cation in some perfects (48–57) is more successful, though it does not ad-

Vance much beyond Oldenberg’s pithy 1906 observation (RV Noten, 
p. 178, cited p. 52 by K.). That long reduplication is frequently 
paired with a short root (type *tārūd-*) and vice versa (type *šuśrūd-*).

The most important part of the remainder of this short work is the 
assemblage of long-reduplicated perfects from the totality of Vedic texts 
(68–89) – an extremely useful conspectus, though almost totally lacking 
in philological commentary. It is with this collection of data, and a form-
by-form, root-by-root philological investigation of it, that this work 
might have begun. In short, though the author should be commended for 
defining the problem and for assembling the data necessary for its solu-
tion, this solution, in my opinion, remains for the future.

Hoffmann, Karl, und Bernhard Forssman: Avestische Laut- 
und Flexionslehre. Innsbruck, Institut für Sprachwissenschaft der Uni-
versität Innsbruck, 1976, 8°, 330 S. (IBS, 84.) Geb. 760 ös.

This is the long hoped-for grammar of Avestan, which brings to-
gather the present insights on Avestan, which are to such a large part due 
to Hoffmann and his school; I think that, beside Hoffmann, especially 
Johanna Narten must be mentioned here. As is well-known, the new ap-
proach to Avestan studies is due to Hoffmann, who, however, did not live 
to see the book published. We are indebted to Forssman for seeing the 
work to completion.

The set-up of the book is traditional: after an introduction and a chap-
ter on the writing system follow the phonology and the morphology, 
the latter two subjects organized as usual for an ancient Indo-European lan-
guage. Very helpful is an extensive (247–282) bibliography per section, 
which we owe to F. Finally there is a complete index of forms (283–330).

The presentation is perfectly clear; a detailed table of contents helps 
the reader to find his way; extensive cross-references further make the 
book easy to consult. There are many overlaps, but I agree with the au-
thors that this is hard to avoid; and it may be instructive.

The main thing one might regret is that the book is not more com-
prehensive. While the phonology, in connection with the interpretation of 
the orthography, is quite comprehensive – it is of course of basic import-
ance – comments on the forms are rather limited. In the introduction it is

R.S.P. Beekes: Hoffmann/Forssman, Avestische Laut- und Flexionslehre

said that completeness is not aimed at, and this is understandable in the 
case of a language where much is still unclear. But it is often not specified 
what is left out. One consequence is that it is not always clear how many 
forms are attested in a given category: sometimes the examples comprise 
simply all the evidence there is, but one cannot know.

Especially the introduction might have been much longer to my mind.

On Proto-Iranian almost nothing is said (32). On the date of Gathic e.g. 
there is just a statement, no discussion (33). The characteristics of Gathic, 
and the position of the dialect, are not discussed (33), there is no attempt 
to reconstruct the Gathic stage, nor are the developments after Gathic 
given. What I miss here, is a discussion of the (linguistic character and) 
value of the different texts. Now only some "wichtige Texte" are men-
tioned. Metrics are hardly discussed, the metrics of post-Gathic texts are 
not mentioned at all. One would like to find more about the development 
of the language, and about the texts written by people who had no active 
command of the language. The – perhaps – important point of different 
dialects is not elaborated upon (35, B2). I would have liked to see a dis-
cussion of the manuscripts. I would have liked to hear more about the use 
of Middle Iranian languages in interpreting the text and establishing the 
forms. A complete list of editions of texts with commentary would have 
been most welcome. It is clear that this does not diminish the value of the 
book, but one always wants more.

I shall now make some remarks on specific points. References are to 
pages, with the sections on that page in brackets.1

35 (B2). I found dialect questions mentioned on p. 53(f), 62(db), 
86(top), 87(f), 96(ec, dh), 103(ce), 106(dd), 106(ca), 127(ce).

At the end of the section on the writing system (39–46) one would 
have wanted a conclusion about the phonemic system of the language. If 
I am not mistaken, the word phoneme does not occur in the book. – The 
section on the origin of the writing system seems to me to be less useful 
for the reader (46–50).

44 (A2). The sign a should now be written without macron, as there 
is no opposition with the sign with macron, and the latter is typo-
graphically very difficult. (Special characteristics of one manuscript 
should not make our transcriptions unnecessarily difficult.)

Phonology:

51 (A). Anaptyxis. It would be useful to refer to the colouring of the 
anaptyctic vowel by surrounding sounds (rather than stating "daneben a 
av oder ö"). as on p. 91 (bb): avo > øvø.

1 For comments I am indebted to Kordland, Lubotsky, Schrijver, Boutkan, Cheung 
and De Vreec.
The set-up of the book is traditional: after an introduction and a chapter on the writing system follow the phonology and the morphology, the latter two subjects organized as usual for an ancient Indo-European language. Very helpful is an extensive (247–282) bibliography per section, which we owe to F. Finally there is a complete index of forms (283–330). The presentation is perfectly clear; a detailed table of contents helps the reader to find his way; extensive cross-references further make the book easy to consult. There are many overlaps, but I agree with the authors that this is hard to avoid; and it may be instructive.

