

The Laws of Indo-European: the State of the Art (1998)

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Since the main compilation of the Indo-Europeanists' laws in Collinge 1985 a revised tally has for some time seemed desirable. Another score of laws was suggested in 1992 (the verbal version in 1991, the resulting publication being Collinge 1995). It now seems worthwhile tidying the list yet again, in several ways. First, some named IE rulings, previously at best sporadic, have really ceased to be generally cited at all. Ascoli's, Darmesteter's, Lehr-Splawinski's, Petersson's, Havlik's, Schmidt's and Torbiörnsson's laws may reasonably be returned to obscurity. Some of the 1985 canon are perhaps beyond their shelf-life: Siebs' is a case in point. Other more recent candidates have not secured enough public approval, or notice, for inclusion. Of those canvassed in Vennemann 1988 none has earned recognition (except the non-IE KLINGENHEBEN, on which— and like statutes— see Collinge 1994: 4061-4063). One might consider a possible second law by Prokosch (see Auer 1989: 1080-1082), on which something is said below under PFALZ.

Secondly, consideration of recent discussions does lead to perhaps six new recommendable laws. They are new *either* in the sense that (like some in the 1995 list) their invention is itself recent, *or* because the title of 'X's law' has been accorded to them only since 1985 (or even 1995) although the original observation of the events they codify was made as long as a century ago. These newcomers, in alphabetical order, are: AĀRYAN, FRITZ, JOHANSSON, LAUTENSACH, LIDÉN and PFALZ.

Their right to the accolade, and to citation in capital letters, is debated in what follows. In each case one needs to know the law's date and place of origin, its domain (the affected languages) and its locus (whether within phonology or morphology or lexis, whether general or dialectal), its precise provision, its degree of regularity, its wider implications for language history or systemic functioning, and finally some diagnostic examples. Then one can sensibly assess its claim to recognition.

Two criteria are vital:

- (i) The pronouncement should have been called 'X's law', preferably in print, not merely by its inventor but by independent scholars (or one such independent scholar, if he/she is recognized as influential in the relevant field);
- (ii) It should concern an event, or set of events, in the evolution of *either* several IE languages *or* a single IE language provided that its operation there involves rethinking some general aspect of comparative-historical study or of linguistic analysis.

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It is *not* a criterion, nor a hindrance to acceptance in the canon, that a ruling has been, or is currently, denounced as false. For one thing, numerous laws have vacillated between acceptance and rejection (BRUGMANN is a celebrated case, where even its inventor despaired of it, yet later revision led to its resuscitation); for another, present or future detractors may still succinctly refer to the disputed ruling as 'X's law', leaving their readers to find out just what it was. Nor do all critics cite the law they attack in correct detail. Of course, of the six laws discussed below some are distinctly less convincing than others.

Despite functional similarity in some cases, the first four laws here displayed are alphabetically ordered. FRITZ and JOHANSSON come last, the former because it may not be quite sure of satisfying the first criterion above, the latter because its citation is certain but its very existence shrouded in doubt. So here they are:—

AĀRYAN

This law handles a dialectal split in the evolving phonology of Armenian, possibly 'around or after the 7th century' (Weitenberg 1996: 104). The ruling's deviser (called, by Vaux 1998: 6, 'the greatest Armenian linguist') dated it sometime between the seventh and eleventh centuries A.D., deducing the shift from the dialect of Van. Hrachea Hakobi AĀryan (or Adjarian, to use the informal transcription he himself used) followed his 1901 paper with studies of four further dialects (AĀryan 1909, 1911, 1952); yet another four were included, in what had by then become known as 'AĀryan's law', by M. Muradyan (1962) and H.D. Muradyan (1973), while Vaux in 1998 lists ten dialects in all (p. 10, now dropping Karchevan). The relevant difference between classical Armenian and these later dialects is found among the set of speakers who were and still are, for the most part, clustered near the southern part of the modern Armenia-Turkey border. (The modern tally of thirty-six dialects is quite other than the presumed ancient set whose prehistoric coalescence prefigured the classical language: on that, see Winter 1966.)

The canonic form of the shift is given in generative notation by Vaux (1992: 272). It is:

$$\begin{bmatrix} - \text{syll} \\ - \text{back} \end{bmatrix} \rightarrow \begin{bmatrix} - \text{back} \end{bmatrix} / \# \begin{bmatrix} - \text{syll} \\ - \text{son} \\ - \text{cont} \\ + \text{voice} \end{bmatrix} \text{ —}$$

(informally: in a word-initial syllable, a back vowel is fronted if it follows an

originally voiced obstruent consonant).

Examples are:

| | | | |
|-------------|---|---------------------------|-----------------|
| <i>bah</i> | > | <i>pāx</i> , 'spade' | [[ba-] > [pæ-]] |
| <i>bolk</i> | > | <i>pōxk</i> , 'radish' | [[bo-] > [pø-]] |
| <i>buk'</i> | > | <i>puk'</i> , 'snowstorm' | [[bu-] > [py-]] |

and, by rightwards cross-syllabic assimilation or 'prosodic' behavior.

| | | | |
|--------------|---|------------------------|--------------------------|
| <i>danak</i> | > | <i>tānāk</i> , 'knife' | [[da\$na-] > [tæ\$næ-]]. |
|--------------|---|------------------------|--------------------------|

This insistence on the obstruent nature of #C⁻ has been challenged. So Weitenberg (1986: 95; 1996: 105) prefers to see initial *y*- as also triggering the vowel shift, and Vaux (1992) includes initial *z*-, *ž*-, *l*-, and (as Weitenberg) *v*-. But the inclusion of /l/ complicates things, as it (like the nasals, here relevant only where assimilation occurs) is [+son]. Vaux now (1998: 174-177) omits non-obstruent initials.

In any case, the basic formulation ignores the voicelessness, in most of these dialects, of the resultant initial consonants. It requires an interim form with fronted vowel after voiced consonant (**bā*-, etc.)—cf. Weitenberg (1986: 95; fn 13)—a situation inadequately documented. Analysts must choose between (1) a fortuitous local congruence of voice loss in C⁻ and fronting of -I⁻; or (2) a loss of consonantal voice followed without delay by the production of a novel vowel (that is, *ba*- > (**pa*-, at once replaced by *pæ*-, etc.) to keep the semantic values distinct; or (3A) a *spread* of a feature rightwards from #C⁻, where it is realized in one guise, to the contiguous I⁻, where it manifests another— or alternatively (3B) a *switch* of that feature's locus from #C⁻ to -I⁻. Of these solutions, (1) represents a convenient co-occurrence. Weitenberg's 'Western Armenian sound shift' (devoicing) is presented (1996: 105-106, 111) as separate from AĀRĪYAN and largely incongruent with it. But interim data (temporary *bæ*- types) are lacking.

