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THE INDO-EUROPEAN MIDDLE ENDING *-r*

EDITH FRANCES CLAFLIN

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[If we accept the Indo-Hittite hypothesis, the existence of middle *r*-paradigms in both Hittite and Indo-European puts the middle ending *-r* back into Indo-Hittite. Even if we assume that Hittite is simply one of the IE dialects, the evidence for medial *r*-forms in PIE is still overwhelmingly strong. The semantic aspect is important. There is virtually no positive evidence for a volitive impersonal in *-r*. But verbal *r*-forms with middle meaning occur in Indo-Iranian, Tocharian, Hittite, Armenian, Phrygian, Venetic, Italic, and Celtic. This proves that not only *r*-forms, but *r*-forms with middle meaning, were inherited.]

In LANGUAGE 12.171–2 (1936) Petersen, referring to the fact that it has recently been assumed by Meillet[1] and the writer[2] 'that the existence of *r*-paradigms in Hittite and Tocharian proves their inheritance from IE', expresses the view that there is 'every reason for adhering to the older opinion that all medio-passive *r*-forms have developed in the individual language groups from a single IE volitive third singular in *-r* (or *-ro* ?).' This makes a fresh review and discussion of the evidence timely.

Before proceeding to consider the positive evidence for inherited middle *r*-forms, however, it is necessary to clear away the debris of certain older theories, which were developed long before Hittite and Tocharian were ever heard of as Indo-European languages, and which now are merely cumbering the ground and impeding the view. It would indeed be passing strange if a hypothesis which was set up to explain what was thought of as exclusively an Italo-Celtic phenomenon should still hold good after the epoch-making discovery of two new languages, Indo-European or at all events of the most marked Indo-European affinities, both of which possess well-developed medio-passive paradigms in *-r*. It might seem unnecessary, in trying to gain a fresh approach to the problem, for us to go so far back as Zimmer's well-known essay on the Italo-Celtic passive and deponent.[3] As a

[1] BSL 32.1–28.

[2] AJP 48.157–75; LANG. 5.232–50.

[3] KZ 30.224–89.

matter of fact, however, Zimmer's so-called 'man'-form theory of the Celtic and Italic *r*-passive has not only had an extraordinarily pervasive influence in the past, but is still coloring the views of scholars. In a recent discussion, for example, Petersen refers[4] for a 'clear presentation of the probable development' to Buck, Grammar of Oscan and Umbrian 178. If we turn to the cited passage we find a brief résumé of the view of Zimmer, as Buck himself explicitly states. Zimmer's theory is in fact, as Odé says,[5] 'startlingly simple and striking' and that is why it has had, and still has, so much influence. And yet, it is now out-of-date and it was never really tenable and certainly is not tenable now. The proof of its untenability I have given in detail elsewhere,[6] but, as I remarked at that time, 'Zimmer's conclusion shows a tendency to persist even after the premises by which he arrived at it have become insubstantial'. It is therefore necessary once more to point out the fundamental unsoundness of the hypothesis.

The essence of Zimmer's theory is that an original active third plural ending *r*, seen for example in Sanskrit perfects such as *viduḥ* (*vidur*), came to be used impersonally. It thus gave rise in Irish and Latin, independently, to two different verbal formations—the passive and the deponent. The deponent Zimmer considered a purely ACTIVE inflection. It was thus supposed to be derived from the original and proper active meaning of the third plural *r*-ending, while the passive developed from the assumed impersonal use or 'man'-form.[7] This highly original conception Zimmer worked out in great complexity of detail in both Irish and Latin, and with an exuberance of imagination for linguistic pre-history which can but excite our admiration. What renders the theory untenable, however, is that Zimmer entirely misconceived the nature of the deponent and so left altogether out of consideration the MIDDLE VOICE. Instead of recognizing the IE middle endings *-to*, *-nto*, for example, in the endings *-tur*, *-ntur* of such forms as Lat. *loquitur*, *loquuntur*, he supposed that these forms arose by the addition of a suffix *-ur*, abstracted from an early form *vehur* (purely imaginary of course), to existing active forms *loquit*, *loquunt* (also imaginary).[8] In this way he conceived of the deponents in both

[4] AJP 53.209 (footnote 36).

[5] A.W.M. Odé, De uitgangen met *R* van het deponens en het passivum in de indoeuropeesche talen 37 (Haarlem, 1924).

[6] LANG. 5.232-50.

[7] KZ 30.224-89 passim; see especially 229-30, 231-2, 237, 257-8, 282-3, and Zimmer's own summary, 287.

[8] L.c. 283.

languages as recent innovations.[9] Since this conception is entirely erroneous and there is no doubt at the present time that both the Latin and the Irish deponents are true representatives of the Indo-European middle voice,[10] Zimmer's whole theory falls to the ground.

Moreover, it cannot be emphasized too strongly that Zimmer had not the slightest notion of positing an impersonal in -*r* in the parent speech. He says distinctly, 'Indogermanisch ist blos eine dritte person pluralis activi conjuncter flexion auf *r*' (KZ 30.268). The 'man'-forms that Zimmer was talking about occur chiefly in Middle and Modern Britannic dialects. Still less was Zimmer's hypothesis designed to explain the origin of Italic and Celtic 'middle *r*-forms' or 'medio-passive *r*-forms'.[11] Zimmer vehemently rejected the very idea that there were any middle forms in -*r* in either Celtic or Italic.[12] What he was trying to explain was the existence in both Irish and Latin, as he believed, of a purely active inflection (the deponent) which showed in Latin *ur*, in Irish *ar* behind the active endings, and on the other hand the passive meaning of some of the Irish *ar*-forms and all the Latin *ur*-forms.[13] What we actually have in Latin, however, is a middle voice in -*r*, which, like the middle voice in other Indo-European languages, is frequently used with a passive meaning[14] and occasionally with an impersonal meaning.[15] In the 1880's, when Zimmer was developing his famous 'man'-form theory, this fact was not generally recognized. But at the present time it is fully acknowledged by scholars so eminent as Wackernagel[16] and Sommer.[17] In Irish the passive forms are often identical with the deponent, which, as we have seen, is like the Latin deponent a lineal descendant of the ancient middle voice. This identity of form, to my thinking, points unmistakably to a common origin.[18]

Though other scholars besides Zimmer have embraced the doctrine of the impersonal passive in -*r*, yet if we analyze their discussions we shall find that for the most part they have adopted without material modification the characteristic features of Zimmer's hypothesis. So

[9] L.c. 261-2; 283.
[10] Cf. E. F. Claflin, LANG. 5.237, 238, footnotes 33 and 35.
[11] Cf. Petersen, LANG. 12.172.
[12] L.c. 230, 240, 257, 267, 268, 281-3.
[13] L.c. 229.
[14] Cf. my paper on The Nature of the Latin passive, AJP 48.157-60.
[15] Id. ibid. 169-70.
[16] Vorlesungen über Syntax², Erste Reihe, 130-37 (Basel 1926).
[17] Vergleichende Syntax der Schulsprachen³ 46-9 (Leipzig und Berlin 1931).
[18] Cf. LANG. 5.240, with footnotes 49 and 50.

Dottin,[19] Vendryes,[20] and Walde.[21] Thurneysen to be sure has altered, but, it seems to me, scarcely improved, Zimmer's theory in two respects. Whatever the weaknesses of some of Zimmer's postulates, he at least starts from something solid, namely the third plural *r*-ending of the Sanskrit perfect; and his theory has a beautiful interior consistency. Thurneysen on the other hand would have us start from an Italo-Celtic infinitive of the goal,[22] which is purely imaginary; and, as I have elsewhere pointed out,[23] in the development of his theory he falls into inconsistencies and indeed contradictions.

If now we turn from theories, which were evolved to explain what seemed like a curiously isolated phenomenon of Italic and Celtic,[24] and confront the positive facts of the Indo-European linguistic world as we see it today, we shall get a very different picture. If we accept Sturtevant's Indo-Hittite hypothesis, the existence of a well-developed medio-passive system, characterized by *-r*, in both Hittite and several Indo-European languages requires us to assume the beginnings at least of such a system in Indo-Hittite. Granting however for the sake of greater cogency in the argument that Hittite is simply one of the IE dialects, the evidence for medial *r*-forms in PIE is still overwhelmingly strong.

The semantic aspect is important. Verbal *r*-forms with clearly-defined middle meaning occur in Indo-Iranian, Tocharian, Hittite, Armenian, Phrygian, Venetic, Italic (including Latin, Oscan, Umbrian, Paelignian, and Marrucinian), and Celtic.[25] There is some difference of opinion in regard to the nature of the Armenian *r*-forms, but they are accepted as cognate with the Italic, Celtic, and Tocharian medio-passives in *-r* by competent authorities[26] and I see no reason for reject-

[19] Cf. LANG. 5.243–4.

[20] Cf. LANG. 5.246–7.

[21] Cf. LANG. 5.247–8.

[22] KZ 37.106.

[23] LANG. 5.245–6.

[24] Cf. Thurneysen's remark (KZ 37.103): Vielmehr kann man nur sagen, dass es im Keltischen und Italischen sonderbare *r*-formen giebt, etc.

[25] For a convenient review of the evidence, see Odé, 3–31.

[26] See Meillet, Esquisse d'une histoire de la langue latine[2] 27 (Paris 1931); Marstrander, Norsk Tidsskrift for Sprogvidenskap 3.243 (Oslo 1929); Pokorny, Die Stellung des Tocharischen im Kreise der indogermanischen Sprachen, Berichte des Forschungs-Institutes für Osten und Orient 3.47–50 (Vienna 1923); Charpentier, Die verbalen *r*-Endungen der indogermanischen Sprachen 6 and 113–18 (Uppsala, Leipzig, 1917).

ing them. The fact that forms such as Arm. *berēr* and *beriwr* occur in a verb that is particularly common in a middle sense in both Greek and Latin has not to my knowledge been mentioned, but it is significant. The two meanings of the Armenian imperfect *berēr*, 'il portait' and 'il était porté', are both easily derived from an original middle force. The dynamic middle easily merges into an active meaning,[27] and the passive use of the middle is everywhere current. Armenian *berēr* Meillet considers comparable in form to Latin *fertur*.[28] Now the so-called passive forms of Latin *ferō* are extremely common in a middle sense—not only in poetry but in prose as well. Caesar says *Rhenus citatus fertur*.[29] And this phrase surely means, 'The Rhine rushes along swiftly', i.e. 'it carries itself', surely not, 'it is carried', still less, 'one carries it'. It was the same no doubt in Armenian, only in course of time the middle meaning faded out, leaving behind an active and a passive meaning both derived from the original middle sense. The testimony of Armenian is important because, like Latin, it represents an archaic type of Indo-European. Armenian, for example, like Vedic Sanskrit and Homeric Greek, gives us a view of the state of affairs in regard to the augment which obtained in the parent speech. In Armenian the augment is attached to past tenses of verbs which would otherwise be monosyllabic, but not to the polysyllabic forms—a strikingly original use which the Armenian language has made of the fact that in PIE the augment was not an integral part of the verbal form.[30]

The extremely interesting medio-passive *r*-forms of Phrygian are often overlooked, but they are well-attested and of clearly middle meaning. The context shows that αδδακετορ means 'he makes for himself' and αββιρετο(ρ) 'he brings' (in his own interest) appears to be a parallel form, also taking a direct object, like a deponent verb.[31] Both verbs are cognate to Greek verbs that occur very commonly in the middle voice.

Venetic *tolar*, if I have rightly interpreted the bronze on which it is stamped,[32] represents a verbal *r*-form of an extremely archaic type.

[27] Cf. Sommer 47 8.

[28] Reference in ftn. 26.

[29] BG 4.10.

[30] Cf. Meillet, Esquisse d'une grammaire comparée de l'arménien classique 93 (Vienna 1903).

[31] W. M. Calder, Journal of Hellenic Studies 31.209 (1911), cf. also 187; Leumann-Stolz, Lat. Gram. 307.

[32] See LANG. 12.23–34.

Not only does it end in *-r*, without the middle ending *-to*, like Irish *canar*, *berar*, Umbrian *ferar*, etc., but it also shows the suffix *-ā* added directly to a form of the base, independent of the present stem, like the archaic Latin subjunctives *attulās*, *advenam*, etc. It is therefore significant that *tolar* is personal, having the subject nominative, *Venna*, expressed. It is equally clear that its meaning is middle, 'Venna dedicates' or 'Venna brings an offering', naturally, in his own interest as a worshipper of the God.[33] Bolling's suggested translation,[34] 'Venna won me as a prize' would be similarly middle in sense.

That the Italic and Celtic *r*-forms are in the mass medio-passives admits no doubt. Brugmann himself, whose exposition Petersen accepts as the best, puts the supposed 'Medialisierung' of the *r*-element into the period of the 'idg. Urgemeinschaft', because, as he justly observes, 'die *r*-Endungen des Ital. und Kelt. ganz vorzugsweise mediale (deponentiale) Endungen sind' (Grundriss[2] 2.3.658.) Italic and Celtic should have each its own weight, since the Italo-Celtic unity, formerly posited, is totally rejected by Marstrander[35] and practically given up by Meillet. In the second edition of his Esquisse d'une histoire de la langue latine (18), Meillet admits that we can perceive this unity only 'dans une lointaine pénombre'.

The comparison of Indo-European languages so numerous and so widely separated geographically proves that not only verbal *r*-forms, but *r*-forms with middle meaning, were inherited.