The main thing one might regret is that the book is not more comprehensive. While the phonology, in connection with the interpretation of the orthography, is quite comprehensive—it is of course of basic importance—comments on the forms are rather limited. In the introduction it is said that completeness is not aimed at, and this is understandable in the case of a language where much is still unclear. But it is often not specified what is left out. One consequence is that it is not always clear how many forms are attested in a given category: sometimes the examples comprise simply all the evidence there is, but one cannot know.

Especially the introduction might have been much larger to my mind. On Proto-Iranian almost nothing is said (32). On the date of Gathic e.g. there is just a statement, no discussion (33). The characteristics of Gathic, and the position of the dialect, are not discussed (33), there is no attempt to reconstruct the Gathic stage, nor are the developments after Gathic given. What I miss here, is a discussion of the (linguistic character and) value of the different texts. Now only some "wichtige Texte" are mentioned. Metrics are hardly discussed, the metrics of post-Gathic texts are not mentioned at all. One would like to find more about the development of the language, and about the texts written by people who had no active command of the language. The—perhaps—important point of different dialects is not elaborated upon (35, B2). I would have liked to see a discussion of the manuscripts. I would have liked to hear more about the use of Middle Iranian languages in interpreting the text and establishing the forms. A complete list of editions of texts with commentary would have been most welcome. It is clear that this does not diminish the value of the book, but one always wants more.

I shall now make some remarks on specific points. References are to pages, with the sections on that page in brackets.\(^1\) 35 (B2). I found dialect questions mentioned on p. 53(f), 62(db), 86(top), 87(f), 96(cf. dh), 101(ce), 102(d), 106(c), 127(ea).

At the end of the section on the writing system (39–46) one would have wanted a conclusion about the phonemic system of the language. If I am not mistaken, the word phoneme does not occur in the book. – The section on the origin of the writing system seems to me to be less useful for the reader (46–50).

44 (A2). The sign \(a\) should now be written without macron, as there is no opposition with the sign with macron, and the latter is typographically very difficult. (Special characteristics of one manuscript should not make our transcriptions unnecessarily difficult.)

Phonology:

51 (A). Anaptyxis. It would be useful to refer to the colouring of the anaptyctic vowel by surrounding sounds (rather than stating "daneben \(a\) \(\hat{a}\)"), as on p. 91 (bb): \(\hat{a}r \ddot{e} \hat{a}r\).

\(^1\) For comments I am indebted to Kordland, Lubotsky, Schrijver, Boukhan, Cheung and De Veen.
52. A development -ινιβι > -ινιβί > υ'ινιβί is assumed. I think that υ' had not become υ, see on 93(a). So I think that the (subphonemic) υ was coloured to u before the following v after which the latter was no longer pronounced. This is confirmed by the fact, pointed out to me by Lubotsky, that several manuscripts write brātrāvya (which is another realization of i-rrāt). The length of the u is to my mind due to the eponym (Beekes 1988, 42).

52. It is suggested that in 'sadāvasa'—the (second) g was replaced by the u of sru-ta- etc. It seems to me improbable, even impossible, that an anaptyctic, automatic, i.e. non-phonemic vowel is replaced by another. Suppose υ had become or (and not υ is as is assumed in this book), it would mean that /sruna/ was replaced by /sryan/. Rather υg/ was replaced by sran-; and the anaptyctic vowel (the first υ) became u under the influence of the following u. That this is the right interpretation is shown by the fact that from the root sri- we have /sřiun-, where the first vowel is in several forms not written; this means that it is not a phoneme.

52. 'sriini; but note the variant -sriiti.

53 (f). β > γ is earlier than i-epenthesis, for ammi cannot go back to αβi. I think that here the opposition between phonetics and phonology must be considered. (γ/β) > γ/β which is realized as (αι/ι); because ι does not show i-epenthesis. No chronological conclusions can be drawn from the subphonemic eponym.

53 (f). A "Lauwandel von δ(γ) < 113) rz (113) (dιδιομ)" is not acceptable: Avestan does not drop a (full, phonemic) vowel because it stood between i and r. We have a voca
cal υ, which is normally written συρ, this means that you have a short, probably optional, vowel-like segment before and/or after the r. After i, the r follows so quickly (because the two sounds are homorganic) that (often) no voca
cal feature is heard. Thus in srii'eti'io no vowel segment (which would have been coloured i through influence of the following o) is heard, whereas you have one in kenarti. In both cases we have Cry with a slightly different realization. No chrono
cological conclusions can be drawn. (Of course, ταω for ττι is possible [exactly because it is non-phonemic]: srii'eti'io-.)

54 (c). That long vowels were more open is possible, but it has to be shown how this could explain the irregularity of the reflexes. — In Leiden, Michiel de Vana is preparing a dissertation on the subject.