Solution (2) demands speedy recognition of a phonological crisis and its prompt remedying, in a fashion reminiscent of Lightfoot's (1979) Transparency Principle. The shift might reasonably affect only some syllables of a similar set, namely those produced by a previous shift of another part: so Latin speakers lowered /i/ to [ɛ] before /r/ only where the /r/ is the heir of a PIE intervocalic sibilant (cf. **sisō* > **sirō* > *serō*, versus unchanged *uirī* whose /r/ is etymological, as in Skt. *vīra*-). Yet languages are notably tolerant of homonymy and ambiguity; the careful avoidance of such muddle is what marks artificial languages as 'abnatural'. Nor is there any obvious cause of the devoicing of #C⁻, which causes the crisis. If devoicing is a later, or a quite separate, shift, solution (2) is ruled out.

That a feature may have been switched (3B), or a quite different feature put in a different place to take over the function, is feasible. In standard British English the contrast between e.g. *matter* and *madder* is still located in the degree of voice or pressure achieved in the central stop, while in much American speech it has been transferred to the length of the first vowel (the consonant after it being in both words a flapped /r/). So consonantal voice may have become dysfunctional in these Armenian dialects (cf. Vaux: 1998: 11). But again, later or separate devoicing makes (3B) quite impossible.

On the face of it, solution (3A) looks less attractive. Rightwards feature *spread* rests on speakers' reluctance to alter every articulatory position or effect in passing to the next segment; the carrying over of voice and the delay of breath-release in BARTHOLOMAE is a good example. But leftwards assimilation, by anticipation, is commoner; and this is so even when vowel height and consonantal voice interplay. Thus, in certain areas of eastern Canada, the onset of diphthongs is raised *before* voiceless stops ('Canadian raising'), as in [lʌʊt] *lout* versus [laʊd] *loud*, or [lʌɪt] *light* versus [laɪd] *lied* (first correctly stated by Morton Bloomfield in 1948). In AĀRYAN the interplay seems to be between consonantal voice and vocalic frontness/backness; a motivation of that linkage and of rightwards spread is needed.

A (3A) answer has been suggested in Vaux 1992 and reinforced in 1998: 176-181. For him, the AĀryan event is a rightward spreading, over the syllable, of the articulatory feature 'advanced tongue root'. In #C- ATR causes expansion of the pharyngeal cavity and lowering of the larynx, conditions decreasing the subglottal pressure and conducing to voicing; in -l- it pushes the tongue upwards and forwards, contracting the buccal space and making the vowel /-back, + tense/. (It is a pity that Vaux presents this process in terms of non-linear, particularly 'metrical', phonology, convenient though this may be. This theory handles syllables in generative trees and structural tiers, and is concerned with the relative prominence both of syllables in structures and of segments within syllables, as displayed in such effects as full enunciation, accent positioning, mora values, and the like (see further below, on Auer's contribution to PFALZ). Switch of the locus of a segmental phonetic feature is an unusual complication; indeed, feature spread largely nullifies the theory.)

Vaux sees ATR spread as occurring in eleven Armenian dialects (1992). Its dynamic role in consonantal voicing is derived from a monograph of 1987 by Loren Trigo. Also, by showing the event (1992; 1998: 177) in tongues as diverse as (Mon Khmer) Wa, (Tibeto-Burman) Jingpho, (Amerindian) Nez Percé and Babine, Mongolian, Somalian, Madurese— and even Buchan Scots English— he gives a strong justification *both* of the shift and its results *and* of the Armenian dialectal treatment as being a single and

local change (the shift being clearly very sporadic and non-universal). Some may object to the relating of vocalic effects caused by the upper surface of the tongue to consonantal effects of the tongue's root (so, indignantly, Sommerstein 1977: 102); but as long as the whole tongue body moves in the same direction, pharyngeal-glottal widening and buccal contracting must occur together. Vaux amasses considerable evidence, and makes a reasonable case (1998: 177-181) against the scepticism of Clements (1993), for whom ATR is purely 'ersatz' and superfluous in a 'unified feature' theory; still, to rename the process 'advanced tongue mass' might add conviction. Even so, uneasiness attends this analysis, on at least four scores:

(1) Why, once again, do the /+ATR/ consonants end up as voiceless in all but three dialects out of ten? To cite modern Mon Khmer reflexes (Vaux 1992: 278) is not to explain a past change over limited time in Armenian; and it is unhelpful simply to dismiss the entire change connection (Vaux 1998: 11). Weitenberg's (1996: 104-106) view of independent events (in time and place) is a valid solution, but it needs a convincing account and a restatement of the law.

(2) Why is the basic event confined to word-initial syllables? (The only exceptions are syllables second in the word, where there has been assimilation to affected preceding syllables.) 'Accent retraction' is a possible factor (so Ačāryan), but debatable as to origin and geographical placing.

(3) Must the triggers include /l/ (- syll, + son)? At least the 1992 formulation needs amending; this segment is simply not mentioned in Vaux 1998.

(4) Why are the incidence rates so variable? At best, in Shatakh dialect, /a/ is fronted in 95% of possible cases; at worst, in Khoy, only 34% of possible frontings of /o/ are found (Vaux 1992: 273).

Yet so much is worth serious debate here, by both Armenologists and phoneticians, that AČĀRYAN looks set to stay on the statute books.

