MYCENAEAN GREEK: A LESSON IN CRYPTANALYSIS [1]

Cryptanalysis, as you all know, is the solving of secret writing without previous possession of the key. Basically it is a study of coincidences, an analysis of them, to eliminate those which are due to chance and to find the reason for those which are not due to chance. The most important part of the cryptanalyst's method, his standard operating procedure, is the tedious drudgery of counting frequencies and noting repetitions and coincidences. The most important weapon in his arsenal is ingenuity. His ammunition, which he uses without stint, is assumptions, which might perhaps be defined as educated guesses, were it not for the fact that for a cryptanalyst to use the word «guess» instead of «assumption» is to be drummed out of the fraternity. Of the latter the most important usually is the assumption of probable words.

It is a simple matter for a cryptanalyst to «break» an alphabetic-substitution cipher in English in this manner. The rest of the solution becomes routine once the cipher is surely broken. If the probable-word method is used, this may prove tedious and boring. Essentially the solving of Linear B is no more than this. Any real difficulty would lie in getting a start. In an alphabetic cipher it may sometimes be sufficient merely to find the letters E and T. In a syllabary as difficult as Linear B you may need three or four times that many high-frequency syllables. Here too the rest of the solution becomes routine and the probable-word (and -ending) method a long, drawn-out process.

A year ago in February all I knew about Linear B and Mycenaean Greek were the «thats». I knew, for example, *that* Evans had found

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[1] This paper was read at the Forty-Sixth Annual Meeting of the Classical Association of the Pacific States (April 18-19, 1958), held at the University of British Columbia, Vancouver, B. C., under the title, «Mycenaean Greek: An Elementary Lesson in Cryptanalysis». It has been revised especially to remove the elementary part of the lesson and to give greater clarity to the methods used. I owe a debt of profound gratitude to Professor Emmett L. Bennett, Jr., for his careful study of much of my decipherment work and his detailed, constructive criticism, as well as for his role of *advocatus diaboli*.

the tablets at Knossos more than a half century ago. I knew *that* Blegen had found them in quantity at Pylos. I knew *that* Ventris was supposed to have solved the syllabary, but I also knew *that* his solution had been challenged. I knew only the «thats». I knew nothing about Linear B itself.

I obtained the Ventris-Chadwick article, «Evidence» [2], and looked to see what sort of Greek he had obtained. It looked like nonsense, and so I closed the journal. An idea had formed in my mind. In Missoula, Montana, due to the inadequacy of library facilities, almost six hundred miles from the nearest classical library, it is virtually impossible to do any real scholarship. Trying it, is a most aggravating task. But if I could get hold of some of the material, what was to prevent me from trying to solve Linear B «from scratch»? I did not *know* that it was Greek since I took seriously the report that Ventris' solution had been challenged, but it seemed to be a logical assumption and besides it should be possible by cryptanalysis to demonstrate whether it was Greek or not. If it was Greek, cryptanalysis should «break» it quickly, that is, reveal a sufficient number of symbols so that one may begin the assumption of probable words knowing that he is on the right track.

Had I known what a superb job Ventris and associates had done, I do not know whether I would have tried it. Probably not! But afterwards I was thankful that I had not known. You have to be a cryptanalyst to know the thrill that comes with such work. It was worth doing for its own sake.

I decided early in my work that Linear B *was* either Greek or a language closely related to Greek, and after two or three months of studying and counting syllables, I found a way to reveal certain statistical resemblances (coincidences) so as to make them obvious by juxtaposition both of the statistics and especially of diagrams. Breaking of the «cipher» as well as the main part of the solution now became automatic, minutes for the first and days for the second. As it turned out, the value of my work lies more in my methods than in my results. Fortunately, like any cryptanalyst, I have notes which show what I did, and

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[2] Ventris-Chadwick, «Evidence» = Michael Ventris and John Chadwick, «Evidence for Greek Dialect in the Mycenaean Archives», *JHS*, LXXIII (1953), 84-103.

when the opportunity is given to me I will be glad to make them available for publication or give them orally, although the latter could best be done in discussion, with the possibility for those interested to examine the materials themselves and to ask questions as each step is taken up.

When I compared my results with Ventris-Chadwick [3] early last summer [4], I found, aside from minor differences, only one really different value. It was a value I could prove as far as such things are provable. But, located as I am, I was unable to find out whether anyone had suggested and proven this value or not during the year or more for which I had no information. Perhaps I should have sent this material somewhere anyway but I did not choose to take the chance. However, a book has just come off the press, a complete, definitive work on the Mycenae tablets, edited by Bennett with transliterations, and with translations and commentary by John Chadwick. I immediately ordered it and received it a week or two ago [5]. The symbol whereof I speak is still transliterated as **85*.

Since the value is provable once discovered, I may assume that no one has yet discovered it. Therefore I shall give an explanation of the reasoning which led me to new conclusions and the value of this hitherto-undeciphered symbol. I shall give in detail my proof of its value and as lagniappe I shall add to this what I believe to be the first reference to writing in writing by the Greeks in Greek. I shall also summarize briefly in the latter portion of this paper the methods to which I found Linear B amenable. The earlier steps of the cryptanalytic process will be considered there.

Since there is no real proof in this kind of work either in deciphering or in interpretation except when known Greek words, spelled exactly as we expect them to be, are used in such a way in more than one context that the differing contexts prove them, our only proof in most cases is the accumulation of coincidences: *mere* coincidences which happen by

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[3] Michael Ventris and John Chadwick, *Documents in Mycenaean Greek* (Cambridge, 1956). «Ventris-Chadwick» will be used to refer to this work but will be treated grammatically as a plural when the reference is logically to the authors themselves.

[4] 1957. See n. 26 on page 86.

[5] Emmett L. Bennett, Jr. (ed.), «The Mycenae Tablets II», *Trans. Amer. Philos. Soc.*, vol. XLVIII, Part I (March, 1958) = *MT*. N. B. This paper was read in April, 1958.

chance if our assumptions are wrong. This is always true in cryptanalysis until the cipher is surely broken. But if you were working in normal English, the final solving of the cipher would remove all chance coincidence. In the Mycenaean work even correct decipherment does not eliminate a large share of the coincidences which happen by mere chance. We may accept with sureness, as just indicated, all Greek words spelled as we expect and used in varying contexts which serve as proof. The less the contextual proof and the greater the change from known Greek, the greater is the percentage of mere coincidence which is being accepted as Greek. And in cases of far fetched assumptions in the interpretation, the odds are in favor of chance similarity rather than correct interpretation.

I am not therefore impugning Ventris' decipherment when I say that a large share of the translation in the Ventris-Chadwick book is imaginative nonsense. The value of the book lies in the Appendices, in the explanatory material, in the easy availability to scholars of the text of many of the best tablets in transliteration, and in the notes, which are absolutely honest.

In working with the signs of more frequent occurrence the statistical evidence itself was strong evidence in favor of a particular value, and it was comparatively easy to accumulate a sufficient number of coincidences, strong and compelling coincidences, to convince myself that the value assumed was correct — when it was. But as I reached the signs of less frequent occurrence, accumulating coincidences and assessing their value became more and more difficult and at the same time more and more important. For this purpose I counted everything which might point to correct decipherment of a value as a mere coincidence, no matter how obvious-appearing proof of correct decipherment it might seem (even *to-so* and *to-sa* as the totaling words or *po-ro* [*po-lo*] and the colt's head). Naturally most of the coincidences which I found were Greek words or names showing up as a result of my assumed values. Unfortunately I found that I sometimes met with quite a few of these even when working with a value which I had to discard as erroneous. I had to decide that such coincidences were by themselves of not great value except in a negative way: if I did *not* find some, my assumed value was quite sure to be wrong. If my value was to be considered as correct, such items had to be reinforced by more compelling coincidences. Obviously words or names not spelled exactly as I expected them to be had to be reinforced by other argument to be of any value whatever. Of great value I considered context in which the Greek word was acceptable. In the case of names a famous name or Homeric name spelled as expected I considered much stronger evidence than just any Greek name. Of greatest value was context which «proved» the correctly-spelled Greek word extracted. But additional evidence which carried weight could be provided by other coincidences; for example, the same value for the same sign in classical Cypriote and spelling alternations. At the same time I kept this rule in mind: *you may not use as evidence whatever clue or coincidence your assumption is based on.* What that means is: you must make your assumption of a single value for each sign before you begin looking for coincidences to prove an assumption. For before you have made your assumption of a single value for each sign it is much too easy to find coincidences.

You therefore assume a value for one reason or another, it need not be a strong reason, and then you look for additional «proof» that your assumption is correct. You may use as this proof all coincidences except, obviously, any that can be proven to be mere accident. You do *not* have to be able to prove that a coincidence is *not* an accident, if you keep in mind at all times that even if your assumptions of values are all correct a certain percentage of minor coincidences are almost without doubt accidental. You must be able to find not only some minor coincidences but also some other more compelling coincidences before you can accept your assumption as proven in order to allow for the probability of coincidences due to pure chance.

It has been my experience that you will occasionally meet with a startling or compelling coincidence even with one or more values wrongly deciphered. You may find a considerable number of minor coincidences if a perverse fate happens to be tantalizing you at the moment. But unless your assumptions are correct, these coincidences (for they would be mere accidents), especially if they include important ones, are limited in number, and there is a mathematical reason for this. For in mathematics the probabilities are multiplied together as fractions. Thus if you meet a series of events, one of which has 1 chance in 10 of happening by accident, another 1 chance in 100, another 1 in 200, another 1 in 500, and another 1 in 1000, the likelihood of their being *all* mere chance is $1/10 \times 1/100 \times 1/200 \times 1/500 \times 1/1000$, that is, 1 chance in 100,000,000,000. Obviously every coincidence, minor as well as compelling, decreases the likelihood of your coincidental «proof» being accidental; obviously too, without compelling coincidences (as without the larger numbers under the line), you would not get very far in dealing with infrequently-used symbols. But if you have deciphered a symbol correctly, even in Mycenaean Greek with much of its material worthless for proof, while it is difficult to assess the point at which proof can be conceded (if I may be permitted to paraphrase Ventris-Chadwick [page 23]), you may reach the point at which estimation of the odds against the results having been obtained by chance may lead to a relative degree of certainty.

As an example of my own obtaining of negative results thus, I shall mention **23,* which appears to have been accepted as *mu* (Ventris-Chadwick, however: *mu*?). I left that space blank. Yet my frequency count adjusted for an equivalent number of syllables (total, initial, medial, final) was for **23*: 5, 2, 3, 0, and 4, 1, 3, 0 for Greek *mu*. Naturally I tried this symbol as *mu*: however, I did not feel that I obtained sufficient results in the manner described to accept it. Recently I checked Ventris-Chadwick's results, but my opinion did not change. I still doubt the sufficiency of the evidence to warrant acceptance [6].