There is virtually no positive evidence for a volitive impersonal in *-r*. Petersen cites[36] only Osc. **sakrafír**, Umbrian *ier*, and Ir. *canar*. Umbrian *ier* is not volitive and Ir. *canar* is neither volitive nor impersonal.[37] As regards Umbrian *ier*, it is not even certain that it is a subjunctive form. In the seventh edition of Meillet's Introduction it is still passing for a present indicative.[38] But even granting that it is a perfect subjunctive, its force is not volitive, but conditional, as it occurs on the Iguvinian Tables,[39] and that is the only reality that we have to go by. Oscan **sakrafír** moreover is embedded in a thoroughly 'middle' matrix (as I demonstrated in detail in a paper on 'The Oscan

[33] See my paper, l.c., 29.

[34] LANG. 12.132.

[35] Norsk Tidsskrift 3.249.

[36] LANG. 12.172.

[37] On OIr *canar* cf. LANG. 12.30-1.

[38] A. Meillet, Introduction à l'étude comparative des langues indo-européennes[7] 235 (Paris 1934).

[39] See Buck, Grammar of Oscan and Umbrian 279.

and Umbrian Verbal Forms in -*r*: a Semantic Study'[40]). To make our whole interpretation of the medio-passive *r*-systems depend on Oscan **sakrafír** alone, especially when this can easily be explained as an impersonal use of a reflexive form,[41] is to stand the pyramid on its apex.

It was Thurneysen who introduced the hypothesis of a VOLITIVE impersonal[42]—on very insufficient evidence. Thurneysen admitted that Celtic alone gave no reason to posit a volitive; also that the fact that in Oscan the simple *r*-forms (without the personal ending *t*) occur only in an imperative sense may be due to chance; nevertheless on the evidence of our fragmentary tradition of Oscan and Umbrian alone he concluded that the shorter *r*-forms must go back to an Italo-Celtic infinitive of the goal.[43] Brugmann, in adopting Thurneysen's theory for the Grundriss, gave it an essential modification. He safeguarded the linguistic fact by assuming that already in the Ursprache the *r*-ending had been transformed into the middle ending which we actually find in the languages.[44]

Even with this modification the theory is superfluous. There is a fallacy at the root of this and all other attempts to explain the origin of medio-passive forms, such as Latin *sequitur*, *sequuntur*, for example, by the hypothesis of an impersonal meaning for the verbal ending -*r*. The fallacy lies in the assumption that if forms showing the middle ending in *t* (*to*, *nto*) have a middle meaning, and if forms showing a combination of this ending with the *r*-ending have also a middle meaning, then the ending -*r* by itself probably has some other meaning. The natural inference is that the *r* also has a middle meaning and was added to verb-forms already characterized as middles in order to strengthen the middle sense. The evidence that language often grows by strengthening existing endings by the addition of others of similar significance lies everywhere at hand. So for instance in English *songstress* we have the feminine ending -*ess* added to a word that was already characterized as feminine by the ending -*ster*. The word 'chief' is superlative in meaning, yet in Shakespeare's English we find it strengthened by the addition of the superlative ending -*est*. The King addresses Hamlet as 'My chiefest courtier and my son.'

[40] Read in Washington at the Seventh Annual Meeting of the LSA, Jan. 1, 1931.

[41] On the use of the reflexive in Romance in the sense of both passive voice and indeterminate subject, cf. C. Barrett Brown, LANG. 12.42.

[42] KZ 37.104-6.

[43] Cf. my criticism of this reasoning, LANG. 5.246.

[44] Grundriss[2] 2.3.658-9.

What we should have been looking for all along is therefore evidence that the ending *-r* had from the beginning a reflexive force. The discovery of Hittite *esari* should have been decisive. Here we have in a very archaic form of Indo-European a verb of naturally reflexive meaning doubly characterized as middle by the ending *-a* (IE *-o*) and the ending *-ri*. Petersen's assumption that the imperative form *esaru* is older than the indicative *esari*[45] seems purely arbitrary. Sturtevant says that the ending *-ru* arose from a contamination of *-ri* with the active endings *-u* and *-tu*.[46] If so, *esari* would be the older form. But in any case a verb of such every day occurrence as that meaning 'sit' would naturally function as an indicative from time immemorial. The root *ēs* is perhaps of all IE bases the one most indissolubly associated with the middle voice. It is given first by Delbrück among IE media tantum and is indeed naturally reflexive like Fr. *s'asseoir* and Ger. *sich setzen*. It is true that active forms of the verb occur in Hittite, beside the middle forms,[47] but that is simply another instance of the reflexive use of the active voice, on which Wölfflin wrote an illuminating essay years ago.[48] I see no reason to suppose that the Hittites sat down differently from other men. Even Hittite kings no doubt seated themselves, like ordinary mortals.

There are in fact no very archaic impersonals in *-r*.[49] Few, if any, facts of IE comparative grammar, on the other hand, are better established than the existence of a middle *r*-ending in the parent speech. This does not necessarily mean that it was co-extensive with the whole IE territory. As Meillet says, 'Le fait peut être dialectal.'

[45] AJP 53.209.

[46] Comparative Grammar of the Hittite Language 262.

[47] Cf. Sturtevant 31, 91.

[48] Archiv für lateinische Lexikographie und Grammatik 10.1–10.

[49] It is a striking fact, to which too little attention has been paid by 'impersonal' theorizers, that the Old Irish passive has a subject in the third person, as in *berir breth foir* 'judgment is passed upon him', Wb. 12d 38. It is in Modern Irish that the use of the passive with a subject is entirely given up and the *r*-form conceived as a 'man'-form, governing the accusative of all pronouns. Cf. Pedersen, Vergleichende Grammatik der keltischen Sprachen 2.394–5; Walde, Über älteste sprachliche Beziehungen zwischen Kelten und Italikern 22. We can thus see the development of the impersonal use in historical times. Zimmer was so impressed with the difficulty of explaining this usage on his own theory of the impersonal as the earlier force of the form that he actually suggested (KZ 30. 255–6) that the use of the nominative with OIr. *doberr*, *doberar* might be an imitation of the Latin construction with *dicitur*, *dicatur*, due to the influence of Irish scholars. This hypothesis is certainly more ingenious than convincing.

That it was fairly widespread, however, is proved by the impressively large number of languages that are now known to have had medio-passive *r*-forms. Moreover, the comparison of Old Ir. *atluchur*, Lat. *loquor*, and Hittite *yahari*, for example, shows that it was not confined to the third person, either singular or plural, but at a very early period was associated with the first person, also. There is practically no evidence for a volitive meaning attaching to it, but incontrovertible testimony to its middle meaning.

THE SOURCE OF THE HITTITE *ḫi*-CONJUGATION

E. H. Sturtevant

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Hittite verbs fall into two conjugations, one of which shows in the first person singular active an ending *-mi* and the other an ending *-ḫi*; it is customary to speak of the *mi*-conjugation and the *ḫi*-conjugation. Now Greek verbs are commonly classified as μι-verbs and ω-verbs. Consequently it naturally occurred to Hrozný, Sprache der Hethiter 101, that, as the Hittite ending *-mi* is identical with Gk. -μι, so Hitt. *-ḫi* ought to be identical with Gk. -ω. The first attempt to carry through this suggestion in detail has just been published by Couvreur, Annuaire de l'Institut de Philologie et d'Histoire Orientales et Slaves 4.551–73 (1936).

Since the author is one of those who accept the laryngeal hypothesis,[1] he must identify the consonant of the ending *-ḫi* with an IE laryngeal. He has some doubt as to whether he should see in it the voiceless spirant, which he writes *ḫ* and I shall write *x*, or the voiced spirant, which he writes ʿ and I shall write γ. Since *x* is held to induce *a*-color of a neighboring vowel, the former hypothesis requires the assumption that IE *bherō* comes from **bhero-ax* or **bhero-xa*; but the Stosston of Lith. *vežù*, etc., is against such a development (cf. Hirt, Handbuch der griechischen Laut- und Formenlehre[2] 468). Consequently he prefers to start with **bhero-γ*.

This, however, causes difficulty; for Couvreur, Hett. *ḫ*[2] 140–94, holds that original γ yields Hittite *ḫ*, as against *ḫḫ* from *x*, while the first person singular of the *ḫi*-conjugation ends in *-ḫḫi* when a vowel precedes the ending (e.g. *da-aḫ-ḫi*). He tries to get around the difficulty by citing such orthographies as *da-a-aḫ-i* in three or four passages (all still unpublished) beside hundreds of occurrences of *da-aḫ-ḫi*,

[1] De Hettitische *ḫ* (Leuven, 1935), reviewed in Lang. 12.210–13, was a summary of a monograph, which has now been published in full under the title, De Hettitische *ḫ*, een bijdrag tot de studie van het Indo-Europeesche vocalisme (Leuven, 1937). In this paper I shall refer to the latter work as Hett. *ḫ*[2].

[2] See Brugmann-Thumb, Gr. Gramm. 69; Hirt, Gr. Laut- und Formenlehre[2] 175; Schwyzer, Gr. Gramm. 246. [Add Bechtel, Die Vocalcontraction bei Homer 78–86 (Halle a.S., 1908). G. M. B.]

etc. He considers *da-a-aḫ-i* an etymological writing due to a feeling that the word is properly **daḫ* with an extension, while the common *da-aḫ-ḫi* is this same **daḫ* with the added *-i* written *-ḫi* because non-initial syllables are rarely written with a sign open at the beginning. Obviously this theory rests upon the assumption that the final vowel of the ending *-ḫi* was felt to be a sort of enclitic, but no evidence is presented to support such a thesis. Instead we have a reference to Hirt, Indogermanische Grammatik 4.120 f., where final *-i* of present personal endings is compared with the element *-i* of nouns. Consequently every consideration leading to the writing of double *ḫ* in *-ḫi* should, by this theory, lead to double *m*, *s*, and *n* in the other present endings *-mi*, *-si*, *-weni*, and *-teni*. Such spellings do, in fact, occur (e.g. *ú-wa-am-mi*—VBoT 24.4.9; KBo. 4.3.1.23; *li-ik-ku-wa-an-ni*—HT 1.1.34); but they are extremely rare. It is perfectly certain that Couvreur's *da-a-aḫ-i* is a scribal error for *da-a-aḫ-*<*ḫi-*>*i*; cf. *kar-ap-*<*pa-*>*an-zi* (KUB 2.15.5.2), on which see HG 47.

The *ḫi*-conjugation ending of the 3 sg. is *-i*, which Couvreur, following Bonfante, IF 52.222 f., identifies with the ει of Gk. φέρει, etc. Elaborating upon Hirt, Idg. Gramm. 4.120–2, 150-f., 154 f., he maintains that Indo-European thematic verbs had an ending *-ei* in 2 and 3 sg. pres. Few will be inclined to follow him here, and I need not point out in detail the difficulties involved; but I must protest against the following statement (556 f.): 'Deux voyelles primitivement séparées par sigma ne se contractant pas en grec homérique (κράτεϊ, μένεϊ), φέρεις ne peut guère être dérivé de φέρεϊ < *φερεσι avec adjonction de la désinence secondaire *-s*.' While there is no categorical proof that εσι became diphthongal ει in Homer, there is nothing against such an assumption, since μένεϊ may be due to the analogy of μένεος.[2]

Couvreur regards the 2 sg. pres. ending *-ti* and the 2 and 3 sg. pret. ending *-ta* as Hittite innovations, but the elaborate hypotheses set up to derive them from an Indo-European ending beginning with *th* (cf. Skt. *-thās* (middle!) and from IE *-to* (also middle!) are not attractive.

The crucial difficulty with the theory is that we have many Hittite verbs that quite clearly belong to the thematic conjugation of Indo-European grammar, and all of them are of the Hittite *mi*-conjugation. I refer particularly to the very numerous verbs with the suffixes *-ske/a-* and *-ya/e-*, whose conjugation is given (278 f. and 281) in my Hittite Grammar. Such forms as *da-aš-ki-mi* 'I am taking', *da-aš-ki-ši* 'you are taking', *da-aš-ki-iz-zi* 'he is taking', *pí-eš-ga-u-e-ni* 'we are giving', *da-aš-ki-it-te-ni*, 'you (pl.) are taking', and *pí-eš-kán-zi* 'they are giv-

ing' contain a suffix ending in a variable vowel, and it is safe to say that all scholars would identify it with the thematic vowel of Gk. γιγνώσκω, etc. Just so the forms *ú-e-mi-ya-mi* 'I find', *ú-e-mi-ya-zi*, *ú-e-mi-iz-zi*, *ú-e-mi-e-iz-zi*, *ú-e-mi-e-zi* 'he finds', *ú-e-mi-ya-an-zi* 'they find' contain a penultimate vowel that can scarcely be anything but the thematic vowel. Opinions differ as to the precise history of the verbs like *ḫa-at-ra-a-mi* 'I write', *ḫa-at-ra-a-iz-zi*, 'he writes', *ḫa-at-ra-a-an-zi* 'they write', *ḫa-at-ra-a-eš* 'you wrote'; but it is generally recognized that the variation of the vowel before the ending stands in some relation to the Indo-European variation between *e* and *o* in thematic verbs. Primary thematic verbs are relatively rare in Hittite, but several such seem to me quite clear; e.g. *wa-aš-še-iz-zi* 'he clothes', *wa-aš-ša-an-zi* 'they clothe'.