LAUTENSACH

In 1899 Otto Lautensach (b. 1852) of Gotha published more widely and definitively a view he had expressed about inscriptional texts twelve years earlier (1887: 5). It concerned the 'temporal augment' in ancient Greek. There, as in Indo-Iranian, a particle /e/ is prefixed to a past tense finite verb form; if the lexical verb begins with a consonant the prefix remains syllabic (as in Skt *a-dāt*, 'he gave') but before an initial vowel or diphthong it is involved in contraction (as in Greek $\acute{\epsilon} + \epsilon\text{-} > \eta\text{-}$). The terms 'syllabic' and 'temporal' distinguish the types. In the latter case, the time of the vowel or diphthongal onset is extended. Lautensach's ruling was that temporal augmentation was avoided in Greek in verbs beginning with $\epsilon\text{-}$ when that preverb meant 'well' or 'easily' or the like, presumably to preserve an

important signal of meaning (1899: 47-49, 146-149). He relied heavily on lexicographical evidence such as Hesychius or the *Etymologicum Magnum*. Suidas (the Suda) was recalled as twice describing the unaugmented (but post-classical) aorist form *εὐλόγησα* as 'typical of Attic' ('Ἀττικώτερον'). Still, epigraphic testimony was available only for the period 320-300 B.C. (The contrast between the normally augmented forms and the Lautensach set has often since been summarized in *εὐλόγημι* 'I was praying', versus *εὐτύχοι* 'I was faring well'.)

Some classical editors fell in line and some did not. Wilamowitz, Ellendt and Lobeck did, indeed, Wilamowitz already ruled likewise in his first (1889) edition of Euripides' *Herakles* (see ad v. 1221)—more is said on this below. Gilbert Murray noted it carefully in the introduction to the third volume of his (Oxford Classical Texts) edition of Euripides in 1909— but, curiously, not in either of the previous volumes, of 1901 and 1904. He listed the six affected verbs in that author and their twenty instances, one of these is doubtful, and forms of *εὐτύχειν* alone account for twelve of the occurrences. Murray believed that all the Attic tragedians deliberately avoided the past finite forms of these compounded verbs (1909: iv). But others, such as Dindorf and Elmsley, ignored the supposed exceptional behaviour of *εὐ-* (for these, and the disarray of other editors, even recently, see Mastronarde 1989: 101). Of grammarians, some accept LAUTENSACH (e.g. Goodwin 1894— or at least by 1930: §545.2— although he accepts augmentation *after* *εὐ-*), while others demur. Schwyzer (1934/39: 654-656) ruled against Lautensach for the classical period, following the judgement of Herodian (2nd. century A.D.). Chantraine (1945: 377) insisted that the augment was always present, but not always initial.

Whether LAUTENSACH is true or false is debated below: that is, for our main purpose, a trivial question. More crucial are the problems it raises about (1) the relation of Greek spelling to speech, (2) the status of this 'well' marker, and of its opposite 'ill' marker, as 'parts of speech' in the relevant languages. As to (1), a written form like *εὐτύχοι* may have indicated a pronunciation [eʊ] as opposed to grammatically expected [ɛ'ʊ]; or it may have honored in writing the lexical argument for not obscuring the prefix even while the spoken form reflected the strict morphological rule. Writing and speech often clash. In French, to change *de* to *des* is graphically to add a consonantal sign but phonically to alter the vowel, and *-s* often has merely graphic (and grammatical) presence, as in plurals. In Farsi conversely, the possessive *ezafe* is spoken but not written (unless the preceding noun ends in *-a* or *-u*): so *manzel e mard*, 'the house of the man', is *منزل مرد*.

On (2) puzzles obtrude. It is fashionable to doubt a connection of Indic *su-*, 'well', with *vasu-*, 'good' (cf. Palaic, Hier. Luwian *wasu-*) < **h₁wesu-* (see Mayrhofer 1986: 125). The origin looks more like **h₁esu-*

(although to link this with $\sqrt{h_{1}es}$, 'to be', displeases those of us who do not rule out the syllable type /eC/ in PIE and prefer $\sqrt{*es}$ for that verb) Then /e(s)u/ and /du(s)/ seem to be safe PIE forms, widespread if only patchily. Of the former, the zero grade is normal in Iranian (cf. OP *huvuspā humartiyā*, '(a province) rich in horses, rich in men') and Indic (cf. Skt. *suga-*, 'easy to reach') and even Celtic (cf. Welsh *hygar*, 'amiable'); but, except in *ι-γής* (if Saussure was correct), Greek prefers the guṇa grade; and then *eu-*, before diphthongization, would collide with *έυ* (neuter of the adjective from $*h_{1}wesu-$). Hence *εδ* readily became an accented and freestanding adverb with reinforced semantic value, even nominalized in *τὸ εὖ ικάτω* 'may the good prevail', (Aeschylus, *Agamemnon* 121 etc.). *Dus-* is more stable Iranian (*duš-iyāru-*, 'bad harvest') and Indic (with sandhi variants like *durbuddhi* etc.) have it in (a) noun-derived adjectives and their renominalizations, and (b) participles—but not in finite verbs. Greek is idiosyncratic in several ways. It rarely employs the pair in a mere intensifier/negator role (whereas Skt. *su-cira-*, 'very long', is a common type), nor as modifying each other (against numerous Indic examples like *su-dus-tara-*, 'very hard to cross', Greek has only two (late) occurrences: *εύ-δυσ-ώπητος* 'easily outfaced', Plutarch, *Moralia* 2. 528e, c. 100 A.D., and *δυσ-ευ-πόριστος* 'not readily procured', Alexander Trallensis 1. 15, 6th century A.D.). And only in Greek are these prefixes used with finite verbs—a boldness paralleled by the use in names, like Homer's *Δύσπαρις* or Euripides' *Δυσελένα*. Even then, the verbs must be derived from substantives, as *εύδαιμονεῖν* ← *εύδαιμων* 'of good fortune'. The only exception, *εύδοκεῖν*, 'be well pleased', is no earlier than Polybius, 2nd century B.C. (Nor does a prefixed verbal adjective entail a prefixed parent verb: *εύβατος*, 'easy to cross', and *δυσεῖρετος*, 'hard to find', do not derive from $*εύβαινειν$ and $*δυσεῖρισκειν$). Therefore LAUTENSACH applies only to Greek; only there do these prefixes and derived finite verbs coincide.