62 (b). The explanation of the short i in Skt. daip. etc. is that he's had not yet merged with o, so that Brugmann's law did not apply, as was shown by Lubotsky (cited p. 253).

62 (db). That am i sometimes written am in inlaut probably shows that we have to do with a phonetic, non-phonemic colouring; before final -m the influence of the -m was of course stronger. Dialects are not to be invoked.

64 (f). That the -o in compounds (for -a) was generalized from forms with a labial, as in datwa, saiva—seems improbable to me.

66—67 (d). "die unrichtige Schreibung "νανωτι" will be a rendering of the (phonetic, non-phonemic) influence of the τ on the preceding nasi
lized vowel.

67ff. The diphthongs are treated in a strange place, under a.

68 (c). The transition -αυρ > -αυτι can best be interpreted as the re
placement of the glide y by a glide determined by the following τ. We may compare OIr., e.g., bintu from *bīthi; It is not necessary to assume a development of aτi > aτ > iτ; this is shown by the fact that we have e.g. muriτ, not *muriτ. See on 69 (c).

69 (a). The remark that "αυ is represented by ατ also durch a und geschlossen o-Laut" is rather disturbing: the a has become more o-phonemic.

69 (b). Note that the two reflexes of ταυ are parallel to the two of ται (Beekes 1988, 36). Thus it seems certain that ται is the reflex in closed sylla
bles.

69 (c). I assume that -αυ first became -ατ, which was later diphthongized to τατ. A metathesis τα > τατ is improbable. See Beekes, 1998, 71 (b). That -κατατα points to an accentuation τατ seems improbable. The notation indicates that the stress caused extra length and there was a short off-glide, as in Dutch when one calls fan (with short a) with much emphasis /fan/ (it may even become /fān/).

72 (cg). That τατ became -τα is shown by nom. sg. baxa and by the i-
tem loc. sg. The dative must have been restored.

73 (c). On xinnia—see Kuiper, 1J 34, 1991, 37.

75 (2). The forms with -αυτο- from daightho- are of course secondary, as the word was hysterodynamic.

78 (c). There can be no doubt that tataτ is trisyllabic in Y 444.

79 (l). I sg. ind. -αυτα, subj. -αυτα-τα, if I understand the text correctly, but τατ is translated as a subjunctive. There are serious difficulties with the number of syllables in these forms. Cf. Beekes 1988, 92; see also Pir
cart, MSS 47, 1986, 163.

80ff. The treatment of the laryngeals is rather disappointing, in view of the unique information provided by Avistan and the amount of work done. Not even the distinction between initial, internal and final syllable is made with regard to the vocalisation. The reflex of laryngeal between vowels is only sketched.

83. I am very unhappy with the transcription it, 66 for earlier y, t. It solves nothing (we still have to decide in every instance whether it was y or τ), while it looks awful and makes reading more difficult and disturbs the comparison with Old Persian and Sanskrit. The two or three cases (which are not given) where there is a difference in inlaut could have been solved by writing only these two or three words with ττ-, αυτ. (I shall here follow the transcription of the book.)

85. In b. and d. the situation is not very clearly presented. Final -τα
52. A development -ørej- > -øyj- > øjj- is assumed. I think that *yj had not become øj, see on 50(ac). So I think that the (subphonemic) ø was coloured to u before the following ü after which the latter was no longer pronounced. This is confirmed by the fact, pointed out to me by Lubotsky, that several manuscripts write *brúnásu (which is another realization of *trina/). The length of the u is to my mind due to the eponthesis (Beekes 1988, 42).

52. It is suggested that in *trúr-to- the (second) ü was replaced by the ü of tru- etc. It seems to me improbable, even impossible, that an anaptyctic, automatic, i.e. non-phonemic vowel is replaced by another. Suppose ü had become or (and not ær as is assumed in this book), it would mean that /surna-/ was replaced by /surna-/i. Rather /g/- was replaced by /sra-/, and the anaptyctic vowel (the first a) became ø under the influence of the following ü. That is the right interpretation is shown by the fact that from the root *tr- we have /tr[u]/, where the first vowel is in several forms not written; this means that it is not a phoneme.

52. *[a]-ni, but note the variant *[a]-ni.

53. (f). β > y is earlier than i-eponthesis, for *aurt cannot go back to æjë. I think that here the opposition between phonetics and phonology must be considered. *[a]-[e]-[i] > *[a]-[i] which is realized as *[a]-ji because øu does not show i-eponthesis. So no chronological conclusions can be drawn from the subphonemic eponthesis.

53. (f). A *Laurnandel von ʉ(i)- (c. *'[i] u r(ø) (a[r])m)* is not acceptable: Avestan does not drop a (full, phonemic) vowel because it stood between ʉ and r. We have a vocalic r, which is normally written øv, this means that you have a short, probably optional, vowel-like segment before and/or after the r. After t, the r follows so quickly (because the two sounds are homorganic) that (often) no vocalic feature is heard. Thus in *stiætætæ* no vowel segment (which would have been coloured i through influence of the following i) is heard, whereas you have one in kinætæ. In both cases we have Cry- with a slightly different realization. No chronological conclusions can be drawn. (Of course, *ær* for *it* is possible [exactly because it is non-phonemic]: *stætætæ-.*

54. (c). That long vowels were more open is possible, but it has to be shown how this could explain the irregularity of the reflexes. – In Leiden, Michiel de Vaan is preparing a dissertation on the subject.