On the other hand scarcely any of the *ḫi*-conjugation verbs of known etymology correspond to thematic presents of similar formation. Typical instances are *te-iḫ-ḫi* 'I place': Gk. τίθημι; *da-aḫ-ḫi* 'I take' : Gk. δίδωμι;[3] *a-ri* 'he arrives', *a-ra-a-i* 'he rises, rebels' : Skt. *ṛcháti*, *iyárti*, *ṛṇóti*.

At first glance it would seem that Hittite *ši-(ip-)pa-an-ti* 'he pours a libation' beside Gk. σπένδει offers support to Couvreur's theory, but the radical vowel is wrong. Hitt. *spanti* demands Gk. ον, αν, or α (< ῃ).

Since the *ḫi*-conjugation cannot be identified with the thematic conjugation, I do not see what possibility is left but the identification with the Indo-European perfect, that was first suggested by Kellogg, Some New Indo-European Coincidences in Hittite 38 f., 41. I have supported this identification in LANG. 2.33 f., 3.161–8, 215–25, 6.32–5, 7.115–7, HG 239–50, 256–60. Kurylowicz, Symbolae Grammaticae in Honorem J. von Rozwodowski 102 f. independently and by a different route came to the same conclusion. The identification has been accepted by W. Petersen, AJP 53.197–200, and by Benveniste, Hirt Festschrift 2.231 and fn. 1. Most of the reviewers of my Hittite grammar, however, have not mentioned this topic, important as it unquestionably is; and I infer that they have not been convinced of the correctness of my position. Friedrich, AOF 9.337, gives explicit expression to his doubts about identifying the *ḫi*-conjugation with the Indo-European perfect. Consequently I list here the reasons which now seem to me cogent; they are not altogether the same as those formerly alleged.

1. I am now convinced that Hittite *ḫ* and *ḫḫ* between vowels must

[3] Couvreur does not accept this etymology; but see LANG. 14.76–7.

be sharply distinguished, although both sounds had to be written in the same way initially and finally and also medially when written next a consonant. I am not altogether sure what was the sound or the history of *ḫ*,[4] but that point need not disturb us here. I am in agreement with Couvreur and Sapir in interpreting *ḫḫ* as a velar voiceless spirant, and in regarding this as a sound retained without important change from original *x*. All who hold the laryngeal hypothesis agree that *x* gave a neighboring vowel *a*-color. Consequently the Hittite 1 sg. ending *-ḫḫi* and the IE 1 sg. perfect ending *-a* can readily be combined on the basis of original *-xa*. Kurylowicz and others ascribe the final *-i* of Hittite to the influence of the *mi*-conjugation, and probably they are right. At any rate, we may be confident that it is ultimately derived from the same source as the final *-i* of the ending *-ti*.

2. The 2 sg. pres. ending *-ti* and the 2 sg. pret. ending *-ta* cannot be connected with any Indo-European active ending except the perfect ending of Skt. *véttha*, Gk. *οἶσθα*, Goth. *waist* 'you know', and the like. Such a connection was first suggested by Friedrich, ZDMG 76.167 and fn. 1, and it must have appealed to many scholars, whatever their opinions about the rest of the *ḫi*-conjugation. For those, however, who, like Couvreur, hold to the hypothesis of three Indo-European laryngeal consonants the connection is difficult; for the *α* of Gk. *οἶσθα* requires Hittite *ḫ*. It is precisely to account for comparisons like this that we must recognize a laryngeal of *a*-color which does not survive in Hittite.[5] I shall in this paper follow Sapir in writing *it̓*. The original form of the ending seems, then, to have been *-t̓a*, and we may plausibly assume that the aspiration in Sanskrit and Greek as well as the vowel quality of Greek and Hittite is due to the laryngeal.[6] The final vowel in the present ending has evidently been assimilated to that of all other endings of this tense in Hittite. Very likely the ultimate source of this vowel is the *mi*-conjugation.

In the preterit the 2 sg. ending appears as *-ta*, without change of the final vowel. One may conclude that before the alteration the final

[4] See LANG. 12.185–7 and Sapir, JAOS 57.77.

[5] See Kurylowicz, Études indo-européennes 254 f.; Sturtevant, LANG. 12.141–3, 211 f.

[6] Gk. *στᾱ-* beside Skt. *sthā-* 'stand' and Hittite *(s)ta-* might suggest that original *t̓*) yielded Gk. *τ*; but there is no difficulty in assuming that Gk. *στᾱ-* is the regular development of **sta̓*+ cons. while zero grade *st*; yielded Skt. *sth-* and this consonantism spread at the expense of **stā-* and **sti-*.

vowels of the present endings of both tenses had a 2 sg. ending *-ta*. But there is another difference that appears in most verbs. While the present ending is appended directly to the stem, an *s* is interposed in the preterit (with or without further change), unless the stem already ends in a consonant; e.g. *pa-aḫ-ḫa-aš-ti* 'you protect' : *pa-aḫ-ḫa-aš-ta* 'you protected', but *me-ma-at-ti* 'you speak' : *me-mi-iš-ta* 'you spoke', *pa-it-ti* 'you give' : *pa-iš-ta* 'you gave', *a-ut-ti* 'you see' : *a-uš-ta* 'you saw'.[7] In several of the related languages also *s* is prefixed to this ending when it refers to past time; e.g. Gk. ἤδησθα 'you knew', ἤεισθα 'you went', ἔφησθα 'you said', Lat. *vīdistī* 'you saw', Toch. A *yāmäṣt* 'you made'. Both in Hittite and elsewhere the *s* appears in some of the associated forms; e.g. Hitt. *da-a-aš* 'he took', *da-a-iš* 'he placed', *pa-ra-iš-te-ni* 'you (pl.) send forth'; Gk. ἤδεσαν, Lat. *vīdistis, vīdero, vīderim*, etc., Toch. A *yāmäs* 'he made'. No doubt Benveniste[8] is right in thinking that many of these forms, including the combination *-st̓a*, are inherited; but the fact remains that the reflex of this *-st̓a* is usually associated with indubitable perfect forms. Consequently the presence of the conglomerate in the Hittite *ḫi*-conjugation may be taken as evidence that that conjugation is in part derived from the old perfect.

The ending of the 3 sg. pret. in such forms of *ḫi*-conjugation verbs as *ak-ta* 'he died', *ku-u-ru-ri-ya-aḫ-ta* 'he made war', *me-mi-iš-ta* 'he spoke', *na-iš-ta, na-eš-ta* 'he turned' cannot furnish an argument either for or against the identification of the *ḫi*-conjugation with the Indo-European perfect, until we are more certain of its phonetic value and history.

3. The consonant stems of the *ḫi*-conjugation present an ending *-i* in the 3 sg.; e.g. *ša-ak-ki* 'he knows', *ši-(ip-)pa-an-ti* 'he pours a libation'. Since final single consonants are not lost in Hittite, this ending goes most easily with the ending of Gk. οἶδε, Skt. *véda* 'he knows'. The vowel is always written with an *i*-sign in Hittite, and it is perhaps probable that *-e* had in this form been changed to *-i* under the influence

[7] The only forms of the 2 sg. pret. with *-tta* after a vowel that are known to me are *ḫal-za-it-ta* 'you called' (KUB 1.16.2.60), *pa-it-ta* 'you gave' (14.1.1.22), *šar-ra-at-ta* 'you broke' (14.1.1.42). There is at least one instance of interposed *s* in the present also, namely *pa-iš-ti* 'you give' (KBo.5.6.3.13), but it is even more exceptional than lack of *s* in the preterit. Such a form as *pí-eš-ti* 'you give' is not in point, since the stem *pes-* seems to have developed a more or less complete conjugation (cf. 3 sg. *pí-eš-zi*); see HG 229.

[8] Hirt Festschrift 2.230. Cf. Sturtevant, Lang. 2.34; Petersen, Lang. 9.28 f.; Götze, Kulturg. 56.

of other present endings. We must remember, however, two other possibilities. (1) There is a strong tendency to write *i* for *e*, especially in final syllables; see HG 50 f. (2) There is nothing to disprove the assumption that original final *e* became Hitt. *i*, although final *e* from a diphthong pretty clearly remained. My interpretation (HG 97, 166) of *utne* as a dual was merely a suggestion; it cannot prove anything.

4. The vowel stems of the *ḫi*-conjugation show final *-ai* in the 3 sg., but other forms from the several stems compel us to divide them into two classes, *a*-stems and *ai*-stems; e.g. *da-a-i* 'he takes' : *da-at-ti* 'you take' and *da-a-i* 'he places' : *da-it-ti* 'you place'. At first glance these forms seem to contain stem *da-* or *dai-* plus ending *-i*; but if *-i* represents IE *-e* such an analysis is very unlikely, especially in the diphthongal stems. What should result in the parent language from the addition of original final *-e* to a diphthong is suggested by such Sanskrit perfects as *śuśrā́va* 'he heard' or *ninā́ya* 'he led'. Within Hittite itself there is a significant contrast between the *ḫi*-conjugation form *na-a-i* 'he turns' and old present (*mi*-conjugation) forms such as *ne-ya-an-zi* 'they turn' or *ni-ya-ši* 'you turn'; for in these forms we have an original diphthong **nei-* plus thematic vowel plus ending, precisely as in Skt. *náyati*. To me the conclusion seems inescapable that Hitt. 3 sg. *na-a-i* is the bare stem and that Skt. *ninā́ya* represents a secondary spread of the consonant stem ending. I have argued at length, HG 246–9, that the stems in *u*-diphthongs also had originally the bare stem in the 3 sg. perfect, and that this is the source of the Skt. type *dádau*. We must assume that, just as this type spread in Skt. to all long vowel perfects, so in Hittite the type *na-a-i* infected all *ḫi*-conjugation verbs of the *a*-class.

5. With trifling exceptions all primary consonant stems of the *ḫi*-conjugation have *a* as radical vowel in the singular; e.g. *ša-ag-ga-aḫ-ḫi*, *ša-a-ag-ga-aḫ-ḫi* 'I know', *ša-ak-ki* 'he knows'; *ši-pa-an-taḫ-ḫi*, *ši-pa-an-da-aḫ-ḫi* 'I pour a libation', *ši-pa-an-ti*, *ši-ip-pa-an-ti* 'he pours a libation'. This harmonizes perfectly with the *o*-vocalism of the Indo-European perfect.

6. Even more remarkable is the variation between radical *a* in the singular and *e* in the plural in several verbs. The most important material follows:[9] *ša-ag-ga-aḫ-ḫi*, etc. : *še-ik-ku-e-ni* 'we know', *še-ik-te-ni* 'you (pl.) know', *še-ik-kán-zi* 'they know', *še-ik-ku-e-en* 'we knew',

[9] Cf. HG 239–41, 283 f., LANG. 11.182 f.

še-ik-ki-ir 'they knew', *še-ik-tin* 'know ye';[10] *a-ar-ti* 'you arrive', *a-ri* 'he arrives', etc. : *e-ir-te-ni* 'you (pl.) arrive', *e-ru-u-en, ir-u-en* 'we arrived', *ir-te-in* 'arrive ye';[11] *ka-ra-a-pí, ga-ra-pí, ka-a-ra-pí* 'it devours' : *ka-ri-pa-an-zi* 'they devour', *kar-ri-ip-tin* 'you (pl.) devoured', *ka-ri-e-pí-ir* 'they devoured', *ka-ri-pa-an-du* 'let them devour';[12] *ḫa-a-ši* 'he opens', etc. : *ḫe-e-ša-an-zi* 'they open';[13] *a-ša-aš-ḫi* 'I beset', *a-ša-a-ši, a-ša-ši* 'he besets', etc. : *a-še-ša-an-zi, a-ši-ša-an-zi* 'they beset', *a-še-šir, a-še-še-ir, a-še-e-še-ir* 'they did beset, *a-še-eš-te-en* 'beset ye'. This alternation offers a striking parallel to Goth. preterits such as *saḫ* 'I saw' : *sēḫum* 'we saw'.

7. Diphthongal stems show a different vocalism in 1 sg. and 3 sg., e.g. *ne-iḫ-ḫi* 'I turn' : *na-a-i* 'he turns'. Since all *i*-diphthongs with short prior member yield Hittite *e*[14] and all *i*-diphthongs with long prior member yield *ai*, we have here the ante-consonantal forms of the two grades of the radical diphthong exhibited by Skt. *ninéya* 'I led' and *ninā́ya* 'he led'.[15]

These detailed correspondences between the Hittite *ḫi*-conjugation and the Indo-European perfect seem to me to compel the recognition of the latter as an important source of the *ḫi*-conjugation. There is no doubt that the present-aorist system has also had considerable influence, just as the perfect system has given the *mi*-conjugation its

[10] There is also one instance of radical *e* in a singular form, namely *še-ik-ta* 'he knew' (KUB 7.8.2.15). The 3 pl. pres. is sometimes *ša-kán-zi*.

[11] There is also a pl. imper. *a-ar-tin*.

[12] There is also one instance of radical *e* in the singular, namely *ka-ri-pa-aš* 'it devoured' (Kub 5.7.1.34).

[13] There is also a 3 pl. *ḫa-aš-ša-an-zi*.

[14] Such an *e* is often written *i*, and this fact has misled Couvreur, Annuaire de l'Institute de Philologie et d'Histoire Orientales et Slaves 4.557, into the remark: 'En hittite, i.e. *ei* devient *i* et non *e* comme l'indique M. Sturtevant (Comp. Gr., §91). Un traitement i.e. *ei* > hitt. *e* serait en desaccord avec le traitement parallele: i.e. *eu* > hitt. *u* . . ' Couvreur is one of those who believe that the sign *u* had the value *o* in Hittite, but in the opinion of most scholars the variation between *u* and *ú* is purely graphic. I can find no proof that the language had the vowel *o* at all, but I have (HG 54) mentioned the possibility that the *u*-diphthongs with short prior member did, as a matter of fact, yield Hittite *o*, which could not be distinguished from *u* by the cuneiform script. At any rate, Couvreur's objection to the change of *ei* > *e* weighs nothing with me. The proof that the change did take place is furnished (1) by *e-ḫu* 'agedum', which is certainly the imperative of **ei* 'go' with an appended particle, and (2) such forms as our *ne-iḫ-ḫi*: 2 sg. *na-it-ti*, 3 sg. *na-a-i* (parallel with this is *u-uḫ-ḫi* 'I see', where Couvreur would perhaps write *o-oḫ-ḫi*).

[15] See Lang. 7.115 f.

3 pl. pret. in *-er*, occasional instances of 2 sg. pres. in *-ti*, etc. The two Hittite conjugations seem to have been in process of fusion at the time when our texts were composed; but, as far as they are still distinct, the *mi*-conjugation belongs to the present and the *ḫi*-conjugation to the perfect.

Supplementary Note: the meaning of the hittite *hi*-conjugation

In the course of his discussion (551–2) Couvreur brings in a semantic argument, which demands an answer.

In the first place, Couvreur argues that the IE perfect had a definite semantic value, and that this is not found in the Hittite *hi*-conjugation. True enough; but the assumed change of meaning is paralleled in Indo-European. The IE perfect is usually assumed to have had the same meaning as the Greek perfect, denoting a present situation resulting from a past act; the evidence for this is chiefly from Greek itself, supported by Latin and Slavic. But the perfect everywhere tends to become simply a preterite, as in late Greek, to a large extent in Latin, and in Germanic. Another development is also shown in certain verbs, of the type of Lat. *memini*, Skt. *veda*, and the Germanic preterito-presents, in which the present meaning comes to prevail. Hittite shows something of both developments, but the *hi*-conjugation is perhaps best compared to the Germanic preterito-presents. In the latter, the old perfect forms became simply present, and formed a new preterite of the so-called weak type; in Hittite, both present and preterite were formed out of original perfect material.

Couvreur's second point has even less to commend it. He says that if there is any distinction at all between the two Hittite conjugations, it is that the *hi*-conjugation is 'perfective-punctual' in aspect, the *mi*-conjugation 'imperfective-durative'. His arguments are three: First, the use of preterite forms in *-s-*, corresponding to the IE *s*-aorist, is limited to the *hi*-conjugation, which is therefore aoristic in force, while the *mi*-conjugation preterite represents the IE imperfect and is therefore durative. Non sequitur. One cannot argue Hittite semantic categories on the basis of prehistoric meanings. Second, the present of *hi*-conjugation verbs often refers to the future, and a punctual present may have future sense. He quotes Hrozný and Marstrander on this, misunderstanding both of them, as neither implies that the *mi*-conjugation present may not also be used as a future. As a matter of fact, it is. Neither conjugation makes any difference

between present and future. Third, the supine in *-wan*, which is combined with the verb *dai-*, *tiya-* in the sense of 'begin to do something', is formed from *hi*-conjugation verbs by using the *-sk-* derivative, which Couvreur says transforms the perfective into an imperfective. He cites the parallel use of the imperfective with verbs of 'beginning' in Slavic. Couvreur is evidently under the delusion that *mi-* and *hi*-conjugation verbs are treated differently in this construction, but this is not the case. See, for a discussion of this Hittite construction, my Hittite Verbs in *-sk-*[67] (Ann Arbor, Michigan, 1936). It may be added that the *-sk-* verb is durative, not imperfective, and may be formed from any type of Hittite verb-stem, of either conjugation; verbs without *-sk-* are, by contrast, punctual.

Couvreur also says that he has made an analysis of the Hittite vocabulary to determine which verbs are punctual and which not. Such a procedure, being based not on form but on purely subjective considerations, is of no value, though it is astonishing how many otherwise reputable linguists, as soon as they come to a consideration of verbal aspect, attempt it. Even so great a scholar as Delbrück tried to list the various aspects into which Indo-European verb roots might be classified. Since the aspect system of the Slavic languages was first called to the attention of linguistic scholars, various writers have been so entranced by it that they have tried to find a perfective and an imperfective classification in almost any language whatever, often with very little basis other than their own imaginations. Linguistic classifications of meaning, or grammatical categories, are always arbitrary, and peculiar to each language. Some categories are widespread, and perhaps a few may be almost universal, but one can never argue for the existence of a particular semantic distinction in a given language except on the basis of the form of that language itself.

The second rule for the relationship of form to meaning is that, while a semantic difference always implies a formal difference, the converse is true only if the formal difference exists within a single word. Where different forms exist, but only in different words, and the words are syntactically equivalent, then we have a purely morphological classification, unaccompanied by any difference in meaning. Thus Latin *rexi* and *regebam* are semantically as well as formally different; they are different forms of the same verb; but *regam* and *amabo* are formally different, but not semantically, because the forms belong to different verbs, which follow different inflectional patterns. If both these types of future were found regularly in all four Latin conjugations, we

should expect a difference of meaning also. Now the Hittite *hi*- and *mi*-conjugations are syntactically equivalent, and the same verb does not normally have forms of both; hence we must not seek for a difference in meaning between them.

It is true that certain Hittite verbs do exhibit forms of both conjugations, though not to such an extent as to affect the pattern of the language. A few such cases need not worry us, any more than such fluctuations as that of Latin *lavo* between first and third conjugation, imply a difference in meaning between the two conjugations. In other instances it is legitimate to seek for individual differences of meaning. One of Sommer's earliest contributions to Hittite was the separation of the forms of the *hi*-conjugation *pai*- 'give' from those of the *mi*-conjugation *pai* 'go' (BoSt. 4.1), and in some other cases, where the evidence is scantier, future study will doubtless bring out differences of meaning hitherto unrecognized. For suggestions in the case of the verbs *arai*- and *zinna*-, see my Hittite Verbs in *-sk*- 101, 107. But it must be remembered that these are individual, not cat orical, differences of meaning.

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PROBLEMS OF TOCHARIAN PHONOLOGY

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§1. The forty odd years which have elapsed since the first publication of a Tocharian text[1] leave much for the comparativist to do. As clear as is the affinity of both dialects A and B[2] to Indo-European, even so inexplicable are still many of the countless special developments that have led to their remarkable appearance alongside the other languages of the family. It is, in fact, the problem of the relationship of Tocharian to the other Indo-European dialects which has most interested scholars from the very beginning, while very little time has been devoted to the comparative study of the two dialects themselves. Indeed, their interrelationship is as yet quite imperfectly understood. As remarked already by M. Sylvain Lévi,[3] in speaking of the vocabulary, the more one observes their fundamental identity, the more one is struck by their divergences. The remark may apply no less appropriately to the phonology.

The phonology of Tocharian has proved disconcerting for the comparativist from the very start. The one order of (voiceless) stops from the three (or four) of PIE renders the etymology extremely ambiguous from the point of view of the consonantism alone, especially when coupled with the fact that the three guttural series likewise fall together. But this is not the worst. The vocalism so far has defied almost every attempt that has been made to bring it to order. In 1924

[1] Hoernle, JASB 62.39 f. (1893).

[2] I use here the name 'Tocharian' in the established (American) sense for both dialects A and B, without regard to the appropriateness of the name for both or even one of the dialects. Lévi's 'langue Ārśi' (= A) and 'langue de Koutcha' or 'Koutchéen' (= B) may be preferable on several counts, but the former is yet quite unfamiliar in English. Should we translate perhaps as 'Arshian'? 'Kuchean' is of course familiar in England through the works of Sir M. A. Stein and others. Cf. Lévi, Le 'Tokharien B', langue de Koutcha, Journal Asiatique 1913 (2).311 ff., 1933 (222).1 ff. (The latter also in Fragments de Textes Koutchéens, Cahiers de la Société Asiatique 1re Série, II, Paris 1933).

[3] Fragments de Textes Koutchéens 31 ff.

Feist was forced to conclude his short survey of the Tocharian vocalism with the comment 'Es herrscht völlige Regellosigkeit'.[4] Schwentner in 1935 could do little better: 'Ein anscheinend regelloses Durcheinander'.[5] But in view of the ambiguity of the Tocharian consonant system, it is all the more imperative that the few vowel correspondences of observed regularity be clearly kept in mind. An example of the extreme indifference to vocalism is seen in Poucha's article 'Die tocharische Lautverschiebung',[6] where, for example, A *tseke* 'sculpture' is connected with A, B *tsäk-* 'burn', and A *ko-* 'kill' is compared to B *ken-* (< PIE $*g^{w}hen$-, with reference to Schrader, Reallexikon[2] 2.76, where B *kan-* is given). The same fault is to be found in Grierson's 'Etymologies Tokhariennes',[7] where the comparisons (chiefly with modern Piśāca dialects) are based almost entirely on consonantal resemblances.

The first step, it seems to me, toward clearing up this maze of conjecture is to make a systematic comparison of the vocalisms of the two dialects. In spite of the scattered state of the published B texts and the possible errors in the earlier interpretations, I believe the time is at hand when this is possible. The start already made by Lévi in the vocabulary to his Fragments de Textes Kutchéens[8] is considerable, and many more comparisons are at hand in the Tocharische Grammatik of Sieg, Siegling, and Schulze,[9] especially in the verb list (421–84), where dialect B is cited chiefly in root form.

I have made use of all pertinent comparisons found in these works or elsewhere, with the addition of a few of my own collection. In trying to establish the original vocalism of forms compared, I have sifted the etymologies proposed—and in this regard great care was necessary—and I have added several more which I failed to find mentioned elsewhere, but which seemed probable enough for my purpose.

In this first attempt at a comparative phonological study, I have picked out some of the more striking phonetic variations of the two dialects, namely the presence of monophthongization in A, and the origins of the vowels *e* and *o* in B for which A shows the greatest variety of equivalents. I have expressly avoided, except in a few citations,

[4] Indogermanen und Germanen 115.

[5] Geschichte der indogermanischen Sprachwissenschaft, zweiter Teil, 5, 2 (Tocharisch).35.

[6] Archiv Orientální 2.320 ff. (1930).

[7] Journal Asiatique 1912 (19).339 ff.

[8] Above ftn. 2. Henceforth abbr. Fragm.

[9] Göttingen, 1931. Henceforth abbr. Toch. Gramm.

forms involving the 'reduced' vowel (*ä*) and *ā*. The origins of the former are too varied, and the value of the notation of length still too uncertain for them to be dealt with at present. The correlation A *e* = B *e* (§**3**, 3) involved forms apparently negated by the prefix *en-*; but since this vocalism for the negative prefix is not constant, it was thought better to treat such forms separately in §**5**.

Interested scholars will, I think, excuse the omissions and oversights, which of course are numerous, due in part, as they all will know, to the scattered state of the B materials. At any rate I hope that this first attempt at a comparative study of the Tocharian vocalism will add its bit to the eventual clearing up of the most confusing maze that Indo-Europeanists have yet had to face.

§**2**. Monophthongization in Tocharian A.[10] One of the most obvious and commonly recognized vowel changes which places the vocalism of A on a less archaic plane than that of B is monophthongization of original diphthongs. The PIE *i-* and *u*-diphthongs give PToch. *ai* and *au* respectively, which remain in B but become *e* and *o* in A. The situation is remarkably parallel to that of Iranian beside Sanskrit.

1. A *e* = B *ai*. A *e-* 'give', B *ai-* (with sb. A *elune* 'gift', B *ailyñe*, etc.) which, according to Pedersen,[11] probably represent a verbal form of a root hitherto known only in substantives as Grk. *αἶσα* 'lot, fate', Osc. *acteis* 'partis', etc. (Walde-Pokorny, Vgl. Wtb. 1.2); that is, a root *ai-* 'give, share', with semantic relationship as in Lat. *pars*, *portio* : Grk. *ἔπορον* 'gave, allotted'. Pedersen[12] also connects B *aik-*, *aiś-* 'know' with Goth. *aih* 'owns', Skt. *īçe* 'own, rule, am able' (IE **ēik̑-*, Walde-Pokorny 1.105). For the meaning compare the Germanic group NHG *können* : *kennen*, etc. To this verb perhaps belongs B *aiśai* (*yāmtsi*) to which corresponds A *eśe* (*yatsi*) '(make) manifest' (Toch. Gramm. 3). A *tre*, B *trai* (masc.) 'three' in all probability represent the nom. PIE **treyes* (Skt. *trayas*, Grk. *τρεῖς*, etc.).[13] The group for 'write, paint', A *pik-*, *pek-*, B *piṅk-*, *pai(y)k-*, with A *peke* 'picture', vbl. sb. *peklune*, B *paykalñe*, is of course related to Skt. *piṅçáti* 'adorns', OPruss. *peisāi* 'writes', ChSl. *pĭsati* 'write', Lat. *pingere*, etc. (Walde-Pokorny 2.9).[14] If the preterit participles A

[10] Cf. also Feist, Indogermanen und Germanen[3] 115 ftn. 1; Fraenkel, IF 50.7.

[11] Le Groupement des Dialectes Indo-européens 19 f. So also now Benveniste, 'Tokharien et Indo-Européen', in Germanen u. Indo-germanen, Festschrift für H. Hirt 1.235.

[12] Groupement 31.

[13] Cf. Meillet, MSL 17.286.

[14] Meillet, Indogerm. Jahrbuch 1.18; Schrader, Reallexikon[2] 2.353; Schulze, Kleine Schriften 260, etc.

pāpeku, B *papaikau* are original perfect forms, we are probably dealing with PIE *oi* (rather than *ei*). Another well-defined IE root is represented in A *tsek-*, B *tsaik-* 'form, shape', with sb. A *tseke* 'sculpture' : Skt. *dehmi* 'smear, anoint', Lat. *fingere*, etc. (**dheiĝh-*, Walde-Pokorny 1.833).[15] For the original vocalism of the pret. ptc. A *tsātseku*, cf. the remark on *pāpeku* (above). It is useless to conjecture the grade represented in A *tseke* (cf. Grk. τεῖχος neut. or τοῖχος masc.) and *kuntistsek* 'potter'. The conditions for the palatalization of the dental to *ts* are not further elucidated by these examples. The root A *l(y)ip-*, *lep-* (e.g. pret. pple *lipo*, *lyipo*, etc., pret. act. 3 pl. *lepar*), B *lyip-* 'be left' is probably cognate with the Germanic group, Goth. *bi-leiban* 'remain', *af-lifnan* 'be left', etc. (Walde-Pokorny 2.403), as Meillet has already suggested.[16] Even these few examples show clearly the normal reflex of the *i*-diphthong, and there are also other rather obvious etymologies bearing out the rule but which I fail to find mentioned. For example A *lit-*, *let-* 'go away, fall from' (= Skt. *cyu-*), B *lait-* 'transmigrate' must be related to Av. *raēθ-* 'die' (pres. *iriθyeiti*, cf. A pres. mid. *litanträ*), and the Germanic group of Goth. *af-*, *usleiþan*, OIcel. *līþan* 'go', likewise in Germanic with indication that the original application was to departing from this life, cf. OIcel. *liðinn* 'dead', *leiði* 'tomb', OHG *leita*, *leiti* 'funeral pomp' (Walde-Pokorny 2.401). Also, though not so identical in formation, A *ritw-* 'be joined, bound', B *ritt-*, with sbs. A *retwe* 'combination, composition', B *raitwe* 'arrangement' (Toch. Gramm. 3.462 f., Fragm. 140), may be compared to the Germanic and Baltic group, Goth. *ga-raiþs* 'fixed, appointed', OE *gerǣde* 'ready', Goth. *garaidjan* 'prepare', etc., Lett. *raids* 'ready', *riedu*, *rist* 'arrange'. The identity with the Tocharian renders imperative the separation of the Germanic forms from the root **reidh-* 'ride', and the connection with the base **(a)rēi-* 'join, fit' (in Grk. ἀραρίσκω, etc.) all the more probable (cf. Walde-Pokorny 1.75, 2.348). The Tocharian group for 'wash', A *lik-*, *lek-*, B pret. *laik-* (Toch. Gramm. 465) may be cognate with Lat. *liqueo* 'be fluid', *līquor* 'be fluid, melt, flow', OIr. *fliuch*, W *gwlyb* 'damp', etc., from an IE **wleiq^w-* (Walde-Pokorny 2.397). For the loss of *w* before *l*, cf. A nom. *wäl* 'king', obl. *lānt*, etc., B nom. *walo*, *wlo* beside nom. *lānte*, etc. : Lat. *valeo* 'be strong', OIr. *flaith* 'ruler, sover-

[15] Schulze, Kl. Schr. 240 ftn. 3, 260; Fraenkel, IF 50.97.

[16] JAs. 1911 (18).633. Smith's (Videnskabsselskabets Skrifter 2. Hist.-Filos Kl., 1910, no. 5) early derivation from IE **leiq^w-*:Grk. λείπω, etc. is certainly to be discarded (cf. Meillet, l.c.), but is still quoted by Schwentner (Tocharisch 35), Lévi (Fragm. 32) and Reuter, Journal de la Soc. finno-ougrienne 47.4.13.

eignty', ChSl. *vladą, vlasti* 'rule', etc.[17] With A *me-, mew-* 'tremble' (e.g. pres. act. 3 sg. *meṣ*, 3 pl. *meyeñc*, verbal noun *mewlune*), B *miw-, maiw-* (Toch. Gramm. 456) can be compared OIcel. *mjōr, mjǣr, mǣr* 'thin, slender' (< **maiwa-, *maiwi-*),[18] but the treatment of the *w* in A is uncertain. Here also perhaps B *maiwa* = Skt. *dahara-* (Fragm. 46, 127), *maiwaññe* 'childhood' (ibid. 89, 127)? A *met-* 'set out', B *mit-, mait-* (Toch. Gramm. 456) may mean more originally 'change (place)', and are in this way to be referred to the group of Lat. *mūto* 'change', Goth. *maidjan* 'alter, falsify', Skt. *mithati, methati* 'associates with, disputes' (IE **meit(h)-*, Walde-Pokorny 2.247). The Tocharian verb shows a semantic development parallel to that of Lat. *meo* 'go, wander', Pol. *mijać*, etc. 'pass by, avoid', from the unextended root **mei-* = **mei-* 'change, exchange' (Walde 2.240, 241). A *wek-* 'lie' (pret. ptc. *wāweku*), B sb. *waike* 'lie' (Toch. Gramm. 473), B *waiyke reki* 'parole mensongère' (Fragm. 58, 67, 153), may be connected with OIcel. *vīkva, vīkja* 'turn, turn aside, dismiss', OE *wīcan* 'yield, give way, fall down', NHG *weichen*, etc., or Skt. *vējate, vijate* 'recoil, flee from', Grk. εἴκω 'give way, retire', etc. (IE **weig-, *weik-*, Walde-Pokorny 1.233 ff.). For the semantic development 'lie' from 'deviation', cf. Ved. *vīci-* 'deceit, seduction' if it belongs here (so Walde l. c.). Here probably belong also A *wik-* 'disappear' (Toch. Gramm. 471 f.) and B *wik-* 'reject, etc.' (*wikäṣlye* = Skt. supraheya, etc., Fragm. 156). Whether or not A *wek-* 'fall to pieces' (Toch. Gramm. 473 with reference to *wik-*) also goes here is a question, but cf. OE *wīcan* also 'fall down'. A further example of an original diphthong is perhaps A *lek* 'appearance, manner' (Toch. Gramm. 48), without equivalent so far as I know in B. One may compare the Germano-Baltic group Goth. *leik* 'body, corpse', *galeiks* 'like', Lith. *lýgus* 'like', etc. (Walde-Pokorny 2.398). But otherwise no strong-grade form is known. Likewise the new diphthong *oi* in B *soyä* 'son' with *o* < *u* (cf. Grk. υἱός),[19] shows monophthongization in A *se*. There are also numerous examples of the equation A *e* = B *ai* for which I cannot as yet offer plausible connections, e.g. A *ep-* 'cover', B *aip-*; A *ekrä*, B *aikarya* 'empty, desolate'; A *āneñci, āneṁśi*, B *anaiśai* = Skt. *su-*; A *keṁ* 'falsely', B *aṅkaiṁ*; A *ne* rel. particle, B *nai* 'in truth';[20] A *treke* 'confusion', B *traike*; A *sem* 'refuge', B *saim*

[17] Lidén, Aufsätze Kuhn 142 f.
[18] Falk-Torp, Etym. Wtb. 744; Walde-Pokorny 2.242.
[19] Meillet, JAs. 1912 (19).116; MSL 17.286; Idg. Jahrb. 1.14; Fraenkel, IF 50.8.
[20] Cf. Fraenkel, IF 50.19; Hermann, KZ 50.307.

(to A *se-* 'lean upon', Toch. Gramm. 4, 479); A *ske-* 'exert oneself', B *skai-*; A *sne* 'without', B *snai*. The relationship of A *pe* 'foot' (du. *pem*, pl. *peyu*, etc.), B *pai* (obl. sg. *paiyne*, du. *paine*) to the PIE **pĕd-*, *pŏd-* remains obscure.[21]

2. A *o* = B *au*. There are several cases where the etymology is clear. A *oks-* 'grow', B *auk-*, *auś-* 'increase' (? cf. Fragm. 113), and A *okṣu*, B *aukṣu* 'old', A *okar* 'plant' : Lat. *augeo*, Goth. *aukan* 'increase', with *s*-increment as in Grk. *ἀϝέξω*, *αὔξω*, Goth. *wahsjan*, etc.[22] A *ko-* 'kill', B *kau-* : OHG *houwan* 'hew, strike', Lith. *káuti* 'strike, forge', etc. (Walde-Pokorny 1.330).[23] A *ko* 'cow', B *kaurṣu* 'bull' : Skt. *gāus*, Grk. *βοῦς*, etc. (PIE **g^{w}ōu-* Walde-Pokorny 1.696).[24] A *klyos-* 'hear', B *klyau(s)-* : Skt. *çroṣati*, Lith. *klausýti*, etc. (PIE *k̂leu-s-*, Walde-Pokorny 1.494 f.).[25] The palatalization betrays the *e*-grade as opposed to *o*-grade in the words for 'ear', A *klots*, B *klautso*, cf. Ir. *cluas* 'ear', W. *clust* 'hearing'. The palatalization in the weak grade forms A pret. ptc. *kaklyuṣu*, absol. *kaklyuṣuräṣ* (Toch. Gramm. 437), must be analogical. A *lok*, B *lauke* (adv.) 'far' : Lith. *laũkas* 'field', loc. *laukè* 'outside', *laukan* 'out'. Here perhaps also the pre-

[21] Cf. Schulze, Kl. Schr. 252 ftn. 4; Fraenkel, IF 50.7.

Professor Sapir (LANGUAGE 12.263) assumes loss of post-vocalic IE *d* in Tocharian citing this group and A *kri* 'will', *käryā-* 'bedenken', which he connects with the IE words for 'heart', Grk. *καρδία*, Lat. *cor*, *cordis*, etc. To arrive at the proper vocalism in the latter case however he starts from an (otherwise unknown) **kr̥d-* reduced form of **k̂red-* from dissyllabic **kered-*. The comparison of Skt. *çrad-dadhāti* 'believes', Lat. *crēdō*, however, is no support for the base, since the first member of these verbal compounds is probably not a word for 'heart', but rather an Indo-Iranian-Italo-Celtic religious term indicative of the magical properties of an object, cf. MIr. *cretair*, W. *creir*, *crair* 'relics (of the Saints), holy thing' (Walde-Pokorny 1.423; Walde-Hofmann 287; and especially Vendryes, Rev. celt. 44.90 ff.). Against this loss of post-vocalic *d*, I would cite B *preściye* 'mire, filth': Lith. *brendù*, *brìsti*, Russ.-ChSl. *bredu*, *bresti* 'wade', etc. (Lidén, Stud. z. toch. Sprachgesch. 7 f.; but we might have here IE *dh* of course); A *kät-k-* 'überschreiten, vorübergehen' (Toch. Gramm. 427), B *kät-k-* 'tomber, passer, arriver à':Lat. *cado* 'fall', Skt. *çad-* 'fall off' (so Meillet, in Lévi, MSS Remains 378 f.); A *āti*, B *ā̆ti* 'grass': Lat. *ador* 'spelt', Goth. *atisk* 'field of grain'.

[22] Fraenkel, IF 50.230. But A *oko* 'fruit' = B *oko*, hence more probably (after Lidén, Stud. z. toch. Sprachgesch. 34) to Lith. *úoga* 'berry', Goth. *akran* 'fruit (of the field)', etc. (Walde-Pokorny 1.173), rejected by Fraenkel, l.c. Cf. below §4, 1.

[23] Fraenkel, IF 50.222 ftn. 2. For Schrader's A *ko-*, B *kan-*, Poucha's A *ko-*, B *ken-*, cf. above.

[24] Schrader, Reallex.[2] 2.255.

[25] Meillet, JAs. 1912 (19).113; MSL 15.327 ff.; Schrader, Reallex. 1.635; etc.

fix A *lo*, B *lau* 'away, off', shortened in proclitic use.[26] A *śo-*, B *śau-*, *śaw-* 'live' with noun A *śol*, B *śaul* 'life' are best derived from PIE **g^{w}yŏu-* from the *u*-extension of the root **g^{w}ei-* 'live' (Walde-Pokorny 1.668 ff.), rather than from the simple **g^{w}yō-* as usually assumed for Grk. ζώω, Att. ζῶ, etc.[27] The parallel B forms in *ai* (*śai-*, *śay-* 'live', *śailñe* 'way of living') are probably from the simple base **g^{w}ei-*, less probably from an (otherwise unknown) extension in *i*, **g^{w}yĕi-*. A *śoṣi*, B *śaiṣṣe* 'world' show parallel formations from the two bases. Here probably also A *śom* 'young man', B *śaumo* 'nara-'.[28] The plural B *śāmna* shows simplification of the labials in the group *-wmn-*. There are likewise etymologies proposed for a few isolated words in A or B which appear sufficiently sure to quote. A *yoṁ se* 'grandson', lit. 'young son' : Lith. *jáunas*, Lett. *jauns*, ChSl. *junŭ* 'young'.[29] B *ṣñaura* 'sinews, nerves', Grk. νεῦρον 'tendon', Av. *šnāvar-* 'sinew', etc.[30] B *śauk-* 'call, name' : Lith. *kaũkti* 'howl' or *šaũkti* 'call, name'.[31] The original *e*-grade of the diphthong is indicated by the palatalization.

But there are other forms found in both dialects for which fairly satisfactory root connections may be proposed. A *koc*, B *kauc* 'high up' may be derived from **qou-d-* parallel to **quo-q-* in Goth. *háuhs*, etc. For dental formation, but with reduplication and weak grade of root, cf. Skt. *kakud-* 'summit, tip'. A *kot-*, B *kaut-* 'split, cut up', are evidently related to Lat. *cūdo* 'strike' (for **caudo*, cf. Walde-Pokorny 1.330, Walde-Hoffman 300 f.). A *koṁ*, B *kauṁ* 'day' (if not loanwords)[32] may be the formal equivalents of Skt. *çoṇa-* 'red', sb. 'redness' (the cerebral appears secondary, cf. Walde-Pokorny 1.368), from a root **k̂eu-* 'shine' seen also in Skt. *çvas* 'tomorrow', Av. *sūrəm* 'early in the morning'. On the semantic side, cf. the usual derivation of Lat. *diēs*, etc. from **dei-* 'shine' (Walde-Pokorny 1.772 ff.).

[26] Fraenkel, IF 50.16 f.

[27] Meillet, Idg. Jahrb. 1.16; Fragm. 37 (quoted by Lévi). Connection with A, B *śwā-*, *śu-* 'eat' (as e.g. Fraenkel, IF 50.7) seems highly improbable.

[28] So Fraenkel IF 50.8 (but eventually with different IE root connection, cf. ftn. 27). The older comparison with Lat. *homō*, Goth. *guma*, old Lith. *žmuõ*, etc. 'man' is certainly to be discarded.

[29] So Schrader, Reallex. 1.246 and cited also by Poucha, Arch. Or. 2.323. Toch. Gramm. gives only *yoṁ* 'Spur'.

[30] Schulze, Kl. Schr. 261.

[31] Fraenkel, IF 50.227 ftn.

[32] Meillet, Idg. Jahrb. 1.19 suggests Turkish origin without closer identification, but apparently with reference to the group of Osmanli *gün*.

A *koṣt-* 'strike, slay', noun *koṣt* 'blade' (Toch. Gramm. 1 f., 434), B *kauṣ-* 'break, kill' recall forcibly the Indo-Iranian group Skt. *kuṣati, kuṣṇāti* 'tears, pinches, kneads', Av. *frakušaiti* 'slays, kills', NPers. *kuštan* 'kill', with different grade of root (cf. Walde-Pokorny 1.331 top). A *cok* 'lamp', B *cauk-* 'light up' may be derivatives of a guttural extension of the root **dheu-* 'shine' in Skt. *dhavala-* 'dazzling white', *dhāvati* 'cleans, rinses', Grk. θοός· . . . λαμπρός, θοῶσαι· . . . λαμπρῦναι Hesych. (Walde-Pokorny 1.835). A *tor*, B *taur* 'dust' are probably from **dhour-* derivative of the extended base **dhewer-* from **dheu-* in words for 'vapor, smoke, dust', frequently transferred to mental states (Walde-Pokorny 1.835 ff.), cf. Russ. *durʹ* 'foolishness', *durětʹ* 'lose one's mind', Ukrain. *dur, dura* 'stupor, giddiness', Grk. θοῦρος 'rushing, furious', etc. (if with genuine diphthong, cf. op. cit. 842 with lit.). I am inclined to derive A *pot-* perhaps 'honor, flatter', B *paut-* (cf. A *potarṣk-*, B *pautarṣke* 'respectful') from the IE root **bheudh-* in Skt. *bodhati* 'be awake, perceive, notice', Grk. πεύθομαι, πυνθάνομαι 'find out', etc. (Walde-Pokorny 2.147 f.). The semantic development 'be awake', 'give heed', 'honor' is straightforward enough, and no more remarkable than that of Lith. *baudžiù, baũsti* 'punish' (probably also from an earlier 'notice, heed'). A *lop-*, B *laup-* 'soil, stain', aside from the final labial, recall Grk. λῦμα 'filth, dirt, disgrace', Lat. *lutum* 'mud', Ir. *loth* 'dirt' and with strong grade, as in the Tocharian forms, W. *lludedic* 'muddy' (op. cit. 2.406). A *tsuk-, tsok-* (in imper. *p-tsok*, subj. act. *tsokam*), B *tsauk-* 'drink' (supplementive to present A, B *yok-*) may be cognate with Lat. *dūco*, Goth. *tiuhan* 'draw, lead'. The semantic relation 'draw' > 'drink' is common enough, cf. *pocula Lesbii, nectaris sucos ducere* (Hor.), etc., or μέθυ, οἶνον ἕλκειν, etc., or the relation of NE *draft* to *draw*. The B root form *tsok-* (cited Toch. Gramm. 460) is puzzling, unless it has its vowel by analogy with the present *yok-*. A *nut-* 'disappear', beside B *naut-* 'destroy', shows only a weak grade. The group of Goth. *naus*, ChSl. *navĭ* 'corpse', Lett. *náwe* 'death', *náwét* 'kill' is hard to separate (IE **nāu-*, Walde-Pokorny 2.316). For the remaining examples of A *o* = B *au* I fail to find plausible etymologies, e.g. A *o-, on-* 'begin', B *aun-*;[33] A *oñant* (meaning ? cf. Toch. Gramm. 6), B

[33] Connection with any one of several roots in *au-*, or *eu-* is of course possible, cf. e.g. **au-* 'weave' (Walde-Pokorny 1.16), possibly orig. 'begin weaving', cf. Lat. *ordīrī* in the wider sense 'begin'; or perhaps **eu-* 'put on', of clothing, etc. (op. cit. 1.109f.), had a more general sense originally. But such connections cannot of course be demonstrated.

auñento; A *krop-* 'collect', B *kraup-*; A *lotk-* 'turn, become', B *klautk-*, with sbs. A *lotäk* 'manner, way', B *klautko* (Toch. Gramm. 467); A *muk* ('etwa 'nachlassen' ', Toch. Gramm. 456), with adj. *mok* 'old', sbs. *mokone* 'age' (?), *mokats* 'strong', B *mauk-*; A *mrosk-* 'get tired of, renounce', B *mrausk-*; likewise in deriv. suffix, cf. A *prākrone* 'firmness', B *prakrauñe, prakrāwñe* (from A *prākär*, B *prākre* 'firm').

§3. B *e*. The vowel *e*, rare in A except as a result of monophthongization, is extremely frequent in B, and corresponds to at least three different vowels in A, *a*, *o* and *e*. By far the most usual however is the first.

1. A *a* = B *e*. Where the etymology is at all clear we seem to be dealing usually with original *o* which apparently became *a* in PToch., remaining normally in dialect A but becoming *e* in B[34] : A *ak*, B *ek* 'eye' : Grk. ὄσσε, Lat. *oculus*;[35] A *kam*, B *keme* 'tooth' : Grk. γόμφος, OIcel. *kambr*, etc.;[36] A *tkaṁ*, B *keṁ* 'earth' : Grk. χθον- (χθών, χθονός), Hitt. *tekan*;[37] A *rake*, B *reki* 'word' : ChSl. *rokŭ* 'appointed time, goal' (beside *rekǫ* 'say', but also *rěčĭ* 'speech, accusation'); A *lake*, *leki*, *leke* 'sleeping place' : Grk. λόχος 'ambush, childbirth'. In the middle participle we find A *-amāṁ*, B *-emane* beside Grk. *-όμενος*.[38] In other forms we may have original *o* but with less certainty, so for example : A *kanweṁ*, B *kenīne* 'knees' (dual) : Grk. γόνυ, Arm. *cunr* (but also Skt. *jānu*, Lat. *genu*, Goth. *kniu*, etc.); A *krant-*, *krañc-* (stem of *kāsu* 'good'), B *krente*, perhaps, with Pedersen[39] : Ir. *carae*, *carat* 'friend', i.e. a present participle of the type of Grk. *φέροντ-*, Goth. *baírand-* (not Lat. *ferent-*).[40] Others have no close cognates, but the root connections are clear, and belong clearly to the *e/o* series. The vocalism *o* may be assumed for them as representing the type of Lat.

[34] I fail to find evidence to support Meillet's statement (Introduction[7] 99) that Tocharian keeps IE *o* and *a* distinct. For the cases of retained *o* in both dialects cf. §4, 1.

[35] Meillet, JAs. 1912 (19).113.

[36] Reuter, Jour. de la Soc. Finno-ougr. 47.4.9.

[37] Meillet, JAs. 1911(18).147, Idg. Jahrb. 1.19; Kretschmer, Glotta 20.66 f.; Benveniste, Hirt-Festschrift 2.235. But no one seems to have envisaged the difficulty that Toch. *ṁ* does not equal final *m* but *n*. Greek and Hittite show simply the extension of *n* from *m*, phonetically correct in final position, to the oblique cases (Hitt. gen. *taknas*), cf. Sturtevant, Hitt. Gramm. 136. The root has *m*, cf. Grk. χαμαί, Lat. *humus*, etc. (Walde-Pokorny 1.662 ff.).

[38] Cf. also Meillet, MSL 17.284.

[39] Groupement 28.

[40] Contrary to Meillet's assumption (MSL 18.18) based on the palatilized *ṣṣ* from *sk*.

toga beside *tego*, Grk. λόγος beside λέγω, etc.[41] Here I might list A *war*, B *were* 'odor' : Goth. *warei* 'cunning', OE *waru*, OHG *wara* 'attention, heed', from the root of Lat. *vereor* 'fear', Grk. ὁράω 'see' (Walde-Pokorny 1.284 f.). The semantic shift between physical and mental, or between the various senses is usual (cf. Lat. *sentīre* > Fr. *sentir*). A *warpi*, *warpiśke*, *warpäśke*, B *werpīśke* 'garden' may be *o*-grade forms belonging with Latin *urbs* reduced grade of **wer-b(h)-* (Walde-Pokorny 1.275).[42] A *wartsi* 'retinue', B *wertsye*, *wertsiye* 'company' can be from dental formations to a radical element **wer-* seen in words of similar meaning, cf. Skt. *vṛnda-* 'host, crowd', OIr. *foirenn* 'division, crowd', OE *weorn* 'troop, multitude', and for suffix (but with other root form) Skt. *vrāta-* 'troop', OE *wræþ* 'flock' (Walde-Pokorny 1.265 f.). A *kapśañi*, B *kektseñ* 'body' might be compared directly with OIr. *cucht* 'color, outer form, kind', OIcel. *hǭttr* 'mode, appearance' (**qŏktu-*, Walde-Pokorny 1.456). The *p* of the A form is then purely dissimilative. Quite uncertain is the gradation of A *naweṁ* vbl. of *nu-* 'roar', cf. B *newe* 'noise', but perhaps **now-* : Skt. *navate*, *nāuti* 'roar, shout', Ir. *nūall* 'cry, noise' (Walde-Pokorny 2.323), and of A *yśalm* 'sense, sensual pleasure', B *yśelme* 'kāma-' which Pedersen[43] compares with Grk. ἐ-θέλω 'wish', ChSl. *želati* 'desire', but no *o*-grade forms are elsewhere attested (cf. Walde-Pokorny 1.692). Obviously from the root **weq^{w}-* are A *wak*, B *wek*, 'voice' but whether we are to compare Lat. *vōx*, Skt. *vāc-*, with lengthened *o*-grade, or Grk. ὄσσα (**woq^{w}yə*), or the *s*-stem Grk. ἔπος, Skt. *vacas-* is dubious. The usual comparison is with Lat. *vōx*, etc., but I doubt if this is correct, in view of A *knān-* 'know' : Lat. *(g)nōsco*, Grk. γιγνώσκω, etc.; A *āknats*, B *aknāts* 'foolish' : Lat. *nōtus*, Grk. γνωτός. Likewise it would appear that A *ṣar*, B *ṣer* 'sister, from PToch. **ṣäṣar* by syncope (cf. Toch. Gramm. 65), would represent PIE **swesor-* rather than the nom. **swesōr*.

A few forms could point to original *a*, with which IE *o* apparently fell together. So the verbal root A *kare-*, B *ker-* 'laugh' (Toch. Gramm. 426, Fragm. 122) : Lat. *garrio* 'chatter', Grk. γαρριώμεθα·λοιδορούμεθα (Hesych.),[44] but there are other possibilities. In Iranian loanwords

[41] Cf. Meillet, MSL 17.284.

[42] Reuter (Jour. Soc. Finno-ougr. 47.4.13) revives again the ghost of labial development from labiovelars by deriving the group from **wer-g^{w}-* an otherwise unattested formation (cf. Walde-Pokorny 1.272).

[43] Groupement 20 f.

[44] Poucha, Archiv Orientální 2.324.

an *ā* is usually treated in the same fashion, e.g. A *pare*, B *peri* 'debt', cf. Av. *pāra-* 'debt', Sogd. *'prtk* (*apartak*) 'culpable';[45] A *paräṁ*, B *perne* 'position, dignity', cf. Av *xvarənah-* 'majesty', Sogd. *parn* (Toch. Gramm. 18); A *ratäk*, B *retke* 'army', cf. MPers. *ratak*, NPers. *rada* 'series, ordo, acies'.[46]

The syllabic liquids seem to have developed the vowel *a* in PToch., if I am right in comparing A *talke*, B *telki* 'offering' with Goth. *dulgs* 'debt', Ir. *dliged* 'duty, law', etc. (Walde-Pokorny 1.868). The original meaning seems to have been 'what is due'. Perhaps we have *r̥* in the verbal root A *ar-*, B *er(s)-* 'arise' : Skt. *r̥ṇoti r̥ṇvati*, Lat. *orior*, etc., or (an *o*-grade ?) Grk. *ὄρνυμι*, *ὀρούω*.[47]

The simplest view of A *nas-*, B *nes-* 'be' (e.g. pres. A *nasam*, *naṣt*, *naṣ*, etc., B *nesau*, *nesäṁ*, etc., cf. Toch. Gramm. 444, Fragm. 132), would be to equate it, as Meillet does,[48] with Grk. *νέομαι* 'come, go (back, away)', Skt. *násate* 'associates oneself with, joins', as a normal *e*-grade of an unaccented thematic class, but it is possible (and semantically better) to compare rather Grk. *ναίω* (**νασ-ɟω*) 'dwell, be situated; cause to dwell', fut. *νάσσομαι*, aor. pass. *ἐνάσθην*, etc. If this shows a reduced grade of the root of *νέομαι*, etc., then Toch. agrees with Greek in its development. Cf. Walde-Pokorny 2.334 f.

The numeral forms, nom. sg. masc. A *sas*, in composition *ṣa-* e.g. *śäk ṣa-pi* '11', *wiki ṣa-pi* '21'), B *ṣeme*, *ṣe* (*śak-ṣe* '11') deserve special consideration. Meillet[49] assumes an *o*-grade of the root in Grk. *εἷς* (**ἐνς*), *μία* (**σμια*), *ἕν*, Lat. *sem-per*, etc. (Walde-Pokorny 2.488 ff.). It is possible also that we have representatives of **sm̥-* as in Grk. *ἅ-παξ* 'once', Lat. *sim-plex* 'simple', though this is commonly only prefixal. Cf. the discussion of *n̥* below §5.

But there are some cases of A *a*, B *e* which cannot reflect in any fashion a PToch. *a*, whether from *o* or *a* or a reduced vowel of some sort. Here most notably A *mañ* 'moon, month', B *meṁ* 'moon', *meñe* 'month', obviously from **mēn-* : Grk. *μήν*, Lat. *mēnsis*, Goth. *mēna*, *mēnōþs*, etc.[50] The vowel of A *want*, B *yente* 'wind' likewise goes back to *ē*, though perhaps shortened prehistorically : Lat. *uentus*, Goth. *winds*, etc., PIE **wē-ntós* to **wē-* 'blow' (Walde-Pokorny 1.220).

[45] Meillet, MSL 19.159.
[46] Schulze, Kl. Schr. 257.
[47] Meillet, MSL 19.159.
[48] JAs. 1911 (17).456.
[49] MSL 17.284 f.
[50] Schrader, Reallex.[2] 2.70, etc.

That of *A mańk* 'blame, sin', B *meńkī* 'less; lack', *meńkītse* 'inferiority' was originally short if Meillet's[51] comparison with Lith. *meñkas* 'petty, weak', ChSl. *mękŭkŭ* 'weak' is correct, as may be true also for A *swase*, B *swese* 'rain' if from **suw-eso-* (suffix as in Skt. *rajasas*, *tamasas*, Lat. *creperum*)[52] : Grk. ὑετός 'hard rain' (**suw-etos*).[53] Provisionally, based on these few examples, one might posit a development of original *mĕ-*, *wĕ-* to *me-*, *we-* in PToch., maintained in B, but become *ma*, *wa* in A (under the influence of the labial ?). If this is true we can further define the vocalism of the verbal root A *malyw-*, B *mely-* 'press, trample on', cf. Lat. *molo* (**melo*), Ir. *melim* 'grind', but also Goth. *malan*, Lith. *malù* id., and, more especially for both sense and suffix, Goth. *ga-malwjan* 'crush' (Walde-Pokorny 2.286). This development also renders the forms in A *wa-*, B *we-* (cited above as possible *o*-grades) yet more ambiguous.

There are still other examples of this dialectal correspondence for which I can propose no satisfactory etymology or probable root connection. I cite a few here without intention of making an exhaustive list: A *añcwāṣi* 'of iron', B *eñcuwo* 'iron'; A *arämṅ* 'countenance', B *ere* 'bimba-' (cf. also *arämpāt* 'rūpa-', B *erepate*); A *arkämnāṣi* possess. adj. in *-ṣi*, cf. B *erkenma* 'burial ground' (Toch. Gramm. 26); A *pal* (in *märkam-pal* 'dharma-'), B *pele* 'pious', neg. *em-pele* 'impious';[54] A *prańk* 'moment', cf. B *preńke* perhaps 'circumstance, occasion' (Fragm. 139); A *praṣt*, B *preke* (*preśyaṁ*, *preściyai*) 'time';[55] A *yats*, B *yetse* 'hide'; A *saku*, B *sekwe* meaning ? (cf. Toch. Gramm. 47); A *sark*, B *serke* 'family'; A *spaltäk*, B *spelke*, *speltke* 'effort'.

In A *akälyme* 'zugewendet', B *ekalymi* 'vaçya-, soumis', the variation is evidently in prefix (cf. A *kälyme* 'direction', Toch. Gramm. 248, 285). A *anapär*, *anaprä* beside B *enepre* 'before' and A *ane* 'into', *aneñcāṣ* 'out of' beside B *enenkā* 'interior' are all probably various derivatives of the adverbial stem **en-* (Walde-Pokorny 1.125 ff.).

2. A *o* = B *e*. Forms with this correspondence are much less usual than the preceding (§3, 1), and seem to rest for the most part, where there are clear etymologies, on PToch. *a* (IE *o* or *a* or Iran.

[51] JAs. 1912 (19).112; Walde-Pokorny 2.267.

[52] Cf. Brugmann, Grundr.[2] 2.543.

[53] Meillet, JAs. 1912 (19).115 f.; Boisacq, Dict. étym. 999.

[54] Pedersen's comparison (Groupement 32) with MHG *un-bil* 'ungemäss' is surely erroneous because of the A vocalism.

[55] Holthausen's comparison (IF 39.66) with NHG *Frist*, etc. wrecks on the guttural of the B forms, and also on A *tāpärk* 'now', which is probably related For the Gmc. group cf. Walde-Pokorny 2.34.

a in loanwords), just as do the majority of the examples of A *a* = B *e*. The special development here probably lies with dialect A, but I fail yet to find a reasonable explanation for it. Here belong A *ñom*, B *ñem* 'name', cf. Grk. *ὄνομα*, Goth. *namo*, Arm. *anum* (IE *[o]*nomen-*). The *e*-grade would be otherwise unknown.[56] Similarly A *ṣom* sg. obl., *ṣome* pl. nom., B *ṣeme*, *ṣe* '1' (but A nom. sg. masc. *sas*, fem. *säṁ*, in composition *ṣa-*, cf. above **§3,** 1), probably also *o*-grade forms, cf. Grk. *ὁμός* 'common, like, same', Goth. *sama* 'same', etc.[57] Is it worth noticing that both *ñom* and *ṣom* show palatalization of the initial consonant, indicating that *o* in these cases is of secondary origin ? A *porat*, B *peret* 'ax' are probably from an Iran. (Sogd. ?) **paraθ* = Skt. *paraçu-*.[58] The other forms showing this correspondence in radical syllable are of uncertain etymology. One, A *omäl*, B *emalle* 'hot' (with derivative substantives A *omlyi*, B *emalya*), I might connect with Lat. *amārus* 'bitter', Du. *amper* 'sharp, bitter', Skt. *amla-* 'sour' (Walde-Pokorny 1.179). For the semantics cf. Slav. *gorĭkŭ* 'bitter' : *gorĕti* 'burn'. Another, A *poto*, B *peti* perhaps 'reverence' (Toch. Gramm. 3 with ftn.), is connected by Sieg, Siegling, and Schulze (l.c.) with the verbal root A *pot-*, B *paut-* with orig. diphthong (above **§2,** 2), but this is most dubious in view of the B vocalism. Dare I suggest that we have here an *o*-grade derivative of the root seen in the Germanic group, Goth. *bidjan*, *baþ*, etc. (IE **bhedh-*), for which no satisfactory etymology has yet been offered (cf. Walde-Pokorny 2.139 f.)? For A *oṅk*, B *eṅkwe* 'man' (beside *oṅki*? Fragm. 134, cf. **§4,** 1) I can offer no suggestion. The variation can be one of suffix in A *cmol*, B *cmel* 'birth' (: A *täm-*, B *tem-*, *täm-* 'beget, be born'), and appears certainly to be one of prefix in A *opyac*, B *epyac*, *epiyac*. The A form reminds us of *opärkā* adv. beside *pärk-* 'rise' and *oklop* beside *klop* 'sorrow'. The meaning of these forms (and hence the value of the prefix) is uncertain (Toch. Gramm. 249).

3. A *e* = B *e*. Unfortunately most of the forms showing this correspondence remain without plausible etymology. The most likely comparison is perhaps that of A *ents-*, *eṁts-*, B *eṅk-*, *eñc-* 'take, seize' with Grk. *ἐν-εγκεῖν* (redupl. aorist).[59] The relation of A *eṁts*, *ents* 'selfishness', B *eṁtse* 'envy' and of A, B *eṅkäl* 'passion' to this verbal stem is uncertain, and semantically difficult (cf. Toch. Gramm. 2, 7).

[56] Meillet, JAs. 1911 (17).451; MSL 17.284.
[57] Ibid.
[58] Lidén, Studien zur toch. Sprachgesch. 1.17 ff., but without B *peret*.
[59] Meillet, MSL 18.28; Fraenkel, IF 50.227 ftn. 1.

A pret. *weñ-* abbreviated *we-* (pres. *tränk-*), B pret. *weñ-*, pres. *wesk-* 'say' are difficult to separate from A *wak*, *B wek* 'voice' (cf. above **§3**, 1), probably therefore from **weq^w-ne-* and **weq^w-sko-* : Skt. *vákti*, *vívakti* 'speaks, says', Grk. *εἶπον* 'said', etc., but without exact formal equivalents elsewhere.[60] The gen. sg. of the interrog.-relat. pronoun A *ke*, B *ke-te* (where *-te* is a postposition) can reflect PIE **q^{w}eso* : Hom. *τέο*, ChSl. *česo*, OHG *hwes* (Brugmann, Grundr. 2.2.359). A *twe*, B *tweye* 'dust' are perhaps from **dhwes-* : Grk. (Hom.) *θέειον*, Att. *θεῖον* 'brimstone' (**θϝεσ-[ε]ιον*), from the root of *θύω* 'rush, storm, rage', etc. (Walde-Pokorny 1.844). The *e* of B is the final increment frequently not found on the corresponding A form, cf. A *war*, B *were*; A *kam*, B *keme*, etc. The *y* is the glide developed in hiatus. For other examples of the correspondence I fail to find outside connections, e.g. A *e-*, *en-*, B *en-* 'command, punish', A *ek*, B *ekañi* 'wealth'; A, B *yepe* 'knife'; A, B *yerpe* 'disc, orb', etc. The list can be considerably lengthened. It is not possible to draw a final conclusion from such doubtful etymologies as I have cited here. However, such as they are, they point to the retention of IE *e* in both dialects. On the other hand we saw IE *ĕ* > A *a* in **§3**, 1. As yet I see no solution to the problem.

§4. B *o*. The normal in A is *o*, rarely *a*. There is no sure example of *e*.

1. A *o* = B *o*. The only case entirely free of etymological ambiguity is the numeral 'eight', A *okät*, B *okt* from PIE **ok̑tō[u]* (cf. Grk. *ὀκτώ*, Goth *ahtau*, etc.). Likewise *o* seems original in A *orkäm*, B *orkaṁñe* 'darkness', cf. Grk. *ὀρφνός* 'dark', Arm. *arǰn* 'dark brown' (Walde-Pokorny 2.367).[61] The original vocalism of A, B *oko* 'fruit' is ambiguous. Lidén[62] is probably correct in connecting it with Lith. *úoga* 'berry', ChSl. *agoda*, *jagoda* 'fruit', Goth. *akran* 'fruit of the field', etc. (Walde-Pokorny 1.173 f.). Therefore probably *ɔ*, since it seems probable that *o*, *a* and *ɔ* fall together in Tocharian as in Germanic, Baltic, and Slavic. The reason for the retention of IE *o* (or better perhaps the reversion of PToch. *a* to *o*) in both dialects in the first two cases cited is obscure.[63]

[60] Meillet, JAs. 1911 (18).148; Toch. Gramm. 2.

[61] Benveniste, Hirt-Festschrift 2.236.

[62] Stud. z. toch. Sprachgesch. 34, but probably not from *ā* as he assumes.

[63] Sapir (LANGUAGE 12.179 ftn. 15) invokes Brugmann's *ȯ*, that is *o* not of the *e/o*-series (and for which he substitutes *ǫ*), to explain the retention of A *okät*, and in *opäśśi*, *opśi* 'dexterous, skilful', which he connects with Lat. *opus*, etc., as opposed to IE *o* in gradation with *e* which gives A *a*. This explanation comes to

In a few cases, some of them of doubtful etymology to be sure, *o* in both dialects seems to be the reflex of a more original *u*. So particularly in A, B *poṣi* 'side, wall' if Fraenkel[64] is right in comparing Lith. *pùse*, Lett. *puse* 'half, side'. For dialect B this development has the indisputable support of *okso* 'ox' (no A equivalent) : Skt. *ukṣaṇ-*, Goth. *aúhsa*; *soyä* 'son' (A *se* with monophthongization) : Grk. *υἱός*; *kokale* 'wagon', beside A *kukäl* with more original vocalism (*u* from *e* due to influence of the labiovelar, cf. Grk. *κύκλος*, PIE **q^{w}e-q^{w}los*);[65] perhaps also *orocce, orotse* 'large' (no A equivalent) : Skt. *urú-*, Grk. *εὐρύς* 'broad'.[66] For the same development in dialect A can perhaps be counted *por* 'fire', from **puwōr* > **puwr* > **pur*, cf. B *puwār*, or *pwār*, or perhaps original **pur* simply with *r* of nominative but vocalism of oblique cases gen. **punés*, etc. (for inflection cf. Walde-Porkony 2.14, but read A for B).

Slim though our evidence is for the development of *u* to *o* in A, still we may be allowed to apply it to a few other cognates which show *o* in both dialects. So perhaps in A *kos*, B *kos, kosa*, etc. 'yāvat-' if from the interrog.-indef. *u*-stem **q^{w}u-*, but the differentiation from the interrog.-relat. pronoun A *kus, kuc* (Toch. Gramm. 176 ff.), B *k$_u$se, k$_u$ce* (Lévi-Meillet, MSL 18.418 ff.) is remarkable. Here also may be mentioned the pronominal adj. 'all, each, every', A *poñc-, ponts-*, etc. (nom. sg. *puk*), B *po*, pl. *ponta* (cf. Toch. Gramm. 161 f., Fragm. 138). In spite of Lévi's (Fragm. 38) reference to Meillet's connection with Grk. *πᾶς* (*παντός*, etc.) the resemblance is probably purely fortuitous and Brugmann's[67] derivation of the latter from **k̑wā-nt-* : **k̑eu-* 'swell' (Walde-Pokorny 1.365 ff.) remains preferable. However a similar semantic origin is possible. I would suggest connection therefore with Lith. *puntù, pùsti* 'puff up, swell', Lett. *pūst* 'blow, breathe', cf. especially the nasalized form Lett. *punte*

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grief on at least two points. First *ā* (*ǫ*) also gives A *a* in PIE **oq^w-* (Walde-Pokorny 1.169 ff., Brugmann, Grundr.[2] 1.153 ff.), cf. A *ak* (B *ek*, cf. above 3, 1) 'eye', and in PIE **potis* (Walde-Pokorny 2.77 f., Brugmann, l.c.), cf. A *pats* 'husband' (no B equivalent). Secondly IE *o* (alternating with *e*) gives also A *o*; cf. *ṣom* sg. obl., *ṣome* pl. nom. of *sas*, fem. *säṁ* 'one' (above §3, 2 with reference), and also Grk. *ὀρφνός*, A *orkäm*, B *orkaṁñe* probably stand in ablaut relationship to Grk. *ἔρεβος*, Goth. *riqis*, etc. cf. Walde-Pokorny 2.367; Benveniste, Hirt-Festschrift 2.236.

[64] IF 50.229.

[65] Schulze, Kl. Schr. 239; Benveniste, Hirt-Festschrift 229.

[66] Pedersen, Groupement 39.

[67] Begriff der Totalität 23, 35, 53, 60.

'bump, belly', all from a dental extension of an onomatopoeic *pū- 'blow, swell'. For meaning, cf. from the guttural extension Skt. *pūga-* 'multitude, quantity', *puñja-* 'heap, mass, quantity' (Walde-Pokorny 2.80). The nom. A *puk* shows the addition of the particle *-k* (Grk. -γε, Goth. *-k*, etc.) common to pronominal forms.[68] The presence of the more original vocalism in A *pu-k* beside B *po* recalls A *kukäl* 'wagon' beside B *kokale* (cf. above).

One is tempted also to connect A *klyom*, B *klyom(n-)* 'noble' with A *klyos-*, B *klyaus-* (above, §2, 2), as representing a weak grade of the root (**k̑lu-m-*) but this leaves the palatalization of the preceding *l* unexplained.

That Toch. *o* has yet other origins is shown, however, by the comparison of A, B *yok-* 'drink' with Hitt. *ekuzi* 'drinks',[69] and also by that of A, B *kroṅśe* 'bee' with OHG *hornuz*, OE *hyrnet*, Lat. *crābrō*, Lith. *šìršė*, Russ.-ChSl. *sŭrŭšenĭ*,[70] which appear to be various derivatives from PIE **k̑ₑrəs-* (*k̑r̥s-*), cf. Walde-Pokorny 1.406 f. This development of *r̥* is assumed by Fraenkel for A in the isolated *orto* 'up' (: Skt. *ūrdhva-*, Av. *ərəδwa-*, Lat. *arduus*).[71] But the form could as well be derived from **urdh-*, weak grade of the root in Skt. *vardhate* 'increases, grows', Grk. *ὀρθός* 'up-right, straight' (Walde-Pokorny 1.289).

For the rest of the examples of A, B *o* which I have collected, I have no suggestion, e.g. A *oṅk*, B *oṅki* 'man' (beside B *eṅkwe*, above, §3, 2); A, B *ottsoyce* (B = asecanaka-, cf. Fragm. 134); A *klyokäśś-*, B *klokaśne* 'pore'; A *kₒraś*, obl. stem *krośś-*, B *krośce* (*krost-*, *krośc-*, *krośś-*, but also *krauśś-*! cf. Toch. Gramm. 43 f.) 'cold'; A *ṣotre*, B *ṣotri* 'sign'; likewise suffixal in A *śmoññe*, 'place', B *śmoññe*, *ścmoññe* 'base' (: A *ṣtäm-* 'stand' ? Toch. Gramm. 433), and A, B *ykorñe* 'negligence' (: *yäk-* 'be careless').

A particularly puzzling case is the relationship of A *poke* (sg. *ā*-case *pokeyā*, du. *pokeṁ*, pl. obl. *pokes*), B *pauke*, but obl. *pokai* 'arm' to PIE **bhāĝhus* (in Dor. *πᾶχυς*, OE *bōg*, Skt. *bāhu-*, etc.).[72] If there

[68] Toch. Gramm. 306 f.; Meillet, MSL 18.416. Holthausen (IF 39.65) takes the *k* as radical and connects with Grk. *πυκνός* 'close, thick', but does not envisage the forms in *-nt-*.

[69] Sturtevant, Hitt. Gr. 91; Benveniste, Hirt-Festschrift 2.235.

[70] Schrader, Reallex.[2] 2.645; Walde-Hofmann 283; Benveniste, op. cit. 234.

[71] IF 50.6. But on the interrelationship of the forms cited, cf. Walde-Pokorny 1.148, 289; Walde-Hofmann 64 f.

[72] Cf., for example, Meillet, Idg. Jahrb. 1.18; Schulze, Kl. Schr. 255 ftn. 1; Fraenkel, IF 50.7.

were any other evidence for it, one might be tempted to assume an original gradation *bhā[u]ĝh-/*bhəuĝh-/*bhŭĝh-, B *pokai* then representing the weak grade. The situation would be exactly parallel to that observed in the case of Dor. φᾱγός 'oak', Lat. *fāgus*, OE *bōc* 'beech', beside Icel. *beyki* 'beech(woods)', Russ. dial., Ukr. *boz* 'elder' (PIE *bhā[u]ĝos/*bhəuĝos/*bhŭĝos, Walde-Pokorny 2.129).[73]

2. Only a very few cases of A *a* = B *o* have been noticed. In only one of these is the original vocalism quite clear: A *pracar*, B *procer* 'brother' from PIE *bhrāter- (Lat. *frāter*, etc.), but this does not agree with A *mācar*, B *mācer* 'mother' from PIE *māter-. The comparison A *waṣt*, B *ost* 'house' with Grk. ἄστυ 'city', Skt. *vastu-*[74] (PIE *a*) is complicated by the loss of the initial in B. B *solme* 'whole', from which I cannot separate A *salu* 'totally', has been compared[75] with Grk. ὅλος, OLat. *sollus*, Skt. *sarva-* (PIE *o*). The comparison of A *praski*, B *prosko*, *proskye* 'fear' with the Gmc. group OHG *forhta*, Goth. *faúrhtei*, Arm. *erkiul* 'fright', etc.[76] (Walde-Pokorny 2.48) tells us little about the original vowel.

3. The only case where A *e* might correspond to B *o* is the difficult group A *es* beside B *oñi* 'shoulder'. But the forms hardly seem cognate, and, if so, their relationship to Skt. *aṁsa-* Grk. ὦμος, Lat. *umerus*, Goth. *ams*, Arm. *us*, is uncertain.[77]

§ **5.** IE *n̥-. There are various correlations in A and B which apparently involve forms negated by the inherited prefix. But there is considerable diversity in its appearance. For example we have A, B *en-* (by assimilation *em-*) in A *empele* 'powerful', B *empele* 'impious, frightful', to A *-pal* (in *sne-pal* 'unrighteousness', *märkam-pal* 'dharma-', cf. Toch. Gramm. 24, 240, 248), B *pele* 'pious'. The same appearance seems probable in the case of A, B *eñcare* 'aniṣṭa-, apriya-' (Toch. Gramm. 48, 79), possibly for *eñ-ciñcare* by haplology, to A *ciñcär* 'lovely',

[73] In his criticism of my paper on Tocharian vocalism (LSA meeting Chicago, December, 1936), and later by personal letter, Professor Sapir suggests the presence of a sort of *u*-epenthesis in Tocharian. That is, PIE *bhāĝhu-s > Pre-Toch. *pāku- > PToch. *pāᵘku- > *pauk- whence A *pok-e*, B *pauk-e*. This view is exceedingly fascinating for these particular words, in as much as it affords an explanation also for B obl. *pokai*, which may then represent the vocalism inherited from another IE case, e.g. dat. *bhāĝhewai, loc. *bhāĝhēu, *bhāĝhewi, etc.—provided, of course, that IE *ā* may give Toch. *o*.

[74] Schrader, Reallex.[2] 1.443.

[75] Meillet, MSL 18.386.

[76] Cf. Holthausen, IF 39.65 without the B forms.

[77] Meillet, JAs. 1911 (18).150; Schulze, Kl. Schr. 255 ftn. 4; Fraenkel, IF 50.7.

B *cäñcare, cäñcre* 'priya-'. Isolated B forms are *eneṅka* 'except' which seems to be a neg. cpd. of a verbal adj. to *eṅk-* 'take' e.g. 'non compris' (Lévi, MSS Remains),[78] and *eśuwacca* 'having not eaten' (*śwā-, śu-* 'eat'), with loss of nasal before sibilant as in *piś* 'five', *misa* 'meat', but the quality of the vowel is retained, showing that the loss is not of the same antiquity.[79] On the other hand we find *an-* in B *am-plā-känte* 'without coming to an agreement',[80] *anākänte* 'anindita-'[81] (for *an-nākänte* ?) and apparently also in *anaiwatse* in Lévi's doubtful translation 'déplaisant' (Fragm. 112), to which A **ānewāts* (dat. pl. *ānewātsnac* and sb. *ānewātsune*) corresponds. Sieg, Siegling, and Schulze (Toch. Gramm. 9, 19, 68 ftn.) make no attempt at interpretation or derivation. It seems probable to me that, as negative forms, they are derivatives of the verbal root A *e-*, B *ai-* 'give' (cf. above, **§2**, 1) with a suffix *-wāts* (op. cit. 19). The meaning of the unattested simplex would be near that of Grk. αἴσιος 'boding well, favorable' from the same root. A development parallel to that of Grk. *ἀ-*, Skt. *a-* is hardly to be assumed on the evidence of A *āknats*, B *aknātse* 'stupid, ignorant' (vb. *knā(n)-* 'know').[82] We have rather dissimilatory loss of *-n-* before *kn-*.[83] The relation of B *emprarkre*, which Lévi translates doubtfully 'court' (Fragm. 116), to A *apärkär* (Toch. Gramm. 262 without interpretation) is uncertain. Cf. A *pärkär*, B *pärkre* 'long' ? Similarly we find B *empalk* . . . beside A *apälkā* (or *amälkā* ? Toch. Gramm. 321). The meanings of both are obscure (Fragm. 116, Toch. Gramm. l. c.), and so comparison with the verbal root A *pälk-*, B *pälk-*, *palk-* 'appear, shine' is to no purpose.

There are actually then only two probable correspondences for the negative prefix, namely A, B *en-*, and A *ān-*, B *an-*. These are possibly to be explained as variant sandhi forms, originally *en-* from **n̥-* (*ₑn-*) before vowels, *ān-*, *an-* before consonants? This appearance (*ān, an*) for *n̥* is to be assumed (with further reduction, under conditions unknown) for A *känt*, B *känte* '100' from PIE **ḱm̥tóm*, and per-

[78] Manuscript Remains of Buddhist Literature Found in Eastern Turkestan, ed. by A. F. R. Hoernle, Oxford 1916.

[79] Cf. Pedersen, Groupement 32 ftn., but for B *ekalymi* 'subject' (to which corresponds prob. A *akälyme* 'zugewendet') cf. above, **§3**, 1 end.

[80] Meillet, Idg. Jahrb. 1.14; MSL 18.20.

[81] