

E. Seebold, *Das System der indogermanischen Halbvokale. Untersuchungen zum sogenannten 'Sieversschen Gesetz' und zu den halbvokalhaltigen Suffixen in den indogermanischen Sprachen, besonders im Vedischen.* Heidelberg 1972 (Indogermanische Bibliothek). 360 p. DM 76/84.

The author starts with a useful discussion of the terms and their phonetic and functional significance, though I am not sure that his proposal (semisorants and semivowels are to be classed together as Halblaute = semisounds?) will work. I think one should retain the current usage as much as possible. On the functional level we should distinguish syllabics and non-

syllabics, on the phonetic level vowels, consonants, semivowels and resonants (i.e. semi-consonants?). – It is strange that the author does not point out the incorrect and confusing use of the terms light and heavy *syllable* (preceding the semivowel). Firstly the form of the syllable depends on the realization of the semivowel ($\hat{a}t/ya > \bar{a}/ti/y$). It is particularly confusing that by a ‘light syllable’ in this context is meant a long *syllable*: *atya* = *at/ya* (or better *at/tya*, but this is not relevant here). To remedy this he sometimes speaks of *CYA* (instead of *YA*), but very inconsistently, as is indicated by the frequent use of *(C)YA*. The real difference is between long and overlong (überschwer) syllables, a principle recognized by the author (see below). I shall here speak of light/heavy ‘sequence’, to indicate the problem.

The author points out that a sequence *CRA* as against *CRA* can be either explained by a separate phoneme \bar{R} (which nobody seems prepared to accept for PIE) or by positing a variant [\bar{R}] of /*R*/. This complicated question is connected with the shwa secundum, e.g. when * $\bar{e}uHs-$ is posited for Skt. *tuviṣ-*. This aspect is not discussed. (It is more urgent with liquids and nasals.)

The first half of the book (25–175) is a ‘Forschungsbericht’. Starting from observations before Sievers, the author discusses Sievers’ ‘law’ from 1878 (only two pages!), Edgren’s statistics, and Osthoff’s contention that this law operated also for *r*, *l*, *m*, *n* and for initial consonants. Then follows a sharply critical evaluation of Kuryłowicz’s views (up to *Akzent und Ablaut*). The final conclusion is typical, p. 42: “Es wird hier nicht klar, wie phonetisch und phonologisch bedingte Verteilung, wie Lautgesetz und Analogie gegeneinander abzugrenzen sein sollen.” Fortunately the author does not “base himself on an idea of Kuryłowicz’s” (many of which I consider unhappy), as is almost a fashion today. Edgerton (1934, 1943 and 1962) added a ‘converse of Sievers’ law’ (*iya > ya*), which was a mistake, as this was exactly Sievers’ rule; he thought that it was a matter of allophones, which was also Sievers’ view; and that it was true of *r*, *l*, *m*, *n* also, which was already supposed by Osthoff in 1884. His real contribution was the formalization which impressed many scholars (more for its form than for its truth, it seems). For the ‘converse’ Edgerton’s alleged example (*sunvāḥ* with etymological $u + \bar{u} > v$) is much more easily explained analogically (for the only other Rigvedic instance, *anvartitā*, see now Ingrid Kühn, MSS 28 (1970) 89–104.). This means that there is no evidence for the ‘converse’. Of course, instances with original *iHo* etc. do not show this ‘converse’ either. The conclusion that the theory must be abandoned seems therefore unavoidable. (F. E. Horowitz, *Sievers’ Law and the Evidence of the Rigveda*, The Hague 1974, 39–48, in a very clear chapter now arrives at the same conclusion, and has the same explanation for *sunvāḥ*.) The parallel phenomena with *r*, *l*, *m*, *n* are discussed; for a strict parallelism the evidence is certainly not enough. The author stresses the differences between the different semivowels and ‘semi-sonorants’. He severely criticises Edgerton’s method (new metric interpretations, no discussion of the more important counter-evidence). – Lindeman (1965) started from Kuryłowicz’s views and tried to correct them. He studied only the anlaut. His conclusion was that the sequences semivowel-*a*-vowel are still consistently syllabic, but that Sievers’ law acted upon them for a short time. This last addition, as the author says, “gewissermassen als Zugeständnis an Kuryłowicz”. The evidence for it is extremely meagre. The author does not consider the first conclusion established, because *seṭ-* and *aniṭ-* roots cannot be contrasted in the same morphological categories. It is a pity that the author does not distinguish between *seṭ-* and *aniṭ-* roots in the relevant part. Lehmann (1968) assumed that a laryngeal counted as a full consonant and ‘made position’ when Sievers’ law operated. He seems unaware of the fact that the idea had already been suggested by Kuryłowicz in 1927 and by Kuiper, *Notes on Vedic Noun-inflexion* 28, n. 2. Lehmann weakened his case by including the supposed sequences *HC* which the author has no difficulty in refuting (Kuryłowicz was right in restricting research to *CH*). The cases with *CH* (*hāvīya-* < **ghouH-ijo-*?) are in principle possible. The author concludes that the idea is “zunächst einmal widerlegt”, but adds “dass die Beispiele viel zu sporadisch sind”. This is something different: the hypothesis, then, has neither been proved nor disproved. Regrettably the author did not consistently check this possibility. It is, of course, a matter of relative