As an example of the piling up of coincidences with positive results to the point where they become tantamount to proof (as it seems to me), I shall use symbol **65,* especially since, as far as I know, its value has still not been generally agreed upon, although various scholars have at least guessed at its correct value or assumed it as possible for analytical reasons. My cryptanalysis gave me its value as *ju* before consultation

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[6] Someone else may wish to test **23* himself. I bequeath to him my list of words using this symbol. The references are to the numbers of the pages in Bennett's indexes on which the words and their occurrences are listed. *MLB* = Emmett L. Bennett, Jr., *A Minoan Linear B Index* (New Haven, 1953) and implies Knossos; *PT* = idem, *The Pylos Tablets: Texts of the Inscriptions Found, 1939-1954* (Princeton, 1955). In the main part of my decipherment, I depended entirely on *MLB*. When I reached the point where statistical analysis lost its effectiveness due to the decreasing frequency of the remaining symbols, I began to use *PT* also both for the additional vocabulary and for the texts of the tablets. This list contains not necessarily all occurrences of **23* but everything I managed to find worth including in both *MLB* and *PT*. The symbol does not occur in the Mycenae tablets.

da-ra-23-ro, MLB 4; *a-23-ta-wo, a-23-ta-wo-no, PT* 209; *sa-23-ta-jo, MLB* 34, *PT* 220; *qo-ro-23-ro, PT* 220; *ti-23-nu-we, MLB* 36; *pi-ja-23-nu*[, *MLB* 43; *si-23-ta, PT* 226;]*ja-23-ta, MLB* 55; *o-23-ka-ra, MLB* 60; *ki-23-ko, MLB* 63; *ka-23-ko-to, MLB* 72; *23-da, 23-ka-ra,*]*23-ti-ja,*]*23-ki-ti, MLB* 28; *23-to-na, 23-jo-me-no, 23-ti, 23-ti-ri, 23-ta-pi, 23-ko, PT* 217.

of the text. In view of the fewness of its occurrences it was necessary to study its contextual coincidences with great care before acceptance. The results were favorable. When I checked my results against Ventris-Chadwick, I found that they had *not* accepted this value [7].

I assumed that **65* was *ju* on the evidence that it was preceded by *i* in the majority of cases (7 out of 12 possibilities), combined with its low frequency, its non-appearance initially, and two processes of elimination; that is, *ju* was the only space left vacant in my reconstruction skeleton (or «grid») for a *j*-syllable, and other candidates for the position were eliminated [8]. Now observe my list of **65*-words and consider my additional evidence [9].

wi-ju-te-u	*MLB* 44	*a-ju-na*	*MLB* 18
ri-ju-no	*MLB* 53	*a-ju-ma-na-ke*	*MLB* 18
i-ju (i-ju-qe)	*PT* 219	*i-ju-ke-o*	*MLB* 33
e-pi-ju-ko	*PT* 223	*sa-ju*	*MLB* 34
pe-ju-ka	*PT* 239	*e-ju-to*	*PT* 224
di-ju-pa-ta	*MLB* 12		

Only eleven words! Even if the assumption of *ju* is correct, if this group runs true to form, two-thirds should be names (mostly unknown), and one-third words. Of this one-third, perhaps forty per cent should be recognizable Greek words: less than two. Yet two or even three minor coincidences can easily occur by pure chance. We must find

[7] It was interesting to me to note that John Chadwick lists the value *ju* for **65* in the pull-out sheet at the end of his interesting popular account, which appeared after this paper was first written: *The Decipherment of Linear B* (Cambridge University Press, 1958).

[8] The only other candidate that seemed a possibility (for similar reasons) was **48*. I eliminated it at first because it seemed to be a Knossian symbol (I found one word in which it appears in the Pylian tablets), for which reason I assumed that it probably did not belong to the syllabary proper (**65* is definitely both Knossian and Pylian). When I looked for confusions in spelling I found that **48* seemed to alternate with *nu-wa* (or *nu-wo*) in four words and with *wa* in one (five cases out of six or seven possibilities). This eliminated it finally as a possibility for *ju*.

[9] These words are all from *MLB* and *PT*. **65* does not occur in the Mycenaean material. N. B. There is no originality in my assumption of Greek words. What I had noted others had noted too.

compelling coincidences to «prove» a value with so few occurrences. Let us try.

First let us look (as I did) for spelling alternations, possible spelling confusions of the same words which may offer help in identifying the value of the symbol. Compare *wi-ju-te-u* with *wi-ja-te-wo* and *wi-ja--te-we* (*PT* 226). If our assumption of *ju* is correct, we may assume that we are dealing with different forms of the same word. We cannot prove these are the same word, however, and so we must treat this as a coincidence: a four-syllable word which is circumstantial evidence that we may be dealing with a *j*-syllable. And if it is a *j*-syllable we may again assume it to belong in the only empty *j*-space (*ju*), unless we assume a sixth vowel. Alone it would not be very significant as a coincidence since *ja* has a fairly high medial frequency, and we do not yet *know* that our symbol is *ju*. But in view of the following confusion again with *j*- it takes on added significance. When we are dealing with only eleven possibilities, two different confusions with *j*- are not to be expected by accident.

Since *ri*- is an infrequent initial syllable in Greek, it is not surprising to find that some of the entries on *MLB* 52 and 53 are due (apparently) to a single word, a frequently-used place name, *ri-jo-no,* of which the scribes were not always sure of the pronunciation. This time we have a word of only three syllables, but the infrequency of initial *ri* and the fact that we are dealing with the second confusion with a *j*-syllable, this time with a different vowel, lends much weight to the assumption that **65* is a *j*-syllable, and if so, *ju.*

However, in another spelling alternation we have corroboration of the vowel *u*. For in addition to the many occurrences of *ri-jo*-forms, and to *ri-ju-no* and two cases of *ri-ju*[, we have (and should give thanks to the scribe for) *ri-u-no*. This is excellent corroboration, for *u* seldom appears after *i,* since it would be used there only for a digamma or for *ju.* Either **65* is *ju,* or we are being pursued by a very perverse fate.

We have *i-ju* (*i-ju-qe*) where the word «son» makes good context, as Ventris-Chadwick (pp. 176-78, 395) are not alone in thinking. **u-ju* would have been a rare piece of luck, apparent proof of the value all by itself. But we have *i*- not *u*-. Such resemblance in a sign-group of merely two syllables to just any Greek word could be dismissed entirely. The real coincidence lies in the fact that the sign-group resembles an important Greek word of the very meaning that some scholars, at least, want for the context (Ventris included). And the coincidence becomes even more interesting and important because, despite philological arguments against *i-ju* = **u-ju, i-ju* for **u-ju* may be considered as orthographically possible in Mycenaean writing. For there is a tendency in pronunciation to produce an *i*-sound before *j*, as a result of which examples of confusion between the *u*- and *i*- vowel sounds in Mycenaean writing exist[10]. But keep in mind that we cannot assume that *i-ju does* equal **u-ju* (υἱός, «son»). We have a coincidence here, to be sure, but however interesting and important a coincidence, no more than that.

I have, however, been fortunate enough to find another coincidence connected with *i-ju*, a weighty, compelling coincidence which as far as I know has not been previously noted by anyone. Assuming *i-ju* to be the nominative singular of an *u*-stem, was it possible that there existed in the indexes an oblique form or a nominative plural beginning *i-je-*? I did find such a form: *i-je-we* (Tn 316. r10). In the context this word is dative. Adhering to Ventris-Chadwick (page 286) for the remainder, and disregarding the queried and now unwarranted emendation, *i-je-<re?>-we*, we have:

di-ri-mi-jo / di-wo i-je-we GOLD BOWL 1

«To Drimios, the ——— of Zeus: one gold bowl.» We are not dealing now with whether the word means «son» or not. This is a completely separate coincidence. *i-je-we* would be the dative of a nominative **i-ju* in either event. And finding this form is an incredible coincidence unless our assumption of the value *ju* for **65* is correct so that the word we are dealing with really is *i-ju*[11]. By this time one may be forgiven

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10 I explain this point at greater length and give examples on p. 75 below.

11 I tried to estimate in various ways what chance there was of finding *i-je-we* purely by accidental coincidence. (Fortunately there was no nominative in *-e-u* to confuse the issue.) If we treat the word *i-je-we* as an accidental coming together of the three signs, as in estimating the chances of a certain throw of the dice, on the basis of the initial frequency of *i*, medial frequency of *je*, and final frequency of *wa, we, wi*, and *wo* totaled together (for the last syllable should not be limited solely to *we*), in my final count for Mycenaean Greek we have: initial *i*, 186 occurrences in a total of 2721: roughly 1 chance in 15; medial *je*, 38 occurrences in 4723: roughly 1 chance in 124; final *wa* (43), *we* (97), *wi* (5), *wo* (101) = a total of 246 occurrences in 2716: roughly 1 chance in 11. Multiplying $1/15 \times 1/124 \times 1/11$, we get 1/20,460: roughly 1 chance in 20,000 of such an accidental coming together of the three signs. This is not a true estimate

for having complete faith in the value assumed, but to complete the proof one ought to be able to read an actual Greek word with confidence. Or else one might even query, «Of what value the proof, if we *have* proven the value?».

We do have this necessary completion of the proof. Consider first *e-pi-ju-ko* and *pe-ju-ka*, correctly spelled historically for *e-pi-zu-go* and *per-zu-ga*. *per-* for *peri-* is acceptable outside of Attic-Ionic and perhaps before *j* (that is, *i*) even there. ζυγόν (ζυγός) is found in Liddell and Scott listed for technical meanings in carpentry or building. ἐπιζύγιον, ἐπιζυγίς -ίδος, ἐπιζύγωμα, all have meanings which fit into this category. If the two words are adjectives they are both attested: ἐπίζυγος and περίζυξ -υγος. Now consider that the context in which our two words are found is carpentry or building [12]. The coincidences are therefore more than just Greek words, but Greek words in context which «proves» them.

It seems to me that we have reached our «relative degree of certainty» and that we may consider the value *ju* for *65 as proven. But if more proof is needed we have a clincher, *di-ju-pa-ta* (L 1568. 2), διύφαντα, an attested Greek word to which no change need be made, one that deals with *weaving*, on a CLOTH tablet. And again we have a Greek word in context which «proves» it. Obviously, it seems to me, the piling up of coincidences, some of them compelling and convincing in themselves, has proven the value as far as proof is possible in work of this kind, and I see no reason personally why this value should have been questioned by anyone.

My argument, it should be remembered, is *not* that every single coincidence is a non-coincidence. But you will not get such a convincing piling up of coincidences if they are *all* mere chance. It is because the value under discussion is *ju* that I was able to find such

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of our chances because this is not an accidental coming together of the three signs (every language has its linguistic «rules», which would affect the result), as well as for other reasons: e. g., the results would be changed somewhat if we made the count only for 3-syllable words. However, this rough estimate does give us an idea of how unlikely it is that the finding of *i-je-we* was mere accident.

[12] As I have indicated, others have worked with the possibility of the value *ju* for *65 and have noted some of the coincidences. See L. R. Palmer's discussion of the last two words and also of the following word in «Observations on the Linear 'B' Tablets from Mycenae», *Bull. Inst. of Class. Studies Univ. of London*, No. 2, 1955, p. 43.

striking coincidences, along with other scholars, to be sure, some of whom perhaps did not evaluate the picture as a whole.

And now let us turn to the hitherto-undeciphered or erroneously-deciphered symbol whereof I spoke earlier. I have thought of this symbol as Old Pighead (**85*) and of his partner-in-arms as the Hitchhiker (**43*). The most interesting part of their story, I think, is the method by which their values became clear to me.

When I found the values for *i* and *u,* I placed them in the top (open-vowel) row of my reconstruction skeleton, but I continued my search for the *j*-slurred *i* and the *w*-slurred *u.* If they did not exist, there might be a reason why they did not, for the slurs exist in pronunciation and a people who so carefully put them in elsewhere might have put them in, in these cases also. Their existence in pronunciation is obvious; for example, just as dissyllabic *ia* becomes *ija,* so does dissyllabic *aï* become *aji.* And the Mycenaeans were perfectly capable of supplying any symbol they needed, once they decided to replace their own writing system, whatever it was, with that of the Minoans. In addition, I felt that if their syllabary proper were put down in the form of a reconstruction skeleton or «grid», every space would be filled if we could find all the answers.

