

KHOTANESE METRICS AGAIN

by R. E. EMMERICK

The following abbreviations are used:

- E E. Leumann, *Das nordarische (sakische) Lehrgedicht des Buddhismus*,
Abh. für die Kunde des Morgenlandes, XX, Leipzig 1933-6, repr.
Liechtenstein 1966.
- KT H. W. Bailey, *Khotanese texts*, 1-6, Cambridge 1945-67.
- SGS R. E. Emmerick, *Saka Grammatical Studies*, i, Oxford University
Press, 1968.
- Z R. E. Emmerick, *The Book of Zambasta*, Oxford University Press,
1968.

At the International Congress of Orientalists held in Ann Arbor, Michigan, in 1967 I read a paper on "Khotanese Metrics", which was subsequently published in *Asia Major*, n.s. XIV.1, 1968, 1-20. A briefer statement of my conclusions on the subject appeared the same year in *The Book of Zambasta, a Khotanese poem on Buddhism*, Oxford University Press, 1968, 437-440.

The problem which confronted me when I set about preparing a new edition and translation of *Z* was that the two scholars who had in the past specialized in Old Khotanese had put forward precisely opposite views on the nature of Khotanese metre. According to Ernst Leumann, Khotanese metre was wholly quantitative, whereas Sten Konow held that it was purely accentual. The conclusion which I reached at the end of my own examination of the material was that the Old Khotanese metrical system as seen in the *Book of Zambasta* represented a transitional stage when the stress accent was beginning to replace quantity, but that quantity as the basic principle was fully superseded only in Late Khotanese poetry.

At the time when I was formulating my conclusions concerning Khotanese metrics on the basis of a thorough examination of the material, H. W. Bailey informed me of his own intuitive conclusion, a statement of which was formulated in an article¹ in the *Sir J. J. Zarhoshti Madressa Centenary Volume*, Bombay 1967, 5, as follows: "The prosody of the Saka

¹ An offprint of this article reached me in Chicago on 21 March, 1968 by the kindness of H. W. Bailey.

poems is purely indigenous. We seem to meet it at a transitional stage when the originally quantitative close of the verse has recently been replaced by stress, the initial part of the verse having always been free as to quantity, and then likely to have been grouped around stresses in the extant texts." This conclusion I believe to be substantially correct.

That there is nothing intrinsically improbable about such a development as it is proposed to see between the Old Khotanese and Late Khotanese systems it is hardly necessary to demonstrate to classical scholars. Such developments are in fact to be expected. It may suffice to quote here from W. R. Hardie, *Res Metrica*, Oxford University Press 1920, (repr. 1934), 222: "In its beginning, moreover, it [=the rise of accentual verse] is peculiarly entangled and perplexing. Accent supplants quantity sporadically, at long intervals (as in Hilarius), or the verse is half invaded by it, or (as in Commodianus) there is a strange mixture of accent and pseudo-quantity - things which happen also in Greek verse at Byzantium."

The fact is that we have a very considerable amount of Late Khotanese poetry as well as of Old Khotanese poetry. In the case of Late Khotanese poetry it is hardly conceivable that anyone would dispute the statement that its metrical system was based on the accentual principle. No Late Khotanese poem has verses which have consistently iambic or consistently trochaic endings. That fact alone is a significant difference from the situation found for Old Khotanese poetry, where metres A and B have regularly trochaic and metre C has regularly iambic endings.

Late Khotanese verses almost always have trochaic endings no doubt for two reasons. Firstly, most Khotanese words are either disyllabic or trisyllabic. In Late Khotanese, disyllables and trisyllables usually have a trochaic ending because of the loss of unstressed medial syllables. Secondly, the most frequently occurring metres in Old Khotanese, which were taken over and given a new form in Late Khotanese, were metres A and B, and those originally had a trochaic ending.

Metre B is continued in Late Khotanese, for instance, in the *Jātaka-stava*. There we find predominantly trochaic verse endings. In many cases the ends of the verses are marked in the MS. Such verse ends are marked after iambic endings in some places e.g. *stavā* (1v2-1), *tvare* (6r4-22), *sata* (7v3-28), *si' dai* (20r1-84) *skalana* (34v1-150). Such words could not have concluded verses in metre B in Old Khotanese.

Similarly, as I showed in my earlier article on metrics (p. 20), the Late Khotanese version found in P 4099, 251-4 of the Old Khotanese verses in Z 8.29-33 actually rewords the verses in order to avoid the iambic endings of the Old Khotanese. The reason was no doubt that metre C could not be continued in Late Khotanese, where the quantitative system has been wholly given up.

Whatever, then, the structure of Old Khotanese metre may have been,

Late Khotanese metre was based exclusively on the accentual principle. Nevertheless, it is unmistakably a continuation of Old Khotanese metre and bears a distinct relationship to it. It is hardly surprising that the late accentual system was preceded by a system where accent is in the process of superseding quantity.

It was not, however, by this route that I formed my conclusions concerning the nature of Old Khotanese metre but rather from an objective analysis of the whole of the Old Khotanese material. The fact that these conclusions were given confirmation by and gained perspective from subsequent examination of the late Khotanese material is merely an added reason for having confidence in them.

The only written, published response that my article has brought forth is a recent article by Manu Leumann, "Zur altkhotanischen Metrik", *Asiatische Studien*, XXV, 1971, 456-80. Manu Leumann clearly feels that the elaborate and ingenious metrical theory invented by his father and subsequently defended by himself has been attacked and that he is therefore obliged to devote a further 25 pages to its defence, "Meine Revindikation", as he calls it (p. 458). Nothing personal was, however, said, nor intended in my discussion. Indeed, the only strong criticism I allowed myself to make could only be construed as directed towards those who ignored the evidence of the verse endings, which, as the Leumanns had already pointed out quite correctly, proves the existence of the quantitative principle (p. 3). To reject the quantitative principle in favour of a system based exclusively on stress is to cause a major retrogression in research into the Old Khotanese metrical system. That is worth repeating in view of the controversial nature of this subject.