62. (b). The explanation of the short ø in Skt. anæ- etc. is that he- had not yet merged with *o, so that Brugmann’s law did not apply, as was shown by Lubotsky (cited p. 253).

62 (db). That *an̂ is sometimes written *an in ital probably shows that we have to do with a phonetic, non-phonemic colouring; before final -m the influence of the -m was of course stronger. Dialects are not to be invoked.

64. (f). That the -o in compounds (for -u) was generalized from forms with a nasal, as in datu-ø, *zaizø*- seems improbable to me.

66–67 (hd). "die unrichtige Schreibung "nörah" will be a rendering of the (phonetic, non-phonemic) influence of the t on the preceding nasalized vowel.

76f. The diphthongs are treated in a strange place, under æ.

68 (ic). The transition -æae > -ææ can best be interpreted as the replacement of the glide y by a glide determined by the following æ. We may compare Olr., e.g. *blua from *bhis. It is not necessary to assume a development of æa > Æ > Æ; this is shown by the fact that we have e.g. *miθí, not *mæθí. See on 69 (ic).

69. (ja). The remark that *au is represented by æo also through a and geschlossen o-Laut" is rather disturbing; the æ has become more ò-p.-

69 (jb). Note that the two reflexes of *au are parallel to two of *ai (Beekes 1988, 36). Thus it seems certain that *au is the reflex in closed syllables.

69. (ic). I assume that *-æia first became -ae, which was later diphthongized to -æo. A metathesis æg > øg is improbable. See Beekes, 1998, 71 (b). That -ææ-v takes points to an accentuation æa seems improbable.

70. The notation indicates that the stress caused extra length and there was a short off-glide, as in Dutch when one calls 'fan (with short æ) with much emphasis fahan (it may even become fahän).

72 (cg). That *ææ became -æ is shown by nom. sg. baxa and the i-stem loc. sg. The dative must have been restored.

73 (c). On sínö- see Kuiper, IJ 34, 1991, 37.

75 (2). The forms with -æor- from daitha- are of course secondary, as the word was hysteronastic.

78 (c). There can be no doubt that tütä is trisyllabic in Y 44.4.

79 (f). I sg. ind. -æa-æi, sub. -æa-æi, if I understand the text correctly, but tátä is translated as a subjunctive. There are serious difficulties with the number of syllables in these forms. Cf. Beekes 1988, 92; see also Pír- ars, MSS 47, 1986, 163.

80ff. The treatment of the laryngeals is rather disappointing, in view of the unique information provided by Avestan and the amount of work done. Not even the distinction between initial, internal and final syllable is made with regard to the vocalisation. The reflex of laryngeal between vowels is only sketched.

83. I am very unhappy with the transcription u, uo for earlier y, û. It solves nothing (we still have to decide in every instance whether it was y or û), while it looks awful and makes reading more difficult and disturbs the comparison with Old Persian and Sanskrit. The two or three cases (which are not given) where there is a difference in anlaut could have been solved by writing only (these two or three) words with æ, ææ, æo. (I shall here follow the transcription of the book.)