One of my assumptions in this work was that everything about the syllabary must make sense. There must be a good reason for everything. You may find a good reason and still be wrong but you are more likely to be right then than if you accept anything at all prompted by wishful thinking. And so I continued to look for an *i* to go with *ji* and a *u* to go with *wu,* since I decided ultimately that the *i* and *u* I had found, **28* and **10,* although used for *i* and *u,* were in the syllabary actually *ji* and *wu.*

I could not feel that this last point was really proven unless I found the symbols for *i* and *u,* unless I could fill the two spaces in the top row which were left empty when I moved **28* and **10* to the spaces for *ji* and *wu.* Some evidence I had found, and a thorough search of all the material would probably supply more. For there is a tendency with *j* to produce an *i*-sound before and after, and with *w* to produce an *u*-sound before and after, both of which when slowly pronounced aloud for purposes of writing sometimes cause confusion in pronunciation and therefore in orthography.

Since silent reading and writing had to await its invention a long time even after the alphabet came into use, I was able to make one of my most important assumptions early in the work: the scribes spoke the words aloud or in some manner formed the sounds vocally and wrote and spelled on this basis. Whatever was uttered was written. In any attempt to formulate rules of orthography for Linear B this should be the first «rule». Each sign represents the sound that the individual scribe made approximately at the moment he wrote it. Thus, if a word occurring twice on a tablet was pronounced somewhat differently each time, we may find two different spellings of it on the same tablet.

As a corollary to this I was able to make another assumption which I now found important. The omission of final consonants implied a true syllabary, yet a true syllabary would have made the written language unintelligible even for the Mycenaeans unless hundreds of symbols were used to take care of the consonant-clusters which are necessary for understanding. Finally I realized that I was thinking in terms of a concept which had not yet been invented in Mycenaean times, the syllable as we understand it today. Nor, to be sure, did the Mycenaeans have any of the concepts which the exact terminology developed by scientific linguistics makes available to our thinking. They were obviously aware that words could be broken up into separate sounds: each sign of a group stood for one of these sounds. The signs did not stand for phonemes or graphemes or syllables but for what were perhaps felt by them at their stage of language-understanding as indivisible sounds or minimum, utterable sounds. Each sign of the syllabary stood for what was to them a single, blended sound or a single blend of sounds. It stood for a consonant plus vowel and any additional sound which blended with and modified the vowel.

The Mycenaean scribe was not prevented from including consonants necessary to them (not us) since the extra consonants which are most necessary for understanding, while rapidly attached, are really separate sounds. Each extra consonant in a consonant-cluster which is pronounced with the following vowel is not merely a consonant sound but is rather a consonant-plus-vowel sound expressible by a sign in Linear B. We call both *pan-* in *panta* and *prô-* in *prôta* syllables, but *pan-*, as is shown by their spelling practices, was for the Mycenaeans a single sound or a single blend of sounds, while *prô-* was not. Fortunately anyone can demonstrate the difference between these two types of syllables for himself just by pronouncing the two syllables aloud at speeds varying from normal to very slow. *prô-*, as you thus analyze it aloud, you will find to be two separate sounds of consonant plus vowel each; *pan-* you will not. *prô-* pronounced slowly is easily seen to be *po-ro′*, with a strong emphasis on *ro*. In normal speech the *po-* is pronounced so rapidly that the *o* virtually disappears. Undoubtedly the scribe in writing as well as in reading pronounced the syllables slowly.

It is thus obvious that the use of a vowel in *po-* and with any extra consonant which was spelled out was not a spelling convention. Neither did any spelling convention determine which vowel was used. The sound the scribe actually uttered as he wrote determined the vowel and therefore his choice of sign just as the sounds he actually uttered always determined his choice of signs. This method, as might be expected, led to variations in spelling, and Ventris-Chadwick provides numerous instances. For an excellent example of variation in spelling caused by variation in actual pronunciation see *du-ma*: *da-ma* on page 391. Now try the pronunciation experiment again, pronouncing *dma* at speeds varying from normal to slow. Note that while you can sometimes pronounce it *da-ma′*, at some speeds almost in spite of yourself the sound will come out *du-ma′*. However the syllable seemed to the scribe as he uttered it while writing, *du* or *da*, so he wrote it, and we find examples of both in the tablets.

Returning now to the confusions in pronunciation forced by the natural phonetic laws of the sounds *i-j* and *u-w*, we find, as I have already implied, that they are preserved as evidence for us in the tablets. Note, for example, the frequent confusion between *u* and *wi* before *j*: *me-u-j-* vs. *me-wi-j-* (*PT* 214; *MLB* 24); *di-u-j-* vs. *di-wi-j-* (*PT* 208; *MLB* 11). This cannot be a confusion between what the scribe thought of as an *oo*-sound and a *wi*-sound. That would not make sense. What you have is the conflict in natural pronunciation between the tendency to produce an *u*-sound after *w* and *i*-sound before *j*, a confusion in sound between *wu* and *wi* before *j*. If the scribe were thinking of the symbol as *u*, not *wu*, a confusion of this kind would not take place, and certainly not frequently. Nor may we think of this as an alphabetic switch. These symbols stand for syllabic sounds, and we cannot assume that we have here the *letter* upsilon and the *letter* digamma trading places.

Note also the confusion between *ra-wa-ra-* and *ra-u-ra-* (*PT* 232; *MLB* 58, 59). Repeat the experiment, if you will: pronounce *ra-wra* aloud at different speeds and note how the sound comes out *ra′-wu-ra′* some of the time despite the twentieth-century Mycenaean spelling rules, which the Mycenaeans, of course, never heard of. I repeat: whatever came out the scribe wrote. *wra-* came out *wa-ra′-* or *wu-ra′-* and therefore appears both ways in the spelling. This confusion also would not appear if the scribe thought of the symbol as *oo*, not *wu*, for the confusion is between the vowel sounds and there is no confusion about the *w*-sound. That is there.

A form like *o-u-ru-to* (*PT* 234) is tantamount to proof that **10* is *wu* in the syllabary rather than *u*, despite the fact that it is used for *u*, if Ventris-Chadwick are right (page 189) in their interpretation: *ho wruntoi*, for the important thing there is the *w*, not the vowel, and an *oo*-sound could not replace that in a syllable written as pronounced and would provide definite interference to understanding in reading. On the other hand, the consonant in *ji* and *wu* would not supply much interference to understanding either in writing or reading when they are used as *i* and *u*, even initially. The emphatic but erroneous statement (pages 79 and 189) to the effect that *u* is used for *w* plus silent vowel because there is no sign for *wu* is due, aside from inability to locate a symbol for *u*, to thinking in terms of an alphabet rather than a syllabary [13].

When I moved **10* to the space for *wu*, I also changed **28* to *ji* since it was my feeling that the latter was *ji* with which I began this reasoning, and also because I had felt from the beginning that whatever was true for one was true for the other. Either the two symbols were *u* and *i* or *wu* and *ji*. I trusted my intuition on that score. Others might not, but I had no proof to offer. To «prove» my theory in its entirety I either had to find the symbols for *i* and *u*, aside from **28* and **10*, or I had to find the reason why they were missing. That the symbols for the two empty spaces in the top row of my reconstruction skeleton existed I felt sure.

My cryptanalysis had left me with two extra vowels, obvious vowels, virtually never used except initially: the aforementioned Hitchhiker (**43*) and Old Pighead (**85*). This should be kept in mind as I kept it in mind during all my present reasoning. Over and over again I had tried to make them *i* and *u*, but, of course, I had failed, since they were not.

I turned my attention to the question, what characters in the original Minoan predecessor of Linear B might have been used as a basis for the open vowels. If this predecessor were Semitic, the origin of the Greek

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[13] Additional evidence of the same sort may be found in Ventris-Chadwick, p. 79, *sub lit. w*. Such telescoping as *e-wa-* for *e-wu-wa-* = εὐα- is haplology when the interpretation is correct.

alphabet lent support to the theory of *wu* for *u* and *ji* for *i* and finally aleph-*a* (*'a*) for *a*. But that did not take care of *e* and *o*. As *j* and *w* were both used across the board with all five vowels, was aleph too used similarly with all five? I then remembered that as a beginning student I had been troubled because the Greeks had used both a rough breathing and a smooth breathing. I was taught that the smooth breathing merely meant the absence of an aspirate. Then why did the Greeks not simply leave the vowel without a breathing when there was none? Was it because they thought of all their words which began with vowels as beginning not with or without an aspirate but as beginning with an aspirate or with a certain something else? And was that something else the glottal stop? Other languages which do not use the glottal stop medially or as an actual letter begin initial vowels with a glottal stop, as, for example, German. We even use it in English sometimes, as in an urgent whisper or in saying a word like «absolutely» emphatically.

This would explain *a, e,* and *o,* but did not help me one whit in the matter of *i* and *u*. And I still preferred *ji* for *i*, and *wu* for *u*. The change back and forth of *i* and *j*, and of *w* and *u*, I could understand, I could feel, I could embrace emotionally. But glottal stop to *a*: that was meaningless to me. It was a phonetic shift I could not feel. Yet the Greeks were obviously aware of it.

The Greeks were aware of it! That would explain why the Greeks could go so quickly from the West Semitic «syllabary without vowels» to the vowels alpha, iota, and digamma-upsilon. They had had a centuries-old tradition of using *ji* for *i*, *wu* for *u*, and *'a* for *a*! Glottal stop to alpha and back again! What would happen to Greek vowels if one appended such a sound to them? I wrote down, doodling, as it were, and using ' for the glottal stop, what is shown here:

'α	'ε	'ι	'ο	'υ
αα	αε	αι	αο	αυ

As I puzzled over the letters something struck me. Pause in your reading for a moment and study the shift of ' to α and back again with each of the five vowels and see whether something does not strike you too.

aa, ae, and *ao* would have been meaningless to the Mycenaeans as single blended sounds, and therefore *'a, 'e,* and *'o* became open vowels, perhaps pronounced when initial with a glottal stop, although undoubtedly the glottal stop disappeared when the vowel was used medially or finally.

But *ai* and *au* did make sense as single blended sounds, as we know from later Greek, even if the syllabary shows that the Mycenaeans pronounced upsilon in diphthongs as a distinct enough second sound to cause its inclusion in writing, as the *i* of a diphthong was not. In other words a diphthong with *i* was a single blended sound, but one with *u*, as *eu*, while a blended sound, retained the identity of the *u* so as to be on the borderline between a diphthong and two syllables. *au-*, however, does not seem to occur as an initial diphthong spelled *a-u-* corresponding to the frequent initial *e-u-* and to medial *-a-u-* after a consonant as seen, for example, in *pu-ra-u-to-ro* (dual of πυραύστρα) in Ta 709. 2, which will be discussed as part of the proof of **85*'s value. This fact, I now assumed, was due to the existence and use of a separate sign for open *au*, **85*.

I now again took my two extra vowels, **43* and **85*, and slipped them into the spaces for *i* and *u*, but this time I tried them out as *ai*- and *au*-diphthongs [14]. Since *ai* is much more frequent than *au*, **43* = *ai* was easy to prove, and when I compared my results with Ventris-Chadwick, I discovered that this value had been found by the probable-word method and known from an early date. I also found that the pighead, **85*, as *au* was correct [15]. To demonstrate it to others I have reworked my proofs. I worked with the sixteen items shown here [16].

[14] There is indication that for two separate syllables two symbols were used: *a-u-po-no* (*a*-privative? *MLB* 13) and the consistent *a-i-* (five times) in *a-i-qe-u*, *a-i-qe-wo*, and *a-i-qe-we* (*PT* 209). *a-u-qe* (*MLB* 13: Sd 0402) is an obvious mistake for *o-u-qe*.