chronology. Relevant is the rise of the surd aspirates, *rotHo- > rátha-. First it should be observed that here a sequence of two consonants is reduced to one without compensatory lengthening of the preceding vowel (so that some time before the laryngeal might not have counted as a full consonant). Secondly the hypothesis implies that the aspirates still did not exist even a short time before the Rigveda: if the laryngeals were separate phonemes (in this position) and their effect with regard to Sievers' law is still seen in the Rigveda, they must have existed till shortly before the beginning of the Rigvedic tradition, which means that there were at that time no aspirates. There is, however, no reason to doubt the existence of these aspirates. The origin of the aspirates is probably of Proto-Indo-Iranian date (e.g. *sákhā*, Av. *haxa*, OP *Haxāmaniš*); see Kuiper, *Notes* 27f. (It was certainly not a PIE development, as Greek does not show it; see my *Development* 179ff.) If this is right, any influence of laryngeals on the length of the preceding syllable with regard to Sievers' law is excluded. After resonants they might have been retained longer. Here the date of Brugmann's law is relevant. It is probably of PII date, but it does not give an indication of the *disappearance* of the laryngeals in this position. The only instance for which the possibility is considered by the author is *sakh(i)yá-*. It probably had a laryngeal because of the aspirate (not "wegen seines ungewöhnlichen Paradigmas"; it has a normal hysterodynamic inflexion, a type accepted by the author). If the laryngeal was a factor, we would expect the same relation between *y/iy* in the inflexion of *sákhā* as in the derived *sakhyá-*. But *sákhā* has *iy* 7 times and *y* 20 times (i.e. $\frac{1}{4}$ *iy*), while *sakhyá-* has *y* 34 times against *iy* 110 times (I counted in Grassmann 33:89), i.e. 1 *y* to 3 or 4 *iy*, which is almost the opposite. The forms of *sákhā* have *-ye* 9 times: *-iye* 0, *-yuh* 8: *-iyuh* 2, *-yā* 3: *-iyā* 5 times. This means that the author is right in considering these forms together with *i-* and *devī-* stems, where the high number of instrumentals with *iy* can be ascribed to the influence of the *vrkīh-* type. This means that *sákhā* gives no evidence for influence of the laryngeal. Cf. also Av. *haše*, *hašqm*. For *sakh(i)yá-* a different explanation is necessary. Here 15 out of 33 instances of *y* occur in books 1 and 10, but only 31 out of 89 instances of *iy*, i.e. *y* is more frequent in the younger parts. The forms show a remarkable distribution: *-iyam* 18: *-yam* 20, *-iyāya* 25: *-yāya* 2, *-iye* 17: *-ye* 8 (of which 6 in books 1 and 10), *-iyā* 17: *-yā* 0. The difference between the first two might be explained as due to the metre: *sakhiyām* can be $\sim\sim$ as well as $\sim\sim$, *sakhiyāya* always begins with $\sim\sim$. As a sequence of three short syllables is inconvenient, it is understandable that *sakhiyām* is preferred. (But *-iye* and *-iyā* could also give three short syllables.) It seems, then, that here *iy* is the original form, which requires the special suffix *ya* (on which below). Only the high number of exceptions perhaps requires further explanation. Was it influenced by the stem *sakhy-* of *sákhā*? I think conclusive evidence for the influence of the laryngeal is found with the comparative suffix *-(ī)yas* (p. 285). There are no forms of which it is sure that they are simple Sievers' variants (only *nāviyas-* 1.105, 15; but *nāviyas-* would fit as well). It is most probable that there were originally such variants and it seems that these have been transformed into *-īyas*, also because the *-yas* forms always follow a light element. As there are only very few *-yas* forms, it is probable that these are old (and do not replace Sievers' forms). Then *tāvyas-* is old, and here a laryngeal is sure. This means that **teuH-īos-* did not have a Sievers' form, and that the laryngeal remained consonantic and left no trace. (Therefore the *ī* in Skt. *-īyas* does not come from disyllabic roots as supposed Brugmann *Grundr.* II 1, 551, Benveniste, *Origines* 84f.) Also for *śréyas-* a Sievers' form **kreiH-īos-* would more probably have resulted in **śráyīyas-*. This treatment is confirmed by Gr. μέζων < **meǵh₂-īos/n-*. If ἀρείων ἀριστος contain the root *ape* < **h₂erh₁-* seen in ἀρετή, Myc. *arjoh-a* may represent **h₂erh₁-īos-*. If μάλα μάλιστα has a disyllabic root (**mlh₂-e/-is-*), **μελλον*, the supposed antecedent of μάλλον, can continue **melh₂-īos-*. This proves, I think, that *H* did not count as a full consonant with regard to Sievers' law. It must have been a full consonant once, so we must assume that when it disappeared (in these positions) the Sievers form automatically disappeared too. However, the author is prepared to make an exception for the group *Csy* (p. 315, 336). I have pointed to the parallel behaviour of *H* and *s* in *Development* 123f. This leaves the possibility that *H* (and *s*) never did count the same as other consonants in this respect.