85. In 5 b. and d. the situation is not very clearly presented. Final *-æ
becomes LAv. -e (GAv. -a). There are two problems. One is the dat. abl. ins. dual -bau, where we find only very rarely -ple (there is no *-bpe). The form -biius has two problems: there is no -e, and -b did not become -b-. The latter problem holds also for dat. pl. -bua and ins. pl. -bit. I see no good explanation for it. Perhaps the two problems are connected. The it may have been restored after the dat. pl. ending. -- The other problem is that beside -she < *-šyva we also find -šthe in the pronouns. A glance at the paradigms in Reichelt, pp. 208–212, convinces me that the *th forms are due to the feminine, where this sound is regular in several forms. Apparently this 'variant' of b was considered typical for the pronouns. So there is, again, no reason for dialect forms. (If this solution is correct, it shows that synoptic paradigms are a necessary instrument.) -- The sequence -še- in inalt is discussed in Sc. and partly in 5d. (buaiba) is Gothic and belongs to § 34. If maišibes is analogical, LAv. vabibes may represent the regular development. But I do not have the material. 86 (tc). Note that zbina- is always trisyllabic in Gothic, which proves /zunaḷjays- < *³paH-eiš-.
88 (b). "nur durch Senkung des Gaumensgelbes": this means nasalization of the preceding vowel. I am not sure that this is what the authors mean.
88 (d). That -q for ą < *-sans would be conditioned by a preceding labial seems phonetically improbable.
90 (aa). "y did not become ąr, i.e. with a phonemic vowel, in Avestan. All instances point to a vocalic r, which was realized with a short, non-phonemic vowel segment in front and/or behind it. See on pp. 52, 53 and hereafter.
91 (bc). That LAv. arte from ąwe has no anaptyctic vowel after the r is understandable: we do not have ar (two phonemes) > ar, but r (realized with subphonemic vowel segments) > ar.
91 (bd). On ātum see on p. 53. pazirabuš cannot be analogical if the ąs are non-phonemic (as I assume). A purely phonetic explanation seems obvious: after a consonant cluster the rise of a prep-vowel is to be expected earlier than after (vowel +) single consonant. (It did not count as a cluster, the group being homorganic, in nazastu the n- is word initial.)
91 (bd). Comments of Kotland and Lubotsky lead me to the following solution of fraorat. In *fraoras the first prop-vowel was coloured to ą, then the y disappeared before the ą, and *fraoras = fraoras. So the "shift of the syllable boundary" is secondary. Probably it is just a matter of realisation, not of phonemic change.
95 (b). Here, as in several other places, one would have liked to have a table so that one could see the whole complex of the developments at a glance.
97 (cg). I wonder whether there is another explanation for the type murziius < "magja than contamination. The form looks so typical Avestan that one supposes a phonetic development. Suppose that between au and ii an a was inserted, as the (unexplained) a in basu- beside buu-, then -ai- could become, as happens more often (p. 68 id.; cf. also 67 ± 0. -ai- before yu, -zii- > -iri-, which gives -auzua. This would be parallel to -banaia-ca, banaia from "baha", p. 57 (top). Cf. also gaha-bhiauus in Y 9.8 in good manuscripts. The length of the first a must be considered together with the whole length of the word. Influence of -auzua "woe" seems improbable, but the form is interesting. Comparing auai ("a-aiat), auoodlei-. "woe-cry" (vooi-ter-), vaat, baaa- and vaaii ("vaii-aiat) it seems obvious to analyse auzua as "a- uai-. For "gai cf. Pokorny 1110. So this form also has -ai- from *-ai-. compare also vooi-, where the short i is suprising.
101 (ce). "Dialektal " ist der jav. Lautwandel von j zu palatalen i."
Could this not simply be a recent development?
102 (dd). *zii- becomes zii- or zii-, for which any analogical differences are assumed. I wonder whether the first form could not be due to analogy after the full grade forms.
103 (dg). The correspondences to Skt. k, gr. zt are not systematically treated. Cf. 1001. If we assume:

\[
\begin{align*}
\text{PIE} & \quad \text{before back vowel} & \text{before front vowel} \\
\text{PIE} & \quad *t̄b > & \text{Av.} & \quad *t̄k \\
\text{PIE} & \quad *d̄g > & \text{Av.} & \quad *d̄k \\
\text{PIE} & \quad *d̄g > & \text{Av.} & \quad *d̄i
\end{align*}
\]
and:

\[
\begin{align*}
\text{PIE} & \quad *t̄k > & \text{Av.} & \quad i (-, not zi) \\
\text{PIE} & \quad *d̄g > & \text{Av.} & \quad e
\end{align*}
\]
we are left with zai (from zm- < *gmn < *dgm-?) and xiasiei. Cf. Beekes 1988, 77 ff.
104. The type buk.baxii- is explained by Lubotsky as a phonetic development; to appear in the GS Schandler.
107 (ca, eb). The appearance of xii for buu or ybii is still unclear. I wonder whether the notations indicated a phonological opposition (in spite of the argument that y was a pheme because of analogical raspu, p. 106 eb).
108 (eh). If unexpanded "huuu- became xii"-, one would think of a (conditioned) regular development rather than of a dialectal form.
129. The forms that do not have -m < -n after labial may be due to analogy (-ant -, 3 pl.) rather than to the fact that they represent -nt, the -t of which had probably long since disappeared when this law operated.
110 (s). The loss of the dental before buu- is rather loss of the first
becomes LAv. -e (GAv. -ai). There are two problems. One is the dat abl -ins, dual -bivirus, where we find only very rarely -e cle (there is no -e in bhe). The form -bias has two problems: there is no -c, and -b- did not become -r-. The latter problem holds also for dat. pl. bius and ins. pl. blit. I see no good explanation for it. Perhaps the two problems are connected. The -r may have been restored after the dat. pl. ending. — The other problem is that aside -she < -sheya we also find -sibhe in the pronouns. A glance at the paradigms in Reichelt, pp. 208-212, convinces me that the sb forms are due to the feminine, where this sound is regular in several forms. Apparently this variant of -b was considered typical for the pronouns. So there is, again, no reason for dialect forms. (If this solution is correct, it shows that syncopic paradigms are a necessary instrument.) - The sequence -ja- in inflix is discussed in Sc. and partly in Sc. ibiyab- is Gothic and belongs to § 34. If eichshe is analagical, LAv. eichhet- may represent the regular development. But I do not have the material.