But the behavioral pattern remains unclear. Prepositions and preverbs are just two aspects of the same basically adverbial items in many languages: thus a Swahili adverb becomes prepositional if *ya* is postposed to it. They retain some power to stand alone: so *σύν*, 'with', is regularly compounded in *συνποιεῖν*, 'to act together', but varied by separation in *ξύνηκακῶς#ποιεῖν*, 'to jointly ill-treat' (Thucydides 3. 13). Therefore even preverbs can claim treatment in accordance with the general rule that semantic elements in a compound stand closer to the root than do grammatical markers. Thus, if independent, they precede the augment, as in OP $*ava-a-stāyam > avāstāyam$, 'I set up', *ἀν-έ-βη*, 'he went up', etc. Yet if they markedly transform the lexical meaning (as when *ἀνα-*, 'up', turns *-γιγνώσκειν* from 'know' to 'read') they really deserve to be equated with lexical-semantic elements (as in *οικο-δομεῖν*: 'to domicile' or literally 'to house-build'), and to

have the augment precede them (as in *ᾠκοδόμησα*). The degree of non-grammaticality is always debatable; if LAUTENSACH rules out initial augment, and ignores inset augment, in *εὐ*-verbs (1899: 149), he is judging the manuscript reading at Lysias 9. 14 *ἠὺροσέτησα* to be a falsehood and implicitly impugning those at Aristophanes, *Plutus* 835 and Xenophon, *Apology* 26 (*εὐρηγοετ-*) which have as much textual authority. Possibly these prefixes hover between independence and compositional existence; possibly they are a sort of 'part of speech manqué'. Apart from Greek *εὐ* (see above), in Sanskrit *duṣ* actually becomes a class IV verb in its own right, meaning 'to spoil'.

If we accept that Polybius' *δυσπρέστον* is typically post-classical (see LSJ, s.v.) whereas Plato's (*Theaetetus* 169d4) *ἐδυσχεραίνωμεν* is the pre-4th century expectation, then we have close lexical status for *εὐ/δυσ-* in the 5th century and earlier: cf. Euripides' *συν-ευνυχεῖν* (e.g. at *Hippolytus* 1117). Then, *prima facie*, the augment should precede these preverbs, and LAUTENSACH is at least relevant. But the textual evidence is full of pitfalls. For instance, at Sophocles, *Oed. Tyrannus* 262, *μη()δυστύχησεν* may show absence of the augment but just as easily its prodelision or crasis with preceding *ε̄*, and of the possible readings at Euripides *Ion* 1457 neither *ὡς τὰ πρόσθ' ἐδυστύχει* nor *ὡς τὰ πρόσθε ἔδυστύχει* is any more certain than *ὡς τὰ πρόσθε δυστύχη*, which contains no verb at all. Anyhow, LAUTENSACH is rejected for classical and pre-classical Greek by the law's recent namer, Mastronarde (1989). He is on Herodian's side, and stresses the absence of epigraphic testimony before 350 B.C. He appeals (1989: 104 fn 8) to the negative comments of his Californian colleague, Leslie Threatte, an appeal justified by the latter's more recent (1996) pronouncement in his second (morphological) volume on Attic inscripational grammar. There Threatte writes (1996: 499): 'Attic inscriptions do not provide the crucial evidence for the Classical period' (i.e. of the Lautensach question), and he indicates that this is so until the end of the fourth century. Still, he makes it clear that *ἠὺ-* is increasingly rare as the initial syllable of *any* verb beginning with *εὐ-* or *αὖ-*, and had completely died out in Attic inscriptions by 200 B.C. This Lautensach himself observed (1899: 47-48); and it is accepted that *ἠὺ-* as the augmented form of an *εἰ-* verb is only ever found with *εἰκάζειν*. (And Herodotus has long been credited with failing to augment verbs beginning with *αι-*, *εὐ-* or *αὖ-*: see Mastronarde 1989: 105 fn 12.) The absence of evidence from papyri leaves this trend unconfirmed but unchallenged (see Mastronarde 1989: 105); in any case Schwyzer long ago noted (1934/39: 655) the growing post-classical nervousness over the whole concept of the temporal augment.

It can safely be concluded that after about 350 B.C. pragmatic writers felt uneasy at applying a strict morphological rule to the detriment of a handy semantic marker. Textual variation was then transmitted through the Hellenistic period into the early Middle Ages. Yet, rather than settle for

merely giving LAUTENSACH a later operational date than did its inventor. It might be supposed that fluidity in *spoken* usage, as a precursor, had begun to occur far earlier, perhaps even before the ϵ/η graphic distinction became available in Athens in Eucleides' archonship (403/2 B C). Lautensach had doubts about his own manuscript evidence; but he may have glimpsed the beginning of a long shift in Greek tense formation. Debate should continue, hence his ruling is now worth logging as a named law.

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One more point: Wilamowitz, as noted above, had a pre-1899 comment of a Lautensachian kind in his (1889) first edition of his commentary on Euripides' *Herakles*. The wording is unchanged in the 1895 re-edition; and the ruling is given confidently and without mention of any other scholar. As he must have been preparing the commentary around the time of Lautensach's 1887 remarks, Wilamowitz may have taken them on board at once, or he may have discovered the non-augmenting practise (true or false) for himself. Linguistic history—like any history—has numerous cases of different people reaching the same conclusion at about the same time. But without more evidence one hesitates to re-name the ordinance 'Wilamowitz's law', or even 'the law of Lautensach and Wilamowitz'.

LIDÉN

In 1899 (the same year as LAUTENSACH) a law was put forward within the history of Balto-Slavic. Evald Lidén (1862-1939) opined that /w/ was so weak in word-initial position in those languages as to have vanished before liquids. That is:

*w > Ø / # — RV (R = r, l)

His examples, here given in modernized form, were such as these:

| | |
|--------------------------------|---|
| *wāg ^h -, 'strike' | OCS <i>uraziti</i> , 'stab'— versus Russ. <i>razitʹ</i> , 'strike' (cf. Gk. $\rho\acute{\alpha}\sigma\sigma\omega$, $\rho\acute{\alpha}\tau\tau\omega$) |
| *wran-, 'wound' | Skt. <i>vraṇa-</i> (cf. Alb. <i>varrë</i>)— versus Russ. <i>rana</i> (all 'wound'); |
| *wri(s)t-, 'turn' | OE <i>wridan</i> , 'twist' (cf. OSwed. <i>vrīpa</i>), NE <i>writhe</i> (but ON <i>rīða</i>)— versus Lith. <i>risti</i> , 'roll' (whereas full grade <i>veržti</i> , 'pull tight', has v-), Lett. <i>rist</i> ; |
| *wlit-, 'appear' | Got. <i>wlits</i> , 'appearance', OE <i>wlite</i> , 'beauty' (but ON <i>litr</i>)— versus Lith. <i>lytis</i> , 'sex'; |
| *we ord ^h -, 'grow' | (cf. *we ord ^h in Skt. <i>vardhate</i> , 'grows', Gk. $(\tau)\omega\theta\acute{\iota}\varsigma$ '(grown) upright'), Got. <i>wrisqands</i> , 'bearing fruit' (but ON <i>roskinn</i> , 'grown')— versus Russ. <i>rod</i> , 'origin, family, age'. |

Obvious questions arise. Why does Old Norse (or early Icelandic) go the same way as Balto-Slavic? Why is Old Swedish not similarly affected, given its nearer contact? Are all the cited forms equally relevant (e.g. for PIE *wlit-*), considering the semantic spread?