[15] Although my reasoning led me to correct and provable results, it is not therefore a necessary conclusion that this train of assumptions was at all points entirely right. Since no counterparts of **43* and **85* have been found among Linear A signs, it is quite possible that the Mycenaeans in adapting the syllabary did not go so far as to change the values of signs for *'i* and *'u*, but rather that they used two new symbols for *ai* and *au*, perhaps suggested by Linear A *'i* and *'u*. In that case symbols for *'i* and *'u* may possibly be extant among the undeciphered, infrequently-used symbols of Linear B.

[16] To avoid confusing those who have worked with Ventris-Chadwick I am using their method of indicating values except for the syllable *au*, which is my assumption. The sign-groups are all from *PT* 245 and *MLB* 78. On the basis of the 1957 Pylos tablet An 1281 (to which Prof. Bennett drew my attention) the sign-group *au-[to-]ja-te-wo* must now be restored as *au-[ke-i-]ja-te-wo* (see *PT 1957* = Carl W. Blegen and Mabel Lang, «The Palace of Nestor Excavations of 1957», *AJA*, LXII [1958], Part II [by Mabel Lang], 183 and 190. Cf. Ub 1318.1,

au-te	*PT*	*au-u-te*	*MLB*
au-ro	*MLB*	*au-ke-wa*	*PT*
au-to-a₂-ta	*PT*	*au-de-pi*	*PT*
au-to-ai₂-ta-ra	*PT*	*au-de-we-sa-qe*	*PT*
au-to-jo[	*PT*	*au-ri-mo-de*	*MLB*
au-to-ai-ta[	*MLB*	*au-ri-jo*	*MLB*
au-[to-]ja-te-wo	*PT*	*au-ta-mo*	*PT*
au-to[	*PT*	*au-ta₂*	*MLB*

A glance at Liddell and Scott shows that initial *au*-words are likely to begin with *auto-*, but I could not expect frequent compounds with *auto-* here. Or could I? In the sixteen words you see six *au* followed by *to* in, possibly, not more than four different words. Since none of these forms occurs where any context can be surely developed, can it by any lucky chance be determined whether even one of these words actually is a compound? Note the fragmentary *au-to-ai-ta[*. This symbol for *ai* occurs 39 times in my count [17], of which 37 are initial. Of the two non-initial occurrences, one seems to be the beginning of the second part of a compound [18], and the other is this. We may accept this sign-group as a compound. We have additional corroboration of

2 [*ibid.*, p. 184]). There was an additional fragmentary word, which perhaps ought to be *au-to-ai-ta[* logically, but is listed by Bennett as *au-ai-ta-to[* (*MLB* 78) and again by Bennett as *]to-au-ai-ta* (*KT* = Bennett, Chadwick, and Ventris [ed.], «The Knossos Tablets», *Bull. Inst. of Class. Studies Univ. of London,* Supplementary Papers, No. 2, 1956, p. 17, C 1582.b).

I used only *MLB* and *PT* in my work, but there are two occurrences of **85* in the Mycenae tablets: *au-ja-to* in Au 102.5 is probably a personal name as has been assumed; *au-te-ra* (αὐστηρά?) appears in the fragmentary Oe 128 (*MT* 86):

au-te-ra WOOL 1
pi-ko-da-ke WOOL 2,

in which it is tempting to see a descriptive contrast with the second word (πεκ-) in the first, but it is more likely, I suppose, that we have here wool for Miss Harsh and Mr. Combtooth.

[17] See n. 16 above for an additional occurrence and the obvious reason for not using it.

[18] Pylos Ng 319.1: *de-we-ro-ai-ko-ra-i-ja; de-we-ro* appears as a separate word (*PT* 228). On *PT* 228 *de-we-ro-a₂-ko-ra-i-ja* should be read with an *ai* for the typographical error, *a₂*. See both of Bennett's versions of the text (*PT* 35 and 182); see also *MLB* 50.

composition in the separate coincidences: for *au-to-ai-ta*[, *ai-ta-ro* (*PT* 227), and for *au-to-a$_2$-ta*, *a$_2$-ta* (*PT* 217; Ventris-Chadwick; Ἄνθας), using the same infrequent symbol, a_2.

By far the largest part of the Mycenaean material is proper nouns [19], mostly personal names. It would mean a great deal, more than anything I have yet mentioned, if *auto-* could be shown to be well-attested as the first part of compound names. But of course! Automedon, Autolycus, Autonoos — and still better: Homeric! If the symbol were not *au*, all this would be incredible coincidence. We have a place name: *au-ri--mo* + *de* (Aulimos?), but without the necessary reference books, I do not know whether it is attested. *-mos*, however, offers no trouble as the end of a place name. As for *Aul-*, Jebb says (Soph. *Elec.* 564): «Aulis was so named from the channel (αὐλός), as other towns were named from αὐλών 'a valley'.» There are three usable words which I feel sure are personal names: 1) *Au-ta-mo*, perfect spelling for αὐθαίμων (αὔθαιμος), «brother, kinsman», 2) *Au-ri-on*, which appears personified (Liddell and Scott), 3) *Au-ke-wa*, Augeas, a famous name — and Homeric. The last I consider a valuable corroborative coincidence.

Of the sign groups which I take to be common nouns let us begin with *au-to-jo*[in Eb 156.2, where I adhere to Bennett's reading of the damaged tablet on page 146 of *PT*. I give the text in full for those who may wish to check the context before reading ahead in the case of this extremely-interesting coincidence:

wo-ze-qe
e-u-ru-wo-ta te-o-jo do-e-ro ka-ma-e-u[
ai-ti-jo-qo e-ke-qe to-jo-qe au-to-jo[ ]*ma*[

Note that our word is preceded by *to-jo-qe*, which Ventris-Chadwick (page 410) translate, «and of this». Now,

1) if we accept *au-to-jo* as the correct reading (I have great faith in Bennett's readings of the tablets and so I was inclined from the beginning to accept it),

2) if we accept *au-to-jo* as the complete word, that is, *-jo* as the last sign of this group (I was inclined to do so, following Ventris-Chadwick),

19 Ventris-Chadwick, p. 92: «At least 65 per cent of the recorded Mycenaean words are proper names...»

3) and if we accept Ventris-Chadwick's translation of *to-jo-qe* as «and of this» (I do; the acceptance of 1 and 2 virtually compels the acceptance of 3 because of the obviousness of the two words together),

we have in *to-jo-qe au-to-jo* the earliest extant occurrence of ὁ αὐτός (in the genitive), «the same». What more beautiful coincidence could anyone ask for? Added to the previous ones it strengthens the case for *au* to the point of sureness, it seems to me. However, we have not one, but two clinchers, Greek words spelled as expected and really «proven» by the context.

There are four more sure words, but two are noun and adjective of the same word so that we have only three to work with: 1) *au-te,* 2) *au-ro,* 3) *au-de-pi* and *au-de-we-sa-qe* [20].

1) *au-te* appears in Ta 709.2, broken at right end (now joined to Ta 712 by a third piece: *PT 1957* 178, 182):

au-te 1 *pu-ra-u-to-ro* 2 *pa*$_2$*-ra-to-ro* 1 *e-*[

as transliterated by Ventris-Chadwick (No. 237) except for the first syllable. They translate, «one *brush,* two fire-tongs, one fire-rake ...». In using the form *puraustrō* (of ἡ πυραύστρα) below, I am in agreement with their first choice: I read the first two words as αὐστήρ and *πυραύστρω. Note the two identical stems side by side and try to estimate their possibility of happening by chance. αὐστήρ is attested as μέτρου ὄνομα (Hesychius), but ἐξαυστήρ is «a flesh-hook for taking meat out of a pot» (see Liddell and Scott for references). Consider this definition in the light of the immediate context within the tablet. We have here the sort of proof it is hard to gainsay.

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[20] In *au-u-te* the scribe may have added *u* from force of habit of adding *u*'s in diphthongs although it was not needed here. On that assumption in Od 666 (*KT* 66):

]*KE-ME-NO* *85*-u-te- a-pe-i-si*
to-so o. WOOL 14

the only occurrence of this word, the possibly facetious thought occurs to me that perhaps so much wool was owing because the measure (αὐστήρ) had been mislaid. I presume that *a-pe-i-si* can be third person singular (εἶμι), but I do not profess even to guess whether the Mycenaeans would have used this verb with such shade of meaning, tense, and voice.

2) *au-ro* (= αὐλός), Sd 0402, is a Homeric word. To save discussion of unnecessary points I quote transliteration and translation from Ventris-Chadwick (No. 270) except for the word *au-ro*: «... horse-(chariot without wheels) ... assembled...»

o-u-qe a-ni-ja po-si

«and there are no bridles attached...»

... *o-u-qe au-ro* ... (= οὔτε αὐλοί).

Consider the Homeric definition, «pipes or grooves (into which the tongue fitted)»; that is, the pipes or grooves, through which the missing reins, and/or other leather would have gone: neither the reins, and so forth, nor the attachments therefor, and *actually missing from the ideogram*! We may consider the value as having been definitely proven. Old Pighead, **85*, equals *au*-diphthong.

By the acrophonic principle, since this symbol seems to be Mycenaean, not Minoan, I obtained indication that my *reasoning* was right. I have placed the symbol in the space for open upsilon. And this symbol is the ideogram for pig: ὗς. The first sound is that of upsilon [21]. There is one chance in eighty of obtaining initial upsilon by accident according to my count.

We may now turn to *au-de-pi* and its adjective *au-de-we-sa-qe*: the Mycenaean references to writing (including *ka-ru*-words, *to-qi*-words, and *so-we-n*-words), and thus the earliest extant references to Greek writing in Greek [22]. Ta 721.1 (Ventris-Chadwick, No. 245) reads as follows:

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[21] Study of the syllabary as used in available texts makes it obvious that the Mycenaeans paid no attention to the aspirate for writing purposes.

[22] I shall continue to adhere to Ventris-Chadwick except for the change from **85* to *au* and shall give all the references to their numbering. The occurrences of *au-de-pi* and *au-de-we-sa-qe* are listed on *PT* 245; of *ka-ru*-words, *PT* 241; of *to-qi*-words, *PT* 207; of *so-we-n*-words, *PT* 214. Of the tablets involved (all Pylos Ta) only Ta 210 is not given in Ventris-Chadwick: *ta-ra-nu a-ja-me-no e-re-pa-te-jo au-de-pi so-we-no-qe* FOOTSTOOL 1, «One footstool inlaid with ivory speech-signs and -sounds (?)». Aside from this tablet, *au-de-pi* appears in Ventris-Chadwick, Nos. 239, 242, 245; *au-de-we-sa-qe*, 237; *ka-ru-we--qe*, 235; *ka-ru-pi*, 246; *to-qi-de*, 239, 240; *to-qi-de-qe*, 235; *to-qi-de-jo*, 241; *to-qi-de-ja*, 237; *to-qi-de-we-sa*, 235; *so-we-no-qe*, 245, 246; *so-we-ne-ja*, 237.

ta-ra-nu a-ja-me-no e-re-pa-te-jo au-de-pi to-qi-de-qe
ka-ru-we-qe FOOTSTOOL 1.

«One footstool inlaid with ivory...» Note that *au-de-pi* is obviously instrumental plural and may be considered as *audesphi*. Ventris-Chadwick (page 338) are in agreement with this except for *au-*.

The question now is, how may a people refer to writing when writing first appears and when writing is done by means of a syllabary? I would answer, 1) by the method whereby it is inscribed, which has given us, for example, the feminine γραφή, «writing», and the neuter γράμμα, «a letter»; 2) by reference to what the writing portrays; in other words, «voice, speech, sounds», which the syllabary certainly represents, «words, ideograms», and so forth. Corresponding to γραφή, we have αὐδή, «voice, speech», and also γῆρυς -υος, «voice, speech». And corresponding to γράμμα, we have, I think, the neuter related to αὐδή here. The nominative singular of *au-de-pi* would probably be **au-de* (later it might have been *αὖδος -εος).