It is clear that no amount of argumentation has any chance of convincing Manu Leumann. Despite the objective presentation of "Khotanese metrics", Leumann has replied with subjective vehemence: "In general terms, the metrical laws in Emmerick's article have not been derived from the received text but have been read into the text according to previously-formed opinions concerning the historical state of the language and concerning a metrical system with accented cadences" (p. 480). What purpose remarks of that kind serve I find it impossible to imagine. Had my aim been to attack Leumann rather than to examine the material afresh, I might well have said in similar vein that Leumann had not derived his metrical laws from the received text but had emended them into it according to a previously-formed conviction that the verse was wholly quantitative and that the metrical pattern of the longest verses was historically related to that of the Greek hexameter.

Indeed, one is tempted, having been provoked into making the remark, to follow it up. For while it is stated² that a long is equal to two shorts as in

² E.g. Z. p. xxix.

the Greek hexameter or the reverse as in the Greek anapaest, what proof is offered? None whatever.³ I had intentionally avoided making this assumption as it necessarily prejudices the result. Yet, in one place where he asks whether the long syllables in cadence 1 (- - - -) can be replaced by two shorts, Leumann complains: "For Emmerick it is an axiom that no substitution is possible; obviously he considers it so unthinkable that *he does not even mention the possibility* of such an assumption, presumably because he is prejudiced by his attachment to the metrical structure of the hexameter ending in the classical languages." (p. 460). Who is prejudiced by what?

The nearest I came to admitting the possibility of the substitution of a long for two shorts, on the other hand, which might also have led me in the opposite direction, was when I was discussing the cadences of spondaic verse (p. 12). In such verses the most frequent cadence is ˘˘˘˘. It accounts for 80 per cent or more of the verse endings. The next most frequent cadence is ˘˘˘˘ (cadence 2). This might, in the abstract, incline one to see here a proof that a long may stand for two shorts. But the remarkable thing about this cadence is that in almost every instance the second long is represented by a syllable that is the result of a secondary contraction of two syllables. Thus, as pointed out there, ʃʃāvā rraysgu (Z 2.77b) can be regarded as inherited from a time when this was pronounced *ʃʃāvā(k)a rraysgu ˘˘˘˘. In other words, the quantitative principle is already by the time of our texts being obscured by phonological developments within the language itself. I was thus led by the evidence *away* from the assumption that such substitution originally obtained.

This important observation concerning cadence 2 has not, of course, received discussion by Leumann. He simply quotes the fact that we agree in finding these two cadences in the trochaic metres and then proceeds to say (p. 458): "The quantitative patterns of these verse endings correspond then in principle to those of the Greek hexameter. The no doubt overhasty and untenable connexion which my father made on this basis, as the preservation of a common Indo-European inheritance in both languages, in no way affects, of course, his analysis of the Khotanese verses according to a quantitative metrical system; but it has all the same discredited this analysis in the eyes of fellow research workers." It should be recalled that Ernst Leumann continued till his death to believe that Khotanese represented a parallel branch of the Aryan language group standing beside the Indian and Iranian branches. In this context an ancient quantitative metrical system related to the Greek hexameter naturally made sense. It makes, of course, considerably less sense when we know that we are dealing with a late Middle Iranian language.

I notice that Manu Leumann does not repeat his earlier opinion concerning his father's "connexion of this A-line with the Greek hexameter and the

Nibelungen-line by way of an Indo-European metre that can be reconstructed and which would be continued still almost unchanged in this A-line" (Z p. xxiii). At that time Manu Leumann was prepared to say: "I too see in fact no other way to explain meaningfully the similarity between hexameter and A-line." It should further be borne in mind that the dactylic hexameter is usually considered to have been not an inherited Indo-European metre but an Ionian invention.⁴

Nevertheless, common sense should be allowed to prevail, and it should be clearly stated that there is no a priori reason why an inherited Indo-European feature could not be conceived of as having survived in one particular branch of Iranian only. But it must first be proved.

The equivalence of a long and two shorts is, of course, not an equivalence that is ascribable to Indo-European metrics. It has developed in particular languages but must in each case be demonstrated to be a valid principle of the metrics of that language. This equivalence is not found in Vedic metrics, which most closely resembles Greek metrics. It is a later Indian and a later Greek development. It should not therefore simply be *assumed* for Khotanese.

The fact that cadence 1 is also the cadence of the hexameter is by itself not sufficient to justify us in thinking that there is any particular connexion between the two, especially since the remainder of the verse, even on Leumann's assumptions, bears no resemblance to the hexameter and is certainly not dactylic in character. Moreover, cadence 1, in so far as it characterizes the spondaic metres A and B, has in Khotanese the function of distinguishing them from metre C. That is, cadence 1 - - - - contrasts with - - - - (cadence 6). In other words, the function of cadence 1 is not to characterize the rhythm of the verse, as in the hexameter, but to mark it off clearly from the other variety of verse used in Khotanese. This is naturally especially important if, as may have been the case in Indo-European,⁵ the part of the verse preceding the cadence is not quantitatively fixed.

I now turn to what is perhaps for some the crux of the matter. What form do the verses take when the cadences have been removed? Here again I find myself in partial agreement with Leumann. Each verse is characterized by a caesura in the middle dividing the verse into two pādas. In metres A and B the first pāda of the verse is characterized by the same type of cadence as the second pāda. Metre C has cadences of the types found in metres A and B in its first pāda and its own special cadences in the second pāda. But what remains of the pādas after the cadences have been removed is the main cause of our disagreement. The range of metrical patterns as well as their length in terms of number of syllables is so varied that I prefer to

³ The remarks made by E. Leumann in *Neue Metrik*, Berlin and Leipzig 1920, 34-5, can hardly be considered a serious attempt to prove this.