On the face of it, here is merely the Balto-Slavic testimony for a shift which, although patchy and unpredictable, is both phonetically understandable (and a reasonable phonological alternative to initial *wr-*) and tolerably widespread in Indo-European. The move is very clear in Latin, as from **wrād-*, 'root' (NE *wort*) to *rādix*, or from **wlāmā*, 'wool', (Skt. *ūrṇā*, Lith. *vilna*) to *lāna*. The effect there could be of cluster reduction, given original **=wrH-* (or even **=HwrH-?*), but the normality of the extrusion even in **=wRV-* is shown by e.g. Homeric *ρίζα* (as against Lesbian *ροισδα*), 'root', where there is no laryngeal. Again, there is *ῥάπτω*, 'sew', from **wr(V)ṛ-*, alongside Lith. *verpti*, 'spin'.

In 1976 van Windekens suggested that exactly the same shift was part of the history of Tocharian, at least in four words in **=wr-* and three in **=w/-*. The reaction of subsequent commentators has been adverse, it is summarized by Anreiter (1987). For **=w/-* Anreiter believes that the normal Tocharian result would have preserved the *w-*. On the words cited, he rules that (1) oblique (participial) A/B *lānt*, 'ruling', is formally obscure (despite A *wāl*, B *walo*, 'king'), (2) A/B *lik*, 'wash', is preferably related to PIE *leig^h*, 'lick, wash', (3) A/B *lūk*, 'cut off', seems to fall together with Lat. *laedere*, OIr. *slat*. Of van Windekens **=wr-* words, Anreiter connects A *rapurñe*, 'desire', rather with Indic *rabh*, 'embrace' (from PIE *rabh?*), while A *ram* B *reme*, 'witness', seem to him relatable to Gk. *ῥεῖν* 'speak', only by a circular semantic argument; as to B *orotstse*, 'big', he links that with A *orto*, 'upwards', and to Hier.Hitt. *ura-*, 'big', and hence to a PIE root *e or*, 'grow tall'. There remains only A *ri* B *riye*, 'city', as a candidate for having an original *=wr-*, supported by Thraco-Phrygian *ροία* and Hesychius' lemma *ροίαν*. Thus any Tocharian loss of *w-* before *-r-* is, at best, a possible but very occasional simplification. (Interestingly, although Lidén himself is quoted as writing on Toch. *ri(ye)* in 1916, he is not otherwise mentioned in the discussion.)

To assign the title 'law' to Lidén's ruling may be justified among Balto-Slavists (and is accepted by e.g. Frisk, 1979: 688, as if well-known there). For more general acceptance it needs a provably wider domain and/or more interesting implications. It is sometimes proposed to extend the observation to other initial consonants (not stops) in Balto-Slavic, such as *-m-*; but no results are yet agreed. The name has been given, and the naturalness of the change commends it. But at present a lack of enthusiasm in the outer world is only to be expected.

PFALZ

A law which is first cousin to AČĀRĪYAN was proposed in 1913 when Anton Pfalz (1885-1958) reported a finding which applied over a limited domain but had wider systemic implications. That domain was some southern dialects of German. The observed shift was that of a consonantal feature (whether by change or conditioned choice) as prompted by the preceding vowel's value in respect of what seemed a quite different feature. PFALZ states (1913: 9) that after an (acute) accented short vowel a following obstruent will be realised as [+ fortis], while after a (grave) long vowel the consonant will be [+ lenis]. At first this ruling was based on some Austrian dialects; it was later extended to some Bavarian speech-groups (cf. Pfalz 1936; Hinderling 1980). Typical examples (as given) are:

- /fēta/ 'cousin' (NHG *Vetter*) versus /fē:da/ 'feather' (NHG *Feder*);
- /v'í:ssn/ 'know' (NHG *wissen*) versus /v':sn/ (sic) 'meadow' (NHG *Wiese*);
- (diphthongal) /la:it/ 'suffers' (NHG *leidet*) versus /la:ɪd/ 'people' (NHG *Leute*).

The coincident opposition of types of accent (*acute* short versus *grave* long) was in Pfalz 1913: 5 (derived from Schatz), it seems incidental and inessential to the shift.

Once a new consonantal opposition is achieved it is claimed to take over some morphological duty: so in Bavarian usage [hunt] plural stands against [hund] singular, the latter having a lenis final segment. (But Hinderling, 1980: 28, cautions that in word-final syllables ending in a vowel the whole opposition is lost.) On the other hand, the otiose root-final contrast between /i re:ɪd/, 'I speak', and /du ret-st/, 'you speak', is conjugationally tolerated,

The process was interpreted as a transfer of a single feature, the same in both *I'* and *C'*, by later commentators on Bavarian speech patterns (so Kranzmayer 1956; Rennison 1981). For them, fortis consonants are phonologically long (as well as having greater phonetic duration), and lenis consonants are short; hence the result of PFALZ is an exchange of length values, producing the regular opposing of /V:C/ to /VC:/. In terms of duration, this is the phenomenon acknowledged in English pairs such as *bēð-bēf*, *bīð-bīf*, etc.; the experimental evidence for it is well attested. There are resemblances to the mechanism of a socially based difference in classical Latin, where lower class *āllec*, 'fish sauce', *ōlla*, 'pot', *hīnmuleus*, 'fawn', had U-variants *ālec*, *aula*, *īmuleus*. More recently, Auer (1989: 1089)— whose own solution is discussed below— has proposed that in phonological length a single fortis equals a geminated lenis obstruent, a neat 2-to-1 calculus known in classical quantitative verse, and already implied by Hinderling (1980: 27 on

[si:f] = [siff]) Cf. Vaux on AĀRĀYAN, above.