We are not discussing whether the word exists: here it is. The only question is its meaning. I call it, roughly, «speech-symbol, sign, syllable, ideogram». The translation of this line would be, and what incredible coincidences if we are wrong!, «One footstool inlaid with speech-symbols, and with *to-qi-de* (*stoi-chi-*), and with (speech-) writing.» For *ka-ru-we* (*ga-ru-we*), of course, is a correct fit as to spelling, declension, and feminine gender for γῆρυς, «speech, voice», as Ventris-Chadwick realized but thought «nonsensical» (page 345). The form and spelling, *stoi-chi-de*, is not attested Greek, but the root was, a tremendous favorite with the Greeks, and is frequently associated with *writing*! Possibly it is here a row of figures or a line [23]. If the accepted derivation is right, this would not be the only case of *qi* for *ki* [24]. And is it mere chance that this word has a related stem which may refer to «sounds», στοιχεῖα?

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[23] See Ventris-Chadwick, Fig. 23 on p. 346, in favor of this, and for the contrary, pp. 335-36, where they make their own suggestions *re* this word.

[24] 1) *qi-si-pe-e* = *xiphee*, «two swords» (Ta 716; Ventris-Chadwick, No. 247 and p. 407: credited to Bennett); 2) the symbol *qi* = *ki* by the acrophonic principle as the first sound of κριός (I assume the symbol to be Mycenaean, not Minoan); 3) *to-qi-* = *stoi-chi-* here, an assumption of a non-attested form based on context. The use of the labiovelar seems to be in a state of more or less complete confusion. At the present stage of my studies I assume that there is similarity in pronunciation to (and therefore confusion in writing with) *p* before

Line 2 of the same tablet (Ta 721) has the word *so-we-no* in the same context. It is my opinion that chance coincidence, mere similarity of sound, plays too great a part in the farfetched assumptions and mere guesses frequently indulged in, in work of this sort, and I hesitate to point out that *so-we-no* may be related to the IE word for «sound», which has an *sw-* in its history (Ernout-Meillet). However, there is no question that in this second line *so-we-no-qe* replaces, in a virtually identical line, *ka-ru-we-qe* of the first line:

ta-ra-nu-we a-ja-me-no e-re-pa-te-jo au-de-pi
so-we-no-qe to-qi-de-qe FOOTSTOOL 3,

lending support to the idea that it too is a word which represents the portrayal of speech or sounds. The coincidence of four words out of four is too much to be *all* chance. At the least, there can be no question concerning *ka-ru-we* (*ka-ru-pi*), «speech-writing», and *au-de-pi* «speech-symbols».

In these two lines then and elsewhere in the Ta tablets (for example, 722.3: *ta-ra-nu a-ja-me-no e-re-pa-te-ja-pi ka-ru-pi*; 709 [712].2: *so-we--ne-ja au-de-we-sa-qe*; 713.1, 2: *qe-qi-no-me-na to-qi-de*) we have footstools, tables, and so forth, inlaid with or decorated with such things as syllabary signs, ideograms, written words, a row of figures or a line, and so forth, in ivory usually. Nor is the possibility excluded that one of these words refers to numbers.

At any rate, note this one last point. Certainly if the footstools were inlaid with *speech* (*ga-ru-we* [Homeric γῆρυς]) in ivory, it has to have been *written*: written speech.

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a, e, o, and consonants, *k* before *i* and *u,* but also *s* before *e* and *i* (see L. R. Palmer, «Observations», *loc. cit.* and n. 19). At times, too, the *q*-row as a whole may have been confused with the *p*-row (it is possible that pu_2 should be placed in the *qu*-position in the grid). However, I accept the confusion *ra-qi--ti-ra*$_2$ for *ra-pi-ti-ra*$_2$ in Pylos Ab 356 as evidence for a sound resembling *p* of the labiovelar before a consonant in *normal* pronunciation. When the scribe in his slow pronunciation aloud, as he wrote, actually said, *ki,* before a consonant plus *i,* it could become a true syllable for him, and I accept the confusion shown by 1) *qi-si- (xi-)* and 2) *qi-ri (kri-)* as evidence of a sound resembling *k* of the labiovelar before *i.* Example 2) seems especially obvious to me because once the initial sound of χριός was thought of for possible use in the syllabary, even before it was placed in it, it would have instantaneously become *ki,* capable of confusion with the *ki*-like sound of *qi,* and no longer would have been «vowelless» *k* before *ṛi.*

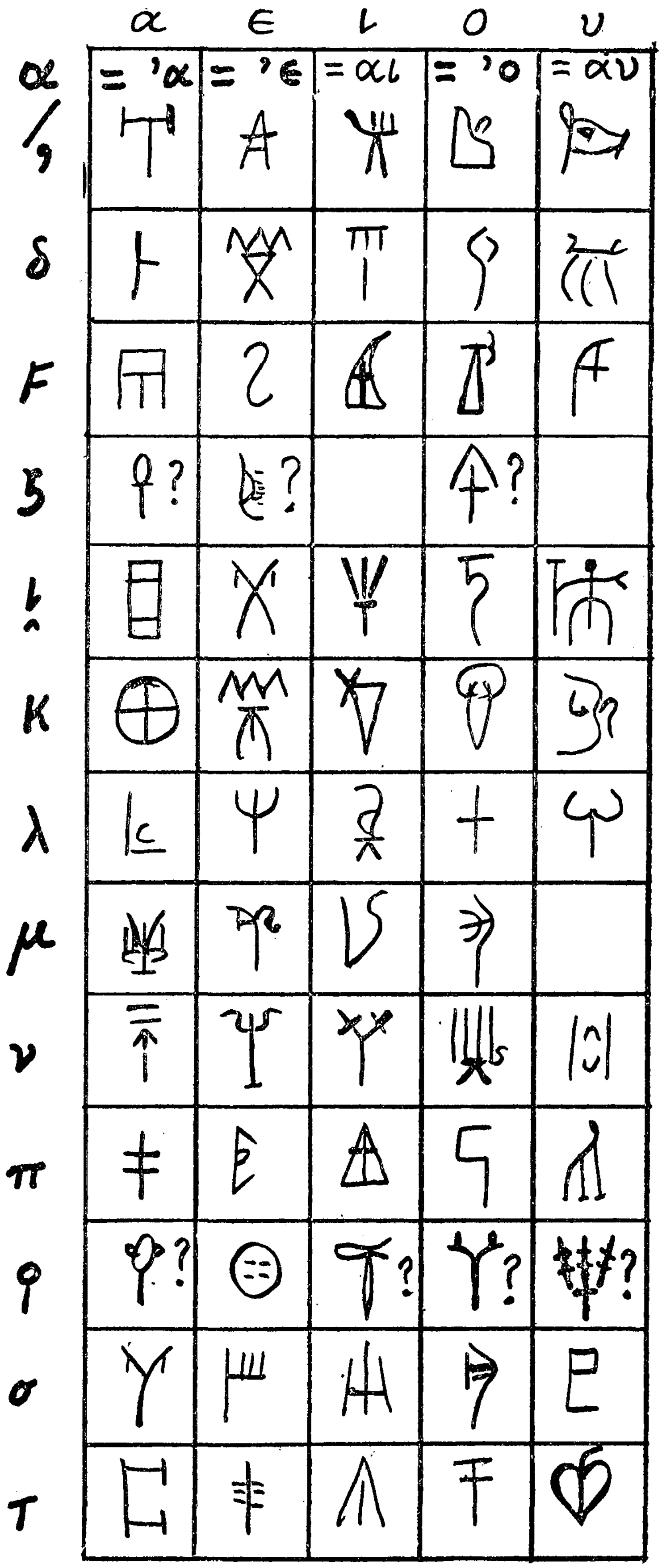

Fig. 1. The Mycenaean syllabary proper (incomplete)

And now that I have described in detail a small part of my work, I shall endeavor to round out the cryptanalytic picture in brief, explaining my methods but with a minimum of detail. I shall try to illustrate what cryptanalysis can do and how one who has never worked on Linear B could by cryptanalysis find out the values of most of the symbols for himself [25]. Figure 1 shows my own results by these methods [26].

Perhaps as a bit of a *tour de force* but also in order to test their worth individually, I worked separately with the different methods I used or noted and shall therefore deal with them separately, but it must be kept in mind that ordinarily the cryptanalyst would dovetail all methods he could think of and not handicap himself by working with each one by itself. It should be particularly noted that I had intended originally to use what I have called methods 2 and 3 together almost as a single method, and any test of their validity should combine the two. The probable-word method, that old stand-by and most useful adjunct of cryptanalysis, I have called method 1, since at first I had intended to depend primarily upon it. In an inflected language probable endings may be included with this method. After assuring myself that it would work, however, I dropped this method entirely, that is, until the syllabary was surely broken by statistical methods and all the more frequent signs had been

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[25] I cannot, of course, in this paper go into all aspects of the solution, the preliminary study, the false steps, the individual questions which troubled me. I had to think through the matter of dialect, find out by statistical experiments with negative results that consonants at the ends of syllables were omitted, make new assumptions, and always assume and test: assume and test—and throw away.

[26] The question marks in Fig. 1 do not indicate doubt of Ventris' solution. They and the figure as a whole show the state of my reconstruction skeleton at the point where I ended my *independent* decipherment study and compared my results with those of Ventris. Although comparable to the famous «grid», this reconstruction skeleton is a cellular structure which was prepared to receive the various signs *after* they were solved. My placing of the infrequent symbols is not necessarily a corroboration of Ventris: 1) statistical analysis is accurate and dependable only with high frequencies; 2) if we each assigned a value to an infrequent symbol on the basis of obvious clues in the text, we would be likely to agree, right or wrong; 3) insofar as I found my results identical with those of Ventris I did not check them any further. I have left my values in Greek to illustrate my own method. I used λ for both liquids, not knowing, of course, that Ventris had used *r*. The figure does not include some obvious values which I had deciphered at the time but which do not seem to belong to the syllabary proper; e. g., **25, *33, *76, *62*, etc.

given (correct) values. I accepted the handicap of dropping this always-helpful method for good reason.

It was a glance at the latter pages of Ventris-Chadwick, «Evidence», at a conglomeration of hyphenated syllables which was supposed to represent Greek, without knowledge of the material and without study, that caused me to assume that Ventris had not solved the language of the tablets. Later I realized that if there had not been a sound basis for acceptance of the solution, it would not have been generally accepted. Then when I discovered by statistical study that final *s* and *n*, and presumably the final consonants of syllables, and diphthongal *i* were not included in the writing (assuming the language as Greek), I realized how foolish I had been to judge so precipitately. Of course, the transliteration at first glance would look like a complete mess rather than Greek. But I decided to go ahead with my attempt at a solution, not only for my own satisfaction because I was now interested, but also because I might get somewhat differing results even if the original solution was right in the main. I then became troubled by the fact that if I continued by the probable-word method I would have nothing to offer as proof of my work, nothing which would give anyone reason to believe me, and so nothing which would be of value to others, if my solution were to prove essentially the same as Ventris' [27]. I had no evidence as yet that any assumptions of mine were right, and so, no matter what I guessed, I could switch to another method. I decided instead of trying to use frequencies and other statistics as a helpful guide to the probable word, to try to use them for actual «breaking» of the code and insofar as possible for actual solution (methods 2 and 3).

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[27] Unprovable claims may be part of my story but do not become a part of scholarship. Only that which may be checked by other scholars becomes that. After Ventris' work was made public, proof of an independent solution became an impossibility except by producing a different set of values. I have not done so. Therefore the only aspect of my work which could be of value to scholars is my methods, and my future interest will lie in giving a complete elucidation of them (which the necessary limitations of this paper prevent) and in the judgment of scholars as to whether they can provide value for the future and corroboration for the present. I feel confident that others can use my methods to obtain similar results, despite individual variations and differences in inspiration, intuition, and, most important of all, luck. This would provide corroboration of my work, which would then be a corroboration of Ventris' solution since the values of all the more frequent symbols were extracted by me by totally different methods.

I shall try to illustrate how a cryptanalyst might go about dovetailing the methods available to him as each supplies him with clues, by pointing out the assumptions I had been prepared to make before I decided to forego the probable word. By my methods you begin trying for actual solution, that is, to «break» the code, from the moment you begin work on it. You would therefore make assumptions, test, fail, and throw away. Since you do not know *how* to make your counts, you would make short, casual, even careless counts. Your first attempts would not work, and so why waste too much time? But they would give you ideas. Fortunately, for a start, you have the classical Cypriote syllabary [28] whereby to guide yourself: more error, to be sure, but a comparison of the two syllabaries shows that there is a relationship there. You should assume error to begin with and each time a new clue reveals to you how to improve your analytical study you should do the necessary work over again. Not until you begin to have faith in what you are doing would you begin the long, careful counts that you will need.

So I worked and before long it was obvious that **8, *38,* and **61* were *a, e,* and *o,* although not till I made my final counts could I be absolutely sure which was which. Meanwhile I obtained a copy of the Mycenae tablets [29] and saw what Mycenaean texts were like: Au 102 was the first tablet I studied. I noted that **78* seemed to be «and». My counts showed that it could not be *te,* but Buck's *Dialects* showed that as late as the fifth century B.C. an Arcadian inscription still used a differentiating letter (*san*) before the epsilon: a clue? Obtaining a loan of *Scripta Minoa* II, I tried the CCS values of those symbols which obviously resembled Linear B symbols as I studied the contents of the tablets, and thus I found *po-lo* and the colt's head (Ca 895): a real clue! The tremendous frequency of **36* and its feminine counterpart **57* (as study of the texts revealed) in final position sent me to the reverse index, which showed that they must be non-initial *o* and *a*: *o* and *a* after vowels? Buck's *Dialects* showed that a better assumption was *o* and *a* after *i*

[28] CCS will be used hereafter for «classical Cypriote syllabary».

[29] Emmett L. Bennett, Jr., «The Mycenae Tablets, a Transcription», *Proc. Amer. Philos. Soc.,* XCVII (1953), 422-70. Other works referred to in this section are: A. J. Evans, *Scripta Minoa,* vol. II: *The Archives of Knossos,* ed. J. L. Myres (Oxford, 1952); C. D. Buck, *Introduction to the Study of the Greek Dialects* (2nd ed.; Boston, 1928); idem, *Comparative Grammar of Greek and Latin* (Chicago, 1933); C. D. Buck and W. Petersen, *A Reverse Index of Greek Nouns and Adjectives* (Chicago, n. d.).

with the *j*-slur. Occurrences of final **36-*36* thus became the ending *oio* after *i*: *jo-jo*. Another good clue! I speculated concerning the totaling word **5-*12*. Reading Homer I ran into the word τόσσος. A clue? The feminine was **5-*31*. The frequencies of the three symbols were good for *to, so,* and *sa*. What then about **5-*12-*45* and **5-*31-*45*? *-de*? I assumed that. Buck's *Grammar* gave κόρϝος as an example of the digamma, and I could not keep from thinking of the child group, **70-*42*. The frequencies corroborated *ko*. I could then assume **70-*54* as the feminine *ko-wa*. Going back to Au 102.14 and assuming *a* for the first syllable, by the previous assumptions you would now have: *a-to-po-*32*. Play around with that word. Is it possible we have another labiovelar (with *o*) to add to **78* («and»)? And now the further you would look with the aid of these, if right, the more possibilities would show up. And add to this the possible results of an analytical study of the final syllables and a careful study of the ideograms for probable-value and -word assumptions. But this is sufficient to illustrate what might happen and what did happen as I studied in preparation for the real task.

As I said, I dropped the idea of using the probable-word method, and so these assumptions remained mere guesses, for I had done nothing to check their validity. It had given me ideas, however. I now assumed that in analyzing frequencies I must cope with a row each of digammas, labiovelars, and *j*-slurs after *i*. Other errors in the original assumptions had to await analytical study of my final frequency counts for their correction; for example, the separation of *d* and *t* and the combining of *l* and *r*.

I now turned my attention to the problem of preparing frequency counts of Greek and of Linear B which would offer a valid comparison. In making frequency counts it is important to use the same type of material for your plain-language counts as for the material you are trying to solve, since the high-frequency words help determine the letter- and syllable-frequencies. If the wrong kind of material is used, too many of the high-frequency words will be different, and these differences may distort the frequencies you are studying to the point of disguising them. This I found to be the outstanding problem in any attempt to use frequency comparisons in the solution of Linear B. There was no possibility of making a frequency count of any comparable material and all attempts to make efficient use of frequency counts in solution were doomed to failure from the outset. I made a turn in the right direction when I made a *subjective* syllable-count of Arcadian and Cyprian material given in Buck's *Dialects*: I skipped repeated material, omitted one-syllable words, and whenever a word was repeated I stopped counting it (except for changed forms) after a couple of repetitions or so. I made the same type of subjective count for the Pylian tablets. Thus I prevented the repeated words of each (which were not likely to be the same words) from distorting the frequencies as much as they might have for purposes of comparison.

In making my Greek counts I kept all consonants and all vowels separate including eta and omega. My reason was that there seemed to be too many symbols in Linear B if the consonants were combined as in the CCS, even if we allowed for the considerable number of foreign-languages symbols that the Greeks were presumably forced to use. I was still using this method when I made my final counts. Having made my count, I would then experiment with the combining of different groups, favoring CCS combinations since the CCS was my starting point. Experimenting thus, and comparing my results with the subjective Pylian count for total, initial, medial, and final frequencies as well as for order of frequency, total, initial, medial, and final, I obtained some interesting resemblances of pattern. I had not yet invented the method I used later of drawing the patterns on graph paper. Visualizing the patterns mentally gave me «better» results than accurate drawings would have, thus misleading me, fortunately, to my next experiment.

For this experiment, my final counts, I took *MLB* and counted every group in the index except fragments which might be partial repetitions of other groups. I counted each group just once, no matter how many times it appeared in the tablets. In my Greek count also I counted each word only once, allowing more than one form for Greek words I assumed to be important since *MLB* too includes more than one form of some words. For my Greek count I prepared by a mostly-random, but partially-subjective, method a vocabulary somewhat comparable to the type of vocabulary which I assumed from previous study to be in the Mycenaean tablets. I used Homer, for I considered that Homer represented the language of an earlier day frozen at a certain time, with two changes, those which the conservatism of the epic bards could not keep out, due to time and place, and the all-important changes in pronunciation, which in those days could hardly have been prevented. In Homer then we should have a reasonable representative of the language of the Mycenaeans if we could change the sounds back to a sufficient degree so that the pattern of frequency would not be altered out of all recognition. It would not be necessary to attain perfection. In cryptanalysis one certainly has to expect variations of twenty per cent in frequencies. Accuracy and finding the sort of material that will supply exceptional goodness of fit will help make the work easier, but even then only up to a certain point. Beyond that you are working with the law of averages and must not expect too much of that law.

I took my Homeric dictionary [30] and went through it from beginning to end, picking out for the most part concrete and proper nouns, and adjectives which could modify such nouns, making some attempt to space the words I took with a degree of evenness. I was careful to include one or more forms of words which seemed to me to be the sort likely to exist in the tablets as well as actual words I had already assumed. This latter was an error in method, for I prepared only 2500 syllables to compare with 10,000 plus, and thus I included by non-random method in each case the equivalent of four times as many syllables as I had intended. On the average both the advantage and the error were negligible. Yet, after further thought, I can say that ideally one ought to include the same number of syllables by this method as in his code count. And one ought to learn all the possible word-content of the tablets beforehand by a study of them, and especially the ideograms, before beginning to prepare the Greek vocabulary. All Homeric proper nouns should probably be included. To keep the choice random one should probably take the words from Homeric text in the order of appearance, choosing those that seem best on the basis of previous study of the tablets. One should probably include non-Homeric words that seem good for the tablet-content, perhaps limiting himself to those attested early. If one takes his words directly from the Homeric vocabulary as I did, he should space them as rigidly as possible the same distance apart. I was rather careless about that because I was not quite sure of what I was doing when I started. And if at the time of preparing his final count one has some ideas of actual words in the Mycenaean texts, he should include them.

I assumed that most of the words should be left in the nominative singular, but I made some changes, usually at random, to nominative

[30] Georg Autenrieth, *A Homeric Dictionary,* transl. and ed. by Robert P. Keep (New York, 1888).

plurals, genitive singulars, and other oblique case-endings. I tried to include a little of everything I could think of; for example, I added a few *si*'s, *phi*'s, *qe*'s, and *de*'s. What was more important was to change etas back to original alphas, put back digammas and restore koppas (labiovelars) as far as possible. Except for the change from eta to alpha I

TABLE I

COMPARISON OF GREEK AND *MLB* COUNTS

	MLB COUNT									GREEK COUNT							
	Frequencies				Order of Freq.					Frequencies				Order of Freq.			
B	T	I	M	F	T	I	M	F	GS	T	I	M	F	T	I	M	F
36	111	1	19	91	1	60	27	1	α	144	134	2	8	1	1	55	28
8	100	90	5	4	2	1	55	32	λο	119	1	53	65	2	50	3	2
59	93	13	40	40	3	14	5	4	λα	106	12	64	30	3	18	2	8
2	90	3	40	47	4	48	5	3	το	104	16	33	55	4	14	9	3
60	83	13	57	12	5	14	1	16	λε	97	10	73	14	5	20	1	15
57	82	3	29	50	6	48	12	2	ịο	94	0	10	84	6	55	34	1
38	79	59	12	8	7	2	40	24	τε	91	24	47	20	7	9	4	12
5	79	12	29	39	7	17	12	5	χο	91	24	37	30	7	9	7	8
4	75	15	44	16	9	11	4	14	ε	79	71	4	4	9	2	45	37
70	70	22	29	19	10	5	12	11	τα	76	15	23	38	11	15	15	4
27	68	8	51	9	11	30	3	22	ο	76	38	6	32	11	4	41	7
10	67	9	34	25	12	25	8	7	υ	76	9	39	28	11	22	6	11
61	62	33	9	20	13	3	45	10	χα	71	31	33	7	13	5	9	32
42	61	11	25	25	14	21	17	7	πο	70	41	16	13	14	3	26	17
53	60	5	52	3	15	40	2	35	χε	69	21	37	11	15	11	7	20
77	60	27	26	8	15	4	15	24	λι	60	4	46	10	16	35	5	23
75	59	8	27	24	17	30	14	9	Fο	60	9	22	29	16	22	17	10
44	59	18	36	5	17	8	7	30	πα	57	30	18	9	18	6	21	25
52	51	2	15	33	19	50	32	6	ịα	52	0	18	34	19	55	21	6
28	46	18	21	7	20	8	24	26	με	51	25	25	1	20	8	12	50
41	45	7	30	9	21	33	10	22	νο	48	0	12	36	21	55	30	5
54	44	10	24	11	22	22	18	18	πε	46	27	18	3	22	7	21	40
37	43	7	32	4	23	33	9	32	δα	46	17	25	4	22	13	12	37
39	42	12	21	9	24	17	24	22	ι	43	21	16	6	24	11	26	33
6	41	3	26	12	25	48	15	16	σι	42	3	26	13	25	40	11	17

Each group is listed in its own order of total frequency. The *MLB* count has been proportionately reduced to approximately 2500 syllables. B = Bennett's numbering. T I M F = total, initial, medial, final. GS = Greek syllables.

did not waste very much time, and I did very little to proper nouns since I had no work of reference available dealing with them. It was just as well that I wasted little time: the labiovelar was in a positive state of confusion, especially before *a*, and the digamma had already begun to disappear. It would have been futile to try to do a perfect job for purposes of comparison.