⁴ Cf. R. Schmitt, *Dichtung und Dichtersprache in indogermanischer Zeit*, Wiesbaden 1967, 307-313.

⁵ See e.g. A. Meillet, *Aperçu d'une histoire de la langue grecque*, ed. 7, Paris 1955, 139.

assume that this part of the verse was not defined. Ernst Leumann regarded it, I think, as a game, certainly as a challenge, to devise an elaborate system to cater for all the possibilities that occur. He assumed that two shorts could be substituted for a long and vice versa and this not only in the usual way so that --- could be resolved into ---- or -- but also so that shorts arising from resolution of longs could be combined with neighbouring shorts to make new longs: thus, -- could be resolved into ---- and regrouped to --- . Since the only constant factor in such a group is the more, this type of metre is called by Leumann *Morenmetrik*. -- , ---- , and --- have in common only that each group consists of five mores (shorts, when fully resolved). The cadence of metres A and B he treated as a metrical unit and regarded as a heptad since it contained seven mores. He then postulated an ideal form in which each pāda of a verse in metre A consisted of a pentad followed by a heptad.

The system of mores enables a considerable variety of syllables to be included in its description and at the same time a considerable variation in their number. Since a pentad may contain from three to five syllables and a heptad from four to seven, a total variation for a verse of between 14 and 24 syllables is thus accommodated.

It will readily be seen that the so-called ideal form of metre A already embraces a vast number of possibilities when account is taken of the resolutions that are admitted as outlined above. Thus, the ideal form is supposed to be:

$\text{---} \text{----} / \text{---} \text{----}$

The pentad may, however, be resolved: --- , --- , or --- and the heptad: ---- or ---- . In other words, the pentad embraces many metrical patterns but excludes the dactyl, which might have been expected here, if we had a metre related to the hexameter. At the beginning of the verse the pentad can be emended into the text usually without excessive difficulty. Various methods are employed. A long vowel may be shortened, e.g. *dātinau* 2.1c is scanned --- ; two syllables may be contracted into one, e.g. 2.3a *biśśā hālā yā* is scanned ---- (vowel shortening of *hālā* to **hālā* and *hālā yā* 'zweisilbig'); a heavy syllable may be scanned as short, e.g. 2.8a *ysamaśśandai* is scanned --- ; an extra syllable may be inserted, e.g. *ni hāmāte* in 2.99c is read *ni ju hāmāte* and scanned ---- ; but some verses nevertheless must be declared defective, e.g. 2.130c *ūca puve [-] ne byaure*. This last is admittedly rare as Leumann usually manages to think of a suitable word to insert.

Although it is not too difficult to emend a word into the verse to achieve the desired metrical structure, it is much more difficult to remove the attested words on a large scale. Sometimes indeed Leumann does in fact throw two words out of one verse as, e.g. 13.55ab:

(*śītā*) *hastāṣṣai rūvu vīrā bodhisatvā (vari) dyāñāte ysamthu*,
where *śītā* and *vari* are both deemed dispensable. By and large, however,

recourse is had to another device, the so-called *Nebenformen*. These provide a lot more elbow room as they enable the number of mores to be increased or decreased before the caesura and the final cadence. Thus, if we want only two mores before the final cadence instead of five we can use *Nebenform* A 3 or A 4, if we want three we can use *Nebenform* A 1 or A 2, while if we want an extra more, six in all, we can use *Nebenform* A 5.

The following *Nebenformen*⁶ are officially recognized by Leumann for metre A:

A 1	---	----	/	---	----
A 2	---	----	/	---	----
A 3	---	----	/	---	----
A 4	---	----	/	---	----
A 5	---	----	/	---	----

As can readily be seen, just as what remained after removing the final cadence could not always be emended to provide a suitable pentad, so when a pentad was interpreted into the beginning of the verse what remained before the caesura could not always be made to provide the required heptad. Instead of a heptad, A 1 and A 2 enable us to have an ennead, A 3 and A 4 a decad, and A 5 a hexad. One misses an octad, of course. This is attested by only two verses, 11.43ab and 22.138cd, which Leumann simply described as "noch singulärer" (even more singular), without explaining why they do not deserve classification as A 6. I might add that they would also complete the symmetry by providing the missing possibility of having four mores before the final cadence instead of only two, three, five, or six as in the "official" categories. Moreover, metre C is allowed the luxury of six *Nebenformen*. But perhaps the curtailing of the *Nebenformen* was because (*E* p. xxvi): "I am conscious in regard to the following presentation of the details that the exposition, the more exact and complete it is to be, can even less avoid the false impression of a confusing and unregulated manifoldness."

Even the *Nebenformen* have their variations. Thus, A 1 is acknowledged to have "a couple of times" ("ein paar mal" *E* p. xxviii) -- instead of ~ before the caesura: 5.10cd, 13.9ab, 16.59ab, 22.280cd. Similarly, I presume 22.211cd is 'A 5?' (*E* p. 269) by virtue of having -- instead of ~ before the cadence.

What is, however, truly remarkable about Leumann's quantitative theory is that despite the elaborate arrangements made to accommodate all possible sequences of long and short syllables, not only does he find it necessary to emend very many verses, but a large number of words are not scanned according to the quantitative principle at all. Thus, words like *uysnora* and *balysüstā* are scanned --- and not -- as one would expect. This is said to be due to "metrical shortening" or the "ictus law", but it is

⁶ In their "ideal forms", of course, and without indicating all their possible resolutions.

admitted that the words were "probably accented - 2 1" (*E*, p. xxxiii). Similarly, words like *utāra* and *praysātā* are scanned - 2 1 instead of - 1 1 when the metre is thought to require it.