The immediate problem is the awkwardly uncertain status of the fortis/ lenis opposition itself in phonetic theory and experiment. Chomsky and Halle (1968: 326-329) used 'heightened subglottal pressure' as a source feature, and described its complex interplay with glottal constriction and voicing. But fortition, or tension, forms no part of their scheme, and (heightened or increased) subglottal pressure, used above in relation to AĀRĀYAN, is not a fashionable concept at present. Twenty years ago it was usual to teach that a plus or minus value for /voice/ parallels, or even entails, a plus or minus value for /lenis/ in consonants: so O'Connor (1973: 127, 129, 140) links [- voice] in English with [+ fortis] and [+ length]. Yet in 1975 Ladefoged did not write so, and in 1984 Lass (177-183) employs fortition/lenition very loosely, to introduce a phalanx of assorted co-features and shifts, on a cline of strong to weak. Admittedly, in that year Kohler (1984) made a late but firm stand for such a 'power feature' as an essential phonetic factor; but more recently Laver (1994: 344) relates only some activities of the vocal tract, and rather vaguely, to 'overall muscle tension', and judges that this 'remains to be confirmed empirically'. Well, well-known indeed among empiricists are Ladefoged and Maddieson. In 1996 (302-306) they roundly deny any correlation of the tense/lax distinction with tongue setting (contrast Vaux on AĀRĀYAN); and in 1997 (589-618) Ladefoged accepts fortis as a phonological value, but only within a 'pulmonic' feature which is itself one 'airstream' alternative (voicing has a separate status, under 'laryngeal' possibilities). No doubt 'pulmonic' subsumes 'subglottal'; but it is interesting that for both Chomsky-Halle and Ladefoged it is an exotic feature, notably characteristic of Korean. In recent phonetic manuals virtually no index has entries for 'fortis' or 'lenis'; nor are they in the 'feature tree' proposed by Halle in 1995 (2), or in his whole discussion of Clements' theory.

On the English *bēd*-*bēf* trade-off, Maddieson makes another telling point (1997: 624-629). He reports on experimental measurement of the durational shifting in more complicated sequences like *send* versus *sent*. It seems clear that the actual time difference between *-ñ-* in *send* and *-ñ-* in *sent* is, proportionately calculated, practically twice that between *-ē-* and *-ě-* in the same context. So this business is no simple transfer; it is rather a segmentally variable device, perhaps for preventing the loss of something as sequences vary. As to what that something was, many competing suggestions are apparently on offer. On all these interlocked matters, one cannot help feeling that historians would welcome some sort of peace process among phoneticians.

If only voice and its relation to accent were at issue, PFALZ would be at one with VERNER in linking voicelessness with preceding stress. But vowel length is not part of VERNER. As to LACHMANN, its (debatable)

phonological solution would be the double converse of PFALZ. In LACHMANN it is *loss* of voice in C' which appears to induce length in the preceding V.

In 1989 Auer reformulated, as concerns PFALZ, the length argument. Whereas the English duration switch in *bēd*-*bēf* etc. is purely phonetic, it is in Bavarian (for Auer) a phonological necessity. In what is claimed to be a mora-timed language the speaker must, it is said, maintain across sequences the correct mora count. (This is an application of the type of compensatory change codified by Hock, 1986: 435, as 'loss with mora retention'.) Auer pronounces on PFALZ that 'in a canonical Bavarian (phonological) word, the number of morae is three' (1989: 1090)—though the word is an unhandy unit of sequential metrical control. It follows that what occurs, in the Auer solution, is really an 'anti-Pfalz' adjustment: it is the vowel which is lengthened in order to keep up the mora count if the following consonant (whether a syllable coda or not) is, in mora terms, insufficient. This is claimed to happen even if the adjustment is itself inadequate by reason of an inescapable lack of mora value in a coda. The mora rating assigned to various Bavarian consonants needs experimental quantifying if this argument is not to be circular. Still, Auer has at least justified the naming of the law, and made debate easier.

* * * *

In passing, it is worth noting that Auer (1989) refers to 'Prokosch's law' in connection with Germanic mora counting. This is treated on a level with those of Streitberg (known; see Collinge 1985: 181-182) and of Pfalz (as above). This ascription derives from Vennemann (1988: 30), who fathers on Prokosch the ordinance that the distinction between accented and unaccented syllable tends towards an optimal length realization, of bimoric versus unimoric structure. (On this 2-to-1 calculus, see above.) What Prokosch actually said (1939: 140 §50) was that (1) in Germanic 'short accented vowels in open syllables are lengthened', and (2) in MHG four original accented syllable types are reduced to only two, open-long and closed-short (and NHG, within the accented syllable, preserves this economic opposition of /VC/ to /V/). Prokosch did not claim that this standardizing event had universality, nor that a 2-to-1 relation existed, nor did he speak of a law. Auer is suitably sceptical (1989: 1098 fn 20); and one sees no need for 'PROKOSCH II', at least on the Indo-European level. (On the original PROKOSCH, see Collinge 1985: 254-255.)

And now for the two whose right to the title is debatable:

FRITZ

In recent years the laryngeal hypothesis has been supported and controlled by a series of pronouncements called (or worthy of being called)

laws. An especial area of concern has been the conditioning of the complete loss of /H/. MEILLET (see Collinge 1985: 117-118) led the way with a package of provisions for that loss; other possible laws in this sector are KUIPER on Vedic (1947, 1955), EICHNER on Anatolian (1973), JOSEPH on Celtic (1980, 1982) and PINAULT (1982)— the last alone concerning IE in general. (On all these, see Collinge 1995: 41-46.) The three last-named laws interestingly agree in displaying an increased sensitivity to syllable boundary as a crucial factor in laryngeal behavior. It is, however, critical here to distinguish between on the one hand the evolutionary transmuting of /H/ from a consonantal (spirant) segment to something else— vowel length, or vocalic insert, or velar trace— and on the other hand its mere, even sudden, omission. That is, we must set apart (1) e.g. **eh₁* > -ē [e:] and (2) e.g. **hro* > *ro*; and these separate processes will be hereafter termed respectively 'transmutation' and 'loss' (Lindemann's 1990 attack on RIX rests on this very point.)