In the following part Sievers' law outside Vedic is studied. Half of this section (pp. 64–98) is devoted to Germanic. In Gothic there is a grouping according to whether a light or a heavy syllable (or a 'Nebensilbe') precedes. (I don't see why "nach wechselnden Auslautgruppen", i.e. *tau-/taw-, sto-/stau-* is mentioned separately with the first group: there is no difference between *tau-, sto-* and *lag-*.) As regards the *jan*-verbs, the author rejects the hypothesis that the distribution with the original causatives (*eje*) and the denominatives with *e-je, i-je* is analogical. In this view there is only the (very early) effect of Sievers' law. This is improbable because it supposes that a non-functional difference would have been introduced in a uniform category. Also there are several indications of a recent Sievers-like tendency, e.g. the redistribution of nom. sg. fem. *-i* < *-ih₂* and *-ja* < *-jā* (*-ieh₂*). The explanation mostly followed (and here made explicit for the first time) supposes for *C-je* Sievers' variants *je/ije* > PGm. *ji/tji* > *ji/i*, for original *eje* > PGm. *iji* > *ii* > after light element *ji*, after heavy *i*. This supposes a Sievers' effect twice over. This again supposes either a very long operation of the law, or two different ones. In the last case an explanation is also possible with only the latter effect: if Gothic developed *ii* into *ji* or *ei*, then it is probable that also *je* > *ji* after heavy element became *ei*. This means that Gothic in itself does not prove Sievers' law for PIE. Old-Icelandic too has evidence for very late Sievers-like effects (e.g. the distribution in the case of the causatives is consistent, and this cannot date from (the old) Sievers' law), but the presence or absence of *e*, e.g. in nom. sg. masc. *ja*-stems *-r*: *-er* must reflect PGm. *jaz*: *ijaz*, i.e. real Sievers' forms. The runic inscriptions give only little evidence, but this fits exactly: after heavy 'sequence' they always have *ija*, after light ones three times *ja* and six times *ija*. That in the last case two suffixes must be assumed, seems evident from *harja, -harjaR* (cf. *κοίρανος* < **korj-*) against *-warijaR*. (Cf. now W. Krause, *Die Sprache der urnordischen Runeninschriften*, Heidelberg 1971, 94f.) The Finnish loans do not allow definite conclusions. Germanic counter-evidence is refuted. (The section on p. 96f. on the operation of two contemporaneous sound laws is methodologically important.) The conclusion for Germanic is that sure evidence for Sievers' law (i.e. as a PIE inheritance) is hardly found, though some Old-Icelandic facts probably presuppose it. Schlerath's study of Avestan is discussed. The author rejects his conclusions for *tuw/θw-* in the personal pronoun: the opposition between the nominative and all the other cases is essential. The evident explanation, that **tū* = **tuH* and that **tuvam* results from **tuH-am* whereas the accusative **tvām* never had a laryngeal, is not mentioned. It is one of the most clear instances that show that laryngeals can explain exceptions to Sievers' law. With the *ya*-suffixes after heavy 'sequence' (almost) only *iya* is found, after light 'sequences' *ya* and *iya* (*āvīšiya-, manahiya, māzainiya-, raiḍiya, zaviya-* and *mariya-* 12 *iy*: 3 *y*, *nairiya-* 4 *iy*: 6 *y*). For Baltic the author thinks Sommer's theory as a whole the most probable but he does not consider it reliable to use it as a basis for research in other languages. For Slavic a principle for the distribution of *i* and *ij* has not been demonstrated. In Latin the only interesting point is the opposition *audire: capere*, of which the first occurs after a heavy 'sequence' and after a light 'sequence' when *i* follows a resonant or *v*. The author rejects the explanations suggested and proposes a new one, which is certainly worth considering though not all steps can at present be definitely proven. He starts from two Sievers' variants, *-jesi: -ijesi*. In the second the *i* was lost and contraction followed, *-ies, -iis* > *-is*. In the first *i* would have been lost after stop (> *is*), but was retained in other cases (**veniis*), where the *i* was later vocalized (**venīs* > *-is*). Celtic is not clear enough. In Greek no independent evidence has been found (not for *-vFω/-vúω* either; *-vω/-avω* rather has a different origin though it is *distributed* in accordance with the law). However, for the comparative suffixes *-yωv* and *-ωv* are distributed according to the law. This is strongly denied by the author (124f; and by Seiler 18), but he is evidently too sceptical: the exceptions are few, some of them can be accounted for and Mycenaean (not yet known to Seiler, and not mentioned by Seebold) removes some of them. See Ruijgh, *Etudes* 100, *Lingua* 36, 1975, 92f; Perpillou, *BSL* 69, 1974, 99–107. The fact is rather important: it adds Greek to the list which, according to the high requirements of the author, contains only Vedic and perhaps Germanic. For Hittite too nothing has been found (only 2pl. pr. Middle *-duma* < **-duya* seems a Sievers' form).