In *E* pp. xxxiii-iv quite a large selection of such words, closely printed, is given. At the time it was clear that Manu Leumann himself felt unhappy about these irregularities. Indeed, he mentioned the possibility that they may give scope for criticism of the system, concluding: "Yet it is to be observed that the number of these deviations or explanations, insofar as one would dispute them, can do no more at first than lead us to assume that the metrical patterns have not been handled quite so rigorously, and not to the further assumption that they have been wrongly set up or are based on false principles" (p. xxxiii). In his latest article, on the other hand, Manu Leumann dismissed these irregularities briefly as "recognized shortenings . . . in the case of a few common words" (p. 457). The examples of "unverständliche Kürzungen" cited in *E*, p. xxxiv: *haysānandai* 12.112 scanned - 2 1 1 and *anandiśāte* 12.114 scanned - 2 1 1 1, are not further discussed. One would have thought it quite absurd to contemplate such scansion in a quantitative system.

The method adopted by Manu Leumann in his latest article on the subject of metrics is simply to assume once more the correctness of his father's system and to explain the new cadences set up by me as being attempts to get round ("umgehen") metrical patterns in the Leumann system. He constantly cites the advantages of his system over mine.

One such advantage is truly remarkable. Leumann claims that his system is characterized by a "Fuge fest", mine by a "Fuge variabel" (p. 460). What a "Fuge" is, Leumann does not explain. In the case of my metrical system, the position of the "Fuge" can be determined, according to Leumann, only through the intuition of the observer (p. 459). In his system, on the other hand, the "Fuge" is fixed by the fact that the cadence which follows always has "exactly seven mores". Since the cadences proposed by me do not always have seven mores, I am deprived of the advantage therefore of locating with accuracy this "unsichtbare Fuge" (p. 458 ult.). Most people would understand "caesura" by "Fuge", but since it is "invisible", I have refrained from rendering it. According to Leumann, "This is prosodically formed normally by the word end or by the *Fuge* between the members of a compound, also no doubt between stem and termination." (p. 459). Thus, to take an example where Leumann and I would agree about the cadence as in 22.315b, the "Fuge" would occur between *ā-* and *-nanda* in the name *ānanda* (p. 473). What use that is I cannot imagine. It must also be borne in mind that according to Leumann's system any number of *mores* from two to six may precede the cadence. The "Fuge" can only therefore be discovered, for what it is worth, by counting back mores from the end of the line.

In the final summing up, Leumann refers to p. 18 of my article: "The metrical system recovered in this way has certain distinct advantages. Principally, it fits the facts, accounting as it does for the evidence both for an accentual system and for a quantitative system, and it does this without the necessity of emendation of the received text." Instead of contrasting with this advantages of his own system, Leumann comments: "According to Emmerick the scansions *ā i ū* do not constitute a textual emendation, *ā i ū* of the manuscript conceal the already valid new pronunciation *ā i ū* by means of an older, traditionally preserved orthography. Yet from a philological point of view the scansion - 2 1, for example, of a spelling *vasuta* is a textual emendation." (p. 480).

The reader of my article will, of course, look in vain for any comment of the above kind from my pen. The vowel *-a-* in *vasuta* is short in Old Khotanese and Late Khotanese alike. There is no question of a "new pronunciation" with *ā*. It is true that I allow it to be scanned - 2 1, but that is according to the general principle that an accented or stressed syllable may be counted as heavy (or long, according to the terminology used) since the accentual system is beginning to replace the quantitative system. It is quite different from Leumann's scansion of *hvāñāte*, for example, as - 2 1 1 1. In my system *hvāñāte* can be scanned *only* as - 2 1 since the stress is on the first syllable. But in Leumann's system it can be scanned - 2 1 1 or - 2 1 1 1 as his metrical system requires. Thus, in 2.57b *hvāñāte hāde* is scanned - 2 1 1 1, as one would expect, because this provides the usual cadence. But in 2.68a *hvāñāte* is scanned - 2 1 1. Why? Because when the cadence *balysā utāri* - 2 1 1 1 is removed what remains must, according to the patterns prescribed by Leumann, not exceed five mores. As *itai* provides two of these, *hvāñāte* cannot be allowed to provide more than a total of three. Hence, it is scanned - 2 1 1 without regard to quantity or accent or any consideration other than the requirements of Leumann's theory.

We see now why the "unsichtbare Fuge" has such importance for Leumann: it is not until the "Fuge" is located that we know how many syllables must be scanned to provide a total of not more than five mores. That is why in the example quoted above from 2.3a, when the clear cadence *nāma biraṣṭā* - 2 1 1 1 had been removed, it was necessary to make *biśā hālā yā* scan as - 2 1 1 1 with **hālā* for *hālā* and *hālā yā* as two syllables.

It is I think here that we are at the heart of the matter. For how can anyone who has not previously read the verse, counted back the mores and decided how to distribute them, possibly be expected to know what metrical form *hvāñāte* in 2.68a is supposed to have? And if the reader or reciter does not know that and cannot be expected to know it, what meaning does this metrical system invented by Leumann really have? Even if it were able to

⁷ He frequently scans it thus (see *E* s.v. *hvāñ-*, p. 529).

account for the facts, which it palpably is not, what practical function could it possibly have? I submit that even if it could account for the facts, could accommodate without emendation all the forms taken by all the extant verses, this metrical theory would still have no basis in reality. It would just be a huge algebraic formula and no metre at all. No doubt the setting up of such a formula could provide those so inclined with a wonderful pastime, but it has absolutely nothing to do with the attempt to discover the metre which the composers of the verses were actually using.