After MEILLET (or at least after Kortlandt's 1975 laryngeal version of it), no specific law has handled intervocalic loss of /H/. Indeed, Matthias Fritz (1996: 4) sees that event as generally acknowledged ('nach einem allgemein anerkannten Lautgesetz'). He cites Mayrhofer (1986: 124, 145) in support; in fact, the latter writes of hiatus as the immediate result. Sensitivity to IE syllable structure starts with Hermann (1923: 348-366); and as a consequential ruling one may cite $H : \emptyset V S : V$ (the loss of intervocalic syllable-initial laryngeal.)

Now is proposed a new and more restrictive law, perhaps to be known as FRITZ (see below). That scholar puts it forward, and calls it a law, in a paper (1996: esp. 1, 5-7, 9-10, 17) which refines the wider statute by demanding in the relevant context (a) a preceding sonorant, (b) a following full vowel, (c) a two-stage process:

- $$(1) \quad *H \emptyset RS : V^p$$
- $$(2) \quad R : R \left\{ \begin{array}{l} \# \\ C \end{array} \right\} : V^p$$

— or, as he puts it, '**R.HV* : **RV*'. (The symbol V^p introduced above denotes that the following vowel shall not be the vocoid realisation of a sonorant (semivowel); and the second stage of the shift produces a consonantal value of the preceding sonorant and so deletes the open syllable with /R/ and any awkward hiatus. What is intriguing in particular is the suggestion (Fritz 1996: 9 fn 32) that the two types of /H/, the neutral and the α -coloring, underwent the loss process at different times (first h_1 , then h_2). (Also, h_2 is chronologically set between them; this complication may be

ignored by those of us who do not believe in its parallel existence at any time.) Fritz's suggestion has two dividends: (1) here is an example of 'phonological diffusion' (parallel to 'lexical diffusion'), where the speed of a change depends on segment type—much as in PROKOSCH a 'ratio of change' is noted in Germanic dialectal consonant modifications, and (2) the split of Anatolian from the other groups of IE languages may be timed as occurring between the h_1 and h_2 participations in the Fritz shift (the evidential basis being as in Melchert 1994: 54-55). This is a most worthwhile set of pronouncements on phonological processes and relative chronology.

Fritz also considers along these lines the evolution of Sanskrit nasal infix verbs (1996:8). In the ninth class, where the infix is within a root syllable of the type /C(e)H/, the third plural present form (e.g. * $ph_2H+enti$) has zero grade of the root plus the ending found in the athematic conjugation (which has a full vowel). This sequence / ηHV / gives the FRITZ input / $R_3\$HV^P$ / and the result is therefore / R_3V /; Sieversian / $\#tiwa$ / is then relevant to this formation as a whole, giving $pr_2\eta kinti$ and by analogy $pr_2\eta\ddot{a}$ -. (Fritz ventures to explain fifth class and seventh class forms likewise, but by analogy.)

Some difficulties remain. The Fritizian rule is obfuscated by idiosyncratic derivations in individual languages, and especially by disagreement over where, and if at all, laryngeals are placeable in various roots. But that H -loss is favored by preceding open syllables, by initial position in the next syllable, and by a following full oral vowel—these are notions important enough to prompt discussion, and FRITZ seems a handy term of reference.

JOHANSSON

In one of his last publications Oswald Szemerényi (1913-1996) discussed the etymology of the IE word for 'thousand'. He proposed to derive it from a root * $g^h ed$ -, meaning 'to seize, to hold' (1994: 89-91— a paper which actually appeared in January 1995). This root's metaphonic form was claimed to underlie the Indic word for 'hand': that is, $hastā$ - is from * $g^h od-to$ -, —which, of course, involves an unusual Indic reflex of the PIE sequence $-d-t$ -. Szemerényi's defense was that this etymon constituted 'a further example of the development of h to s even in Indic. This is the so-called law of Johansson, not generally accepted, not even mentioned in Collinge's *Laws*' (1994: 91). One accepts this remark as a clear challenge. But is Johansson's ruling of sufficient weight and reliability as to deserve canonic status after all? He himself put forward no law; it is hard to find in him any very positive statement on it. But his coverage is worth reviewing, if only to decide whether any statute can be described here, let alone defended.

Johansson published two sizeable articles on the evolution of

contiguous dental stops in IE (1903, 1906), in which his concern was to improve on the existing statements (e.g. those by Kögel) on the matter. Leaving Indic aside for the moment, Johansson's view was that:

- (a) in Italic, Celtic and Germanic the ultimate result was normally *-ss-* (but *-s-* after a long vowel), and *-st-* before *-r-*, this last case represented the retention of the historically intermediate form;
- (b) in Balto-Slavic, Greek, Illyro-Albanian, Thracio-Phrygian, Armenian and Iranian the normal, and virtually omnipresent, result was *-st-*.

From these facts he concluded that the shift-process had already gone as far as *-t̥t-* in PIE itself, which is just what Brugmann said in the *Grundriss* (1.624).

Now Indic, as is generally agreed, mostly offers *-tt-* for such sequences, but with enough traces of assibilated forms in its cognate lexicon for this result to have arisen from a late shift-reversal, removing the previously introduced cluster-easing sibilant. One might, therefore, credit Johansson with seeing even *-st-* as a late PIE shift-stage. But there is no certainty here; Szemerényi earlier (e.g. 1990: 108-109, where fifteen modern contributors to the historical picture are cited) judged that to assign *-st-* to PIE was impossible. (Interestingly, Meid (1964/5: 236) for whom any IE *-st-* always needs a particular explanation, sees as the norm an IE 'Stufenfolge' *-t-t- > t̥t- > ts > ts > ss*.)

Johansson regarded *-D-t- > -st-* as the initial change in Indic (1903: 272, 273) and rejected the idea that only *-tt-* was offered there (1903: 292, against Wackernagel 1896(1957) 178 §152c). He called *-tt-* in Indic secondary, despite its frequency, and saw it as analogical ('*durch Systemzwang*', 293)—which is not necessarily the same solution as the shift-reversal noted above.