A short chapter is devoted to the 'Begründung für die Sieverssche Regel'. The author rejects the theory of the influence of the syllable boundary, of which the essential point is that consonant groups at the beginning of a syllable were not tolerated (C_i was then changed into $C_i\bar{i}$). I do not quite understand the author's counter argument. I would say that there is no indication that a group C_i was avoided: it is rather frequent in word initial. Also one would then expect that groups of two stops were even more strenuously avoided. The author objects to theories based on syllable boundaries (p. 131): they "beruhen durchweg auf Zirkelschlüssen. Aus bestimmten sprachlichen oder metrischen Erscheinungen wird auf die Silbengrenze geschlossen, darauf werden diese sprachlichen oder metrischen Erscheinungen mit Hilfe der Silbengrenze erklärt." I think this criticism is too general to be convincing, and it is slightly misleading in that it mixes up "sprachlich und metrisch". Our idea of the (PIE) syllable rests primarily on the metrical evidence. This is confirmed by the testimony of ancient grammarians and of inscriptions. If the conclusions thus arrived at can explain linguistic developments, this is only a confirmation. Then the point might come where linguistic phenomena induce us to reconsider some minor point. I don't see that this normal process is a *circulus vitiosus* (cf. the interaction of the study of material and of theory). The author's own theory (based on an idea of Kuryłowicz) is that the essential thing is that overlong (überschwer) syllables (Silben) were avoided. He then says (p. 132): "Wir brauchen dabei zur Bestimmung der Schwere einer Silbe die Silbengrenze nicht zu kennen, sondern rechnen einfach von Silbengipfel zum nächsten Silbengipfel". Again I do not understand this. To decide whether a syllable is heavy one must be able to say what a syllable is. This could be remedied by leaving out the term syllable altogether, but in that case the explanation is reduced to zero: it simply repeats the conditions of the phenomenon. Overlong is defined by the author as $\bar{V}CC$ or $\bar{V}CCC$. In the latter case, according to accepted theory (Szemerényi, *Einführung* 100), the syllable boundary was after the first consonant: $\bar{V}C/CC$. This would mean that the syllable was just long, not overlong. If, then, the existing theory is correct, it contradicts the author's theory, however vague it is formulated.

Pp. 133–150 other explanations of the opposition $CYA:CIYA$ are reviewed. That which ascribed it to the accentuation (Fick) is dismissed, as there are no indications that this was a determining factor. The opposition productive: unproductive (Kuryłowicz, worked out by Nagy, *Greek Dialects and the Transformation of an IE Process*) does not seem useful either: Nagy's attempt is rejected by the author (as it is by Strunk, *IF* 78, 251–259), because it requires too many secondary hypotheses. Others supposed different suffixes. This possibility, specially that of two nominal suffixes, notably as proposed by Arnold, *Vedic Metre* 81–107, is kept open. The theory of Specht, who assumed that $i\bar{o} : i\bar{i}o$ originated from $i + o : \bar{i} + o$, is combined by the author with Meid's observations on lengthening of the vowel before a suffix. He takes it as his working hypothesis. I shall come back to it below. The idea that the use of variants was merely a metrical question was worked out by Sihler, *PIE Post-Consonantal Resonants in Word-Initial Sequences* (Diss., Yale 1967). The author accepts Sihler's thesis that the occurrence of the variants was "metrisch bedingt", but suspends his judgements on his theory of the *origin* of the variants. To my mind, however, *this* is the essential point – it is the subject of Sievers' law – and here Sihler's theory seems to me very improbable: the postrigvedic development iya , $uva > ya$, va gave variant forms, and they would have induced iya , uva beside original ya , va .