86 (7c). Note that xhine- is always trisyllabic in Gothic, which proves /ou/ < /bH-ee/. 87 (8). "nur durch Senkung des Gauenseges"; this means nasallization of the preceding vowel. I am not sure that this is what the authors mean.

88 (d). That -g for < *-sbs would be conditioned by a preceding labial sounds phonemically improbable.

90 (aa). *r did not become *r, i.e., with a phonemic vowel, in Avestan. All instances point to a vocalic *r, which was realized with a short, nonphonemic vowel segment in front and/or behind it. See on pp. 52, 53 and hereafter.

91 (bc). That LAv. ar is < *ar has no anapctic vowel after the *r is understandable: we do not have *ar (two phonemes) > ar, but *r (realized with subphonemic vowel segments) > ar.

91 (bd). On *átruhm see on p. 53. Panahvist cannot be analogical if the *s are non-phonemic (as I assume). A purely phonetic explanation seems obvious: a consonant cluster the rise of a prop-vowel is to be expected earlier than after (vowel +) single consonant. (It did not count as a cluster, the group being homorganic, in narahit the *s- word initial.)

91 (bd). Comments of Kortlandt and Lubotsky lead me to the following solution of frasor. In *frasor the first prop-vowel was coloured to *s, then the *s disappeared before the *u, and *frasor = frasor. So the "shift of the syllable boundary" is secondary. Probably it is just a matter of realization, not of phonemic change.

95 (b). Here, as in several other places, one would have liked to have a table so that one could see the whole complex of the developments at a glance. 97 (c). I wonder whether there is another explanation for the type maunyotia < *mauniyot from contamination. The form looks so typically Avestan that one supposes a phonetic development. Suppose that between *au and *e an a was inserted, as (the unexplained) a in bayah- beside *baunyotia, *au could become, as happens more often (p. 68 id.; cf. also 67 a, on *ai before *yù, *stir > *stir-, which gives -anuyotia. This would be parallel to banaunyia-ca, banaunia from banajia, p. 57 (top). Cf. also gaunbanaunyia Y 9.8 in good manuscripts. The length of the first *a must be considered together with the whole problem of the length of vowels. Influence of -anuyotia "woe" seems improbable, but the form is interesting. Comparing anuyotia ("a+yai), annaetlit-, voita- "woe-cry" (voiti-tara-), vauit, bara- and vaiot ("vaiot+yai) it seems obvious to analyse anuyotia as *a+uyotía. For "yai cf. Pokorny 1110. So this form also has *ai- from *a-ai; compare also voita-, where the short o is surprising. 101 (ce). "Dialektal ... in der jav. Lautwandel von *j zu palatalen *x."

Could this not simply be a recent development? 102 (dd). *zn- becomes *zn- or *zin-, for which again dialectal differences are assumed. I wonder whether the first form could not be due to analogy after the full grade forms.

103 (dl). The correspondences to Skt. kṣ, Gṛ. x are not systematically treated. Cf. 1001. If we assume:

- PIE *tḥ > Av. *ṭḥ
- PIE *tṛ > Av. *ṛ
- PIE *dḥ > Av. *ḍh
- and: PIE *tṛ > Av. *l (i.e., not *xṛ)

we are left with zane (from zm- *yṃ < *dyem-) and xiati. Cf. Beeckes 1988, 77 ff.

104 (f). The type buk.baxati- is explained by Lubotsky as a phonetic development; to appear in the Gr Schindler. 107 (ca, eb). The appearance of x as for hou or poth is still unclear. I wonder whether the notations indicated a phonological opposition (in spite of the argument that y was a phoneme because of analogical eubai, p. 106 eb).

108 (eb). If unstressed "hou- became x-", one would think of a (conditioned) regular development rather than of a dialectal form. 129. The forms that do not have m < n after labial may be due to analogy (-ant-, 3 pl.) rather than to the fact that they represent -nt-, the t of which had probably long since disappeared when this law operated. 110 (s). The loss of the dental before bau = is rather loss of the first
part of an unusual cluster in anlaut than dissimilation; cf. *pāt > tā (and *jig- from *djig-*, p. 101).

112. The chapter on sandhi is very succinct, as is the next on the accent. Sandhi almost only regards developments before enclitics (which is not usually called sandhi).

112(A). With mātra- *marakats- with the stress on the syllable before the enclitic.

**Morphology**

In the chapters on inflection I miss paradigms that show the different inflectional types. E.g. p. 139, the *l*-stems should have been split up into separate categories. I remember that it took me quite some time as a student to figure out what types you have. So p. 148 with the difficult paradigms with *-ant-.*

118 Dar. I have always wondered how a PII. innovation (*-aiia) could be replaced by the older form. The forms must have coexisted, as in the nom. pl. *-aia* (whatever the origin of the latter form).

125 (4). It is surprising to find the now completely outdated notion 'schlechttonig' (also p. 71b).

125 (5) idem for the notation intr. (PIE) *-el-ą (for *-el-bh).*

129 (6). The nouns in *-arc* require comment. (138 (1). Note that the other stem of *maz- is mazantu* on p. 147.

142 (1). On *zruuos* see Lubotsky in the Korylowicz Memorial Volume.

137 (B). The nouns in *-arc* require comment. (138 (1). Note that the other stem of *maz-* is *mazantu* on p. 147.

152, acc. Lubotsky points out to me that *fabdrā* does not exist, as had been seen by Benveniste (see Duchesne-Guillem, Les composés de l'Avesta, 119).

153 (2). Nom. sg. ntr.-*er*. The latter form, as far as I know, can nowhere be demonstrated.

153 (2). *hxari* is syllabic in the Gathas; note that initial *hxur-* is always syllabic in the Gathas (Beekes 1988, 15). (On *hxari* see Beekes 1998).

155. The loc. sg. *zravatā "ist dialektal oder verderbt:" this seems another instance where a dialectal form is considered too soon.

160. Several times from here on the bracket is printed too low, e.g. 1.2; this is only a technical accident.

162.1. The presentation of these (pronominal) forms is very unclear.
part of an unusual cluster in anlaut than dissimilation; cf. *pāt > tā (and *jīg-
from *dīj-, p. 101).

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not usually called sandhi).

112(A). With *madraka- cf. *marakat- with the stress on the syllable
before the enclitic.

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inflectional types. E.g. p. 139, the t-stems should have been split up into
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'schleifentöpf' (also p. 71h).

125 (5) idem for the notation instr. (PIE) * dél- / for *-el/er/.

129.1, acc. -am: from *-agam. Often one would like to have more
comment, but it must be admitted that this might have made the volume
weeks as large.

130, loc. *tāto note that this is an i-stem.


134, loc. Add: *tāti, with the ending from the s-stems (Skt. -sw).

135. (j). On *ṛehu- see Lubotsky in the Kuryłowicz Memorial
Volume.

137 (B). The nouns in -aṣc require comment.

138 (1). Note that the other stem of maṣ- is mazānt- on p. 147.

142 (1). On *zruaun- see Lubotsky, FS Beekes 144f.

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162f. The presentation of these (pronominal) forms is very unclear.
duction the authors state that not all forms in the grammar may have been correctly identified. But the grammar gives a very welcome, solid overview of what we know of the language. As stated above, I would have liked a more elaborate handbook, but the present grammar is a well suited instrument for further study. It is a monument of Hoffmann’s work, for on every page one notes the results of his research.

Addendum

The phonemic system of Late Avestan.

I suggest the following phonemic system for Late Avestan. (All further letters will be discussed in the notes.)

\[
\begin{array}{cccccccc}
p & f & b & \beta \\
t & \theta & d & \delta & s & z \\
c & j & i & \hat{i} \\
k & x & g & \gamma \\
h & \eta & \eta \theta \\
y & v \\
r & \hat{r} \\
m & n \\
a & i & u & e & o_1 & o_2 & (\eta) & \eta \eta \\
\end{array}
\]

Notes:

\(\beta, \gamma, \delta\) are largely allophones of \(b, d, g\), but they were probably phonologized (cf. ins. pl. “afnut”).

\(\hat{r}\) is the word final allophone.

\(\hat{r}\) (which I propose to write for \(\hat{r}\)) is the palatalized \(\hat{r}\) of \(\delta\).

\(n\) can be interpreted as a sequence of two phonemes, \(\eta\delta\) (there is no opposition \(n^\eta: n^\delta\)).

\(g\) is unreleased \(g\), only in Gae.-Avestan.\(\eta\theta\) is mostly an allophone of \(b\) between \(a\); but it is unconditioned in \(\eta\theta\theta\theta\), in Avestan.

\(\eta\theta\theta\) is the palatalized \(b\) between \(a\): \(\eta\theta\theta\theta\). \(\eta\theta\theta\theta\theta\) can be seen as a sequence of \(\eta \theta \theta \theta \delta\).

\(\eta\) written \(u\), \(\eta\) and \(\eta\) are allophones in Avestan.

\(\theta\) is the allophone in Avestan.\(\eta\) is this a voiceless \(\eta\) “mit Reibegeräusch”, it could be put beside \(e\). From stressed \(\eta\).

\(m\) is a voiceless \(m\), in \(hmp\). (When written alone, it may stand for \(hmp\).)

\(n\) is the allophone before \(\eta\) – \(n\) is the allophone before stops and affricates.

\(\eta\) is a phoneme because of \(\eta\) < “\(\eta\) – \(\eta\) is not a phoneme; in \(\eta\) it can be regarded as an allophone of \(\eta\).

\(\eta\) is not a phoneme. In the type \(\eta\) \(\eta\) \(\eta\) it can be regarded as an allophone of \(\eta\), in \(\eta\) \(\eta\) as an allophone of \(\eta\).

\(\eta\) from “\(\eta\)”. At a later stage it became a laryngeal: see on p. 259c.

\(\eta\) from “\(\eta\)”.

\(\eta\) is the opposition “\(\eta\)”. I see no reason to assume that \(\eta\) was a phoneme.

The vowel system is quite unusual; Kortlandt calls it most improbable. It is due to a number of recent changes. Thus several vowels were phonologized because of word final developments. Several phonemes are rare, and will probably disappear soon.

Bibliography


Beekes, R. S. P. Avestan -6,-7, duo. MSS 58, 1998, 7–11.

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The arrival of the Umgang of Greeks and Romans with fremden Sprachen – not beschränkt auf the praktische Seite der Verständigung with Angehörigen fremder Völker fremder Zunge, sondern in the weistesten Sinne verstanden – hat bei den beteiligten Disziplinen nicht immer the Aufmerksamkeit gefunden, die ihm angemessen wäre. An Anregungen hatte es allerdings nicht gefehlt, z. B. in einem Aufsatz von Michel Lejeune (Conferences de l’Institut de Linguistique de l’Univer-
quium angeregt, das dann wenige Tage nach dem historischen Fall der Berliner Mauer an der Universität des Saarlandes stattfand (vgl. Zum Umgang mit fremden Sprachen in der griechisch-römischen Antike, hrsg. von Carl Werner Müller, Kurt Sieg und Jürgen Werner, Stuttgart
duction the authors state that not all forms in the grammar may have been correctly identified. But the grammar gives a very welcome, solid overview of what we know of the language. As stated above, I would have liked a more elaborate handbook, but the present grammar is a well-organized instrument for further study. It is a monument of Hoffmann’s work, for on every page one notes the results of his research.

Addendum

The phonemic system of Late Avestan.

I suggest the following phonemic system for Late Avestan. (All further letters will be discussed in the notes.)

\[ p \ f \ h \ \bar{h} \ j \ k \ l \ m \ n \ o \ p \ q \ r \ s \ t \ u \ v \ w \ x \ y \ z \]

Notes:

- \( \bar{h} \), \( \bar{g} \), \( \bar{d} \) are largely allophones of \( h \), \( d \), \( g \), but they were probably phonologized (cf. ins. pl. \( -\bar{b} \bar{a}t \)).
- \( \bar{f} \) is the word final allophone.
- \( \dot{s} \) (which I propose to write for \( \bar{h} \)) is the palatalized \( s \) of \( -\bar{ata} - \).
- \( \dot{m} \) can be interpreted as a sequence of two phonemes, \( \dot{n} \) (there is no opposition \( \dot{m} \) vs. \( \dot{n} \)).
- \( \bar{g} \) is unreleased \( g \), only in \( G\text{-}\bar{g}h \), \( G\text{-}\bar{g}r \).
- \( \bar{g}h \) is mostly an allophone of \( g \) between \( a \); but it is unconditioned in \( \bar{g}a\bar{b}u \), \( \bar{m}a\bar{b}a \).
- The palatalized \( b \) between \( a \)’s: \( \bar{g}a\bar{b}u \). - \( \bar{g}b \) can be seen as a sequence of \( \bar{g}h \) vs. \( \dot{g}h \) (cf. the notations \( gh \), \( gh \)).
- \( \dot{g} \) written \( w \), \( y \)- and \( \dot{g} \)- are allophones in aslant.
- \( \dot{a} \) is the allophone in aslant.
- \( \dot{t} \) is a voiceless \( t \), in \( b\text{-}m \). (When written alone, it may stand for \( h\text{-}m \).)
- \( \dot{r} \) is the allophone before \( a \) - \( \dot{a} \) is the allophone before stops and affricates.
- \( \dot{e} \) is a phoneme because of \( \text{-} a \) vs. \( \text{-} a \) - \( \dot{e} \) is not a phoneme in \( \text{-} a \) it can be regarded as an allophone of \( i \).
- \( \dot{a} \) is not a phoneme. In the type \( \text{-}o\bar{e} \) it can be regarded as an allophone of \( a \), in \( \text{-}a\bar{a} \) as an allophone of \( u \).
- \( \dot{a} \) from \( *-\text{-}ah \). At a later stage it became a diphthong \( \text{-}ah \); see on p. 69c.
- \( \dot{a} \) from \( *-\text{-}ah \).
- \( \dot{a} \) in \( \text{kar} - \) - \( \dot{a} \) I see no reason to assume that \( \dot{a} \) \( a \) was a phoneme.

\( \ddot{a} \): e.g., from \( -\text{-}\dot{a} \). There is no distinction between \( \dot{a} \) (only in one manuscript instead of the other sign) and \( a \).

\( \ddot{g} \): cf. \( \ddot{a} \) in \( \text{zanh} \). I see no reason to assume an opposition between \( \ddot{a} \) and \( g \).

The vowel system is quite unusual; Kortlandt calls it most improbable. It is due to a number of recent changes. Thus several vowels were phonologized because of word final developments. Several phonemes are rare, and will probably disappear soon.

Bibliography

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