Returning now to *vasuta*- I would at this stage add a little to what I have previously said, for *vasuta*- seems to me to provide just such a transitional form as one might expect to represent - in a situation where accent was replacing quantity. There can be little doubt that the later pronunciation *vasva*- was already known at the time of the copying of this poem if not of its composition. Thus, *vasvātā* occurs in 2.176b, where we expect *vasutā* as emended by Leumann (p. 462, n. 3). It may be that in this case the scribe first wrote *vasvā* as he spoke, then added *tā* to correct it to *vasutā* forgetting to change *vā* to *su*. This is what Leumann suggests. But such forms are fairly frequent of occurrence and one wonders naturally whether there is not some linguistic reality behind them just as has been found in a great number of the words emended by Leumann. In the case of this particular word I have noticed only *vasvāte* in Kha vi.12.122 KT 5.179,⁸ where the context is not clear enough to enable the form to be interpreted, and *vasvatattetu* in Kha i.303a.123 KT 5.171.⁸ *vasvata*-⁹ is apparently a contamination of *vasva*- and *vasuta*-. Similar are the forms *hvete*, *hvetā* "he said" from the *Saṅghātasūtra* beside *Z hvate* and *hve* (SGS i. 223). More complicated is *byatanu* beside *byanu* "obstruction" also both in the *Saṅghātasūtra*.

What I am suggesting then is that whether the scribe who wrote *vasvātā* in 2.176b intended to write *vasutā* or not, he would clearly have had no difficulty in regarding the first syllable as heavy. Note further *vasvu* 11.34 and *vasve* 24.652. In the former case, *vasva svabhāvu* "pure in nature" provides a clear cadence 1 - - - - - . Leumann prefers to interpret this as *vas[u]va svabhāvu* - - - - - , no doubt because *vasuta* often forms part of a cadence with a trisyllable of the pattern - - - as e.g. in *vasuta aggamjā* 2.146b. But it does occur also with a disyllable at 9.3c *vasutā hāde*, which I would regard as a normal cadence 1. For Leumann it is only part of the cadence of A 2 with resolution.

Nevertheless, the cadence - - - - - , while not extremely frequent, occurs often enough to leave no doubt about its existence. In addition to the examples of cadence 4 cited by me on p. 13 of "Khotanese metrics" the following may be mentioned:⁹ *hāmāte bitandā* 2.221d; *tcamāña hvarindi*

⁸ In neither case do I have at hand the means of checking the reading.

⁹ Further examples are quoted below on p. 151.

4.33b; *hamatā hvarimi* 4.50b; *ttāmāra jadina* 4.92d; *parrātu yādāndā* 6.58b; *hāmāte paramjā* 12.45b; *ratana hāmāre* 16.51b; *kṣamāte pravaja* 22.173b; *damānu ggaysimjyo* 22.276d; *ratāna hāmāre* 22.142d. In each case the stress is clearly on the first syllable.

Leumann believes (p. 461) that my cadence 4 was set up to dispense with some of his resolutions of cadence 1. The above cadences for him simply show a resolution of the first long in - - - - - so that one has - - - - - . My first example on p. 13 has, however, already a long syllable: *drainu ratanānu* 12.24b. This example is not referred to by Leumann. In his edition of *E* he felt obliged to remove it forcibly:¹⁰ *drainu rat(a)nānu*. He emended it from *ratanānu* to *ratnānu* and then scanned it - - - ! Actually, *ratanā*- appears in Late Khotanese, as expected, as *rana*-. Of this argument Leumann could hardly avail himself as *ratana*- occurs frequently in this poem and he has everywhere else scanned it as trisyllabic. Even *dr(r)ainu ratanānu* he scanned elsewhere as expected (12.19c, 55a; 22.231cd, 238c, 267d). Like *vasuta*, *ratana* often forms part of a cadence with a trisyllable of the pattern - - - as e.g. in *ratana vicitra* 22.248b, but it is found also with a disyllable: *ratanu nāsta* 24.387c.

Discussion of *vasuta* brings me also to the question of the scansion of the instrumental-ablative singular of the *a*-declension (Leumann, pp. 463-4). The cadence of 22.243a (*a*)*ysmūna vasutāna* I regard as cadence 4, as Leumann presumes. That is, I consider *vasutāna* to have been accented on the penult. For Leumann it is, of course, an example of the resolution of the second long in cadence 1 (- - - - - for - - - - -).¹¹ *dātēna* I scan similarly - - - with the accent on the penult. *vratāna*, cited by Leumann from 23.103a, is probably also to be scanned in this way. *pacadāna* 22.317c is lumped together with *vratāna* and *vasutāna* by Leumann, but it is clearly different. It is in fact like *kādāna*,¹² beside which *kādna*, the precursor of Late Khotanese *kāna*, is found already in *Z*. Thus, we have for example *pacadna* in P 3513 8ov1 KBT 64 (= *pacadni* Ch i.0021b, b45 KBT 154). *pacadna* occurred earlier at *Z* 19.77d.

Leumann is right, of course, in saying (p. 464) that the stress cannot have been on the penult in such words as *kādāna*, *padamāna*, *pacadāna* etc. since this syllable is later lost. But whether Leumann likes it or not, the subsequent development of words like *dātāna* and *vasutāna* is different.

¹⁰ Similarly, the clear example at 2.144d *ttatvata vātāga* is dismissed by Leumann as "ein Überlieferungsfehler" without further comment.

¹¹ Similarly, (*sku*)*tāna puvāta* at 22.155b is for me cadence 1, but for Leumann *skutāna puvāta* represents the same cadence with resolution of the first long. *skutāna* I expect to develop to Late Khotanese **skvena*, but it has not yet turned up.

¹² In the Khotanese *Sūrangamasamādhisūtra* this word is consistently spelled *kādāna* (see my edition, Oxford University Press, 1970, p. xx), a spelling not found in *Z*, although the word is common there. It may simply be an archaizing spelling by analogy with words like *ratana*- "jewel", which developed via *ratāna*-, with medial weakening, to Late Khotanese *rana*-.