Pp. 150–154 give a discussion of the Middle Indic and Oscan-Umbrian evidence, which has never been studied in this respect, but allows no conclusions.

Then follow (154–175) the conclusions and the working hypothesis. In "The influence of the metre on the anlaut-variation" the variants of $tvám tvám$ etc. are studied. It is found that after light syllable $tv-$ occurs, but after heavy syllable many more unexpected cases of $tv-$ than expected of $tuv-$. On the other hand the distribution exactly fits the metre. It is (first) concluded that the distribution is "metrisch bedingt". I am not convinced that this conclusion is permissible. As the words (variants) do stand in verses, they naturally conform to the structure of the verse (unless there is something wrong with the verse). (Cf. on this aspect my

study on the wordforms in the Greek hexameter in *Glotta* 50, 1–10). Of course the place of a form in a verse is metrically conditioned. But this is not relevant to our main problem, the *origin* of the variants. Therefore the only right conclusion is given at the end (p. 162): “Wenn die *Verteilung* tatsächlich nach dem *Metrum* geregelt ist, besagt dies selbstverständlich noch nichts über die *Herkunft* der Varianten”. The author convincingly demonstrates (165–169) that the metre is not relevant for the inlaut variation.

The chapter Zusammenfassung und Arbeitshypothese (169–174) is not quite satisfactory. It is stated that “von dem ursprünglichen Sieversschen Ansatz fast nichts mehr übriggeblieben ist: Nicht die völlige Abhängigkeit des Gegensatzes *CYA* : *CIYA* von der Schwere der vorausgehenden Silbe und nicht der Ansatz eines ursprünglichen *CIYA*, das nach leichter Silbe zu *CYA* ‘verkürzt’ wurde”. What remains is that after a heavy syllable only *-iya-* (= the syllabic variant) occurs, a rule the author wants to be called “Anschlussregel (die Regel der Beschränkung der Anschlussmöglichkeit von Halbvokalen an Konsonanten)”. This would be part of “Das Prinzip der Vermeidung überschwerer Silben”. The author then accepts the idea that there were two suffixes, $i\bar{o} < i + o$ and $i\bar{i}o < \bar{i} + o$, the long \bar{i} being due to lengthening.

As to Sievers’ law, the principle given seems to me to be the *explanation* of Sievers’ observation, an explanation, it is true, that starts from *CYA* and not from *CIYA*. I also agree that there is also a further factor involved (explaining *CIYA* after light syllable). But, apart from this addition and the change in the explanation, Sievers’ central observation to my mind stands: the realization of *ya* is conditioned by the length of the preceding syllable, and I would think it unfair to deny it the name of Sievers’ law. (On the other hand I would agree to delete Edgerton’s name from it; cf. above.)

As to the two suffixes I would prefer Burrow’s hypothesis of a suffix *-iHo-*. This is dismissed by the author p. 145: “Für ein solches laryngales Suffix gibt es keinerlei Begründung, – es scheint mir deshalb auch keiner weiteren Diskussion wert zu sein”. This approach rather testifies to a general disinclination towards the laryngeal theory. When we assume that \bar{a} was (exclusively or mostly) a consonant and a separate phoneme that could occur in all positions – and *this* is the essence of the laryngeal theory – then this hypothesis should be taken seriously. For such a ‘laryngeal suffix’ see Hoffmann, *MSS* 6, 35–40 (*-Hōn-*). It should be recognized that it is very difficult to demonstrate such a suffix and that this kind of evidence is one of the ways of proving the presence of a laryngeal. (It might be left open here whether we have to posit *i-Ho* or *iH-o*, as Burrow does, *Skt. Lg.* 185. Perhaps even both may have occurred.) Compared with the idea $i\bar{i}o < \bar{i} + o$ a suffix *iHo* is more probable. First I think the lengthening of *i* before *vowel* (in ‘Meid’s’ suffixes it occurs before consonant) very improbable. There is no parallel for it in PIE and it contradicts everything we know about this language. Secondly, it assumes the existence of a long \bar{i} which does not result from $i + H$ (the same problem we have with the supposed PIE variant of the comparative suffix $-(\bar{i})\bar{i}os-$). One might ask on whom the burden of proof rests, but I think we cannot solve the question unless it is demonstrated that in one instance a long \bar{i} cannot derive from $i + H$. These two reasons induce me to prefer *iHo*.

Half of the book (176–307) consists of a study of the Vedic material for the semivowels. The morphological categories are studied one by one from the more loose to the most close connection between y, ν and the preceding element.

Kompositions- und Satz-Sandhi (176–184) is very limited. It would occur only if there is a strong unity and it would be recent. – Very convincing is the interpretation of the \bar{i} -stems and the type *devī* (184–190). After heavy syllable *-iy-* (*-iyai-*, *-iyāh-*, *-iyā-*) is almost always found in the instr. sg. and the gen.-loc. du. (in accordance with Sievers’ law). In the other cases $-y-$ is as frequent as *-iy-*, but $-y-$ can be shown to be recent. After light syllables we find both *-iy-* and $-y-$ (56:45) in the instr. sg. and gen.-loc. du., in the other cases 253 times $-y-$ against 21 times *-iy-*. The author convincingly explains the high number of *-iy-* forms in the first two cases on the basis of the identity of the endings in only these cases with the $\nu\bar{r}k\bar{i}\bar{h}$ -inflection. It could only be added that for the growing influence at the end of the Rīgvedic period of the $-y-$ forms, which evidently started in the cases that were not supported by the $\nu\bar{r}k\bar{i}\bar{h}$ -inflection, an important

factor will have been the very high percentage of forms with light preceding syllable: 254 as against 50, i.e. 83%. (P. 187. Nach leichter Silbe ISg *devī* 21 γ , not 15.)

Zero grade verbal stems in \check{i} , \check{u} (p. 192–194) are for the first time studied consistently. A clear set of rules is found: if the anlaut has a consonant group, it has *-iy-* (*kṣiyānti*). If not, it had *-iy-* (*ciyantū*), unless preceded by augment or short reduplication (*juhvatī*). I think the roots in \check{i} , \check{u} should be set aside (*bruvānti*, *suve*), because here only *-iy-* can be expected. In Seebold's discussion a diachronic problem has disappeared in a synchronic description. (It is interesting to compare verbs of the type *syāti* < *sH- $\check{i}é$ -, which do not conform to these rules, neither as sHya- nor as sya-.)

On pp. 196–199 (verbal stems in \check{i} , \check{u} before suffix) we see difficulties as there are several exceptions to the rules of the preceding paragraph (e.g. *ahuve*: *ahve*, *suvāná*-: /*svāná*-/, written *suvāná*-). The author states that there is no distinction between *set-* and *anīṭ-* forms (as stated in the preceding paragraph), but I think that an investigation should start by keeping the two apart. It seems relevant that *suvāná* is from *sū-*, /*svāná*-/ from *su-*. The same problem recurs in the next paragraphs, e.g. p. 202, where Kuiper's article on *ābhva-* (*Lingua* 11, 1961, 225–230) is not mentioned.

Very important is the study of the absolutes (208–218). Those in $\check{y}ā$ which do not conform to the Anschlussregel seem recent (book 1, 2 and 10). Those in $\check{uryā}$ are kept apart "wegen der unsicheren Chronologie" (see below). For $\check{divyā}$, however, I can see no reason to do that (*dīHy-* had the same structure as *dīv-*). Those in $\check{tvī}$, $\check{tvā}$ do not conform to the Anschlussregel at all, which is the more strange as $\check{tvā}$ has *-uv-* very often in the Atharvaveda. The author concludes that the explanation must be found in a later 'correction' of the text. He supposes that after a heavy 'sequence' the original form was $\check{vā}$, which means that $\check{tvā}$ was not a form of a *tu*-noun, but had the \check{t} - that was added to zero grade *anīṭ*-roots. The situation with $\check{vā}$: \check{t} - $\check{vā}$ would have been exactly parallel to that of $\check{yā}$: \check{t} - $\check{yā}$. This explains the Rigvedic problem as well as the situation of the Atharvaveda "wenn angenommen wird, dass die Verallgemeinerung des \check{t} in der gesprochenen Sprache (vielleicht nur in einer bestimmten Mundart) so früh erfolgte, dass die Anschlussregel noch wirken konnte." (p. 217). I think this explanation, which has rather important consequences, is worth serious consideration. (The "morphologische Anstoss" that *tu*-nouns have full grade, the absolutes in $\check{tvā}$ zero grade, need not be a problem: it could be a regular ablaut.) In the same way a form * $\check{v}i$ is reconstructed.

