those who have worked on the subject, even where I may disagree with some of their conclusions.

In 1977 I had several conversations with Anne Kilmer, during her stay in Jerusalem, and I am grateful to her for giving me of her time and knowledge. I am also obliged to Aaron Shaffer who, as Professor of Assyriology at the Hebrew University of Jerusalem, helped his musicologist neighbor to carry out her “Burden of Babylon.” Over and above the tendering of advice on certain points, these meetings also helped me to see more clearly what difficulties must be surmounted when a new bidisciplinary field comes into existence. A minor but not unimportant fact is that the latecomer who has kept back from the first stages of the fray has the unfair benefit of hindsight.

Here I shall mention only one of the problems of our bidisciplinary situation, which has already caused some trouble. This is the two-edged sword of traditional philology. The Assyriologist must present the source document with a philological apparatus — the richer the better. But this will very likely generate misdirections for both partners in the enterprise. Verbal connotations and etymologies may not be taken as guides, nor serve as proof, when searching for the functional meaning of a term. And this applies most strongly when the term, or set of terms, is a “professional” one. After the terms have been explained securely through procedures that are not dependent on the lexical element (cf. “Symphony”!), that element can be taken up as well, but “internal analysis comes before external comparisons” (Barber 1974: 323). Our own task is not fully analogous to the decipherment of unknown scripts, but it is sufficiently similar in principle — especially to the decipherment of scripts of the nonalphabetic kind. What happened there proves that the rule of “analysis before comparison” cannot be circumvented. In musicology, some sharp words on this subject have already been said by Husmann (1961: 69). At certain stages in my own research I actually substituted symbols for the Akkadian terms, so as to keep the verbal element from intruding into the structural investigation: $S_1 \dots S_n$ for the strings, and $M_A \dots M_N$ for the modes. These symbols will not be used here often, except in a few places where they can help to make the reasoning more clear.

Within musicology itself, the newly discovered evidence seems indeed to be “à l’aube de la théorie musicale” (thus the apt title of Duchesne-Guillemin’s 1966 paper), and the implications began to be explored almost from the first. But everything depends on a correct understanding of the texts. The four texts are obviously concerned with certain parts of what we would nowadays classify as practical or elementary theory. Their aim is “the proper division of musical space” (Henderson 1957: 340–341, where the phrase is used in the sense of Greek theory at its most mature stage). The question is how this aim was conceived of here and how we can come to understand it.

For us, to understand what these texts say means to translate them correctly into our own musical language, more precisely: to map their system of musical concepts onto our own standard one. We may not map our own system or any other, such as the Greek, onto theirs. The first way is that “understanding” that we are trying to achieve; the reverse way generates fallacies. This also means that we must recognize which points or areas on our own map have no corresponding elements in the other system. Barber, in his book on archaeological decipherment, states the same principles in different terms (Barber 1974: 15–16). He also emphasizes, as he must, the checking of the decipherment hypothesis, which is equally relevant here. To quote:

It is then necessary to test for the empirical validity of each hypothesis by its consistency throughout the data. If the hypothesis is a structural one, formed on theoretical grounds, this will be a matter of testing all the relevant data for agreement with the hypothesis. (Barber 1974: 195; see also 33)

For us this statement is more provocative than the simple methodological precept that its author meant it to be. Our subject is itself “a structural one, formed on theoretical grounds,” and this means that we have to recognize and evade a logical trap that would not exist in the decipherment of a script or the elucidation of a language. If it is true that the Mesopotamian theory is anchored to the heptamodal-diatonic group (for a definition, see §1.23), then several alternative deductions are equally consistent with the relationships obtaining within this group, and, thus, each of them will test out as consistent with the data on the first round of checks! It is therefore necessary to devise such further checks as will eliminate this choice of possibilities, and leave only that probable one that represents what the creators of this particular “incarnation” of the system intended. My reexamination of the Mesopotamian texts, which forms the main part of this study, is based on this approach.

Part One: The Mesopotamian System

And their appearance and their work was as it were a wheel within a wheel (The first vision of Ezekiel in the land of the Chaldeans by the river Chebar; Ezek. 1:16)

The inventory of the texts and their subsequent presentation are based on the latest published information, as extracted mainly from Kilmer's survey of 1971. Since that survey was meant to describe the history of the discoveries and studies, and also contains many revisions and addenda to its original 1968 presentation, later readings and translations appear there both in the discourse and in the footnotes. Here I shall use the net result, without recapitulations.

Even though there are still so few texts, it seems to me that calling them by their Assyriological *sigla* is already somewhat inconvenient. It will become more inconvenient as the texts increase, and extremely so when copies are found. One presumed copy has already been cited for no. 3 in the inventory. If a scheme for working *sigla* can be agreed upon now, we shall save ourselves trouble later on. I have made up such a scheme, and shall use these *sigla* in the discussion. I shall also use standardized names for the texts, such as "Song Catalogue" alternately with its *siglum* C-Md. If the text has been identified as part of a standard "book" ("series" in Assyriological parlance), the name will generally be taken over here as well. An explanation of the scheme for making up the working *sigla* is given in Appendix B.

In all that follows, I shall try to preserve a clear distinction between text and document. The term "text" will denote the content, while "document," or "tablet," or simply the Assyriological *siglum* (such as CBS 10996) will denote the particular specimen.

Inventory of the Texts

Of the four texts, three are seen to be sections or passages about music that occur within a context of wider scope. For the Procedure Text the context is as yet unknown, since the document is a fragment. The music section may in itself contain statements about different classes of musical constructs (strings only/strings-and-modes/modes only/terms for instruments, etc.). The distinct names and working *sigla* must be assigned to what may be termed the "units of concern," in effect, to paragraphs and not to "texts." This is not what we would do in the case of a medieval treatise — but then what we have here are not treatises. It is difficult to adjust to a culture in which scientific concerns are not communicated discursively (at least not in written form) but only in the form of tables, prescrip-

tions, sets of model problems and lists of terms (for discussions of this situation see, e.g., Neugebauer 1934: 202ff.). But the approach must be fitted to the sources, and there seems to be no other way but to regard the information as a collection of “modular packages.” The following inventory is still organized by documentary units, but in the future such a survey will at least need a parallel listing by units of concern.

Content: The list contains those texts that have been found in Mesopotamia proper and which contain statements that may be defined as theory texts. The notations from Ugarit will be surveyed in §2.1.

Language and script: If not specified otherwise, the language is Akkadian and the script is “mainstream” cuneiform.

Provenience: For most of the tablets, only the name of the locality is known, and not always with certainty. To place the theory of music in its social setting, it is necessary to know whether the document was found in a temple, a palace or a private house, and whether in the context of a school, an archive or a private library. It is also important to know with what other kinds of evidence — written and artifact — the tablet was associated. For the greater part of the tablets in the museums such information is not available.

Date: The dates given apply to the particular tablet, and not or not necessarily, to its contents. Again, since the excavation data are not sufficiently precise, the dates depend only on the characteristics of script, arrangement and tablet shape, and hence can be defined only by period. At present, none of the tablets have a colophon (the Ugarit tablets have colophons but no dating statements).

Order: The order in this list is alphabetical, as generated by the working *siglum* assigned to the “main text” of each document.

Working Sigla: Since the texts, i.e., the content units, are at present documentary *unica*, the working *siglum* is given in the short form, without the added numerical specification of the document.

Bibliography: In general, only the first publication of the text itself is listed here. For further information, see the studies mentioned in the Introduction above. Supplementary information can be found through Borger 1967–1975. The ongoing Assyriological bibliography is the “Keilschriftbibliographie” in the periodical *Orientalia*.² The ongoing musicological bibliography is RILM, abstracts of musical literature (Répertoire International de Littérature Musicale).

² Editor’s note: Today Bayer’s research would have benefitted from tools such as the *Cuneiform Digital Library Initiative* (www.cdli.ucla.edu) and the *Electronic Text Corpus of Sumerian Literature* (www.etcs.lorinst.ox.ac.uk). Her reference to *Orientalia* appears to be to the journal of the same name published by the Pontifical Biblical Institute in Rome.

1) C-Md Song Catalogue

KAR 158. From Assur (Berlin, Staatliche Museen, Vorderasiatische Abtlg.; VAT 10101). Middle Assyrian period, second half of fourteenth to end of tenth century BCE.

The document is an extensive list of songs, apparently both sacred and secular, in eight, not fully intact, columns (4 obverse, 4 reverse), of at least 55 lines each. Cols. i–iv: titles of series of liturgies to various divinities, with totals for each group. Cols. v–vii (as numbered now): titles (= *initia*) of other kinds of songs, similarly grouped and totaled. Col. viii (previously numbered as v): list of totals, apparently extracted from all the preceding groups. The texts to which this catalogue refers have not yet been identified in other sources (Shaffer, oral communication 1978). The groupings and totaling definitions are by diverse criteria, though often by language only (Sumerian/Akkadian). Some of the classifying terms may refer to musico-poetic genres. In two of the groups (two only, out of several dozens!) the classification is by mode terms.

Relevant sections: Cols. vii–viii. In col. vii, 1–5 *initia* of 23 songs are totaled in line 6 as 23 *irātu ša eširte*; followed by *initia* of 17 songs (lines 7–23), totaled in line 24 as 17 *irātu ša kitme*; followed by *initia only*, preserved only to line 55. Col. viii (the total–of–totals) provides two totals for *šīrtu* songs, in *embūbu* and *pītu*, respectively, in lines 14–15. After diverse totals by other criteria, the mode terms appear again in lines 45–52, seven totals for *irātu* songs in the order *išartu*, *kitmu*, *embūbu*, etc. to *qablītu*, with a grand total for this group. This section almost certainly refers to the songs listed in col. vii, although there only the first two mode groups are set off explicitly.

Publication: Ebeling 1919: no. 158, pp. 269–276, autograph (= facsimile drawing) only. Description: Ebeling 1922 (not available to me for the present study). Parallel study with selected transcriptions: Langdon 1921. Although the study of the entire text by the state-of-the-art Assyriological and musicological research is long overdue, Langdon’s interpretations of the musical or presumed musical terms have mostly been disproved in the meantime, most decisively so as regards the mode terms (which he assigned to instruments or etymologized). First correct recognition of the mode terms: Kilmer 1965.

2) K-MdSt Key-Number Table

CBS 10996. From Nippur (Philadelphia, University Museum). Neo-Babylonian period, first half of first millennium BCE.

The document is a table of key-numbers (also called coefficients) for diverse calculations, mostly economic ones. Obverse: only a small part of one column is preserved. Reverse: parts varying in lengths of cols. i–iii are preserved; tops and bottoms not preserved. First dated to the Kassite period, now revised to Neo-Babylonian, i.e., about a millennium later (Kilmer 1971: 132, confirmed orally 1977). The content of the list is generally standard, with only a few entries that were not known previously from similar texts. The “table of key-numbers for musical modes” is the only section of which the subject itself has not yet been found in similar lists.

Relevant section: Col. i (of the reverse); the 19 lines extant, numbered by estimate as 6' to 24'. Lines 6'–10': number pairs apposed to mode terms. In line 11' a new tabulation begins, in which each entry opens with a string-term pair, followed by the corresponding number pair and the mode term. Lines 21'–24' are increasingly fragmentary.

Publication: Kilmer 1960 (with another list, which has no music section), transcription, translation and brief study, and with a photograph of the tablet appended. Readings of the music-table terms and numbers have been partly revised since then (for survey, see Kilmer 1971).

3) L-St/L-Md or L-St nabnītu/L-Md nabnītu String List/Mode List

U.3011. From Ur (London, British Museum). Neo-Babylonian period, as above.

The text is part of a standard series: the bilingual (Sumerian vs. Akkadian) encyclopedic vocabulary *nabnītu* (“creation,” from its opening line). Its divisions are arranged by the parts of the body, from the head to the feet, with the appropriate activities and objects listed for each part. U.3011 represents the thirty-second chapter-tablet, hence its Assyriological designation *nabnītu* XXXII. The theme is “sinews” (information supplied by Aaron Shaffer). This provides the point of attachment for an entire chapter of terms from the domain of music, opening with the nomenclature of the paradigmatic set of nine strings. Then follows the nomenclature of the modes, fragmentary in U.3011 as is the rest of the chapter (see below Fig. 1). *nabnītu* XXXII is thus the earliest encyclopedia-lexicon of music known now, and probably the very first. The composition of *nabnītu* is assigned to the Middle Babylonian period, i.e., the second half of the second millennium BCE (for an illuminating description of the Mesopotamian literature-of-lists, see Oppenheim 1977: 244–249).

Kilmer (1965: 264, note 25) states that “a duplicate fragment is K.9922, cited in MSL 6, 119.” This is a fragment that links up with the reverse of U.3011 but does not duplicate it precisely, and hence cannot, in any case, help to complete col. i.

Relevant sections: Cols. i + ii, Sumerian (i) vs. Akkadian (ii). Lines 1–10: String List, Akkadian *qudmû* to *uḫrû*, and totaled by “nine strings.” Line 11ff.: Mode List, truncated by the diagonal break-off and no more than a textual fragment (for a discussion of this part, see here §1.7).

Publication: Kilmer 1965: 264ff., transcription, translation and study (contents already utilized in Kilmer 1960). Autograph published in 1974 by Gurney (1974: no. 126, Pl. LX).

4) P-MdSt/X-MdSt Procedure Text/Mode-String fragment

U.7/80. From Ur (London, British Museum). Old Babylonian period, second half of eighteenth to end of sixteenth century BCE.

The document is a fragment, with parts of two columns of text, both pertaining to music. It is unclear whether this is an obverse or reverse and what the full extent of the tablet was. Nineteen lines partially preserved, numbered provisionally as 1–19. In the right-hand column (Procedure Text) at least lines 0 and 20 can be restored by textual extrapolation.

Right column: two sets of “procedures” in which relationships between modes are defined by changes to be effected on one or two strings. After the first three (preserved) examples, there is a subscript (line 12), followed by two further examples that present a more complicated case. The text is formulaic, in the “if-then” form. Since there is a cyclic relationship between the elements, it is theoretically possible to extrapolate the first group upward and the second group downward until the cycle (of seven modes) has run its complete course in each group. However, there are reasons for assuming that the full cycle was not gone through (see discussion in §1.43).

Left column: lines 2–13. All truncated at their beginnings. Mode terms, and at least two string terms are legible. The sequence of terms is presumably systematic, given the nature of what is done in the right-hand column, but the contents do not seem to be an actual part of the Procedure Text. The latter is a fragmentary text, while this is a textual fragment. For its discussion, see §1.6.

Publication: Gurney 1968, autograph, transcription, translation and study, with supplement by Wulstan (1968). Gurney proposes two emendations in the Procedure Text, which have been accepted tacitly in all subsequent studies. Here the text will be taken as it appears in the tablet. For discussion, see §1.4. Crocker (1978) explores the textual fragment of the left-hand column.

The fragment K. 9922 has already been mentioned above in connection with item no. 3. An autograph was published by Meek (1920: 165; correct “obverse” there to “reverse”) and reference was made to it in MSL 6, 119. It is part of a lexical

list, again Sumerian vs. Akkadian, apparently related to *nabnītu* but not identical with it (information supplied by Aaron Shaffer). No analytical publication seems to have been undertaken as yet.

A further text (BM 65217) is being prepared for publication by Kilmer. As I have been informed by her, it raises considerable difficulties.

1.2 Some Methodological Considerations

1.21 Are the texts co-systemic?

Hitherto it has been assumed that the texts are co-systemic, i.e., predicated upon identical theoretical concepts. Some changes could be expected to occur in time and in different locations, but it was not assumed that these could amount to a full paradigmatic shift. Such a shift, or even switch, has been suspected — but not fully reconstructed — in the transfer of ancient Greek theory to medieval Europe. The Mesopotamian texts are spread over a considerable range in time and space, and one must at least pose the question whether a paradigmatic shift, or even shifts, could not have occurred along the way.

The Assyriological answers, at least, are largely reassuring. The four texts on which we depend at present are in the same script and in the same language — the most obvious sign of a cultural comity. For this period and area, at least, everything that we know about the Mesopotamian intellectual tradition — the “scribal-religious complex” — makes it reasonably certain that the paradigm has not shifted and that the formulations that we have belong to one “coherent and continuous stream.” (This expression is taken from Oppenheim 1977: 16; for the background, see there, especially p. 14ff. and Chapter 1). Within that mainstream, one can perhaps already glimpse some signs of development and change in time: the mainstream is also fed by a few tributaries on its way. In principle, though, the texts are sufficiently compatible to allow the kind of inquiry that has been carried out on them until now, and will also be carried out here.

There is, however, one exception, and that, I hold, is the notation found at Ugarit. To continue with the metaphor used just now, this is not a further station along the mainstream, after the entry of some new ethnic tributary. On the contrary, a new channel is here drawn from the mainstream, to wend its way elsewhere. The fact that there is a difference in language and ethnicity (Hurrians!) cannot be disregarded, in spite of the overt “Mesopotamization.” But this subject will be discussed in Part Two. For the material from Mesopotamia proper, the evidence from Ugarit will therefore be used only for what it can yield on the general chronological problem.

1.22 The order of investigation

The order in which our texts were discovered, or rather rediscovered, has had an obvious influence on their analysis. Situations of this kind are only natural at the early stage of research. An interpretation of the Key-Number Table will come first, with the help of the String List (Figure 1); this will be applied to the Song Catalogue (Figure 2) and the Procedure Text (Figure 3). It is now possible to look at the four texts in a more detached way and to plan the approach; then it becomes apparent that the Key-Number Table should be taken up only at the last stage, not the first.

The Key-Number Table is a table, nothing more. Being a Mesopotamian table it is even more bare than a European one, say a table of logarithms. Because of the nature of the subject, it is possible to flesh out the bones in several ways, which will all be “true.” The Procedure Text, however, assumes the table and goes beyond it. It therefore has more inbuilt constraints, in other words — more information. The text that has more information must be applied to the one that has less — not the other way around. For a theoretical discussion of the same problem in language decipherment, see Barber 1974: index, s.v. *level*.

We shall therefore take up the Procedure Text first, necessarily together with the String List and the Song Catalogue. The results will then be applied to the Key-Number table. That application will of course be valid only if the decision of what the terms signify has not in fact been read earlier out of the table and into the Procedure Text. Data from the table can therefore be used at the initial stage only if they do not cause our reasoning to bend itself into a circle.

In the table there are fourteen terms that we assume to stand for modes. Seven of these also appear in the Procedure Text and in the Song Catalogue. By the principle just argued, it follows that the seven other terms must be taken up at the very last stage.

1.23 Working Terminology

In the discussion we must use the terms of our own mainstream or school doctrine of music theory. Some of these are essential tools for the task, but they are also the most loaded ones, with a burden of diverse historical and musicological usages. The writer must decide, and the reader must be informed, in which sense such a term is used here. Neologisms may also become necessary. Therefore, in the following, I define my terms. Each definition serves as a premise for its successor(s).

- *scale*: any stock of pitch norms, arranged as an ordered sequence. Specific constraints are not implied.
- *diatonic*: used by extension, for the scalar series of the class Tone-Tone-Semitone-Tone-Tone-Tone-Semitone and the relationships obtained therein. Values of tuning and intonation are not implied.
- *mode*: used solely in the meaning of octave species. None of the other uses of the term (historical or musicological) are implied. If not explicitly stated otherwise, the octave species indicated is understood to be diatonic.
- *D-mode*, *E-mode* and similar: used solely to symbolize a diatonic octave species as a “white-key mapping.” Fixed pitch concerns, absolute or relative, are not implied.
- *H-mode*: used instead of a B-mode. Because of the ambiguity of B (si/si-be-mol). The b flat will be written as B^b.
- *Heptamodal-diatonic group*: denotes the cyclic group of seven diatonic modes. Notwithstanding definition (c) above, “diatonic” is included in the expression because of certain problems that will be raised in the discussion of the Key-Number Table.
- *para-mode*: denotes any scalar construct that does not belong to the heptamodal-diatonic group as defined above.
- “*Dorian*,” “*Phrygian*,” and similar: when in quotation marks, used solely as supplements to the “white-key” definitions of the heptamodal-diatonic group. Hence “Dorian” = E-mode, “Phrygian” = D-mode, etc. No other implications are assumed.

The matter of scalar direction (upward/downward) will be discussed in §1.42, and there it will be proved that the Mesopotamian reckoning is upward. No inquiry can be undertaken here whether the Greek reckoning was indeed downward. As used here, both the “Greek” analogue names and the letter names refer to the scale as reckoned upward.

It goes without saying (but must be said) that the medieval European use of the Greek terms is to be disregarded.

1.3 String List and Song Catalogue

These two texts furnish the starting information. Both documents are fairly intact in the areas concerned, and their reading presents no particular difficulties.

1.31 The String List

The list is the opening section of the thirty-second chapter tablet of the “encyclopedia” *nabnītu*. The composition *nabnītu* has been assigned an approximate *terminus post quem* in the Middle Babylonian period (second half of the second millennium BCE). As in practically all of the literature-of-lists, Sumerian equivalents are apposed to the Akkadian terms. Naturally, it must be asked whether the Sumerian terms can contribute to the operative understanding of the theory, which is our concern here. The answer seems to be negative, since we are trying to avoid going by verbal connotations. As I have been enlightened by Aaron Shaffer, the active part of the text is the Akkadian. Moreover, the Sumerian part cannot be considered as a “Sumerian source”: these terms are largely artificial ones and not real historical relics. Of course, there are often differences between the Sumerian and the Akkadian terms in the lists, but the relationship between the Akkadian and the Sumerian terminology in *nabnītu* must be examined by comparison with such phenomena in other lists, and this is a task to be undertaken on its own. As far as I have been able to judge from examining the material published in MSL, this confirms the decision to disregard the Sumerian side of the String List in *nabnītu*, as concerns the present task. We need only note that the four “back” strings are also “back” in the Sumerian definitions here. To avoid distraction, the Sumerian side is not even included in the presentation of the text; it can be found in the previous publications, such as Kilmer 1965 and 1971.

The text of the String List will be presented as established by Kilmer (1965: 264; also 1971: 133). Since our concern is functional and the reading does not seem to raise any particular problems, we can abandon the strict method of trans-“literation” and give the text in a straightforward transcription. The restorations are also so few and so obvious that there was no need to perpetuate them here by square brackets.

In the specimen, i.e., the tablet U.3011, the Akkadian term for the fourth string is written *A-ba-nu-[ú]*. The reason is not important to us here (for discussion, see Kilmer 1965 ad loc.) We shall use the standard form and transcription ^dEA *bānū*, so as to keep visible the special nature of this term (“god Ea the creator”). The choice of terms in the translation given here is partly my own.

Fig. 1: String List/Mode List (L-St, *nabnītu* XXXII//U.3011).

col.	line		
i	1	qudmûm	fore (string)
		šamūšum	next (string)
		šalšu qatnu	third (string)
		ᵈEa-bānû	god Ea the creator (string)
	5	ḥamšu	fifth (string)
		ribi uḥrîm	fourth of the rear (string)
		šalši uḥrîm	third of the rear (string)
		šini uḥrîm	second of the rear (string)
	10	uḥrûm	rear (string)
		9 pitnû	nine strings

[followed by
Mode List]

That the nine entries are music strings is confirmed by the context. The Sumerian column prefixes *sa* to each string term. In the literary sources there is much and varied evidence on *pitnu* = music string, although not always clear in detail (see Kilmer 1965; supplement in Kilmer 1971: 133, note 16).

The “first forward then backward” sequence of the string terms might call up a surmise that the accordatura is nonscalar, and such of course are known to exist. However, the Key-Number Table features a parallel double nomenclature: the strings are first named as in L-St, but then indicated by numbers that together yield a straightforward ordinal sequence (there from 1 to 7 only, but that *šini uḥrîm* = 8 and *uḥrûm* = 9 can easily be proved). In the case of the four “back strings,” then, the names of the components do not accord with their function in the system as we have it. They are an intrusion from another domain, and here this most probably means an earlier stage. The string paradigm as we have it here, in the theory as it stands, is scalar: the set will be tuned to a scale or to various scales from *qudmû* to *uḥrû* (in the discussion we use the basic forms of the words).

The third string is called *šalšu qatnu*.³ Hence, it has been assumed that this was somehow equivalent to what Western theory terms a “minor” third, and that the set of strings was therefore already predicated on a specific scale, which would thus be *the* basic Mesopotamian scale. The nine string terms would thus be the stations of a little *systema*, analogous to the Greek method. I am unable to accept this interpretation, for a number of reasons that are set out in Appendix

3 Editor’s note: Literally, *šalšu qatnu* means “third, thin.”

A, Excursus 2. In all that follows the assumption will be that the string terms are neutral, i.e., purely ordinal.

If various tunings can be projected on the set of strings, and these are to be scalar, what is the direction? Does *qudmû* have the highest pitch or the lowest? The problem is twofold. For obvious mechanical reasons, the *qudmû* string can only be one or the other: a treble cannot be turned into a bass. Still, it might be possible that in the modal systematization one group of modes will be reckoned to run “upward” and the other, “downward.” The Key-Number Table has been interpreted to demonstrate that this is the case here, but another interpretation of the reversal of the numbers in that table is also possible. Iconographic evidence seems to favor *qudmû* = lowest, as the longest string that is “in front,” with *uhrû* being “in back,” since it is the closest to the player’s chest. But here we also involve a verbal connotation, and, as agreed, this may not be used as a guide or as a proof. I hope that both questions, on the relative pitch of *qudmû* and on the scalar direction, will be resolved by the purely structural analysis of the data as we proceed.

Last, why nine strings? This will also find its explanation, through the Procedure Text. The Key-Number Table, it will be remembered, uses only seven strings, but there will be a proposal, when we come to this text, to account for the limitation — which is peculiar to the “mentality” of that table.

1.32 The Song Catalogue

Our text is part of a catalogue of hymns and songs (see general description in the inventory above, no. 1 = C-Md). The terms that are now known to stand for modes are used to characterize certain groups of songs, but these are the minority in the catalogue: two such groups, against dozens that are totaled and thus classified by quite different criteria. A new and thorough survey of the entire catalogue is certainly needed. At present, there is only Langdon 1921, which can nowadays at best serve for general orientation, and the nearly unobtainable report by Ebeling (1922).

Like the String List, the Song Catalogue (Figure 2) is given here in transcription and not in transliteration. The source is Kilmer 1971: 138 and 147, revised by the remarks and footnotes there. For the restoration of the totaling formula in line 52, see Kilmer 1965: 268, note 59.

Fig. 2: Song Catalogue (C-Md//KAR 158).

		Glossary	
		Akkadî/ Akkadî KI	in Akkadian
		irātu	breast songs/poems, love songs
		KIMIN	ditto
		napḫar	total
		ša	of/pertaining to
		šīṭru	song category, meaning of the word in this context unknown (<i>všṭr</i> = write)
col.	line		
viii	14	13 [?] šīṭru ša ebbūbe Akkadî ⁽¹⁾	
	15	2 KIMIN ša pīte Akkadî	
	45	23 irātu ša eširte Akkadî KI ⁽²⁾	
		17 irātu ša kitme ⁽³⁾	
		24 irātu ša ebbūbe ⁽⁴⁾	
		4 irātu ša pīte	
] irātu ša nīd qabli	
	50] irātu ša nīš GAB.RI	
] irātu ša qablīte	
		[napḫar x irātu Akkad]ū	
<p>(1) Respective entries in the body of the catalogue cannot be located.</p> <p>(2) Carried from vii, 1–6: initia of 23 songs and total <i>napḫar</i> 23 <i>irātu ša eširte</i>.</p> <p>(3) Carried from vii, 7–24: initia of 17 songs and total <i>napḫar</i> 17 <i>irātu ša kitme</i>.</p> <p>(4) vii, 25 to extant 55: initia only, not interrupted by expected rest of mode-totals.</p>			

In the literature, the standardized forms of the terms have already come to be used, irrespective of the local peculiarities of each document. They will be used here as well (except when presenting a source), thus: *išartu*, *kitmu*, *embūbu*, *pītu*, *nīd qabli*, *nīš GAB.RI*, *qablītu*. For reasons that will be explained later on, the transliteration *nīš GAB.RI* is kept in this form.

In lines 14–15 there is an incomplete group of *embūbu* and *pītu* only (the context is intact). The number in line 14 is unclear, being either 3 or 13 (according to Kilmer 1971: 147). In Ebeling's autograph (1919: 271) the number is seen to be at a break, but seems an intact 3. Since the next line says "5 *pāru Akkadū*," 5 *pāru* in Akkadian, it may be the interim total for these two entries. Lines 17 and 18 list 1 + 10 totals, summed up in line 19 as "11 *zamar šēri*" (on the very complicated problem of *šēru*, see Kilmer 1971: 143–144, note 62). The group of seven mode categories that appears in lines 45–51 is, as we know, a complete and canonical set. A complete collection for only two modes is highly improbable. Moreover, the

context of these two isolated entries in lines 14–15 gives the impression of a very mixed lot. The section in the main catalogue, from which these entries have been carried, cannot be located, because the tablet is not sufficiently preserved. We notice, however, that the order *embūbu-pītu* is the same as in the complete set of line 45ff. Further on I shall offer a hypothesis about the “Sitz im Katalog” of the two mode groups, the incomplete and the complete one.

A complete set of seven mode categories is presented in lines 45–51, clearly distinct from the preceding entries and closed off by a total. Unfortunately, the three last totals and the overall total of the set have not been preserved: the number comes to 68 + ? and there is no support for making any estimate. At most, one might venture to continue the sequence on the same pattern as the first four entries: so-and-so much (around 20?) for *nīd qabli*, but less than this for *nīš GAB. RI*. The pattern will not help for *qablītu*, the last. We notice that, whatever the numbers may be for the incomplete group of lines 14–15, there, too, *embūbu* has more than *pītu*, just as in the complete group. This proposal is not as fanciful as it may seem at first glance. To anticipate: the Procedure Text will yield the conclusion that the modes that have the larger number of songs here have another common characteristic — they are the authentics, and those with the smaller number are their *plagals* (*qablītu* is the “maverick” H-mode = mixolydian). But we do not know why there “must” be more songs in the authentic mode than in its plagal.

As we know, the seven categories used here also appear in the Procedure Text and in the Key-Number Table. Here, in C-Md, they classify songs. In P-MdSt they are constructs that form a cyclic group, obtained by the systematic modification of the state of one or two strings. All the historical and ethnomusical data point to the conclusion that these terms must therefore stand for scales. The Procedure Text supplies enough information to peg down the assumption that these scales are octave species and that they are diatonic. Since the Catalogue is not an exercise but rather a classification of actual songs, we might well ask whether here, at least, the terms stand for something more than scalarity. But there is no documentary evidence for this, and there can thus be no hypothesis.

We may assume that the sequence in which the seven terms appear in the Catalogue is not haphazard. The incomplete group in lines 14–15 has *pītu* after *embūbu*, just as in the complete group in lines 45–51. This is no proof in itself, but for the musicologist it would be surprising if the order were haphazard. We also find the same order in the second part of the Procedure Text, and its reverse in the first part. For the heptamodal-diatonic group three orderings are possible: two are cyclic — by fourths or by fifths — and the third is by scalar steps. The Procedure Text “works” most simply if we assume that its ordering is cyclic. When we come to the Key-Number Table, we shall see that it comports very well with

the assumption that there the ordering is scalar, because the table has a pedagogical purpose of its own. In the earlier publications our Assyriological colleagues always considered the differences of sequence as something very troubling, but this is actually a nonproblem. It must be said that the musicologists have also raised some nonproblems on which the Assyriologists found it difficult to allay their colleagues' fears.

1.4 The Procedure Text

1.41 Analogous Textual Formats

Our text is a highly formulaic sequence of statements in the “if-then” form. Five of these statements have survived, fully or in part. Each opens with the term *šumma* (“in case/given that”), which signals that what follows is a protasis (the “if” clause). There are always four lines to the statement, before *šumma* appears again to open a new protasis. Somewhere within these four lines the protasis ends and the apodosis (the “then” clause) begins. But this apodosis is not introduced by a signaling term of its own.

In the first section of the text (lines [0] to 11), the lack of such a signal is not too important, because the statement is relatively simple. The second section after the subscript 12 [x?] NU.SU [y] is another matter. It is obviously a more complicated case, one step further along the way to proficiency. Some additional components appear in the second line of each statement, and here the lack of an explicit apodosis signal causes trouble, because it leaves the interpretation open to several alternatives. This becomes especially obvious if we take the text as it stands, without the two emendations that were introduced when the tablet was first published (for discussion, see §1.42). The precise role of several elements that do appear in the text is also obscured, because of the highly formulaic shaping of the statements. To solve this problem, we must draw upon texts of a similar form, for which there is already a more secure interpretation of the components and their relationships. Such texts are indeed plentiful, and the opening term *šumma* is their identifying mark and symbol. They are the analogues and the congeners of our music text, whose formal pattern is not at all an original creation, devised for the domain of musical theory.

It can be said — and now with more certainty than ever before — that no formulation of a musico-theoretical statement is ever “original” in its pattern. The theoretical approach to music is not among the first concerns of Man the Classifier. When music comes to be taken over into the province of the intellect, certain molds of inquiry and discourse are already at hand, and the new lore is cast into

one of these molds. When a new musical theory is promulgated, it will also be cast in a mold taken from the intellectual environment. It is, of course, significant to note which mold the musical theorist will chose in each period — be he Franco of Cologne or Allen Forte of Yale. The recognition of the mold gives the phenomenon its “Sitz im Leben.” Moreover, especially as we go further back in time, these analogues and congeners can help solve textual cruxes that cannot be solved by applying only internal criteria.

In the Mesopotamian record we find the pattern “if-then” as a standard vehicle for expressing normative decisions, chiefly in the domain of divination, medicine and law. The key term that signals the protasis is *šumma*, in the sense of “given the initial situation that...,” just as in our text; however, the situation is not always a simple one, and the full statement can be rather involved.

Struggles with the interpretation of such complex statements are evident in the study of every domain of what I call the “province of *šumma*.” The most detailed and most acerbic discussions appear in the literature on Mesopotamian law. The jurist will not be satisfied when the Assyriologist has reached the limit of philological certainty and must pepper his translation with bracketed additions, alternatives and question marks. The original mechanism must be made to work again, and open alternatives will not do. The same goes for the mathematician, and for the musicologist. Thus, the complex *šumma* statements, in law and elsewhere, enable us to recognize that we have a problem in certain parts of our text or texts, and the kind of a problem it is.

(a) Divination

Omen texts form the largest part of the Mesopotamian “stream of tradition.” To quote again from our guide:

Such omen collections consist of endless, systematically arranged, one-line entries, each describing a specific act, a well-defined event, the behaviour or feature of an animal, a specific part of its body, or that of a plant or of a human being, or the movements of the stars, the moon and the sun, atmospheric events, and other observable details, of unbelievable variety. Each case is provided with a prediction that refers to the welfare of the country or to that of the individual with respect to whom — such is the basic assumption — the event happened, if it was not purposely provoked to obtain information about the future. (Oppenheim 1977: 16; see also his chapter on divination, pp. 206–227 and its bibliography)

There is a strong probability that the *šumma* formulation was first established in the omen texts, and transferred from there to other domains. “Given that (*šumma*) this-and-that has occurred, [it follows that] such-and-such will occur.” Here reasoning was first formulated, in both senses of the word. There is an early stage, the

collection and accumulation of occurrences considered ominous, during which the pattern becomes established, and the division into subject groups that will later become the standard series. At this stage, this is still a “science of doing,” alias folklore. After all, formula is also the most important tool of oral tradition. But then a process of theorizing sets in: the lists of *omina* become omen tables, exercises in extrapolatory, combinatory and permutatory virtuosity, to leave no possibility unregistered. For the apodoses, the predicated results, there is a kind of stabilization (“stock apodoses”), and the parallel collections of apotropaic and expiatory rituals also do not grow to infinite complexity. The theoretical expansion takes place in the protases “given that this-and-that has happened” becomes, in effect, “given that this-and-this exists.” Formula has become a tool of creation, not only a tool of preservation and transmission, and with this, the fateful step has been taken, from a “science of doing” to the “doing of science.” Let us say, “doing science — Act One.” The view of divination as “la discipline reine, et probablement mère de toutes les autres,” (Nougayrol 1966: 10) is no doubt justified.

The rise of theoretical divination seems to have occurred sometime during the Old Babylonian period (very roughly between 1800–1600 BCE, with Hammurapi, ruling ca. 1792–1750, as the focal figure). This is also the period to which our musical Procedure Text is assigned. Its document (U.7/80) was a lucky find: if the text had been discovered in a later copy we could not have learned from it what can be learned now. To whatever domain of intellectual achievement one turns, the Old Babylonian period is defined again and again as a kind of watershed situation, or creative spurt. The first stage of mathematical achievement in its proper sense also occurred at this time.

A survey of one particular branch of divination will yield a further insight into our own subject. It concerns teratological omens — the implications of extraordinary births, human and animal, that came to be collected and developed in the series *šumma izbu* (“Given that a newborn animal”).

This change [to the theoretical approach] had two aspects. First, the existing omens were systematically ordered. The omens were arranged by subject matter in a sequence based on the protasis and running, as far as possible, from the head to the feet. Secondly, new omens were added in an attempt to make the series all-inclusive. The series consistently gives one omen derived from the left side of the body, followed by an identical omen from the right side of the body, followed by the identical omen from both sides of the body. This consistency and the general all-inclusiveness of the series virtually guarantees us that the majority of the omens in the standardised series were systematically added rather than observed. (Leichty 1966: 132)

We may substitute for “omens” the musical constructs defined by a theoretical system, whether expressed through a Procedure Text or, even more patently, in

the perfect “wheel within a wheel” of the Key-Number Table. The problem of musical theory and musical reality arises, as it must, as soon as there is a theory, in the sense of a “doing of science.”

In the above quotation, the entities of a theoretical system were defined as being of two kinds: observed and contrived. It would be better to divide them into three kinds: the observed (real, though pared down by formalization), the contrived (unreal) and the semicontrived (*realia* distorted by contrivance to fit the system). The three kinds can, of course, be discerned only if the *realia* have also survived. For the musical reality of ancient Mesopotamia (or Greece, or India or China) this does not seem possible. We can be sure that the “perfect” theory comprises all three kinds — but what is which? Moreover, in the arts it may happen that a branch of practice comes to obey theory, so that the initially contrived becomes the real.

(b) Medicine

The domain of medicine uses the pattern of the omen texts for its pairing of illness and cure. There is a large area of activities in which divination and medicine operate together, and the picture is rather complex. Particularly interesting for us is what Oppenheim calls “theoretical medicine,” in which the building of tabulations for the so-called prognostic omnia shows the same unrestricted growth as the systematic omen tables (see Oppenheim 1977: 289ff.). In “practical medicine” the cure will also have a mixture of spiritual and physical technology, as expected. It is the textual form that interests us here. For my example I chose the case and cure of the common hangover. Conjunctions have not been added, but I have marked the boundaries of the clauses.

šumma amēlu šikara ištīma (in case that a man, having drunk strong drink) / his head pinches him / his words he forgets / in his speech he slurs them / his understanding does not hold / that man’s eyes *izzaza* (are fixed = glare?) // *ana balātišu* (for his well-being) / herb A, herb B...[11 herbs listed] macerated / in oil and strong drink / before the divine Gula (goddess of healing) / in the morning before the divine sun shines / before anyone has kissed him (in salutation) / *ištattima* (he will have imbibed) // *iballuṭ* (he shall be well). (Küchler 1904: 32–33, there with German translation)

In the medical procedure texts the apodosis is introduced explicitly, by the stock expression *ana balātišu*, which is then echoed in the equally standard *iballuṭ* at the end. Such a clear pivoting point in the middle is not found in all statements on the *šumma* pattern, and indeed it does not appear — regrettably — in our musical Procedure Text. In the medical text we note the distinction between the initial *ištīma* (preterite = having previously drunk) and the final *ištattima* (present

= having now drunk). Both are given the enclitic particle *-ma*, which articulates the structure in a particularly subtle way. The more complex the statement, the more such devices are needed to assure that the mechanism will indeed work as required. However, if the statement is not preserved intact and the subject matter is not self-explanatory, the complexity of the mechanism will make it more difficult to find out what was intended. This is the problem that we face in our musical Procedure Text.

(c) Law

The domain of law takes over the *šumma* pattern, to pair what must be paired here: crime and punishment or other situations of imbalance and restitution. In the Neo-Babylonian period, by the way, the *šumma* opening is discarded; if more musical texts from this period are found, it will be interesting to see whether this has also happened there. It is in the study of Mesopotamian law that the workings of complex *šumma* statements have been most thoroughly investigated, both by historians of law and by the grammarians. The legal statements also have the most involuted realizations of the pattern — as legal statements do until the present day. Their interpretation often raises problems that are very similar to those of our Procedure Text. We shall therefore apply them directly to our task. Metaphorically speaking, at this point our patient has been “prepped” and is already on the operating table.

We now proceed in two stages. In the first stage (§ 1.42) the text will be established as it stands, with only those restitutions that may be accepted as self-evident. From this we shall already be able to find out the scalar identities of the mode terms. In the second stage (§ 1.43) we shall try to complete the more problematical missing parts of the clauses, with the help of what can be learned from a selection of legal statements.

1.42 The text and the modal values

From the String List comes the nomenclature of a set of nine strings, in a “first forward then backward” order. A cursory check of the Procedure Text and the Key-Number Table assures us that the strings are nevertheless to be tuned in a straightforward scalar sequence. There is, however, no overt indication whether the sequence is low-to-high or high-to-low — whether *qudmû* is the lowest or the highest in pitch. We assume that only one scalar direction obtains throughout the theory (as the most economical hypothesis). The Procedure Text must be made to

yield one of the two alternatives, *qudmû*-high or *qudmû*-low, and also to eliminate the other beyond doubt.

The Procedure Text assumes the full gamut of nine strings, but the Key-Number Table only uses strings 1 to 7. Now, an overview of the music texts in themselves and against their background (including the mathematical texts of all kinds) gives a very strong impression of a striving for minimal redundancy. How efficiently this was achieved for the basic computational tools — the multiplication table and the tables of reciprocals — is described *con gusto* by Neugebauer (1969: 30–34). If the Procedure Text has the full gamut, there will probably have been a need for this. We should thus not only ask why the Key-Number Table makes do with “less,” but why the Procedure Text has “more,” and why nine.

From the Song Catalogue comes a group of seven terms that classify songs. A superficial reading of the Procedure Text offers the hypothesis that the two texts, taken together, are consistent with the assumption that the terms stand for the heptamodal-diatonic group. But since we have also assumed that the string terms are neutral, there are still several alternatives for the assignment of a specific value to each of the seven mode terms. The Procedure Text must not only be made to yield a plausible set of values, but also to eliminate all other candidates. But first the text itself must be established.

For the text, I shall generally follow its first publication by Gurney (1968). However, that “authorized version” also included two emendations, one of addition and one of substitution. I have come to conclude that these emendations are not necessary. The text will be presented here as found and the matter of the emendations will be discussed afterward. Gurney also reconstructed the missing endings for the clauses. I shall leave these out for the present and discuss the possibilities later. Completions that seem beyond doubt are included, but printed in smaller letters so that the reader will again see what portions of the text are actually preserved in this specimen. A translation will not be attempted at this first stage, because it depends on the outcome of the structural analysis. The glossary is also provisional. As in the two texts presented previously, transcription is used instead of syllabic transliteration.

Fig. 3: Procedure Text (P–MdSt//U.7/80).

Glossary				
^{gis} ZĀ.MÍ	Sumerogram for noun <i>sammû</i> , an open-stringed instrument. Here probably used generically for the set of nine open strings. As the clauses are not sufficiently intact, the case-form of the word cannot be inferred.			
Šumma	“If”/“Given that” (initially)			
Talput	vb. <i>lapātu</i> : grip/encompass/play? 2. masc. preterite			
Tennīma	vb. <i>enûm</i> : change/modify. 2. masc. present with enclitic particle – <i>ma</i>			
Lā	“not”			
<i>zak</i> -[...]	vb. <i>zakû</i> : be pure/clear/free/absolved/perfect etc.			
<i>iz-za</i> -[...] [l.4 & 8]	vb. <i>zakû</i> or <i>izuzzum</i> : stand/be stable etc.			

Line		Section	Clause	Restored through
[0	šumma ^{gis} ZĀ.MÍ <i>pit</i> []]	a	C-Md
1.	embūbu[m]		b	
2.	šalša[m qatnam]	I	c	L-St
3.	embūbum izza[] / izā[]		d	l. 7 & 11
4.	šumma ^{gis} ZĀ.MÍ <i>embūb</i> []		a	Pattern
5.	kitmum		b	
6.	ribi uḫrīm	II	c	
7.	kitmum izza[] / izā[]		d	l. 11
8.	šumma ^{gis} ZĀ.MÍ <i>kitm</i> []		a	
9.	išartum la zak[]		b	
10.	šamušam u uḫriam	III	c	
11.	išartum izza[] / izā[]		d	
12.	[x [?]] NU.SU [y]	subscript		
13.	šumma ^{gis} ZĀ.MÍ <i>išart</i> []		a	
14.	qablītam talput		b	Unemended!
15.	šamušam u uḫriam tennīma	IV	c	l. 19
16.	^{gis} ZĀ.MÍ <i>kitmu</i> []		d	
17.	šumma ^{gis} ZĀ.MÍ <i>kitm</i> []		a	
18.	qablītam la zakūtam talput	V	b	Unemended!
19.	ribi uḫrīm tennīma		c	l. 6 & L-St
[20.	? ^{gis} ZĀ.MÍ <i>embūb</i> []]	d	C-Md & II here

Spacing between sections and dashed lines are added here.

The top and bottom of tablet were not preserved in the fragment.

(1) The b-clauses of sections IV and V

In the first publication of the text (Gurney 1968) two emendations were made. IV-b (line 14 was expanded to *qablītam <la zakūtam> talput*, analogous to V-b (line 18). To quote: “It is difficult to see how this can be anything but a mistake on the part of the scribe” (ibid.: 230–231; seconded by Kümmel 1970: 256, note 1). In line 18, however, the possible reading *qablītam* was emended to *išartam*. To quote: “The traces in line 18 would allow the reading [qá-ab-]i-ta-am instead of [i-šá]r-ta-am, but the latter is required here by the sense...” (loc. cit., adopted without comment by Kümmel). These emendations stem directly or indirectly from certain interpretations of the Key-Number Table. We may not apply such interpretations here, and at this point must somehow bring ourselves to act as if K-MdSt were still buried in the tell of Nippur.

The fragment is carefully written. Syntactic assonance might have caused a slip in line 14, if the scribe already had the next section ringing in his head. But the mistake assumed in line 18 is less likely: there is no phonetic assonance between the two words. Since texts of musical theory were not copied very frequently (otherwise we would already have a larger corpus and the subject would have been recognized much earlier), the probability of an ignorant and/or sloppy copyist is low. I find it very difficult to accept the co-occurrence of these two lapses in this copy and in a text of this kind. The proof would be if the text could be shown not to work at all, or with great difficulty, unless these two emendations are put in. I have tried to take the text as it stands and to follow where this may lead. As will be shown, the text works very well thus, and also does more than was expected of it until now.

(2) The c-clauses

In sections I–III the operational term is not preserved. Until now, Gurney’s reconstruction has been accepted — to have *tennīma* here, as in IV–V. But this raises several problems.

IV is the reverse process of III, and V, of II etc. If the same *tennīma* is indeed used here and there, it implies *b* in the first part and sharp in the second part (or vice versa). A pedagogical paradigm cannot be effective in this way (“modify, as the case requires, either by flat or by sharp”). Since *-ma* ends the clause, there could not have been something like “*modify* by tightening/sharpening,” and then “*modify* by loosening/flattening,” (or vice versa). Moreover, there is a crucial difference between the two, in the paradigmatic situation that we assume here — an open-stringed instrument, tuned to a diatonic scale, with no chromatic “spares.” One can sharpen any string temporarily and instantly by finger stopping it near the end with a slight pressure. (The tone quality will be slightly different from that of the free strings, but this will not be decisive under *all* musical circum-

stances, especially when the instrument does not give a solo performance but rather accompanies a singer/singers and/or is part of a mixed ensemble.) One can also sharpen the string permanently, through whatever mechanism is available. Flattening, however, *always* involves a mechanical readjustment. As every string player knows, you have a fair chance of getting close to the desired higher tone when you twist the peg upward, but if you have to flatten, the tension of the string will pull you downward beyond your target. If there is a modal metabole within the piece that is being played on the open-stringed instrument, this difference between sharpening and flattening becomes crucial. You can sharpen temporarily by finger pressure, but you cannot flatten temporarily.

Lastly, we know now that the terminology had separate terms for tightening (“up tuning”) and loosening (“down tuning”) the strings, as expected. These terms are discussed in Kilmer 1965: 263–264, supplemented ead. 1971: 139, note 43. The proposals are tightening *nasāḥu* (sum. gíd.i) or loosening *nē’u* (sum. tu.lu), with a synonym, *rabābu*. Remembering the Arab *rabbāba* (instrument and congeners in the domain of bow and bow-string), the “synonym” requires a question mark. The terms need a separate inquiry under musicological control. In any case, we may not etymologize at this stage. The meaning of *tenni-ma* in sections IV–V must be recovered by structural analysis. We may note, though, that the *tenni-ma* is not derived from *nē’um* (loosen) but from *enūm* (change, general), even if both verbs belong to the same semantic field.

For sections I–III the operative verb will be left unrestored at present. The only assumption is that if the second part is about sharpening the first part is about flattening, and vice versa.

(3) The d-clauses

It is still a moot point whether *iz-za...* in section III should be restored as *izzaz* (stands/is stable) or *izzaku* (is made pure/perfect). The first was proposed by Gurney (1968: 230) and the second by Kümmel (1970: 255, note 3). We may risk the assumption that the difference is not significant. Of course, as in European theory, what is imperfect is not necessarily stable.

In the second part (IV–V) the d-clauses are formulated in a different way. Gurney assumed that the d-clauses in the second part would end like the a-clauses in the first part, to make the presumed mirror relationship even stronger. Section V would thus be as follows:

šumma sammû kitm[umma]
 qabliltam [unemended!] la zakûtam ta[lput]
 [ribi] uḥrîm tennîma
 [sammû embūbumma]

Similarly, section IV would begin with ...*išart[umma]* and end with ...*kitm[umma]*. Later on I shall propose a slightly different reconstruction, but this does not involve the mode terms as such.

(4) Space allowances for restorations

On the tablet, there are no spaces between the words in the longer clauses, and the signs are slightly squeezed together. The longest line that can be reconstructed is line 18, comprising 14 signs (including *t[a-al-pu-ut]* with no spacing between the words. The minimal width of the column can thus be assessed at about 13 “average” sign spaces. This minimum could probably be increased, since in those lines that have word spacing the spacings are quite wide. The left-hand column is too fragmentary to allow the reconstruction of even one full line. As the specimen is a fragment and the subject is not a common one, it seems too risky to make more precise extrapolations, even though much is known about the standards of tablet and column sizes in various periods. At any rate, the suggestions that will be made in the following, such as putting an additional word in the a-clauses, have been tested by the above assumption of column width, and they do not seem to strain the limit of possibility.

For the first structural analysis, we shall take up sections I to III. Each section says that if the initial state (= S) (of the instrument, i.e., of the strings) is thus-and-thus, and a certain operation is performed on a specific string (or string pair), the resulting state will be so-and-so. To obtain a clearer view, we reduce the three statements to formula. The sign Θ will stand for “unspecified operation” and the sign \wedge will stand for “conjunction,” i.e., co-occurrence, with no further implication.

I	S <i>pītu</i> ,	ΘS_3	→	S <i>embūbu</i>
II	S <i>embūbu</i>	ΘS_6	→	S <i>kitmu</i>
III	S <i>kitmu</i>	$\Theta S_2 \wedge S_9$	→	S <i>išartu</i>

Assuming that *pītu*, *embūbu*, *kitmu* and *išartu* stand for members of the heptamodal-diatonic group, which value is to be assigned to each?

The testing has to consider four alternative conditions in which the unspecified operation is either flatten or sharpen and S_1 (*qudmû*) is either the lowest or the highest:

- *qudmû* is lowest and the operation is “flatten.” Then *pītu* is an ascending mode with a major third: either the C-, F- or G-mode.

If *pītu* is the C-mode, *embūbu* is the ascending melodic minor – intrinsically not a member of the heptamodal-diatonic group. This is not decisive in itself, but *kitmu* would then have an augmented second, and two such compromises are not acceptable.

If *pītu* is the F-mode, *embūbu* has two consecutive semitones $S_4-S_5-S_6$; negative again.

If *pītu* is the G-mode, we obtain that *embūbu* is the D-mode, *kitmu*, the A-mode and *išartu*, the E-mode. By extrapolation (reversing the sequence of terms in the Catalogue) we obtain that *qablītu* is the H-mode, *nīš GAB.RI*, the F-mode and *nīd qabli*, the C-mode. Needless to say, the fact that the group comprises two tritonal modes (F and H) is a nonproblem in the present context.

- We have one plausible result, but the other alternatives must be tested as well.
- *qudmū* is lowest and the operation is “sharpen.” Then *pītu* is an ascending mode with a minor third: either the D-, E-, A- or H-mode. Results: all tests lead immediately or ultimately to constructs outside the heptamodal-diatonic group.
 - *qudmū* is highest and the operation is “flatten.” Then *pītu* is a descending mode with a minor third: either the C-, D-, F- or G-mode (the minor third being here “on top”). Results are as in b.
 - *qudmū* is highest and the operation is “sharpen.” Then *pītu* is a descending mode with a major third “on top”: the E-, H- or A-mode.

For *pītu* = E-mode or H-mode all tests come out negative, as in (b) and (c).

For *pītu* = A-mode we obtain that *embūbu* is the D-mode, *kitmu*, the G-mode and *išartu*, the C-mode. By extrapolation, as in (a), we further obtain that *qablītu* is the F-mode, *nīš GAB.RI*, the H-mode and *nīd qabli*, the E-mode.

The tests assumed that the scalar direction is uniform, i.e., that there is no coexistence of ascending and descending scalar paradigms. Such coexistence within one theoretical framework (predicated upon one kind of instrument) is not plausible. Moreover, the text could not be formulated uniformly, as it is, if its task was to be a “Schule der Geläufigkeit” for the inculcation of the modes by solfège-ing or playing them up and down. Neither is the *šumma* pattern suited for such a task. The bidirectionality was proposed because the Key-Number Table has number pairs that “go up” or “go down” (such as 2–6 as against 7–4). Since the table was the first text to arrive on the musicological scene proper, that interpretation has continued to influence the later studies. When we come to the table here – last, not first – we shall see that there is a simpler explanation.

We face two alternatives, and there must be a deciding factor to eliminate one of them. The deduction “*qudmū* = front = lowest string of the lyre/harp” cannot be decisive, since we may not depend on verbal connotations.

The decision depends therefore upon our interpretation of the second procedure, i.e., the two surviving sections that come after the subscript ...NU.SU.... One of the alternatives must comport better with the second part. The criteria are economy and plausible relationships with the comity of traditional theory systems that we already know. The latter criterion is ultimately also one of economy. On these counts, the most satisfactory result is achieved when we adopt alternative (a). There *qudmû* is indeed the lowest string/note and *pîtu* is the G-mode. The operation in the first part would thus have to be “flatten.”

The only reservation against this would be that Greek theory is supposed to have reckoned the direction of the scale downward. But this supposed rule actually does not exist (there is no need to survey that imbroglia here).

In what follows I shall proceed by alternative (a). The testing of alternative (d) will not be carried out here, but as the conditions have been described the readers can do the test by themselves. One internal test of the first part may nevertheless be mentioned. If we take section III, for instance — *kitmu* to *išartu* — alternative (d) makes this G-mode (“Hypophrygian”) to C-mode (“Lydian”), but alternative (a) makes it A-mode (“Hypodorian”) to E-mode (“Dorian”). The Greek analogue seems to favor (a), but of course this cannot decide the issue without other supporting evidence.

The working hypothesis is thus as follows, with four modes extrapolated:

Sequence as in the first part of the Procedure Text:

[qablîtu	H-mode	“Mixolydian”]
[nîš GAB.RI	F-mode	“Hypolydian”]
[nîd qabli	C-mode	“Lydian”]
[pîtu	G-mode	“Hypophrygian”]
Embûbu	D-mode	“Phrygian”
Kitmu	A-mode	“Hypodorian”
Išartu	E-mode	“Dorian”

Sequence as in the Song Catalogue:

Išartu	E-mode	“Dorian”
Kitmu	A-mode	“Hypodorian”
Embûbu	D-mode	“Phrygian”
Pîtu	G-mode	“Hypophrygian”
[nîd qabli	C-mode	“Lydian”
[nîš GAB.RI	F-mode	“Hypolydian”
Qablîtu	H-mode	“Mixolydian”

In the second part of the Procedure Text the terms appear in triplets: *išartu* – *qablītu* – *kitmu*, and then *kitmu* – *qablītu* – [*embūbu*]. The extremes of each (surviving) triplet are thus in the same sequence as the Song Catalogue. At this stage we cannot know if the subsequent statements will also have *qablītu* in the middle each time, because we do not as yet know why they are formulated the way they are. It is therefore preferable not to extrapolate the text downward, except for [*embūbu*] to complete section V. In the first part, the statements are straightforward enchain pairs, and an extrapolation upward is not overly risky. It is preferable to consider what we have done here as an extrapolation to complete the paradigm, but not necessarily the column on this particular document.

For the procedure in the first part it is not important whether each section is to be taken by itself, or whether the student is asked to carry out a “run” through the sections. In both cases flattening must be done, i.e., the string or strings must actually be loosened – tuned down by a semitone. If there is to be a “run,” the risk of a growing distortion seems to be too great. In any case, we feel the need for a countervailing device to true the tuning to itself, not only in relation to its “plagal” or “authentic” parallel. Moreover, this Procedure Text teaches how to turn one mode into another, but not how to tune the strings to the initial mode. It is not a Tuning Text, as it is called in the literature but a Retuning text. We have already seen, and shall see further as we go on, that we are in an environment where all norms that can be measured are put into tables and exercises (and the other norms are also treated as if they were of this kind). The texts that have been discussed up to this stage – String List, Song Catalogue and Procedure Text – make it highly probable that a Tuning Text proper must also exist. To anticipate, the interpretation that will be proposed here, the Key-Number Table is precisely that Tuning Text.

1.43 Explorations

The working hypothesis of the modal values has been set up without a precise translation of the text, and without any assumption of what the second part (sections IV, V etc.) is about. In the literature, the consensus is that the second part is simply the first part performed in reverse: “down by flattening” there, “up by sharpening” here.

Our Mesopotamia colleagues of the “scribal-religious complex” were not in the habit of writing out twice what could be written out once – especially not for subjects involving inversion and reciprocity. Here another part of the environment must be drawn into our field of view: the domain of mathematics, with its procedure texts and their various strata of supporting tables [!!], the numerical place-value

notation that underlies them and the fundament of the two numerical systems (decimal and sexagesimal). Although the domains of the *šumma* pattern are the formal analogues of our text, the domain of mathematics is the technical analogues of both this and the Key-Number Table. And here economy reigns supreme. (For the background, see Neugebauer 1969: Chapters 2–3, with pp. 31–34 on “saving tricks” in the organization of the network of multiplication tables and tables of reciprocals; a longer survey is Neugebauer 1934–1969.) The general impression one gains from these texts is: reduction to essentials and shortcuts and making one tool do for several. This speaks strongly against the assumption that the second part of our Procedure Text is nothing but the mechanical inversion of the first. We are not in our own environment of paper, print and the premise of the idiot infant.

Moreover, the statements in the second part are formulated *differently* from those in the first part. In both, a section consists of four clauses, each of which is given one line. They have been marked as a, b, c and d in our presentation of the text (see p. 51ff.). The a-clause is the “source,” the c-clause is the operation, and the d-clause is the “sink.” The difference is in the b-clause. In the second part (sections IV–V) the b-clause posits an additional mode term between the “source” and the operation. Gurney (1968) emended IV-b and V-b. We must find out under what condition these b-clauses become necessary at all, and one should first try to see whether there could be a condition in which the b-clauses make sense if they are not emended. The emendations were as follows: in IV-b *qablītam* <*la zakūtam*> *talput* and in V-b *išartam* [instead of *qablītam*] *la zakūtam talput*.

At this stage, a precise translation of the text still confronts an overly large number of branching choices: but, and, initially, concurrently, consequently and subsequently will each lead to a different outcome. The information that we already have eliminates some alternatives, but still leaves too many others, and a mechanical exhaustion of alternatives is not possible here.

The solution must be found through a structural approach, with the help of similar texts whose workings are already understood. It is here that the legal statements in the *šumma* pattern come to our aid, and they will serve us a so-called toy. To quote (the emphasis is mine):

Much of the time the decipherer finds himself chasing structural features of an unknown nature. A useful device in these cases is external comparison — not of two hypotheses within the corpus, but of structurally similar hypotheses in two different corpuses, the second of which is of a known nature. This other corpus is the cryptanalyst’s “toy” or model. (Barber 1974: 77; see also index s.v. *toy*)

The “toy” will be five legal statements from the Old Babylonian period to which our document is also assigned. At the very least, the composition of the text is in reasonable chronological proximity. Two of the ordinances are taken from the

Laws of Hammurapi and three, from legal formulary *ana ittišu*. The latter would seem to confirm the existence of strict rules for the formulation of legal statements. Even though we have but this one Procedure Text for music at present, it seems that the same rules were also applied quite deliberately here. This is not surprising: the situation is related to the reasoning of imbalance-and-redress with which the domain of law is concerned, and the *scriptores* of musical theory belong to the same overall professional environment.

The texts will be presented in transcription and not in the trans-“literation.” The Sumerograms have been left in place, with their readings added in brackets. This is because there is also a Sumerogram in our Tuning Text — the writing ^{sis}ZĀ.MÍ for *sammû* (stringed instrument or body of strings). It appears within clauses that are not intact enough to supply clear information about the case of the noun (see discussion above). The intact legal texts will demonstrate how the cases are assigned there.

My retranslations are literal as to the lexical elements, and largely congruent as to word order. The grammatical forms have been translated “functionally” and not literally. The result is obviously not a translation for reading. For the difficulties, see Driver and Miles 1955: 360–361 and the entire chapter. No conjunctions have been added, because there are none in the text and thus we become more conscious of the fact that the conjunctive functions are carried out by other means — which is the basic problem that we face in our music text.

The clauses and subclauses have been numbered so as to set out their relationship. (1) governs the protasis and (2), the apodosis.

(1) *ana ittišu* VII, iv 1–7 (MSL 1, 103)

11	šumma aššatu	In case a wife
12	mussa izīrma	her husband she disliked
13	ul mutī atta iqtabi	“Not my husband are you,” she has declared
23	ana nāri inaddūšu	Into the river canal they shall cast her.

(2) Codex Hammurapi §229 (Driver and Miles 1955: 82–83)

11	šumma ŠITIM [= itinum]	In case a builder
12	ana awīlim E[= bītam] īpušma	for a man a house made
	šipiršu la udanninma	his work he did not strengthen
	É[= bīt] īpušu imqutma	the house he made collapsed
13	bēl E[= bītim] uštamīt	the householder it has killed —
14	ŠITIM[= itinum] šû iddâk	That builder shall be put to death.

**uštamīt* = properly “has caused the death of”

(3) Codex Hammurapi §21 (Driver and Miles 1955: 20–21)

11	šumma awilum	In case a man
13	bītam ipluš	into a house broke
21	ina pāni pilšim šuāti idukkūšuma	In front of that breach they shall kill him and
23	iḫallalūšu	shall hang him.

(4) *ana ittišu* VII, iii 29–33 (MSL 1, 101-102)

11	šumma māru ana ummišu	In case a son to his mother
13	ul ummī atta iqtabi	“Not my mother are you,” he has declared
21	muttassu ugallabūma	They may (first) shave his half [= of his head] and
22	ālam ussaḫḫarūšu	lead him around the town
23	u ina É[= bītim] ušešūšu	And (lastly) throw him out of the house.

The formally present tense of *ugallabū* etc. could serve in the apodosis to say what shall be done, as in all the preceding examples. Here, though, the use is permissive: the punishment may be carried out. No internal or external coercion is implied. The family can just as well decide to give the naughty son a good beating and the “authorities” will not force them to obey the letter of the law or carry out the formal punishment on their own. This use of the present tense can also be applied to the other nuances of permission: “choose to do,” “wish to” and “be able to.” The available information decides which sense probably applies in each case. This grammatical possibility exists in a certain place within our text and it will be explored later in this study.

(5) *ana ittišu* VII, iii 23–28

11	šumma māru ana abišu	In case a son to his father
13	ul abī atta iqtabi	“Not my father are you,” he has declared —
21	ugallabšu	He may shave him,
21	abbuttam išakkanšu	The slave-hairlock he may put on him,
23	u ana KÙ.BABBAR[= kaspim] inaddiššu	and for silver he may give [= sell] him.

For the slaves’ coiffure, see CAD A/1: s.v. *abuttum*. The intention is again that this is what the father may do if he wants. The *u* “and,” in the final clause here and in the preceding, also functions as the rhetorical hinge for the final and most grave declaration.

These two paragraphs have a distinct “swing,” much more so than in the cases of the builder and the housebreaker. Because of their content they stand

midway between a legal ordinance and a moral adage, and hence partake of the nature of poetry.

When a group of texts evinces strong patterning, a fragmentary text that clearly belongs to the same group can be filled in, on whatever level of information the pattern provides. In a specimen of European poetry, for instance, only the time element and perhaps the rhyming syllable could be filled in (u—u—u—u—ose). In an optimal case, such as a numerical table, extrapolation can fill in the precise individual values (but only if one is sure that no “saving tricks” were employed, as in certain Mesopotamian tables — see Neugebauer 1969: 30–31). Where our own case is analogous to the mathematical tabulation, we have already been able to fill in values. But in all other respects we are midway between a minimal and an optimal level of information. The “toy” cannot provide individual values, except for the opening *šumma* (which is mostly preserved in any case), but it does provide the next-best information.

- Every clause concludes with a verbal element, throughout the sections.
- The a-clause has at least four beats (or rather accentual peaks). Hence, the accepted reconstruction as, e.g., *šumma* ^{gis}ZĀ.MÍ *kitm*[*umma*], is less probable than *šumma* ^{gis}ZĀ.MÍ *kitm*[—](verb). The *sammû* may thus be in the accusative, *sammûm* (as the CAD indeed puts it; see CAD L: 89, s.v. *lapātu*).⁴ But the vagaries of musical terminology recommend that we leave the forms unreconstructed.
- The protasis and the apodosis can both be subdivided in themselves into antecedent and consequent parts. For this, the medical text quoted previously (see p. 38) also furnishes a good example: drinking implies consequent hangover symptoms; cure implies *iballuṭ* “he shall revive.”
- The present tense can also indicate feasibility (“may,” “can”). Circumstantial information must be adduced in each case to decide what is intended.

The proposed reconstructions of the forms of our statements are, thus as follows:

Section III

šumma ^{gis}ZĀ.MÍ *kitm*[..] {verb/verb-*ma*}
išartum la zak{ât?}
šamušam u uḫriam {verb-*ma*}
išartum izzakū/izzaz

⁴ Editor’s note: Bayer apparently did not understand the Akkadian nominal sentence; *sammûm* (^{gis}ZĀ.MÍ) is in the nominative in the CAD transcription. Although the CAD reference in L is not translated, it is translated as: “if the *sammûm* is tuned to the *išartum* mode,” in CAD S: 119, s.v. *sammû* mng. 1.

The b-clause probably ends on *la zakât*: it is not a verb but a verbal adjective, and there are sufficient precedents that a verbal adjective can play the role of a verb. The other points have already been discussed. In the a-clause, the ending on *-ma* is not absolutely necessary. As for *kitm*— there, musicological experience warns that one cannot fill it out just by choosing one of the grammatically correct possibilities, because professional idiomatics are involved.

Section IV (unemended!)

šumma ^{giš}ZĀ.MÍ *išart*[..] {verb/verb-*ma*}
 qablītam talput
 šamušam u uḥriam tennima
^{giš}ZĀ.MÍ *kitm*[..] {verb}

Section V (unemended!)

šumma ^{giš}ZĀ.MÍ *kitm*[..] {verb/verb-*ma*}
 qablītam la zakūtam talput
 ribi uḥrīm tennima
^{giš}ZĀ.MÍ *embūb*[..] {verb}

A functional translation of IV and V can now be attempted, with due consideration of the “toy.” Instead of “In case” it will be more convenient to translate *šumma* here as “Given that.”

Section IV

Given that: the *sammû* in/for *išartu* (vb. you having it set/it having been set),
 [thus] *qablītu* you have [also!] (vb. *lapātu*).
 [Next] S₂ and S₉ you having [first] changed/modified —
 the *sammû* in/for *kitmu* (vb. shall be set/be able to play?).

Section V

Given that: the *sammû* in/for *kitmu* (vb. you having it/it having been set),
 (thus) an imperfect *qablītu* you have [also!] (vb. *lapātu*).
 [Next] S₆ you having [first] changed/modified —
 the *sammû* in/for *embūbu* (vb. shall be set/be able to play).

To achieve a more specific translation, the lexical meanings of *lapātu*, *enûm* and *zakû* must be checked.

The verb *lapātu* belongs to the class of “touching.” But this can be delimited further by excluding the “punctive touch,”⁵ either a single touch or several in succession. *lapātu* implies the combination of touch, coverage and adherence. Hence “encompass” is perhaps more suitable in this context than “play” even though there are descriptions of music scenes that do use *lapātu* in the latter sense (see Kilmer 1965: 263; CAD L: 91).

The implication of *enûm* (*tennī-ma*) has already been discussed (p. @@). Its result is sharpening. But the act is probably finger stopping. The basic state of the string would then be left as it was; the string would only be “modified” temporarily.

In IV *qablītu* is posited without qualification. In V it is *la zakû* – imperfect in some way. As noted (p. 43) one need not assume that what is designated as imperfect is therefore unusable or even impossible. Only the context of usage will supply what is meant.

A provisional rephrasing in normal English will thus be as follows:

Section IV

If the tuning is in *išartu*,
then *qablītu* is also possible;
by finger-sharpening S_2 and S_9 –
you can also play *kitmu*.

Section V

If the tuning is in *kitmu*,
then imperfect *qablītu* is also possible;
by finger-sharpening S_6 –
you can also play *embūbu*.

With this reconstruction, the b-clauses in IV and V are no longer senseless. The statements posit a relationship of three scalar constructs. Such a relationship is entirely feasible under a certain condition, which we do not have to invent: the heirs of the Mesopotamian achievement described it, each in his way, and it still exists today, for the ear of the listener and for the hands of the player, as it existed then. The label we use for it has been taken from the terminology of one of the heirs: Byzantium, which assigned it two terms: plagality versus authenticity. Earlier, Aristides Quintilianus (second or third century CE) recognized that melodies that share the same scalar base can be allocated to three rather than to two subgroups. As an epigone of the Hellenistic tradition, he characterized them as *hypatoïd*, *mesoïd* and *netoïd* (cf. Sachs 1943: 249).

5 Editor’s Note: By “punctive touch” Bayer apparently refers to the “plucking” of a string.

We already have the values of the octave species as such, and the implication of *tennī-ma* as an act that results in sharpening. For the second part of the text (sections IV–V) to make sense as it stands, the octave species must be juxtaposed as in the adjacent Table 1 and assigned differing regions of primary and secondary melodic activity. As purely notational convenience on our part, the mode of the a-clause is notated each time as its white-key mapping.

If the situation represented in Table 1 is indeed the true explanation of what sections IV and V “mean,” a veritable host of interlocking conclusions is generated thereby.

- The E-mode *išartu* and the D-mode *embūbu*, to which the Greeks assigned presumably the straight ethnic names “Dorian” and “Phrygian,” belong to the same class as regards the region of primary melodic activity. The A-mode *kitmu*, which would thus correspond to the presumed Greek “Hypodorian,” has its region of primary activity on the opposite side.
- The H-mode *qablītu*, which would thus correspond to the presumed Greek “Mixolydian,” represents the case of a melodic occupancy of the middle area. Its lower region is more important, as regards the melodic activity. The Greek name is anomalous; a satisfactory explanation of the “Mixolydian” is not available, but obviously some close relationship to “Lydian” was expressed thereby. In our reconstruction the same terminology sharing is apparent. “Mixolydian” : “Lydian” = *qablītu* : *nīd qabli*. This correspondence also lends additional support to our scalar identification of the two Mesopotamian names: a similarity of relationship weighs more than just a similarity of components. The above statement of equivalence would seem to imply that something is not quite right: the compound term “Mixolydian” is *qablītu*, while the simple term “Lydian” is *nīd qabli*. But this will find its explanation when we come to the Key-Number Table.
- The disposition of *qablītu* in the middle of the nine-string *systema*, with its “finalis” on the midpoint, has been arrived at through a purely structural hypothesis. But the result is also a terminological fit: *qablītu* means “the middle” (cf. Aramaic and Arabic). At this stage, however, I still avoid any use of the verbal elements. The data are noted, but not adduced as proofs of anything.
- In section V it would ostensibly be possible to emplace *qablītu* within the *kitmu* tuning as a simple H-mode between *SI* and *si* (from S_2 to S_9). But the perfect *qablītu*, as emplaced in section IV, needs at least four tones below its “finalis.” Thus, it can be played when the tuning is *kitmu*, but imperfectly. Apparently, the main melodic activity is in a small range around the middle tone, so that the “imperfect” extremes do not make for an absolute impossibility. The imperfection could, of course, be removed here by setting the

offending intervals right, but this demands flattening. If flattening is not allowed here, since the player cannot pause for retuning a string, it leaves *qablitu* imperfect. This means that our hypothesis about finger sharpening being involved has a good chance of being right. A perfect *qablitu* is achieved under these circumstances by the same action that turns *kitmu* into *embūbu*: the sharpening of S_6 . The d-clause of section V is not sufficiently preserved to show whether this was also said there explicitly. It may well have been. In the d-clause of section IV no other mode can be added.

- The emplacements notated in the table imply that the ninth string and the first string can both play three roles: nonpertinent, pertinent or significant. The first string can also be “strongly significant.” Hence, the ninth string is no longer the puzzling addition that the String List, as such, did not explain.
- If the ninth string is functional, a simpler scheme could also be invented. Each nine-string stretch in a certain tuning yields two octave species: a tuning from D to e, for instance, would present *embūbu* D-d and *išartu* E-e. As we have so few texts, it would be imprudent to decide that such a scheme cannot have existed, but the probability is low: If plagality is taken into account in this text, it strengthens the impression that this musical culture behaves like all the other theory-governed cultures we know: one does not “modulate” between a D-mode and an E-mode. They have no relationship within the actual melodic occurrence. Shock effect changes are always possible, but these will not be demonstrated in a basic training text such as the one we seem to have here.
- The complex workings of sections IV and V make it less easy to extrapolate sections VI, VII etc. One possibility would set up a section VI as *embūbu-to-pītu*, via *šalšam qatnam tennīma* (the mirror of section I). Between this pair, a perfect *qablitu* again becomes possible. In this case, section V would not end with the double statement “you can then play *kitmu* and *qablitu*”; that would be the job of section VI. There are some other possibilities as well: the statement may not be concerned exclusively with the relationship X: *qablitu*: Y. We simply do not have enough information to decide what is supposed to happen next and for how long it goes on.
- The very fragmentary titling statement in line 12 has not been investigated up to this point. Anne Kilmer was the first scholar who tried to establish a reading and propose an explanation (1971: 140, note 47). As this was done solely on palaeographical and philological grounds, and as I did not want to be guided by any of the existing interpretations, I did not take it up during the working out of the present hypothesis. A later check, however, showed that Kilmer’s proposal fitted very well into the result achieved here.

To quote:

Provisional translation [“Not augmented (??)”] of [x?] NU.SU [y] based on NU = *la* “not,” and SU = *riābu* “to augment, increase, replace,” and assuming that this line is a sub-label describing the preceding “Section I” [our I–III, B.B.] Perhaps to be restored as [SA] NU.SU.S[U] “string(s) not augmented.” Another possibility: NU.SU (for SÛ) .U[D] “not embellished” (= *ulluḥu*, see CAD E: p. 79, sub *elēḥu*). (Kilmer 1971: 140, note 47)

The second possibility does not seem to fit, but the first certainly does. The sub-label, i.e., subscript, says that something is “not augmented”; what follows is therefore probably “augmented.” A reminder to non-Assyriologists: in the cuneiform tradition, the titling statements are generally at the end, not at the head, of the text to which they refer. The first part (I–III) somehow makes do with less of something than the second part (IV–V). To put it even more generally, something is done in the second part that is deliberately not done in the first part. We do not know what the proposed *riābu* would mean in a musical context, but, in any case, there is a negative here, which further strengthens the contention that the second part is not simply an exercise in reverse. One possibility could be “Strings not in full range,” i.e., only to the octave, or even only in a heptatonic gamut. The latter is particularly tempting, because the Key-Number Table also names only strings one to seven. But section III seems to contradict this, as it indicates the ninth string as well. Another possibility would be nine strings in the first part and more in the second (together with a more involved paradigm, in accordance with the principle of economy). Many of the instruments in the iconographic and archaeological record have more than nine strings, but the quantity of pitch units on the instrument does not imply anything about the gamut with which the theory operates. Moreover, once we go beyond nine strings as the theory gamut or *systema* (which is the optimal number, capable of doing “the mostest with the leastest!”) – we land in the same theoretical bog in which the Greeks and the Indians landed. I suggest that for the present, there is not enough information to assign a precise meaning to the subscript. Its general sense is understood.

In his new study of this text, Crocker proposes to interpret NU.SU as “stop here; for if you go further you merely duplicate what you have already done at a higher pitch” (Crocker 1978: 100–101). He reaches this conclusion through the accepted assumption that the two parts are mirror exercises and also does not consider the import of SU = *riābu* (to augment). Since my own argument follows a different path from the outset, my conclusions are necessarily different.

- The cycle that ends with section III can be traced backward and thus would begin with *qablītu* (H-mode), to continue with *nīš GAB.RI*, *nīd qabli*, *ḫītu*, *embūbu*, *kitmu* and *išartu*. Crocker indeed proposes this (1978: 101). However, *qablītu* to *nīš GAB.RI* and *nīš GAB.RI* to *nīd qabli* involve the sharpening of

the fourth string. The fact that two tritonal modes are involved (H and F) is not a hindrance, but the fourth string is the string that the god Ea created or instituted — ^dEa-*bānû*. In the Greek analogue, the *mese* (of the “Dorian” mode projected on the *systema*) may not be modified. Could the Ea-string also have been such an immovable point? The analysis of the Key-Number Table will again give rise to this question. I thus suggest that we do not restore the first part of the Procedure Text beyond one preceding section, namely *nīd qabli-to-pītu*. A caution against the mechanical extrapolation of the second part has already been put forward.

- The second part proceeds in the same order as the listing in the Song Catalogue. *išartu* stands at the head of the group when an actual “Diwan” of songs is organized. One cannot ask why this should be so, but only note the datum. Also, this need not mean that the majority of the repertoire was in this mode. For example, the modern Arabic modal system usually puts *Rast* at the top of the list of modes (and calls it “First”), but the mode *Rast* does not dominate the repertoire. The Greeks attributed various good qualities to their “Dorian,” which is the presumed scalar equivalent of *išartu*, but since they had nothing that corresponds to a “Diwan,” a comparison must look to other paths. It is the “Diwan” concept itself that is far more interesting here. I use the Arabic term to denote any written collection of texts for singing that is organized according to a fixed sequence of the modes of the melodies. That the principle has survived, with its core area stretching from the Bosphorus to the Indus, is another datum to note.
- The “Diwan” concept has its parallel in the religious domain. In the Christian Near East this is assumed to be governed by the term *Oktoëchos*. This presumes eight modes. The Mesopotamian system has seven modes (in the Key-Number Table, seven plus seven). A tie in with any *Oktoëchos* tradition does not seem to be possible at present, and this is puzzling.⁶

⁶ Editor’s note: Surprisingly, Bayer fails to cite Eric Werner’s important study of the *Oktoëchos* in which he advanced the idea of its possible Mesopotamian origins. See: Eric Werner, “The Origins of the Eight Modes of Music (Octoëchos),” *Hebrew Union College Annual* 21 (1948), pp. 211–255. See now: Elias Kesrouani, “L’octoëchos syriaque,” in: *Aspects de la musique liturgique au Moyen Âge*, ed. Christian Meyer, Paris: Éditions Créaphis, 1991, pp. 77–91 and the pathbreaking study by Peter Jeffery, “The Earliest Oktōēchoi: The Role of Jerusalem and Palestine in the Beginnings of Modal Ordering,” in: *The Study of Medieval Chant: Paths and Bridges, East and West: In Honor of Kenneth Levy*, ed. Peter Jeffery. Woodbridge, Suffolk: Boydell Press, 2001, pp. 147–209. Jeffery does not address at all Mesopotamian antecedents and focus on the earliest tangible evidence from eighth-century Jerusalem

However, one area of the *Oktoëchos* tradition has also recently been shown to be more problematical than was thought hitherto. This is the area of the Syriac Orthodox Church, whose liturgical language is Syriac (a dialect of Aramaic), and it extends *grosso modo* over Syria and northwestern Mesopotamia (with sectarian divisions within the area and offshoots outside it).⁷ For a relevant study, see Husmann 1971: Chapter 3 “Der ‘Oktoëchos’ des Severus von Antiochien” (pp. 46–58).

1.5 The Key-Number Table

1.51 Analogues

That text that we call *the* Key-Number Table is actually one section of a list of key-numbers, i.e., coefficients, for computations of the most diverse kinds. It is necessary to understand the nature of these lists, so as to enable us to approach our table through its own context and culture.

A key-number list is the equivalent of our modern vademecum of formulae and tables. It is a dispenser of concise numerical information for instant use. Several such lists have been published, and the general principle is understood. But many of the entries have not yet been explained, because corroborative texts are needed to flesh out the extreme “reductionism” of the statement. Here is an example, from the list that has been given the *siglum* Ud (YBC 5022, text and explanation from Neugebauer and Sachs 1945: 132–134).

Line		
17	3, 45	“Of the wall of rammed earth” (?)
18	15	“Of asphalt” (?)
19	30	“Of the triangle”

The numbers are probably to be read in the sexagesimal or mixed sexagesimal-decimal notation. The first number (3,45) is found in other lists in connection with the daily work assignment of manual laborers, but the application here is not yet understood. The second number (15) may be “the coefficient by which one must multiply the area expressed in units of square kuš [square cubits] in order to find the number of sila [a liquid measure] of asphalt to cover the area in question” (Neugebauer and Sachs 1945: 133). The third number (30) is probably “the

⁷ Editor’s note: Only the Melkite and the Jacobite liturgies use the Oktōēchoi. See Jeffery, “The Easliest Oktōēchoi,” p. 156, note 29 and the extensive bibliography there on Syriac chant.

constant by which the product of the length and width of a right triangle must be multiplied in order to get the area” (Neugebauer and Sachs 1945: 133f.).

The Akkadian term for such a key-number is *igigubbû(m)* (derived from the Sumerian). Its literal meaning is “fixed/established fraction.” Here it was used in the more general sense of a standard “coefficient.” The idea was applied not only to absolute coefficients such as geometric ones, but also to empirical constants such as, here, work assignments or pots of asphalt for surfacing. There are also simple standards, such as “30 quarts of barley, load of the wagon,” or “1 double bushel of litter, load of the wagon” (on the tablet that has our musical table, col. iii, 29 and 30; see Kilmer 1960: 283).

Since our table has number pairs, connected by \dot{u} (and), one should look for similar examples. These appear very rarely.

Text A 3553 (Kilmer 1960: loc. cit.)

line

1	30 \dot{u} 1	“coefficient: triangle”
2	5 \dot{u} 6	“coefficient: circle”

Text Ud (see above)

line

21	45(?) \dot{u} 10	“of the segment of a circle”
44	1,20 \dot{u} 15	“ <i>nazbalum</i> of water”

The meaning of these numbers is not yet known, but obviously the task had to be defined in terms of two numbers, but not one.

The key-number lists are thus the acme of terseness, even more so than their modern descendants are. As this entry does not say “For X — to get R, take N and perform O,” but rather “N [is for] X.,” our understanding is blocked because the operation (O) is never specified.

However, these lists do not stand alone. There is another group of texts, the so-called problem texts, in which the operations are actually demonstrated. By correlating the information, it has become possible nowadays to explain some of the obscure numbers. Our own Procedure Text is largely similar to the problem texts (though these are not in the *šumma* form), and will serve us in the same way.

A direct parallel to the musical table, i.e., a sequence of numbers within an overall systemic pattern, does not appear in the key-number repertoire. The reason is that the usual subjects of these lists simply do not have such a pattern. Patterned configurations are found in a different domain: the multiplication

tables, tables of reciprocals, of squares, of roots and of “logarithms.” The metrological tables have a pattern of their own. Thus, the key-number lists and each entry or group of related entries there presume the substratum of number tables.

All the strata are held together by the word lists and “encyclopedias” and bilingual “dictionaries.” “...and their appearance and their work was as it were a wheel within a wheel” (Ezekiel 1:16).

Behind this giant system of interlocking norms, there is nothing. Among the hundreds of thousands of tablets recovered, there seems to be nothing that resembles a “treatise,” neither didactic nor philosophical-speculative. The system develops but after the early (and naturally nebulous) stage it remains essentially the same, an Accountant’s Paradise that was never breached from without nor ever questioned from within. It is important to remember this for our own subject as well, because the circle of fifths also bears within itself the seed of its own destruction, so to speak, since it does not lead back to its beginning. We also know that this opens the way to speculative theory. But it is a matter of cultural climate if that breach is taken as a challenge — or as a threat and therefore patched over and disregarded. Greece chose one way, but Mesopotamia chose another. This must caution us against reading into the texts what their originators very probably did not think of, and if they thought of it — never pursued.

Yet one must not overestimate these achievements. In spite of the numerical and algebraic skills and in spite of the abstract interest which is conspicuous in so many examples, the content of Babylonian mathematics remained profoundly elementary. In the utterly primitive framework of Egyptian mathematics the discovery of the irrationality of $\sqrt{2}$ would be a strange miracle. But all the foundations were laid which could have given this result to a Babylonian mathematician, exactly in the same arithmetical form in which it was obviously discovered much later by the Greeks....In other words, Babylonian Mathematics never transgressed the threshold of prescientific thought. It is only in the last three centuries of Babylonian history and in the field of mathematical astronomy that the Babylonian mathematicians or astronomers reached parity with their Greek contemporaries. (Neugebauer 1969: 48)

This leads us to another domain of ancestry for our table. Cyclic phenomena do not occur naturally in key-number listings for wagonloads, work assignments and even elementary geometry. Their natural habitat is astronomy. An extended discussion of Mesopotamian astronomical techniques is not necessary here (consult Neugebauer 1934/1969, 1955, 1969). A structural analogy to our Key-Number Table indeed appears as expected, and it is most instructive. These are the ephemerides — tables that give the calculated locations of a heavenly body, day by day or month by month for one year (mostly), and thus serve various calendrical needs. In our musical table there appear two sequences of string-number pairs. One is

$1^{\wedge}5, 2^{\wedge}6, 3^{\wedge}7, 4^{\wedge}1, 5^{\wedge}2, 6^{\wedge}3, 7^{\wedge}4$. The other, intercalated with the first, is $7^{\wedge}5, 1^{\wedge}6, 2^{\wedge}7, 1^{\wedge}3, 2^{\wedge}4, 3^{\wedge}5, 4^{\wedge}6$. A cyclic concept is described linearly, and the same is done in the ephemerides: “The main tool for the computation of the ephemerides is arithmetic progressions, increasing and decreasing with constant differences, between fixed limits...we call such sequences ‘linear zigzag functions’” (Neugebauer 1969: 110).

Now an ephemeris is set out line by line, one for each month. The sequence is naturally unalterable, and the complete table thus describes the apparent motion of that heavenly body through a circle of “stations,” generally with respect to the zodiac. The user of the table will read off that month line which concerns him at the moment. He will not run through the entire table from top to bottom. The same holds for all the other tables in his library. It stands to reason that our table is similar. It is ordered so as to be mnemotechnically efficient, but the line-by-line definitions are not to be run through except for mnemonic purposes. In other words, the analogues and congeners of the Key-Number Table make it virtually certain that this is not an exercise to be played through from A to Z. It is a table, and just what this term implies. The present consensus in the literature takes the table for an exercise, and its “realizations” therefore lead to difficulties. The main difficulty arises because the base is the cycle of fifths, and a run through leads to painting oneself into a corner — or out into “the land where the (Chinese) *lü* and the *commata* play.” To save the situation, further assumptions are brought in, but these have no parallels in any known traditional melodic theory. This does not seem plausible. Especially doubtful is the assumption of the “desire to avoid the tritone”; the tritone is a *diabolus in musica* only in European harmonic theory!

Another difficulty arose directly through the fact that the table was the first theory text proper to become known. The Catalogue was already accessible, but had been misunderstood until then. Since the table apposes a set of number pairs to a set of nouns, and these number pairs were soon after identified as denoting pairs of strings — the nouns were taken to stand for intervals. There were controversies about details, but the basic premise was not questioned. The Song Catalogue should have precluded such a hypothesis from the start. Instead, it was forced to yield a saving hypothesis. Thus, *kitmu*, for instance, would still be primarily an interval, but also “the mode called thus because it is characterized by the *kitmu*-interval”; characterization here being taken not in the sense of prominence in a *maqam*, but as an interval in the scale as such — this notwithstanding the fact that if *kitmu* is, say, the interval of the fourth, then fourths are after all found in every scale. What further assumptions this had to involve can be traced through the literature. It seems that there just had to be a conceptualization and naming of the interval, because the Greeks, and European theory as their heir, indeed “had a word for it.” But it is possible to do without interval names (in a

melodic culture), and say instead “from string X to string Y,” as the Greeks indeed also did. Moreover, the Greek interval names, like the ones used subsequently in the European languages, were numerical definitions (diapente, diatessaron etc.). The proposal that in Mesopotamia the intervals were called by non-numerical names should at least have caused some eyebrow raising among the musicologists, as should have the assumption that the double-duty relationship is between the interval and the scale. The comity of traditional theories indeed has this phenomenon of double-duty terms. But care is taken not to cause misunderstanding, and the linkage is never of the kind that has been argued here. We also say “C” for the note and “C” for the scale, but by the scale we mean key, and we take care to say “C major” or, as I do here, “the C-mode” (in the sense of scale, not key).

In the present inquiry the Key-Number Table was not taken as the point of departure. Up to this stage in our “code breaking” the analyses have not produced any names for intervals. There is only a set of diverse nominal forms that are used to label modes (presumably octave species); a set of ordinal names for strings; and a small collection of verbs, adjectives etc. for operations and assessments. As interpreted here, all this fits together quite normally, by all we know of the basic concepts and terminological ways of the comity of traditional theories; and thus it also fits into that comity. Since it is the “Ancestor of them all,” directly or indirectly (as already surmised by the first researchers) the wonder would be if it were otherwise.

1.52 The text

The text is a section of a table of key-numbers (for details, see §1.1). Its writing is difficult to read and the tablet may be the practice copy of an apprentice scribe (information from Anne Kilmer). The source for the presentation of the text here is the “Revised Version” in Kilmer 1971: 134–135 and further passim. As was done with the String List and Song Catalogue, the original transliterations have been turned into transcriptions. Except for lines 6 to 10, the components of the lines are not spaced in the original to form internal columns. Spacing has been added here to clarify the structure. The meanings of the terms will be discussed subsequently.

Key-Number Table

K-MdSt // CBS 10996

obverse, col. i

line

(at least 5 lines broken off)

6	[2 4 SA titur qablītu]m		
7	6 3 SA <i>kitmu</i>		
8	3 5 SA titur išartum		
9	7 4 SA <i>pītum</i>		
10	4 6 SA <i>šerdû</i>		
11	SA <i>qudmû</i> û SA 5-šú	1 5	SA <i>nīš</i> GAB.RI
12	SA 3 <i>uḫri</i> û SA 5-šú	7 5	SA <i>šēru</i>
13	SA šamūšu û SA 4 <i>uḫri</i>	2 6	SA išartum
14	SA <i>qudmû</i> û 4 <i>uḫri</i>	1 6	SA šalšatum
15	SA 3-šú qatnu û SA 3-šú[!] <i>uḫri</i>	3 7	SA embūbu
16	SA šamūšu û SA 3-šú[!] <i>uḫri</i>	2 7	SA rebūtu
17	SA ^d Ea- <i>bānû</i> û SA <i>qudmû</i>	4 1	SA nīd qabli
18	SA <i>qudmû</i> û 3-šú qatnu	1 3	SA isqu
19	SA 5-šú û SA šamūšu	5 2	SA qablītu
20	SA šamūšu û SA ^d Ea- <i>bānû</i>	2 4	SA titur qablītu
21	SA 4 <i>uḫri</i> û 3-šú qatnu	6 3	S[A <i>kitmu</i>]
22	SA 3-šú qatnu û SA 5-šú	3 [5	SA titur išartum]
23	SA 3-šú <i>uḫri</i> û [SA ^d Ea- <i>bānû</i>	7 4	SA pītum]
24	SA ^d Ea- <i>bānû</i> [û SA 4 <i>uḫri</i>	4 6	SA <i>šerdû</i>]

(Remainder broken off)

Remarks (by lines):

6) Partially legible. 7) Number pair unclear, first read as 4 3, emended by line 21. 8) Number pair unclear, first read as 3 6, emended by line 22. 11) 5-šú = *ḫamšu*. 13) 4 *uḫri* = *ribi uḫri*. 15) 3-šú = *šalšu*; properly *šalši uḫri*; number pair unclear, first read as 3 4, emended by pattern. 16) Number pair unclear, first read as 2 4, emended by pattern; *rebūtu* written 4-tu. 21) *kitmu* restored by pattern (relative to other texts), confirmed by line 7 although number pair unclear or wrong there. 22) *titur išartu* restored by line 8, although number pair unclear or wrong there; placement of term problematical, to be discussed. 23) Restored by pattern, confirmed by line 9. 24) Restored by line 10.

We thus seem to have the end of one terse formulation, and most of another, less terse one. The scribe (apprentice?) apparently had difficulties with the terse formulation, and the reason is obvious. In the String List and Procedure Text, and here as well, the strings are named according to the “first forward then backward” convention. But the table also uses another convention — the simple linear sequence (here up to *šalši uḫri* = 7). That simpler convention is obvious better suited to the terseness of the key-number concept.

Deductions as to a “development” are not possible at the moment, although the tablet is from the Neo-Babylonian period (mid-first millennium BCE); any text must be taken as a copy in a chain of unknown length, if evidence for a *terminus* is not yet available.

The terms for strings are already known at this stage, and so are seven of the fourteen nominal forms. In the Song Catalogue and in the Procedure Test these stand for modes, i.e., the octave species of the heptamodal-diatonic group. Sumerian SA = Akkadian *pitnu*, “string, interval; scale.” The conjunctive *ù* has been encountered in the Procedure Text, where it stands for the simple “and”/“and also,” i.e., with no obligation except co-occurrence. An *ù* may also stand for “or”/“either,” but within the present context — however it is interpreted — this is not possible.

Because of the redundancies, vertically and (in the second formulation) also horizontally, the text is self-checking and can therefore be restored. We may thus deduce that the table was indeed one of fourteen entries.

1.53 The interpretation

The table dovetails two groups of seven entries each: the group for which we already have the mode values, and another one. A schematic representation follows, similar to the “terse version” in the upper part of the original. As in our analysis of the Procedure Text, S_n will stand for the n-th string (in the simple ordinal numeration). \wedge will stand for “conjunction” (cf. p. 44 above). Properly, the entries should thus be expressed as for instance, $S_x \textcircled{\text{O}} (S_2 \wedge S_6) \rightarrow S \textit{išartu}$. But since the table itself does not do this — it mentions neither the initial state nor the operation — we shall also leave these components unstated.

$S_1 \wedge S_5$	<i>nīš GAB.RI</i>	F-mode “Hypolydian”
$S_7 \wedge S_5$	<i>šēru</i>	?
$S_2 \wedge S_6$	<i>išartu</i>	E-mode “Dorian”
$S_1 \wedge S_6$	<i>šalšatu</i>	?
$S_3 \wedge S_7$	<i>embūbu</i>	D-mode “Phrygian”
$S_2 \wedge S_7$	<i>rebūtu</i>	?
$S_4 \wedge S_1$	<i>nīd qabli</i>	C-mode “Lydian”
$S_1 \wedge S_3$	<i>isqu</i>	?
$S_5 \wedge S_2$	<i>qablītu</i>	H-mode “Mixolydian”
$S_2 \wedge S_4$	<i>tītur qablītu</i>	?
$S_6 \wedge S_3$	<i>kitmu</i>	A-mode “Hypodorian”
$S_3 \wedge S_5$	<i>tītur išartu</i>	?
$S_7 \wedge S_4$	<i>pītu,</i>	G-mode “Hypophrygian”
$S_4 \wedge S_6$	<i>šerdū</i>	?

The two sets must obviously be analyzed separately, starting with the one for which we already have the values. Its sequence is different from the circle-of-fifths (and “authentic” – “plagal”) ordering encountered in the Song Catalogue and in the Procedure Text, but this is a nonproblem. Here the intention is to obtain an ordering that is mnemotechnically efficient “by the numbers,” and hence 1^5 comes first, 2^6 , next etc. From the musical point of view, the scheme is turned from a sequence of most related modes to one of least related. Punningly apt: what you win on the swings you lose on the roundabouts.

Disregarding the as yet unknown group, we have the following:

$S_1 \wedge S_5$	<i>nīš</i> GAB.RI
$S_2 \wedge S_6$	<i>išartu</i>
$S_3 \wedge S_7$	<i>embūbu</i>
$S_4 \wedge S_1$	<i>nīd qabli</i>
$S_5 \wedge S_2$	<i>qablītu</i>
$S_6 \wedge S_3$	<i>kitmu</i>
$S_7 \wedge S_4$	<i>pītu</i>

The general statement for this group is:

Any pair of integers, minimum 1 maximum 7, with a constant difference of +4 or -3, is necessary and sufficient for the exclusive definition of a scalar constant.

Question: What are the simplest limiting conditions under which this is possible?

Answer: There are three limiting conditions:

- The scalar construct is a diatonic octave species.
- The pair of integers stands for the pair of incidences of the semitone, one for each semitone, when the scale is stepped off from “1” upward. All the other steps are assumed as nonsemitones and (pragmatically) equal, hence (pragmatical) whole tones. Since the step is from one point to the next, but signaled by only one number, the agreement is “The number is that of the upper point.”
- Since a mnemotechnically efficient formulation is desired, the actual “8” (for the semitone 7–8) is exchanged for its octaval equivalent “1.” The table can thus begin with 1^5 . When it reaches what should be 4^8 , this is expressed as 4^1 . The next terms in the mnemotechnic will have to be 5^2 , 6^3 , 7^4 . Hence, there is no need to use any number beyond 7.

The “trick” of condition (c) agreed very well with the environment. Repeatedly, the tables and computations make use of reciprocals, zigzag functions (basically the same concept) and many other devices of shortcut and reduction. The numerical notation is itself based on such a device — the “floating place value,” like

the floating decimal point we use today (for details, see Neugebauer 1969: 15ff.). The substitution of “1” for “8” is therefore something that ties in organically with what the Mesopotamian student has already been accustomed to, when he was learning his arithmetic.

The Key-Number Table is thus a “Plaine and Easie” way of learning and remembering how to tune one’s lyre or harp to any desired mode. It is also an abstract description, but the actual approach is practical. The modes are simply predicated on the set of strings. Of course the set of strings is not pretuned (as the interpretations of the table have argued hitherto, looking for *the* Mesopotamian scale). The table says how to tune whenever you need, and similarly, one does not, on the actual instrument, do a run through from the first to the last tuning. Under extra-classroom circumstances, with professional musicians, the tunings were surely made by ear and melodic memory, but our table belongs to the classroom.

The method will only work when the strings, i.e., “tones,” are named by ordinal numbers. If a similar table-cum-memorizing-jingle is made up where the “tones” do not have numerical names, one might still learn it by heart, but with much greater difficulty. Here we do have a lost tradition whose loss should be bemoaned by everyone who has had to struggle with the “teaching of the modes.” The Greeks could not adopt it, because they used letters for numeration, and so did their European successors, until the arabic numerals came into use in the Middle Ages. But by then ut-re-mi and A-B-C were already entrenched. The Romans, even though they had I-II-III as numerals, ran into difficulties with the following ones, and, in any case, for musical theory they depended on the Greek tradition. Nowadays, to make matters still worse, scalar theory is inculcated on the piano with its black keys. And the “inculcantes” mostly have to learn the “modes” as a dead lore, which does not link up with the musical environment in which they have been raised. The reference here is of course to Western or Westernized musical environment. Where Westernization is not absolute, such as in Israel where this is written, the ancient key-number system can still work – if one can get one’s students to disregard the actual or imagined picture of the keyboard. The author has already carried out such an experiment once, and it worked.

To return to Mesopotamia, the step-by-step tuning method would thus be, for instance, for *embūbu* = $3^{\wedge}7$: 1–2 Tone, 2–3 Semitone, 3–4 Tone, 4–5 Tone, 5–6 Tone, 6–7 Semitone, and implicitly 7–8 Tone. If the instrument has more than eight strings, “8” becomes “upper 1” automatically, and so forth.

Two musical difficulties have still to be resolved. A step-by-step procedure will soon lead to distortion. There must be a truing device to prevent this. Also, if one begins from the lowest string (*qudmū*) and that is set first, the high region of

the string body might turn out to be too high in pitch: the strings would tear, and perhaps the instrument itself would not be able to bear the tension.

The truing device that prevents the scale getting out of joint has already been identified in Greek theory, although there it has been held to be a device for setting up the scale itself. This is the “up and down principle” (Sachs 1943: 229). One starts with the central string, goes down by a fourth, and thence it is possible to go upward by an octave and also to check the central string again with reference to the upper octave point. Here that central string/note is “the Ea-creator” string, most understandably so.

No absolute pitch norm needs to be demanded. The Ea-string itself is tuned by experience and for convenience (of the instrument and of the singer whom it accompanies). If several instruments play together, they will accord themselves by agreement (with the “Chief Musician”). The practice can still be found in the Near East. One might still ask whether the god Ea did not also ordain that his string be not only a relative norm but an absolute one. In other words, the Ea-string would itself be set according to a pitch norm, and that pitch norm would only be the note given by a pipe of which the length was in itself a normative unit, presumably the cubit. (Pipe diameter considerations can be considered as irrelevant in the present context.) The Old Babylonian cubit (kūš) measured ca. 20 inches/50 centimeters. A pipe of this length would sound a tone somewhere around the (present day) F below middle C. The replica of one of the large lyres from Ur, made by Robert Brown in Berkeley, was indeed found to be conveniently tunable in the range implied by this hypothesis (the height of the instrument is about one meter). To confirm such a proposal, definite textual evidence is required. So far I have found only one datum that could be relevant — if more such can be identified. This is not enough, and I prefer leaving the matter for the present as a suggestion that seems to be worth pursuing.

If the Ea-string is set immutably (with or without a normed pitch), then a difficulty would arise when the instrument has to make a “metabole” from *nīd qabli* (C-mode) to *nīš GAB.RI* (F-mode; its plagal). The fourth has to be sharpened. But we need not assume that the fourth string with its Ea-given stability was forbidden to be finger sharpened during playing.

1.54 The seven para-modes

In the Key-Number Table, the second group of seven entries must also refer to modes, and the same method must also apply to these. But a difficulty (for our hypothesis) looms up immediately.

The group is defined as follows:

$S_7 \wedge S_5$	šēru
$S_1 \wedge S_6$	šalšatu
$S_2 \wedge S_7$	rebūtu
$S_1 \wedge S_3$	isqu
$S_2 \wedge S_4$	titur qablītu
$S_3 \wedge S_5$	titur išartu
$S_4 \wedge S_6$	šerdū

The heptamodal-diatonic group has already been exhausted.

If the same method is applied here, the result is not impossible, but difficult to understand. To present it for analysis, it is advisable to begin the sequence with *šerdū*, because here philology must be called in. *šalšatu* and *rebūtu* are “the third” and “the fourth” ones (feminine gender). Accordingly, *šerdū* would be in the first place, *šēru*, in the second etc. One lexical possibility for *šerdū* is indeed *širtu* “first rank” (as remarked by Kilmer 1971: 144 footnote 69), but we know that groups of mode names can be a very mixed bag, because earlier systematizations of varying age and provenience may lurk below. A similar situation has been argued for the nomenclature of the Chinese *Lü*'s (Küttner 1965). Equally mixed is the collection of Arabic mode names.

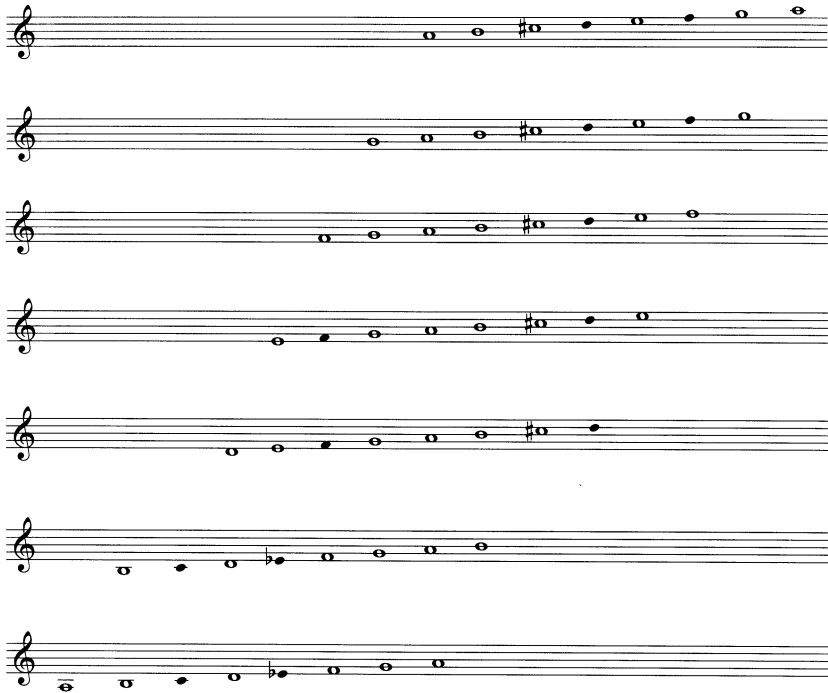
In the following notational description (Table 2), the scales have not been pegged to the same initial note because the pitch is immaterial here, and the emplacements chosen make the situation more obvious. Since *qablītu* is the H-mode (purely as a white-key mapping, of course), *titur qablītu* has been pegged to H. and the others let fall into their (pseudo-) place. The table should be understood as a temporary heuristic device.

Most of these scales call up strong “Balkanic” associations. The actual melodies do not always span the full octave, especially when they belong to a vocal repertoire. It would not be difficult to set up a collection of specimens from the ethnomusicological literature for each scale. If this is mapped geographically, strong clusterings would appear in Yugoslavia, the Caucasus (Armenia, Georgia), and probably also in the region of Turkish and Syrian Kurdistan.

It is also possible that at least some of the constructs in the scheme are in reality scales with augmented seconds, which the table has forced to be described by the two-semitone method.

All this is a surmise, which cannot as yet be proved. The main difficulty would seem to be this: the heptamodal-diatonic group is a recognized theoretical concept, as a group, in the traditional theories from Greece to India, but the “group” set up here has no equivalent as a group elsewhere. At the most, it is a

secondary or parallel group created by the overall application of some external rule, such as the concept of “genus” (nondiatonic, here “diatonicized” forcibly).



At any rate, this smoothly mechanical scheme of fourteen modes altogether is surely a smoothed-over synthesis of several ethnically diverse repertoires. These must have been known, and synthesized thus, much earlier than the Neo-Babylonian period in which this particular table (CBS 10996) was written. As we know, twelve of the fourteen mode terms appear as the notational units of the Ugarit “score.” And these “scores” date to the fourteenth century BCE.

1.6 The Textual Fragments

As I define it, a fragmentary text is one that can be largely completed, out of itself and/or through another copy (or several others) of the same text. A textual fragment, on the other hand, is a true fragment. Information may be available for

going a little beyond the edges of the specimen, but that is all. Two such fragments are included in the material that has been published in transcription until now: the seven or eight lines that follow upon the String List in U.3011, and the remnant of the column to the left of the Procedure Test in U.7/80.

The transcription of this part of U.3011 was published by Kilmer (1965: 264; 1971: 133–134). The autograph copy of the entire tablet is available in UET 7, 126 (Gurney 1974) (Fig. 1). As the text is Chapter 32 of the series *nabnītu*, further copies will surely appear before long as “museum finds,” now that its importance is known. It is therefore not urgent to attempt a reconstruction from the single fragmentary specimen that is available at present.

The second fragment appears to the left of the Procedure Text and is clearly not part of that unit though obviously related to it. The transcription was given in Gurney’s (1968) publication of the tablet, but not taken up for analysis until recently. Crocker has now attempted a partial reconstruction, by the premises of the consensus — that the mode terms stand primarily for intervals (Crocker 1978: 102–104). As he recognized, the variation of the grammatical endings is significant (nominative *-um* against the genitive *-im*). But it seems to me that the extent of loss for the beginnings of the lines is too great. Out of the eleven partially readable terms, five or perhaps even six allow alternatives. In several there is more than one alternative that would fit into the space and comport with what is left. The resulting uncertainty seems to be extremely high, much beyond the danger line. What is already known through the other texts (by any interpretation) is not enough to reduce this uncertainty. I prefer therefore not to attempt this fragment either, as long as no further texts are available.

Part Two: Notation in Ugarit

<i>Schüler</i>	Das sieht schon besser aus! Man sieht doch wo und wie?
<i>Mephistopheles</i>	Grau, teurer Freund, ist alle Theorie, Und grün des Lebens goldner Baum.
<i>Schüler</i>	Ich schwör' Euch zu, mir ist's als wie ein Traum...

(Goethe, *Faust* 1, 2037–2040)

2.1 The Problems

Theory texts are grey, but the hope of being able to hear melodies that were notated three millennia ago is green and golden. In the Introduction I have already described the general scene of the discovery and study of the Ugarit scores. I also stated there that, in my opinion, the true decipherment of the notation has not yet been achieved, and that I was not about to offer an additional decipherment. The task was to go back to the Mesopotamian theory texts and work out their interpretation anew. Since the Ugarit scores are rooted somehow in the Mesopotamian theory, and since there is actually no real consensus on the specifics of that theory, we may be initially skeptical of any decipherment that is offered in such a situation. Moreover, the literature now contains about half a dozen proposed decipherments. Each differs from the other, and none seems to be acceptable to any but its own proponent. Anyone who has some knowledge of the history of the decipherment of ancient scripts and the elucidation of ancient languages will probably conclude that the symptoms are familiar: the redemption has not yet dawned. It is rather cruel, but salutary, to compare our own predicament with what Chadwick described under the chapter heading “Hopes and Failures” in his book on the decipherment of the Linear B script (Chadwick 1960: Chapter 3). Our own situation is perhaps even more difficult, since the object is not a verbal script, and since the work is beset by the troubles of a new bidisciplinary liaison. When the breakthrough will have been achieved, the scholarly scene will look and “feel” quite different. It is only after such a breakthrough that one can properly analyze the various earlier attempts and see what went wrong where and why and who contributed at least a pointer in the right direction. Further on I shall make one critical reference to a particular publication, but this will be in connection with a more general methodological problem.

The relevant publications to date have been included in the list of references. Not all of them contain actual [musical] transcriptions. They range from Laro-

che's first publication of some of the tablets in 1955 to the latest attempt at decipherment (Thiel 1977).⁸

The contribution that I offer here is of a different kind. During my own struggles with the material from Ugarit, I came to certain conclusions about the need for a largely new approach, based on certain aspects that were not considered in the existing publications. These will be set out in the following.

2.2 An Approach to the Decipherment

The coexistence of civilizations of a status equal to that of Mesopotamia was quite rare, but Ugarit seems to have been such a case. There, the technology of the Mesopotamian system of writing (cuneiform signs on clay) was applied to a system that represents a revolutionary advance: an alphabetic script, the sequence of whose letters is already much the same as that of our alphabet. This script was used to record a native literature, to administer a complex bureaucracy, and to write down legal transactions, but at the same time there were scribes well-trained in the Mesopotamian way of writing in Akkadian; in addition, Hurrian was written in Ugarit in both the Ugaritic alphabet and the Mesopotamian cuneiform system. In Ugarit, we also encounter Hittite documents in cuneiform as well as art objects bearing dedications in Egyptian hieroglyphs. It must have been a truly international center, a clearing house for both ideas and merchandise. (Oppenheim 1977: 72)

The Ugarit scores present a mixture of elements from several distinct cultures. Mesopotamia contributes the cuneiform writing system, the quasi-Akkadian formulation of the subscripts, the (surviving) names of the scribes and the mode terms that are the basic components of the notation. The Hurrian elements are the language of the poems (a stylistic analysis is not yet feasible), the (surviving) names of the authors and/or composers, a sizable number of additional components of the notation and perhaps also the genre term *saluzi* in the subscripts. The forms of the number signs in the notation are those used within the alphabetic cuneiform of Ugarit (Laroche 1955: 334). In this context, as well as in many others, "Mesopotamian" and "Hurrian" are general terms that cover a situation that is actually far more complex, but need not be described here (see, e.g., Kammenhuber 1968: 61ff.; on the problem of Sumerian and Akkadian terms borrowed into Hurrian, and vice versa(?), see there: 121ff.).

The picture of the theory texts is wholly different. Whatever diversity of origins may have contributed to the formation of the system is no longer noticeable overtly. The theory evolved where the theory texts themselves were written – in the Land between the Two Rivers. Until now, no musical notation of the

⁸ For an updated bibliography, see Ann Kilmer's article, p. 92 below.

Ugaritic kind, or of any other kind, has been identified in documents that come from Mesopotamia proper (see Appendix A, Excursus 1). In the Ugarit scores the Mesopotamian theory is clearly the foundation on which the notation has been built. But it seems that certain factors of non-Mesopotamian origin have also been at work here, and that the external situation of a different (and multiplex) culture is paralleled by an internal difference as well.

The Mesopotamian system actually offers a ready-made notation, in the form of a solfège of string names or numbers. This would have to be “pretuned” by stating the mode, whereupon the melody could be spelled out by the strings. We still use the same principle in our own staff notation, where we “pretune” the staff by clef and accidentals. Certain supplementary devices would probably be needed for such a string notation, but it would not be difficult to see that the string names are the basic notational components. In the Ugarit scores, however, this obvious way was not adopted. The basic notational components are the terms that stand for the octave species in Mesopotamia. To these are added a number of Hurrian terms, some of which are compounded with the Mesopotamian terms and some of which seem to stand by themselves. There is some similarity here with the case of a new script being made up partly out of graphic elements borrowed from another culture and partly of newly invented ones. What this means for linguistic decipherment is discussed by Barber (1974: mainly 97–98). The case is not wholly analogous, because a musical “statement” belongs to an entirely different communicatory dimension. As yet, we do not have such unequivocal paradigms of grammar and syntax for melody as the decipherers of unknown languages and/or scripts can use to puzzle out the written reflection of speech. That is the main reason why I doubt the feasibility of what Duchesne-Guillemin attempted in her 1975 study (further on this, see Appendix A, Excursus 3).

In summary, there is a strong probability that the Mesopotamian theory texts and the Ugarit scores are not truly co-systemic. If this was so originally, an attempt at the decipherment of the notation nowadays will also not be viable if it goes directly from the theory to the notation. A transformational mechanism seems to intervene and must therefore be sought out. In the following, I shall propose a hypothesis as to why such a mechanism would be necessary and what this implies for the decipherment of the notation.

The Mesopotamian system is predicated on the set of strings of an open-stringed and multistringed instrument. This could be a large lyre or a large harp in any of the diverse shapes that these instruments assumed in Mesopotamia (see Spycket 1972; for the [presently] earliest known depiction of a large harp, see there: 158, and Kantor and Delougaz 1969). The principle is also applicable to a zither, but at this period there were no zithers in the Near East. (Note to non-musicologists: the terms are used as taxonomical norms; for an explanation, see

Sachs 1940: “Terminology”; 454–467.) The open-and-many stringed combination is thus the “instrumental premise” of the theory. A solfège-by-strings notation can be derived from the theory as long as this instrumental premise obtains. But such a notation cannot be derived from the theory, if the culture that wished to have a notation, and use the theory, has a wholly different instrumental premise as the foundation of its art-music practice. What was possible for lyres and harps (and zithers) is not possible for members of the fourth class of chordophones — the lutes. If a culture has its art-music tradition based on the lute, it will need a transforming mechanism if it sets about to create a notation out of a theoretical system that is predicated on lyres and harps.

There are indications that the Hurrians were indeed a “lute people.” Specifically, the lute in question is the so-called long-necked lute. Short-necked lutes (as exemplified nowadays in the Near East by the ‘*ud*) apparently did not come into the region until well after the beginning of the first millennium CE. A brief discussion of the problem of the “northern” provenience of the lute will be found in Appendix A, Excursus 4. No iconographical documentation for Hurrian music making in Ugarit is as yet available. Nor can we adduce something from Alalakh, its neighbor to the North, which was even more strongly “Hurrianized” for a time. From the materials published so far, it is clear that the two sites have not been exhausted by the spade, and also that such materials that were found have not yet been made public. For an interim illustration we can use some finds that do not coincide precisely in their location and/or period with the Ugarit scores but that do fit *mutatis mutandis* as to details, into the presumed ethnocultural image. A thorough discussion would have to go into the complicated problem of Hurrian settlements in Syria and Mesopotamia, and the history and post-history of the (mainly) Hurrian kingdom of Mitanni (for introductory information, see Oppenheim 1977: 61, 71 and see index). For the task at hand we shall limit ourselves to a first exploration of the musical data by the hypothesis that is offered here. That hypothesis can now be stated as follows: the Ugarit scores seem to be written in a lute-based notation; this notation “maps the lyre into the lute,” and this mapping involves a transformation, which must be reconstructed.

Theoretically, one can turn the group of modes very easily into a linear *systema*. The names of the modes would then stand for the points on the line, and thus be double-duty terms for modes and notes (as the standard Arabic system indeed does). But the heptamodal-diatonic group would then yield only seven such points. If the seven para-modes are dovetailed into these to yield — somehow — the semitonal filler notes, there is still the same range. If the terms repeat at the octave, they would have to be qualified by something like “lower X” as against “upper X.” Such compound terms with “lower” and “upper” (Hurrian *turi* and *ašhu*) do indeed appear in the notation (see the useful list of terms in

Kilmer 1971: 143–145), but they might as well stand for adjacent frets or even a grouping of three close frets (lower X / X / upper X).

There is a further obstacle. The little *systema* notation just described is linear. It will do as a tablature (fingering notation) for a lyre or for a one-stringed lute with frets. But one-stringed lutes are folk instruments, nowadays often bowed (such as the Bedouin *rabāb*), and have no frets. Lutes that are used for art-music proper have at least two strings. The iconographical, historical and ethnomusical record seems to favor the assumption that the “Hurrian lute” was indeed a two-stringed instrument. The strings are tuned to a certain interval apart. One finds tunings from a major second to a fifth, with tunings of a third (minor or major) or of a fourth in the majority. Most of the notes can therefore be played on either string. If the frets are simply named from the lowest to the highest, “(fret) X” can mean either of two notes. It does not seem plausible that the system was of this kind.

Lute tablatures can and do make use of still another element that is denumerable: the four fingers of the hand that presses on the string. Indeed, the scores (i.e., fragments) generally show the number signs 1, 2 or 3, with 4 much rarer. The appearance of 5 is still more rare, and its reading is not assured. In h.6, line 5, the last term is — according to Dietrich and Loretz (1975) — *kablite* 2 and not 5; Laroche (1968) reads “5” only there, in h.16 line 12, in h.19 line 7 and, with a question mark, in h.30 line 3. The numbers 6 to 9 do not seem to appear at all, and there are just three appearances of a “10” that is somewhat problematical (h.6 line 5; h.8. line 10?; h.10 line 7). But our grouping (and anyone else’s) cannot be continued in this way. The historical and ethnomusical record must be called in and used with the proper tools — and with the greatest caution. It can be used because the domain of the long-necked lutes is an eminently tradition-preserving one, especially for the two-stringed instruments, notwithstanding regional and historical modifications (see, e.g., Gerson-Kiwi 1973; Baily 1976).

An approach-by-structure is demanded here no less than it was demanded for the theory texts. Here, however, it needs two separate steps: from the general theoretical framework (the Mesopotamian theory) to the theory of notation, and then from that theory to the decipherment of the notational data. There would be the usual going to and fro on the way, as one must do when a hypothesis is worked out, but the three stations and the two steps are still the basic path by which the inquiry must follow. We have the point of departure (the Mesopotamian theory as such), but the step from this to the (unwritten) theory of the notation has still to be taken. As I have argued here, this demands the reconstruction of the transforming principle. This reconstruction must be made with the help of the historical and ethnomusical record. The record is not easy to interpret, since

it has a millennial depth by now: each stage is influenced by what came before it, and also “edited” by what came after it.