Earlier¹³ I quoted the Late Khotanese development of *dātāna* to *dyēna* as confirming the impression I had gained from its metrical use that *dātāna*, usually spelled *dātena*, was accented on the penult and could therefore be scanned ˘ ˘ ˘. Since Leumann brings into the argument *vasutāna* in the cadence (*a*)*ysmūna vasutāna* (22.243a), it is perhaps worthwhile drawing attention to the use of the cadence (*a*)*ysmūna vasvēna* (˘ ˘ ˘) in Late Khotanese poetry (Bhcd 44r3 (4) *KT* 1.222; 45r1 (8) *KT* 1.223). This fact must be borne in mind at the same time as the fact that we have other evidence concerning the development from Old to Late Khotanese. Thus, the adverb and postposition *patāna* developed via *paṃna* to *pana*. The stress no doubt lay on the initial syllable and the word showed the commonly found phenomenon of weakening of the medial syllable of a trisyllable. But we do not have *dātāna* > **dāmna* > **dāna* like *kādāna* > *kādna* > *kāna*, nor do we have a contraction as in *vātāya* > *viya* (see "Khotanese Metrics", p. 16, n. 27). In this particular case the matter is more complicated because -*ātā*- was dissimilated early to -*āte*- (see *SGS* i: 192, 194, 198, 254). Nevertheless, even so, the treatment of *dātāna*, *dātena* is different from that of *hātānai*, *hātenai*,¹⁴ of which the late form *henei*¹⁵ is found already in *Z* 23.44. In the case of this frequently occurring word the spelling *hy-* is never found. Nor is this attributable to the difference of initial, for Old Khotanese *hātā* develops into Late Khotanese *hye*. Thus, *hātā suhā kādāna* (Or 9609. 56v7 *KT* 1.241) = *hye suhi kiṇa* (Bhcd 46v1—15 *KT* 1.223). This development is exactly parallel to that of the nominative **dāte* to **dye*, a development which cannot be documented but which is probable since from the same verb the development of *dāte* "he saw" (*Z*) to *dye*¹⁶ is attested.

Such considerations as I have adduced in the preceding paragraph point clearly in the direction of an accentuation on the penult of *dātena* and *vasutena*.¹⁷ That a development from *dātena* via *diena* to *dyēna*, as suggested by Leumann (p. 463), is theoretically possible I am ready to admit. But when one considers the evidence as a whole, one is led in a different direction. We have to account not only for the metrical evidence but also for the fact that *dātena* developed to *dyēna* and not to **dāna* **dīna*, or **dena*.

It was already clear to Leumann (p. 461 and see above) that I regarded *vasuta-* as having the stress on the first syllable. Yet on p. 464 he allows himself the sarcastic comment: "Then the situation is not helped by appealing for support, only for *dātena* and *vasutāna*, to the inherited oxytone accentuation of *tō-* participles." I did not refer to the "inherited oxytone

¹³ "Khotanese Metrics", pp. 3-4.

¹⁴ See *KT* 6.413.

¹⁵ Not recognized by the Leumanns, but quite certain as demonstrated by H. W. Bailey, *KT* 6.413.

¹⁶ E.g. *Si* 102r3 *KT* 1.38.

¹⁷ In the prose of the *Saṅghāṭasūtra* occurs *vasutena aysmūna* (H. 147 NS 109 41r6 *KT* 5.73).

accentuation of *tō-* participles" at all, nor is it in any way relevant. *dāta-* and *vasuta-* were doubtless accented on the first syllable. The secondary shift of stress in the instrumental-ablative case is no doubt due to the development of a long vowel in the penult. A similar secondary shift of stress occurred in the instrumental-ablative plural when followed by the enclitic postposition *jsa*: *dukhyau* ˘ - but *dukhyau jsa* ˘ ˘ ˘.¹⁸

In discussing my cadence 3a, Leumann refers to "the *dukhyau-* cadences of Emmerick" (p. 465). It is not clear whether or not he accepts the deduction concerning the position of the stress in such cases. His main point is that he scans the ending of the instrumental-ablative plural long whether it is spelled *-yau* or *-yo* and whether it is followed by *jsa* or not. This accords with one of his basic principles that where alternations between *o* and *au* are found in the same word, the *o/au* should if possible be treated as long in all cases. In itself this is, of course, a logical and commendable principle. There are, however, places where this principle is not observed. Thus, *būṣṣātau virā* at 23.153b stands beside *būṣṣato' virā* at 23.166b, but Leumann emends *būṣṣātau* to *būṣṣāto* for the sake of the metre instead of accepting the logic of his principle and scanning *būṣṣātau* and *būṣṣāto'* both ˘ ˘. If he had scanned *būṣṣātau* at 23.153b with a long final, he would, of course, have had to scan its first syllable short: no great problem for Leumann as his frequent scansion of *hvāñāte* as ˘ ˘ ˘ shows. As for 23.166ab, the scansion of *būṣṣāto'* as ˘ ˘ ˘ would at least have the advantage of providing the heptad which I presume Leumann is trying to obtain by throwing out the word *ustamu* before *būṣṣāto'*. As printed by Leumann at *E* p. 309, the verse is a more short according to the requirements of his own theory, but as handed down by the manuscript the verse is several mores too long to be accommodated within even Leumann's scheme.

My comments on the scansion of final *-o* and *-au* on p. 8 of "Khotanese Metrics" are unfortunately not as clear as might have been desired. Examples of a final *-o* (other than in the instrumental-ablative plural ending) counting short when unstressed should have followed. Those I originally selected were from chapter 22: *gyastuvo' iste* (194d); *ggamjsō dātāndā* (211b); *jīngo yādāndi* (224d); *pando nijsaṣḍe* (275b); and from chapter 23: *hastamo bvānde* (372d). The comment that 'final *-au* is too rare to examine' meant that final *-au* seldom occurs except in the ending *-yau* and as a result of contraction. The above cadences are, of course, scanned also by Leumann as ˘ ˘ ˘ ˘, but I was not at that time attacking his system so much as attempting to set up a new one.