Then the survival of the sibilant in apparently relevant Indic contexts was examined under these headings:

(1) Seventeen examples were paraded for consideration (1903: 310-316);

(2) Numerous cases of *-st-* occurring after *i* or *u* (essentially ruki cases) were shown to derive in fact from **-DsD-* in PIE (1906: 112-138), the words were divided into (a) those whose sibilant has to be reconstructed as original from the evidence of other IE languages, and (b) those where some *s*-trace turns up here or there in Indic itself;

(3) Returning to the list in (1), Johansson admitted that five of the examples do stand in a ruki situation as do those in (2), but took them to be none the less valid exceptions to the full reversing movement of (*-DT- > -st- > -tt-* (1906: 139).

The five recalcitrants in (3) are: *mustu-*, *musta-*, *tūsta-*, *busta-* and

kṛṣta- *Mustu-*, 'fist', aligns with the better-known *musti-*, but only as a lexicographers' citation. Mayrhofer (1956-80: s.v.) believes it to be artificial, and calls Johansson's etymology quite beside the point ('gewiss müßig'). Similarly, both *tūsta-*, 'dust', and *busta-*, 'the skin of fruit or the burnt exterior of meat', are found only in lexical lists or scholia. Hence, for *busta-*, Mayrhofer regards a *-Dr-* source as 'völlig unglauhaft'. *tūsta-* is indeed found in texts, as in Pāṇini, 3. 1. 21, with the meaning 'matted hair', but for Mayrhofer it is dialectal—yet that does not rule out a PIE origin. (A form *busa-*, meaning 'chaff' as well as 'fog', has some currency (e.g. *RI* 10. 27-24), but a PIE connection is ruled out by Mayrhofer.) *Kṛṣta-*, 'poet', is equally at home in Vedic (e.g. *RI* 1. 127. 7; 6. 67. 10); but Wackernagel is probably justified in branding it a 'Fremdwort' (1953-90: I. 143, 233— not even a 'Lehnwort').

We are left with *musta-*. All agree that this word denotes a species of grass, *Cyperus rotundus*, the neuter form perhaps being used for the root and the feminine (*mustā*) for the visible plant. But Mayrhofer (s.v.) explicitly rejects an Indo-European origin in view of Dravidian congeners, notably Telugu *muste*. Certainly, both in form and in semantic class, this word is a clear candidate for loan status.

Nor do the non-ruki words in list (1) escape suspicion. Ten of the twelve occasion doubts. (See Johansson 1903: 310ff.) Three are supposed to come from sources with movable *l*-segments, which does not entirely inspire confidence. Thus *paṣṭh-vāh* 'young bull', is derived from **pelth-to-* or **polth-to-* (but Celtic congeners suggested, such as Irish *los*, Welsh *llost*, Bret. *lost*, require **ploth-to*). Secondly, *baṣṭa* 'mutilated', is given a parent **blt-to-* and this is courageously linked with Danish and Norwegian *pölse* and dialectal Swedish *pölsa*, meaning a kind of sausage (it is just as well that Zachariae deemed *baṣṭa-* to be a Prakṛt form only). Thirdly, *kāṣṭha-*, 'length of wood' (cf. *kāṣṭhā*, 'race course goal marker') is said to be one with Gk. *κλάδος* 'twig' (cf. OHG *holz*). All is possible; little is convincing. Two other exempla are merely lexicographical: *lastaka-*, 'bow' (or, more exactly, 'the middle of a bow'), and *masti-*, 'measuring', although the latter may be cognate with Gk. *μέδιμνος* and the Latin set *modus, modulus, modium* s. Another two look like *s*-inclusive forms after all. So *mastu-*, 'sour cream', which Burrow (1945: 115-116) thought Dravidian, is reasonably based by Petersson (as noted by Mayrhofer, s.v.) on **mad-s-tu-*; while *basta-*, 'goat', is given by Thieme (see Mayrhofer s.v.) a convincing onomatopoeic origin, namely from the **psd* root found in Latin (*op*)*pālō* (the **prd* variant giving the Eng. *fart*), a source commended by the odor of those animals. The words *mastu(ka)-*, 'head', and *pastyā*, 'homestead', are certainly known lexemes. But to relate the former to the **men* root of Lat. *ē-min-eō*, *prō-min-eō* is unhelpful guesswork; and although the latter might connect with *pattana-*, 'town' (cf. Lat. *oppidum*), it

could equally do so with Lat. *postis*, which suggests an original *-st-* (compare, as does Mayrhofer, Arm. *hast*, OHG *festi*). As for *sana-*, 'breast' (cf. Arm. *stin*) it is said to issue from **pt-teno-* by a long and rather prestidigitous argument (pp. 324-329), as otherwise it is irrelevant by reason of the initial position of *st-*.

What is left after all this winnowing is the pair *prastha-*, 'plateau', and *asthi*, 'bone'. *Prastha-* ties in well with Gk. *πλάτος*; *πλάτης* and a basic sense 'flat' suits adequately the Sanskrit meanings 'stable' and 'measure of extent' (as well as 'expanse' and 'tableland'). This commends **ple^h-to-* as the earlier form (rather than **pro-s^heH*), and one recalls *prathati*, 'he spreads', and Lith. *plou*, 'flatten'. Mayrhofer's comment '*wohl unrichtig(en)*' is too harsh a reception of this derivation. *Asthi* seems also a possible result of ? *oD-D^h-t*, Latin *ossa* (pl.) agrees, and Gk. *ὀστέον* does not entirely rule it out (cf. *ἴστε* 'you know' < **wid-te*)—there is no need to try to involve *ῥζος*, 'twig'. Unhappily, Alb. *as* may, and Hittite *haštai* (not **hazzai*) does, argue for an original *-st-* form.

There remains precious little to support a supposed law, itself unformulated and given no conditions of operation in Johansson's exploratory essays. It was scarcely wise of Szemerényi to rely on such a thing to justify his etymology of *hasta-*. Truth, one repeats, is immaterial in deciding on recognition of a statute. But width of locus, regularity and power to promote discussion are crucial, and here seem absent. Still, quite a number of Indic *-st-* forms do need explanation (many are cited by Meid, 1965, with no mention of Johansson), and JOHANSSON is conceivably a convenient term. At any rate, it may stand so as a tribute to Szemerényi and his own many notable contributions to Indo-European studies.

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These six laws will, of course, be added to in coming years. 'Cowgill's law' may soon achieve citable status; other old rulings may be revived. The canon is far from fixed.

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(These references are eclectic but comprise the major pronouncements on each law.
The bibliographies of the works cited contain fuller information on tangential matters.)

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Frederick W. Schwink

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