The task demands a separate research effort. I have already made a beginning at this, but a solution is not yet in sight. Actually, the decipherment of the notation is at every instant but one part of a wider inquiry. The following are, in brief, the directions that I think should be explored (and have already begun to explore): (a) the domain of the long-necked lutes (for a direct bearing on the decipherment of the notation); (b) the background of the Greek instrumental notation; (c) the “Early Arabic” theory of the Finger Modes (*aiabi*), which now begins to look as if it should be more properly called “Late Mesopotamian”; (d) the earliest stages of Indian theory, which also seem to hint at a lute-versus-lyre situation; and (e) the background of the modal systems of Persia. The order in which these have been listed is also the ascending order of difficulty of their investigation, from the merely sticky to the almost certainly impossible.

Since 1960, when the field began to be explored, the musicologists who entered it have recognized its wider import. What should become possible from now on, gradually and by the efforts of many scholars, is an understanding of the outline — no more but hopefully no less — of what I have here called the comity of traditional theories. Another name for it now seems even more apt, both as a model and as a metaphor: the Tree of Theory.

Appendix A

Excursus 1 (to Part One): The hypothesis of the “Babylonian notation”

Musicology accepted the rise of the hypothesis, but has not become properly cognizant of its fall. The mysterious arrays of syllables are nothing but another kind of analytical vocabulary for scribal training (though with a curious “prehistory” of its own). Its usual designation in Assyriological research is “Silbenvokabular.” It would be useful to have a resumé of the matter formulated for musicologists and published in a musicological venue. As an interim aid I list the basic bibliography here.

- Proposal, defense and modifications of the “notation” hypothesis: Sachs 1924, 1925, 1941, 1943: 85–87; Galpin 1937: 38–43, 99–104.
- Appearances in standard surveys: Farmer 1957: 248–250. MGG-Not: cols. 1600–1601.
- Recent utilization: Katz 1974.

- Rebuttals, further studies of the “Silbenvokabular”: Landsberger 1933, 1959 (the year of Curt Sachs’ death!); Çiğ and Kızılyay 1959; Sollberger 1965; Nougayrol 1965; Çiğ and Kızılyay 1965 (all in Landsberger Fs.). See also in Borger 1967–1975: vol.1: 52 (entry Çiğ-Schulbücher). Among the studies of the Mesopotamian theory proper, Güterbock (1970) opens with a summary of the “affair” (p. 45), as does Kilmer (1971: 131).

Excursus 2 (to p. 31): The “third-thin” string

Duchesne-Guillemin proposed a certain basic scale for the set of strings, based on her interpretation of *šalšu qatnu* (Duchesne-Guillemin 1966: 150ff.). Wulstan (1968) proposed a different one. Kümmel (1970) doubts both, and attempts a solution of his own. All agree that there is one basic scale. Each such proposal will, of course, influence the interpretation of the other texts, and thus ultimately also the decipherment of the Ugarit notation.

The strings will here be denoted by the ordinal abbreviations, i.e., S_1 = first string (*qudmû*), S_2 = second string etc. We need not decide at this point if the scale runs upward or downward. The assumption is then that *šalšu qatnu* implies that S_2 – S_3 is a “thin” interval. Since it is also assumed that the scales are all diatonic, then S_1 – S_3 must always be a minor third. As argued in the following, this raises more problems than it solves.

- A diatonic scale cannot be specified by one semitone only. Such a “cavalier” attitude is unthinkable for Mesopotamia, if we assume that the complete heptamodal-diatonic group is a musical reality in the culture. Four of the seven modes have the minor third between S_1 and S_3 . Two of these have the semitone between S_2 and S_3 . If the basic scale exists, this is surely not the way to characterize it.
- On the Sumerian side of the String List, S_4 is called “string-fourth small” and S_3 is “string-third thin” as on the Akkadian side. This would imply, by the same reasoning, a scale with two adjacent semitones! Otherwise, are we to assume that the Sumerian and Akkadian side of the list were not meant to correspond?
- As will be shown in the course of the present inquiry, the Procedure Text and the Key-Number Table can be “worked” without difficulty if we assume that the string names are intrinsically neutral, i.e., that the nine-string set is not in itself predicated upon a particular mode. Entities are not to be multiplied except when necessary. If a simpler hypothesis works as well, yields results that are compatible with what we know about scalar theory in other ancient

cultures, and needs less extraneous suppositions than the more complicated hypothesis — the simpler one is to be preferred.

- Verbal connotations should not be taken as guides and not adduced as proofs when searching for the functional meaning of a term. The only verbal connotations that we can use in the present inquiry are the ordinal designations of the strings, taken as simple ordinals of position in a body of strings on the lyre or harp equally.

At present, as I see it, we can only affirm what the “third-thin,” and “fourth-small” and the backward numbering of the sixth-to-last strings do not imply. We do need to know how these designations came about, but there is simply not enough information available as yet. I have at least three alternative hypotheses. All of them are possible, all are plausible — and none of them is more probable than the others. For this reason I shall not even set them out here.

Excursus 3 (to p. 74): Ethnomusical analogues

In Duchesne-Guillemin’s 1975 study, some specimens of ethnomusical materials from the Near East are used as possible structural analogues to the Ugarit melody. The basic idea is that of the cryptanalytic “toy”, and the basic assumption is that of ethnomusical survival. I agree with both, but find some disturbing methodological problems in the way in which the study was carried out. There is no need for a detailed argument here, but our common interest demands the correction of one factual error.

The clue specimen of the undertaking is a rendition of Ps. 137 (*By the rivers of Babylon*) in the “Babylonian,” i.e., Iraqi Jewish tradition. It was taken from vol. 2 of A. Z. Idelsohn’s *Thesaurus of Hebrew Oriental Melodies* (1922), where it appears as no. 95. Actually, however, this is not at all “une pièce”...“un chant,” in the sense of a specific “tune,” as Duchesne-Guillemin terms it. It is the rendition of the psalm text by a psalmodic formula. That formula happens to be the one known to Western Christianity as the *Tonus peregrinus*. In most Jewish traditions of psalmody, the pattern is realized in a melismatic and rather free way. This is very different from the largely syllabic and extremely schematic form in which the same patterns were notated and are sung in Western Christianity. A Western musicologist might well recognize the pattern underneath an elaborate Eastern rendition when hearing a live performance (if he knows what to expect). But if one only sees a transcription, where the “flesh” has obtained equal visual weight with the “skeleton,” it needs a specialist’s familiarization to recognize the pattern.

For a thorough study of the *Tonus peregrinus* in Jewish tradition, see Herzog and Hajdu 1968. For Jewish practices of Psalmody, see Herzog 1972.⁹

Where a psalmodic formula exists, in a culture in which the use of such a device is part of its own musical heritage, there would seem to be no need to notate it expressly: at the most, a verbal or numerical tag could be added to the text. If the psalmodic formula is a new invention or an import from another culture its notation would surely be much shorter than what we see in the Ugarit score; its nature would also become immediately apparent, even when the specific note values are not yet known. Conversely, if the Ugarit score is basically a “tune” — and this is the impression it gives — an ethnomusical specimen of psalmody is not the proper “toy” for its decipherment. That the melody will proceed both here and there largely stepwise (i.e., by tones or semitones) is highly probable; even then, that it might very well not be stepwise is highly probable. But even in that case, it might very well not be stepwise throughout. In any case, this characteristic (if it is indeed found in the Ugarit score) is too general to warrant the specific conclusions that it has been made to produce or to prove.

Excursus 4 (to p. 75): Were the Hurrians a lute people?

In his 1961 study and subsequently, Stauder advanced the claim that the lute originated among the “Bergvölker,” i.e., in the area comprising eastern Anatolia, the southern Caucasus and northeastern Mesopotamia. This would also include the presumed homeland of the Hurrians and their later state of Mitanni. Lately, this has been contested by Rashid in two studies (1970, 1973) with further arguments promised. Rashid contends that the lute already appeared in Mesopotamia in the Akkad period (approximately 2300–2100 BCE), and that it is indigenous to Mesopotamia. His richly documented arguments must certainly be weighed very seriously. It would seem to me, however, that, in the end, the true historical development will turn out to be neither as “pure Northern” as Stauder would have it, nor as “pure(?) Mesopotamian” as Rashid insists.

The appearance of lutes in the monumental art of the Hurro-Hittite area in the period with which we are concerned here (fourteenth century BCE) does not give the impression of a Mesopotamian import. The Ugarit notations have so far been found only with Hurrian texts. The notation seems to be lute based, as I argue here, while the Mesopotamian theory as such is lyre/harp based. This is as far as I can take the argument at present, as a working hypothesis.

⁹ Editor’s Note: Indispensable now is: Reinhard Flender, *Hebrew Psalmody: A Structural Investigation*, Jerusalem, 1992 (Yuval Monograph Series 9).

The early appearances of the lute in Mesopotamia might help to solve the problem that I have already hinted at in §1.54 — the origin of the seven paramodes. Nondiatonic modes seem to be strongly linked with lutes, because of their intrinsic intervallic freedom. An instructive inquiry on what seems to be a similar case of a synthesis from different periods and with a different instrumental base was undertaken by Kuttner for the Chinese system of the *Lü*'s (Kuttner 1965).

Appendix B

A Scheme for Working sigla

Within the cuneiform record, a subject corpus is now being formed by those texts that are primarily concerned with music. As the size of the corpus increases, the texts come to be tagged by improvised names and working *sigla*; and after a time the need is felt to standardize these tags through some kind of overall scheme. Such a scheme is, in effect, a model of the subject. There are a number of technical precepts for the making of an efficient model, but there is also one condition that goes beyond the technical. The model should not only describe the material that is already available, but should also be predictive in order to accommodate what will be added to the corpus by future discoveries.

- In its full form, the *siglum* consists of two parts, separated by a double oblique:

TEXTS//DOCUMENTS. Thus,

C-Md//KAR 158	Song Catalogue
P-MdSt//U.7/80	Procedure Text

For the text part, letter symbols are taken from the list of abbreviations that I created (see below) and combined in a fixed sequence. The document is indicated by its conventional *siglum*, i.e., C = Catalogue, Md = #Mode, P = Procedure, St = String. Obviously, an item need not always be cited by its full double definition.

- The unit for this subject corpus is that extent of statement that represents *one entity of musical concern*. A “music text” or “music chapter,” even a “music passage,” may contain several such units. For instance, in *nabnītu* XXXII, which is a “music chapter,” the String List forms one unit. The Mode List that follows it in the same column is already a different unit (see discussion in §1.1, Introduction).

- The unit of musical concern is defined through the class or classes of music terms with which the statement operates, such as “mode term” or “string term,” or both together. The table of *siglum* components lists those classes of terms that are sufficient for such a definition, for the purposes of the working *siglum*. This is a definition by extension (the field of applicability) and not by intension (a complete description of the contents). Hence, the definition also does not say how the term functions in a particular statement. Function is indicated only in the case of a notation (and there, too, without specifying how the notational result is brought about); this will create an immediately visible distinction between “scores” and “literature.”
- For the classes of music terms, the *siglum* uses two-letter symbols (Md, St etc.). The definition of the unit is complete by a prefixed one-letter symbol, which stands for the context category (such as L- = List, C- = Catalogue etc.).

In the following list the texts known at present are assembled by their full *sigla*, thus creating a self-classifying inventory. Some predicted cases are also included. At this stage the scheme is applied to the theory texts proper.

C-Md//KAR 158	Song Catalogue, featuring mode terms
C-Md, a//KAR 158	Different catalogues of the same kind
C-Md, b//....	
H-Md (Nt), e//RS h.6	Hymn, with mode terms used as notation
H-MdSt (Nt)//...	Hymn, with string terms used as notation, through qualification by mode term
H-Md//...	Hymn, with colophon giving mode term indication of tune (≠ notation!)
H-Md//...	Hymn, mentioning mode terms (same definition as above!)
K- MdSt//CBS 10996	Key-Number Table (implicit: music part only)
K- MdSt//...	Another copy of the same table
K- MdSt, a//CBS 10996	Different formulations of a Key-Number Table
K- MdSt, b//...	
L-St, <i>nabnītu</i> XXXII//U.3011	String List, in series <i>nabnītu</i> tablet XXXII
L-Md, <i>nabnītu</i> XXXII//U.3011	Mode List, in same
L-Xx//K.9922	Fragmentary tablet, not yet divided into units
P-MdSt//U.7/80	Procedure Text, featuring mode terms and string terms
X-MdSt//U.7/80	Left-hand column of above tablet, textual fragment, not yet classifiable
X-MdSt, a//U.7/80	Different fragments, not yet classifiable
X-MdSt, b...	

Notes to theory texts

H-Md (Nt), e: The special numeration for Hurrian texts from Ugarit (Laroche 1968) is used in preference to the general excavation numbers of the tablets. It is shorter, and accommodates joins that have already been made. Some changes are expected with further joins. The most intact “score,” h.6, is RS 15.30+15.49+17.387 (second join made several years after the first!). On a further problem of Ugarit *sigla*, see below.

H-Md: The two cases would appear as H-Md, a //...and H-Md, b//..., perhaps with a conventional name already available for some hymnal compositions (cf. Kilmer 1971: 147–148, note 77). If a colophon is considered as a separate content unit in all cases, the Ugarit scores would have to be split into “notations” and “colophons,” which is factually incorrect. Apart from the notations proper, a hymnal text might appear with a colophon (scribal, and, hopefully, musical) in one copy and without one in another copy. Instead of setting up a Procedure Text of our own for all such alternatives, we cut the Gordian knot by deciding that the position of the term within the text is irrelevant to the *siglum*, including cases of colophon position. The information about colophons will be taken care of by an index.

A crux is at present posed by the Sumerian texts that describe a musical ambience and feature something termed “7 tigi.” This term cannot as yet be assigned to any class, even though there is a strong probability that it is a theory term. Such texts must therefore remain under reserve, and the *siglum* scheme cannot accommodate them for lack of minimal information. We dare not even describe the text as H-Xx..., because “7 tigi” might still turn out to be a set of seven instruments, or a suite of seven hymns.

P-MdSt//U.7/80: The two sections of this text, separated by the subscript ... NU.SU..., come out as one content unit by definition, which is factually correct. Terms such as *sammû* and *tennîma*, which are also featured in this text, need not be taken into account for the *siglum*. There is a *siglum* element either for the mention of a specific instrument or several instruments, but the present text seems to use *sammû* as a generic term for “any stringed (open-stringed?) instrument,” or even for “the set of strings,” which thus coalesces with “St.”

Two texts in alphabetic cuneiform from Ugarit have been proposed tentatively as featuring “Canaanized” theory terms (for details, see Kilmer 1971: 14). Such texts, which are in the Ugaritic language and script, have no less than five alternative identification numbers. Taking one of the texts as an example, we face a choice of RS 5.213 = UT 104 = quondam UM 104 = CTC 163 = 93 (Eissfeldt!). It would seem advisable, in such a situation, to keep to the one constant – the excavation number RS 5.213 – and take care of the other numerations by a reference note.

At present, the language of the text is not indicated by the *siglum*. An expansion might become necessary in the future. We already have one Akkadian-Sume-

rian bilingual (*nabnītu*), with others from the “library of lists” in the offing (see p. 25). There will also be monolingual Sumerian texts. The Ugarit “scores” are a mixed phenomenon, Hurrian hymns with a Hurrianized Akkadian notation and an Akkadian colophon. A similar phenomenon would appear in Ugaritic texts proper. It seems advisable not to put the language criterion at the head of the *siglum*, at any rate. Since the record leads us to expect trilingual lists (Sumerian-Akkadian-X), translations and adaptations, it might even be necessary to put the language indication at the very tail of the *siglum*, after the documentary part; however, we will cross that bridge when we get to it.

The following table lists the elements for composing the text identification part of a working *siglum*.

Elements for Working *Siglum*

Note: If the musical content involves more than one element, these are to be put in alphabetical sequence, irrespective of their position in the text itself.

A-	Astronomy (for ritual calendars, see R)
B-	“Biography,” all evidence on personalities ¹⁰
C-	Catalogue
D-	“Didactic”/ <i>é dub.ba</i> texts (“Literature of the Tablet House”) ¹¹
E-	Epic, including mythology (for Hymns, see H)
F-, G-	not assigned
-Gn	tentative: “General music,” no theory terms/not theory text? ¹²
H-	Hymnic forms, including all non-epic poetry
-It	Interval term(s), other than Md or St
-Ir	Interval ratio(s), explicitly numeric
K-	Key-Number Table
L-	List (vocabularies and all similar)
-Md	Mode terms
(Nt)	Notation, in case of explicit “score”
-Or	Organological term(s), if specific ¹³
P-	Procedure Text
Q-	Administrative, Legal, Diplomatic, <i>Inscriptions</i>
R-	Ritual (≠ texts used in ritual), Omina, Medicine, Magic ¹⁴

10 Personality named, as involved in music making or theory; otherwise assign to Q-; treatment of colophons, see above (p. 83).

11 Literary category ≠ curricular materials as such.

12 To cover all nontheory passages, after context-category designation; standard names for literary texts may be available to be added after the “Gn.” To qualify for inclusion, a text must at least be descriptive of a musical action, and not only furnish a terminological datum.

13 Instrument(s) mentioned by specific name(s).

14 “Ritual” = nominative; prescription text, generally giving only the agenda and not the legenda (and cantanda). The two aspects will often coalesce in omina, medical and magical texts.

–St	String term(s)
T–	tentative: “Treatise on music,” in discursive form (≠P–)
U–, V–, W–	not assigned (“Varia” — category inadmissible!)
X–	Context category not yet discernible
–Xx	Text not yet divided into units of musical concern
Y–, Z–	not assigned

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Cypro-Archaic I period, 750–600 BCE.

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