\check{Y} , \check{v} in suffixes (218–301) is twice as large as the remaining part of the study of the material. The gerunds in \check{t} - $\check{y}a$ - present a problem inasmuch as after light 'sequence' we find $\check{y}a$ - with the simple roots, but $\check{y}a$ - with the compounds (*gūhiya-*, *tūjīya-*: *a-budhyā*-). The author thinks the conclusion unavoidable that these represent two different suffixes. He takes the first type together with the later types that were derived from full grade *a*-stems (*jōṣiya-* from *jōṣa-*) and *a*-stems with lengthened grade (*kāmiya-*), and which follow the pattern of the denominative adjectives. Though it is granted that there are instances and tendencies in Indic to treat compounds in a way different from the simple forms, I do not think it a priori probable that there were two suffixes which so much resembled each other in form and function. I think a phonetic explanation would a priori be much more probable, and I think it can be found. The suffix $\check{y}a$ - to my mind is best understood as representing $\check{i}Ho$ -. If we accept this, the form of the compounds is explained easily by a phenomenon which it is necessary to posit on other grounds, i.e. the loss of the laryngeal in compounds. We then get * \check{n} -*budh-iHo*- > *a-budhyā*- exactly parallel to * \check{n} -*bhuH-o* > *ābhva*- 'monster' (Kuiper, cited above; in general *Die Sprache* 7, 1961, 14–31). It is, I think, a fine proof of the explanatory force of the laryngeal theory in general and of the correctness of Kuiper's theory in particular.

A problem is presented by *pānya* ($\check{Y}y$: $0\check{y}$). Here we must assume a simple $\check{i}o$ -. Also *jūr(i)ya* (once) and *ā-hūr(i)ya* (once) must have $\check{i}o$ -, because * $\check{g}yH-iHo$ - would have given **jūriya*-. *Ajuryā*- has puzzled many scholars. Kuiper (*Lingua* 11, 229) already assumed loss of the laryngeal in compounds: * $\check{g}yH-i\check{o}$ - > **jrya*- (> *-jurya*-). (Here also double loss of laryngeal

from **-ḡrH-iHo-* could be assumed, but with this suffix *jūriya-* cannot be explained.) This means that after all we do have to posit two suffixes, but for the general *-ya-* in compounds the above interpretation remains more probable.

The secondary nominal suffixes form the greatest group, and within this group the denominative *ya*-formations (without *vyddhi*), 243–278. The author carries out a strict formal analysis based also on the accentuation. As regards Sievers' law, it turns out that only adjectives derived from substantives (as opposed to such from adjectives, and neuters and (other) nouns) present a suffix *-iya-*; they have 'preserving' (bewahrend) accentuation as against others with *-ya-* and 'contrasting' (Kontrast-) accentuation. It appears that *dīv(i)yá-* has 25 *y* against 66 *īy*. The *y* is concentrated in two formulas (*vásu . . . divyáni pārthivā*, and *divyāḥ suparnāḥ*). The author concludes that this was the ancient form. It would follow from this that already in the beginning of the Rigvedic period the opposition *ya : iya* was no longer relevant and that *-iya-* was secondarily extended. This means that we could find traces of the original opposition only in some archaic types. He then tries to establish this functional opposition on the basis of the most frequent adjectives, but these are only a dozen. His result (p. 272f.) is: adjectives with *iya* denote typical properties (*nāriya-* 'die für einen *nṛ-* typischen Eigenschaften besitzend'). Those with *ya* indicate "Herkunfts- oder Teil-Relationen" or indicate syntactical subordination ("dient der formalen Unterordnung . . . , d.h. es hat nur syntaktische, keine semantische Funktion"). However, the evidence on which this is based is very meagre. The Anschlussregel has no more than one exception on fifty, but a remarkable exception is *sūr(i)ya-* 125 *y* : 261 *īy* (below).

The verbal *ya*-suffixes are rather regular. In the present, the passive and the future (*syā*) the *y* has been extended to the few instances where heavy 'sequence' precedes. Suffixes with *v* (292–301) rarely occur after heavy 'sequence', which explains that exceptions to the Anschlussregel are more numerous. There is no indication for an independent *-uv-*.

Initial groups that were not included in the foregoing are discussed 301–306. An interchange of *y/īy*, *v/uv* is found with words where "Wortausgang" (Stammvokal + Endung) follows the semivowel or is separated by "eine Folge von Lauten, deren erster teils ein Sonant, teils ein Konsonant ist", a phrase which I do not understand.