The fact that final *-o* when unstressed is frequently (indeed usually) scanned short had, I was suggesting, to be considered beside the fact that *-o* in *-yo jsa* was always scanned long. This I suggested was a further indication

¹⁸ "Khotanese Metrics", pp. 8-9.

of the fact that *-yo*, *-yau* bore the stress when followed by the enclitic *jsa*. That an enclitic should have such an effect upon the accent will not surprise those acquainted with the same phenomenon in Latin and Greek. It is probable that Avestan reflects the same accentual system, as is indicated by such pairs as *tē : taēca* and *čabwārō : čabwarasča*. The latter pair is particularly interesting in that it shows that a long vowel under the accent was actually shortened in pronunciation when it lost its accent due to the presence of an enclitic. If such shortenings can occur in the language itself, it is hardly surprising that they should be introduced into the metrical system.

The fact that words like *dukhyau* form a cadence in A-verses when followed by words of the pattern *---* is for me an indication that *-yau* (or *yo-*) when unstressed (not followed by *jsa*) may count as short. The following are thus examples of cadence 1: *dukhyo bitcampha* (1.50a); *stavyo vicitra* (2.242b); *u'vyau vikalpa* (4.20c); *uvyau' pyuviru* (5.13b); *dukhyau bit-sāmgya* (5.17c); *gguvyo' bajāṣṣa* (8.35a); *ggaryau vataysde* (17.12a); *ggaryau nwoalysde* (17.18a); *spātyau pajustā* (21.34c); *sañyau huṣṣāte* (23.128a); *gyastyau dātāna* (23.137b); *spātyau hambirstā* (23.157d). It is possible for Leumann to scan *-yof/-yau* long in such cases because of the resolutions he allows. Thus, *dukhyo bitcampha*, which for me is *---*, is for Leumann *---* because only the mores are important even in the cadence. Similarly, *gyastyau dātāna* is for me *---* (*dātāna = dātena*, on which see above), but for Leumann *---*. Yet the regularity of the stress on the first and fourth syllables in these cadences can hardly be accidental and in my view defines the rhythm. Curiously enough, it was the recognition of just such endings that led Ernst Leumann to compare them with those of the hexameter in the first place, even though in many cases he had to assume the metrical shortening of a syllable not bearing the ictus as in *busta balysūtu* (11.3d, 72b), which even Leumann scans *---*.

It is, of course, probable that *stavyo*, *u'vyau*, and *gguvyo'* had a metrically short first syllable as is required by Leumann's scansion, but not certain. No instance of *vy* not making position has so far been found in C-endings (see "Khotanese Metrics" p. 7)¹⁹ but on analogical grounds it is no doubt justifiable to draw the inference. Nevertheless, in the case of *u'vyau* Leumann is led into inconsistency. Thus, on my system cadence 1 occurs in 3.31a *tsāṣṭyau uvvau' jsa*, 3.107a *hivvau uvau' jsa*, and 3.107c *hivvau woyau' isa*, all rhythmically alike. For Leumann, however, *uvvau'* in 3.31a has to be regarded metrically as a monosyllable whereas in 3.107a and c it is a disyllable. In 3.107c this is made possible by scanning *hivvau --* but in 3.107a *hivvau* must be emended to scan likewise.

¹⁹ Additional C-endings are now known from a folio from the *Saṅghāta-sūtra* published in *KT* 1-3 (ed. 2), pp. 139-40. Note there the polysyllable *atanantanarya* (v4), badly read by Bailey. In r3 the MS. has *trāmā* and not the grammatically impossible *trāmimā* printed by Bailey.

Leumann complains (p. 466) that *-yau/-yo* counts long even when not followed by *jsa*. The above examples are, however, the only instances I noticed, in a quick look through *Z*, of disyllables ending in *-yau/-yo* forming cadence 1 with a trisyllable of a clear *---* pattern. Even if an example had turned up of (say) *karmyau vicitra*, Leumann would no doubt have scanned *--* as at 13.106c. The amount of evidence is too small to prove anything one way or the other in this case. But it should be noticed that of the twelve examples found only four occur at the ends of verses, where the definition of the rhythm is expected to be at its strongest.

Three trisyllables occur in this position, viz.: *indriyo nuvāta* (14.75b); *mānavyau ppravaindi* (22.205d), and *rakṣaysyau uysnaura* (24.162b). These provide clear examples of cadence 4 according to my system: *---* in each case. For Leumann, however, they extend beyond the cadence and represent in each case *-|---*, that is, cadence 1 with *---* resolved as *---* plus an extra long syllable. Here again we see the worthlessness of Leumann's invisible "Fuge", which in these cases falls in the middle of the stem and not even between the stem and its ending.

The scansion of *uysnaura* as *---* instead of *---*, as seen in the last example in the preceding paragraph, was discussed in "Khotanese Metrics," p. 3. In this particular instance, as in many others, I am in agreement with Leumann in the scansion of this word. But it has occurred to me that the reason for the metrical pattern shown by this and many other words may be only partly that the unstressed syllable is counted short simply because of the fact that it is unstressed. It may be in fact that the syllabication rules in Khotan were not exactly the same as elsewhere. Variations are after all known to occur. One has only to think of the development found in Latin metrics, which allows *pātris*²⁰ to be scanned *--* if the syllabication is *pāt-ris* but *---* if the syllabication is *pā-tris*. The latter possibility is unknown to Vedic metrics. But I see no reason why it may not have developed among the Khotanese, and in particular why it may not have developed in groups with sibilants, which in Iranian languages have a greater significance than in Indian.

Thus, the real reason for the apparent exception among the words allowed in C-endings, namely *aysmū* (see "Khotanese Metrics" p. 7), pronounced "azmū", may be that its syllabication was *a-ysmū*. In the same way the scansion *---* in the following words can be accounted for by Khotanese syllabication: *vistāta*, *pastāta*, *aysmūna*, *uysnora*, *gyastūna*, *uskyālstu*.

It will be recalled that *tc*, pronounced *ts*, and *js*, pronounced *dz*, do not make position and are treated as single consonants. Previously I thought that this may have been a survival from the time before Old Iranian *č and *j

²⁰ The vowel *-a-* is short however the word is scanned. No variation in the scansion of *mātris* is possible, on the other hand, because the vowel of the first syllable is long.

had developed into *ts* and *dz*.²¹ But it seems to me now to be more likely that the determining factor is rather the presence of a sibilant in the consonant cluster whether as prior or as latter element. Thus, *nātca* counts *~* because of a syllabication as *nā-tca* and not because the quantity of the syllables was fixed at a time when *ts* had not yet developed from **č*.

Whether, however, the scansion *~* of a word like *balysūstu* can be explained in the same way is much more doubtful. Usually one finds that only such clusters are allowed to begin a syllable as are admitted at the beginning of a word. Of the words so far discussed, those with *tc* and *js* as well as those with *st* and *sky* (e.g. *skyāta-* "time") would straightway be covered by this principle. Those with *ysm* and *ysn* would not. At the beginning of a word neither is found without an anaptyctic vowel. For *ysm* I can quote only *ysimariye* from P3510.3.3 *KBT* 49, in which Bailey proposed to see **zmarata-* "emerald" (*BSOAS*, xxiii, 1, 1960, 29), known also in NPersian *zumurrud* and other words cited by him. In the case of *ysm* one thinks at once of *ysānāh-* and *ysānāj-* "to bathe" from the Old Iranian root **snā-* with extensions (see *SGS* i.113). Within a word, the situation is somewhat different. There, as *haysn-*, *haysnāta-* "bathe" (*SGS* i.148) shows, the anaptyxis is not necessary and *ysn* can begin a syllable. But I see no possibility of justifying a syllabication *ba-lysūstu*. The first syllable is scanned short simply because it is unstressed. Nor is it likely to be merely by chance that the syllable that is counted short in the other cases listed above (*vistāta* etc.) is unstressed.

Without support from C-endings I see no way of determining at present whether the cluster mute plus liquid admitted of the same possibilities of syllabication or not. Thus, *tcabriye* in 2.44a scans *~* in the cadence *śśāre tcabriye*, but the scansion of the first syllable of *tcabriye* as short may be by virtue of its being unstressed. Only if a word like *tcabri* should turn up in a C-ending would there be a strong case for assuming such a syllabication as *tca-briye*.

Leumann cites (p. 457) two examples of the shortening of a syllable containing a long vowel: *ātāsa-* and *ānanda-*. In both cases, the shortened syllable is unstressed so that no problem is at first sight presented. Yet when one considers the Late Khotanese development to *ātāsa'*,²² one naturally wonders whether the stress did not lie rather on the initial syllable. If, however, one looks at the Old Khotanese occurrences in the verses, it will be seen, that the word cannot have been stressed on the initial syllable. *ātāsi ni*

²¹ "Khotanese Metrics", p. 7. In a letter (7 December 1968) commenting on that article, G. Morgenstierne expressed the view that since "*tc* and *js* probably were single phonemes, i.e. dental affricates, the occurrence of *tc*, *js* in short syllables needs not to be based on a pre-Khotanese stage **č*, *j*."

²² Note the following Late Khotanese occurrences: *ātāsiq'* P3513.33r3 *KBT* 59; *āvāsiā'* P3513.16r3, 17v2 (bis) *KBT* 55; 69v4 *KT* 1.246; *āvāsiq'* P3513.14v1 *KBT* 54; *āvāsiā'* *jsa* Si. 145r3 *KT* 1.88.

rūva (4.26b) could be scanned *~* (cadence 4) as could *ātāsi uysnora* (4.112d), *ātāśā hamamggā* (9.4b), etc., but it would be difficult to scan *sarbandi ātāsu* (2.234c), and *puḍgalā ātāśū* would have to be hypermetric at 5.69c. *ātāsi kho balysūstā* (9.6a) would have to be scanned as 3b with *kho* stressed. In fact, it seems clear that (*ā*)*tāsi ni rūva*, (*ā*)*tāśā hamamggā*, (*ā*)*tāsi uysnora* and (*sar*)*bandi ātāsu* are examples of cadence 1 while *puḍgalā ātāśā* and *ātāsi kho balysūstā* represent cadence 4.

It looks therefore as though Old Khotanese *ātāsa-* developed to Late Khotanese *āśa'* (as in *ḡS* 19v1 (83), 23r4 (101)) and that the spellings with *-ta-* and *-va-* are pseudo-historical or semi-historical. One recalls the occurrence of intrusive *-ta-* found occasionally already in Old Khotanese as in *byatanu* beside *byanu* "obstruction" cited above.

In order to round off this second contribution to Khotanese metrics on a slightly less discordant note I would like to point out that Leumann and I scan many cadences in the same way. Moreover, if we compare the cadences I have set up for metre A with those formulas used by Leumann, it will be seen that cadences 1 and 2 are his ideal form A, cadence 3a corresponds to his *Nebenform* A 2, cadence 3b could be equated with a resolved form of *Nebenform* A 3, and cadence 4 could be a resolved form of the "*Nebenform* A 6", which, as we saw above, he did not set up because only two examples are found. The main difference, apart from that of the general principle, is that I allow all four cadences in both *pādas*, whereas Leumann admits in both *pādas* only cadences 1 and 2 and confines the others to the first *pāda* of a verse.