I made my final Greek-syllable count from this prepared list of Homeric words, and thus obtained total, initial, medial, and final frequencies for Greek to compare with those from my *MLB* count proportionately reduced to approximately 2500 syllables. I also estimated the order of frequency for each syllable for total, initial, medial, and final use as part of my comparative study. In Table I this comparison is illustrated with the twenty-five most frequent syllables of each group. I show the values for the Greek syllables as I finally corrected them after separating *d* and *t* and combining *l* and *r* [31].

I had been studying the patterns of frequencies and of orders of frequency for total, initial, medial, and final use from the beginning, but mentally only. It was not until after I had made my final counts that I invented a method of portraying a picture of the pattern for the frequencies of each syllable as well as for the order of frequency by drawing them as accurately as possible on graph paper according to the figures given in Table I (but with the values for *l* and *r* listed uncombined). I received a bad shock. I had stumbled upon something breathtaking, a method whereby almost at a glance a sufficient number of values became apparent to «break» the syllabary by matching the patterns, especially of order of frequency, of the Mycenaean signs and the Greek syllables. And one of the matched pairs destroyed a most cherished and long-cherished assumption. **2,* CCS *lo,* which I was confident was *lo*

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31 I made my count (as I always did) keeping all consonants and vowels separate as in Classical Greek. I then experimented with different combinations but came back to the combinations of the CCS except for separating *d* and *t*. Since I had confidence in the probable words *to-so* and *to-so-de,* I could assume **5* = *to* and its probable feminine counterpart **59* = *ta.* Consult Table I for their frequencies. If our assumptions are correct you would not add the frequencies of *do* (32 14 12 6) and *da* to those of *to* and *ta.* Moreover, since I felt sure that Mycenaean **11* and **2* had their CCS values *(po* and *lo),* I had expected that **1* would have its CCS value *(da/ta).* But I had already assumed a sign for *ta.* I compared the counts for Greek *da* and for **1:* 46 17 25 4 *vs.* 38 14 19 5. **1* therefore was probably *da* and again I could assume that Mycenaean had separate symbols for *d* and *t.* From *to-so-de* I assumed that **45* = *de.* Their frequencies were 30 5 9 15 *vs.* 29 6 4 19. Obviously **45* could not be *de* combined with the high-frequency *te.* I kept the *d* and *t* separate with confidence, buoyed up because my theory that more consonants had to be kept separate than in the CCS was in this instance true. The shock of having to combine *l* and *r* did not come until after the pattern study.

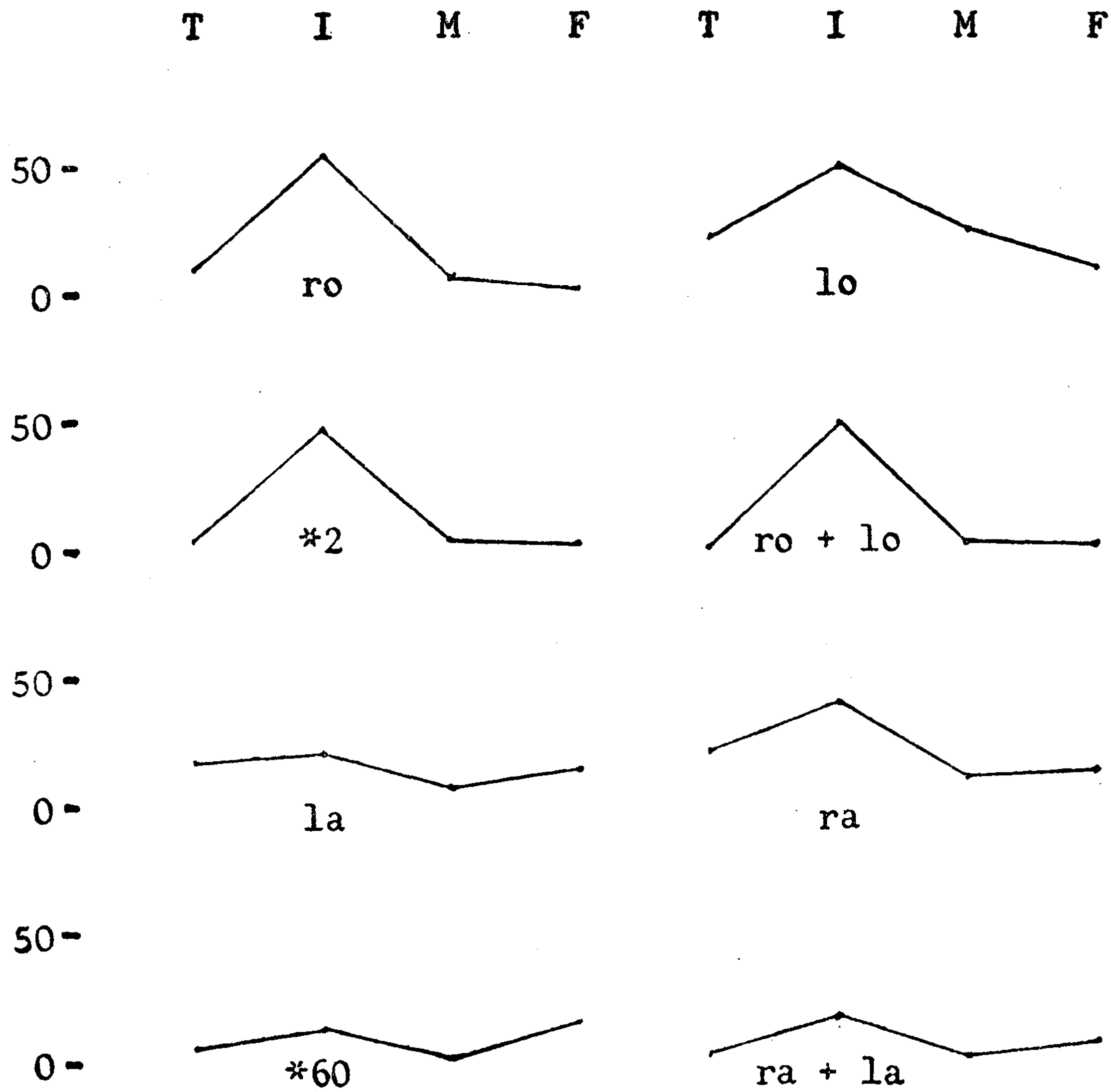

Fig. 2. Patterns of order of frequency illustrating the combining of *l* and *r*. Each horizontal line equals 5; for example, *2 reads for T I M F: 4 48 5 3.

with a confidence built in by feeling sure of it for several months, seemed obviously to be *ro*, not *lo*, according to the graphs (as may be easily seen from Figure 2). **60*, the feminine counterpart of *2 as a final syllable, however, resembled *la* more than it did *ra*. I was sure that the consonant was *l*, the masculine turned out to be *r*, and the feminine *l*. Both *2 and **60* had too high a frequency for either *l* or *r*. And searching for the answer to the riddle I noticed that I had too many graphs of the *l*- and *r*-pattern as compared with the same type of pattern in the Mycenaean columns. Ridiculous though it seemed to me, the vagrant thought, the remote possibility, did occur to me that the answer to the puzzle might be that **2* was both *lo* and *ro* and **60* was both *la* and *ra*. It is

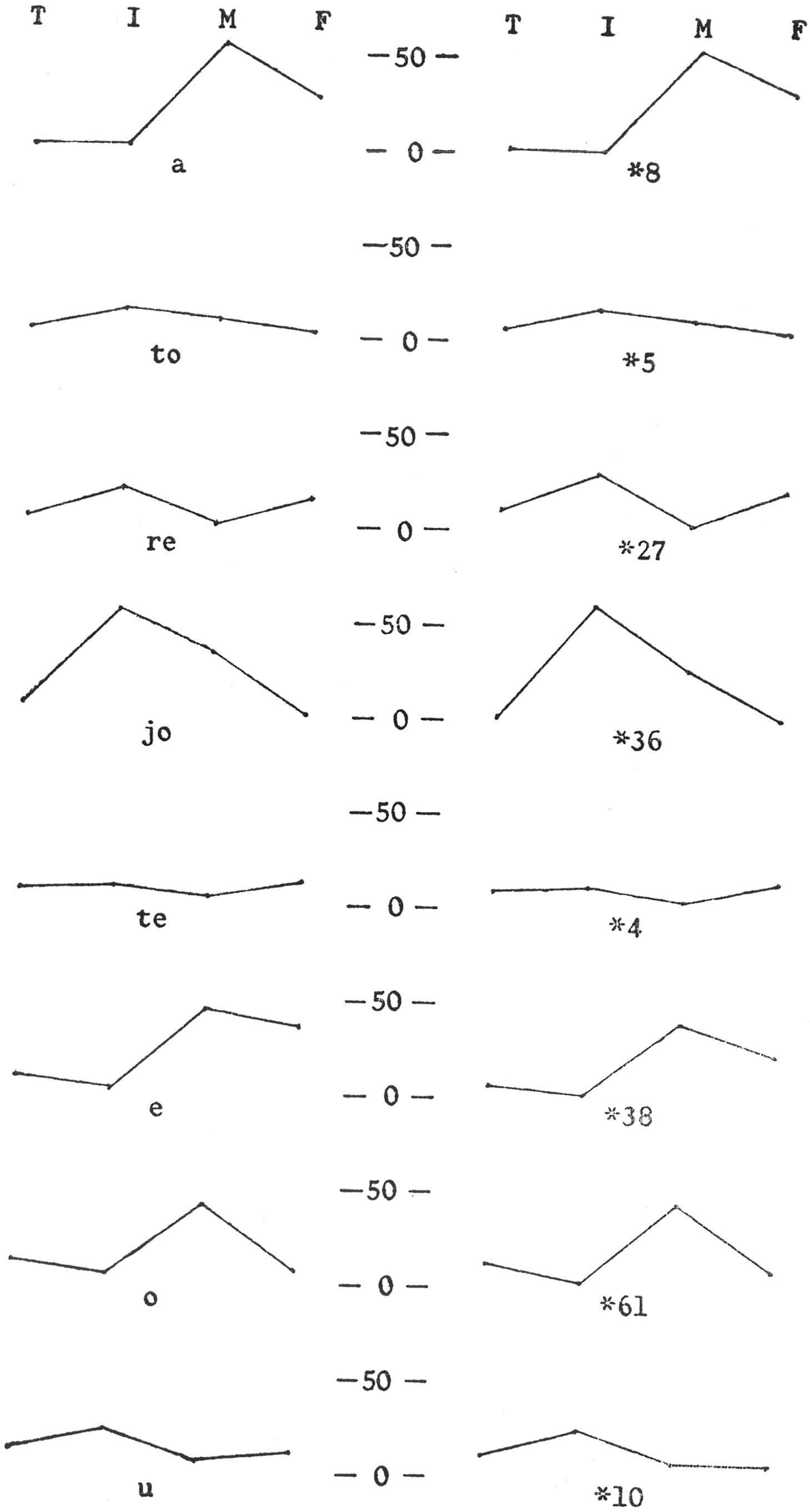

Fig. 3. Patterns of order of frequency illustrating matched pairs

standard operating procedure to test every possibility in cryptanalysis, and so I tried combining the frequencies of the *l*- and *r*-syllables for each vowel. *lo* combined with *ro*, and *la* combined with *ra*, gave me perfect fits for *2 and **60*, and I was easily able to locate Mycenaean *le* (*re*) and *li* (*ri*), of which I had had no previous inkling.

In Figure 3 I give as examples eight more matched pairs resulting from a study of the twelve most frequent Greek syllables, making ten with *lo* and *la* (Figure 2). Two of the twelve required more careful study and the aid of the frequency patterns and of the process of elimination for correct evaluation. Seven more almost-perfect fits could be spotted, however, from a study of the next thirteen Greek syllables. One could be sure immediately of the values of about fifteen signs, but by careful study, comparison of the drawings of the patterns of frequency, and a check of the groups in *MLB* for probable pairs of masculine and feminine endings one could raise the total well above the nineteen already mentioned. Yet the ten most frequent would have been sufficient, I am sure, to «break» the cipher. Naturally if one studied the text for probable words and added a study of the CCS endings one could increase the number of probable correct-values very quickly. I did not go any further with this method but I made a quick check to ascertain whether I was on the right track: I lined up against these results by method 2 all the assumptions I had been prepared to make earlier for the probable-word method and as a result of study of CCS. Insofar as the signs overlapped, the results were identical. The cipher, therefore, was «broken».

I had intended, however, to break it by method 3 with the help of method 2, and so now, as a *tour de force* and for whatever value there might be in the experiment, I laid these results aside and went to that method, using as an aid the frequency study illustrated in Table I. For this method I had prepared two cellular structures, rectangular skeletons, for each Mycenaean sign and for each Greek syllable, one for initial and final use, the other for medial and total use, and with their aid I had made a count of every syllable which preceded and followed every syllable in *MLB* and in my Homeric vocabulary of 2500 syllables. Even though I used a simpler method for the little-used syllables and signs, I still prepared almost three hundred rectangles of the type illustrated in Figure 4 [32]. I put the results of method 2 out of my mind but began

[32] It will be noted that I made this count after I had come to the realization

with a study of the rectangles for *a, e, o, jo,* and *ja,* comparing them with **8, *38, *61, *36,* and **57,* since I had recognized early that these signs were initial and final vowels and this much, therefore, could be assumed. The rectangles for the Greek identified those for the Mycenaean signs, of course. *a, e,* and *o* were easily identified by the number of cells showing high frequencies following. The highest frequency following initial Greek *e* was *u,* while *a* was followed by *u* only once. Would this prove a clue to the Mycenaean sign for *u*? Looking at the charts for **38* (= *e*), I studied the eight highest frequencies which followed. I found that the signs for *re, ko, ra, ke,* and *pi* (their values then unknown) showed even higher frequencies following **8* (*a*). Therefore they could be eliminated, leaving **10, *53,* and medium-frequency **75.* But **53*'s chart showed it to be followed by 51 *jo*'s and 31 *ja*'s, and so **53* had to end with *-i* and was eliminated. The charts for **10* and **75* showed a remarkable sameness of pattern, but the frequencies shown on the **10*-charts were far higher than those on the **75*-charts. Therefore **10* must be *u* (see Figure 4) and **75,* which showed a similar pattern as an ending and probably represented an oblique case for the nominative ending in *u,* must be ϝ plus vowel. Since it was followed by final **10* (*u*) 4 times, it was probably *we.* As these signs and more were identified it became easier to extract others as long as I was dealing with high-frequency signs. After about thirty signs were identified, it began to be increasingly difficult to assign values to additional signs, and it became necessary, as I went on, to check the vocabulary in *MLB* for results and finally to check actual text in *PT.*

The obvious limitations of space prevent the reproduction in this paper of the rectangular charts, without which it is impossible to illustrate much more of the method than I have. But as an example of how simple the method can be at times compare the chart for NU (**10*) with that for *u.* (The capital letters refer to the labels I gave the signs *at*

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that *d-* and *t*-syllables had separate signs but before I realized that *l* and *r* should be combined into one row of signs. Actually I had already had a glimmer of the truth but had not yet done the tests which proved it, and I was not ready emotionally to accept the truth in this case until I had what I considered ironclad proof. As far as the separate columns for eta and omega are concerned, before I made the count I had stopped worrying about keeping them separate from epsilon and omicron. But at the time I drew the skeletons I had not yet made my decision to stick to five vowels only.

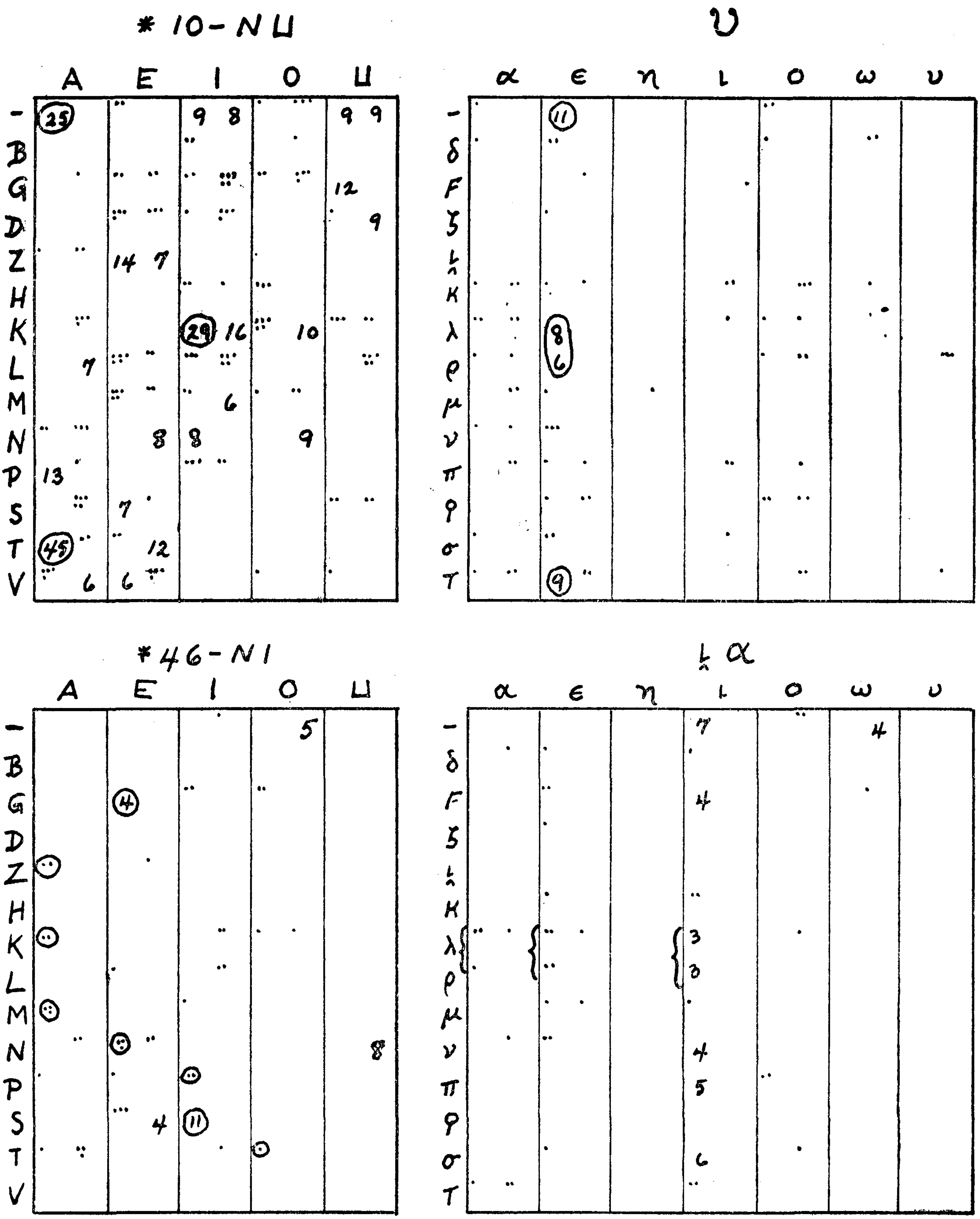

Fig. 4. Rectangles illustrating method 3. In each double column the cells on the left indicate syllables which precede, on the right those which follow. Each dot indicates one occurrence but the larger numbers have been changed to Arabic numerals for greater clarity. Several rows of infrequent Mycenaean syllables have been omitted. The Mycenaean count included 4 times as many syllables as the Greek. The capital letters refer to the labels I gave the signs *at random*. Note that these labels are arbitrary and *absolutely meaningless*.

random. These labels are arbitrary and *absolutely meaningless,* but such labeling is standard operating procedure in cryptanalysis.) Three high frequencies stand out on each: the signs, TA, KI, and A, apparently corresponding to the syllables, *re, te,* and *e.* We already know that A has the value *e.* TA must have the value *re* and KI, *te.* A quick check with the frequencies on Table I will corroborate this. The frequencies need to be used constantly with this method, both for this type of check and to help decide which rectangles to compare with each other. Naturally you should try those whose frequencies are most nearly alike first. Finally you must use the process of elimination to help with signs which at first resist identification.

The chart for *ja* is given as an illustration of how interesting and helpful a pattern one may find. Note that the main preceding syllables end in *-i* and that a secondary group ends in *-e.* In the case of *jo* the main preceding syllables also end in *-i,* but the secondary group ends in *-o.* Final *u* shows virtually no preceding syllables except those ending in *-e.* By comparing with these three charts the corresponding three Mycenaean charts, it is possible to pick out first most of the syllables ending in *-i,* a good many then of those ending in *-e,* and some finally of those ending in *-o.*

The chart for NI (**46*) is given to illustrate how even a low-frequency sign may respond to treatment. Because it was such a low-frequency sign I had deciphered a large share of the syllabary before I tried this. I noted that it was preceded by SI (*i*), 11 times. The circles on this chart represent additional syllables ending in *-i.* Therefore NI must represent *j-*? It is followed by 8 NU (*u*), of which 7 are final NU, as well as final digamma-syllables. Therefore NI must represent ?*-e.* Put the two together and you have its value: *je.* But usually you have to work much harder than that and often ingenuity is needed. Once the cryptanalyst is not sure, and usually even if he is sure, he will turn to the text to check each value, since in the final analysis context alone can show him whether he is right or not. When I had deciphered about thirty signs I checked these against my previous results, which as far as they went corroborated my values derived from method 3. This method then and certainly methods 2 and 3 combined will not only «break» a cipher like Linear B but can actually solve the greater share of it before detailed study of the texts begins. Not till then did I begin the contextually corroborative studies such as I described in detail in the first half of this paper. From this point on the probable-word method, which can be used at any stage of the work, became the only effective means of solution. There is no method of deriving the values of most of the low-frequency signs without including intensive study of the sign-groups in which they appear — in context.

ADDENDUM.—Since this paper was first written and after the information concerning *85 was made public, the following article appeared: M. D. Petruševski and P. H. Ilievski, «The Phonetic Value of the Mycenaean Syllabic Sign *85», *Živa Antika,* VIII (1958), 265-78. This journal is not available to me, but Dr. Emmett L. Bennett informed me of the confirmation of my value *au* for *85 by these two scholars.—Since this paper was intended to deal chiefly with methods and describes the actual reasoning and methods whereby I achieved the results contained herein, no attempt has been made to change it or bring it up to date. To do so would vitiate its main purpose. For more recent and more advanced work of a similar nature the reader is referred to my paper, «The *Jēsōn* Tablet of Enkomi», *HSCP,* LXV, 39-107.

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