Chapter VIII (307–332) gives the conclusion of the Rigvedic material. First those instances are put together where a correction of the text seems necessary, with a clear theoretical exposition. A second paragraph treats the sequence *ūr*, which is almost always followed by *y*, *v*. Here the conclusions of the laryngeal theory – which, it should be reminded, is not a hypothesis that can be simply left out of account (we are here not concerned with the number of laryngeals), but a study of the behaviour of what was reconstructed earlier as *ə* – are at variance with the author's suggestions, which, it should be added, are pure *ad hoc* suggestions. First, he thinks that *-tūriya-* stands for **-turiya-* from **-tṛHya-* with *r > ur* and *H > i*. However, all relevant material shows that *H* (that is *ə*) could not be vocalic in this position. We must retain, then, that *ūr* from *rH* still conformed to Sievers' law. Then the author concludes from *prthvī* "dass *ə* vor unsilbischen Halbvokalen optional schwinden konnte". I think this is correct (see above on *tavyas-*), but *prthvī* does not prove this. *Prthvī* is a recent analogical creation (cf. Kuiper, *IJ* 9, 1966, 224). If the assumption would nevertheless be valid, (**rHya->*) *gya* would have given *urya*, and *ur* would have been replaced by *ūr* only after the composition of the Rigveda. This seems not very probable, also given the fact that *ajuryá-* and *pūrya-* are retained in the text. In this connection I would rather recall the problem of the development of *rH-Co > ūrCa*, where a long syllable (*rH*) is replaced by an overlong one (*ūr*): to retain the (normal) length of the syllable *ur* would have sufficed, i.e. the lengthening of the *u* is not explained. However, for several instances (of *ūry* etc.) there are proper explanations. For *pūrvī* e.g., the *v* (1 *uv* : 80 *v*) seems analogical, as in *sādhvī*, *svādvī*, *prthvī*. But I have no explanation for *pūrva-*. It is a pity that all the material is not presented together here. The author's suggestions about *sūr(i)ya-* are unsatisfactory. He starts from a sequence *səul-*, which is given by **sāwel- < *seh₂u-el-*, but *súar/sūr-* rather point to **suH(e)l-*, with a metathesis which must be assumed more often.

(A form **sau̯-o-* (>*sūra-*) is improbable: a sequence *ʃo* is only acceptable if a laryngeal disappeared between them. A development *ə > u* may be theoretically acceptable, but it has never been demonstrated.) Given the sequence *suHl-*, the oldest form must have been **suH₂lyā-*. (The general problem whether *TRi* was realized as */TRI̯i/* or as */TR̥i/* has not been put in this book.) Here *ya* is regular, but the form is trisyllabic. I don't know what the regular development of **suH₂ryā-* was. If *sūryā-*, then this should date from after the Anschlussregel, or we would again have a trisyllabic form (*sūriyā-*). Rather **suH₂ryā-* went to **sūryā-*, which was replaced by *sūryā-* early enough to give *sūriyā-*.

A paragraph is devoted to the anlautproblems. The discussion of this problem is somewhat scattered: pp. 56, 146, 191, 192, 196, 301, and 323. As happens often, the discussion here is very theoretical as no examples are given. It would be better to begin with the last paragraph, which is also the most satisfactory. The author maintains that *Cī/CiH + A > CIYA* and *CI + A > CYA* unless it is preceded by a consonant group (> *CCiYA*) or a group that is not tolerated (*yv, rv*). He concludes that there is no evidence that the metre ever *caused* the rise of a variant. This again is more clear than earlier remarks. – As to the chronology of the deviations, some appear in the recent sections of the Rigveda, others show a higher frequency in the second book and the second half of the first (the author concludes to a separate dialect), others (non-syllabic forms after heavy element) appear in all parts.

The appendix (333–337) shows that Avestan and the Aśokan inscriptions appear to reflect the same system as that of Vedic.

In the last chapter the results for PIE are discussed. It is stated that only Germanic has a comparable *Anschlussregel*, that this does not *prove* that it existed already in PIE but that this is nevertheless probable. The author asks himself whether the rule or the underlying structural principle (avoidance of overlong syllables) was PIE. Here it may be relevant that in Greek prosody overlong syllables are avoided. Then a model for the system of semivowels of PIE is given, i.e. an alternative to Edgerton's system. It is much simpler inasmuch as liquids and nasals are left out. I must say that I don't see how the model follows from the preceding. E.g. it is stated (345) that when a sequence of two syllabic semivowels arises, the first is non-syllabic (giving *KYUK*, not *KIWK*). This may be true, but it does not follow from the preceding study. We then get assertions like "Ich halte . . . für die einzige ursprüngliche" (346), without further proof. (The absence of laryngeals is felt time and again. E.g. *īya* is regarded as derived from *īiio > iūjo > ijo*; *iHjo* would be easier.)

I have taken much space for the discussion of this book, because I think it deserves it. We often read that painstaking analysis is necessary, and are also often disappointed by the results. Here we have a book which is an example of how such research should be done, and of what the results can be. It is always a pleasure to read Seebold's clear and well thought-out discussions. It is a most valuable contribution for which we must be very grateful. It is a pity that the author is so sceptical about the laryngeal theory, but this is not too grave: the book provides a good base for a systematic study of the rôle of the laryngeals in this context.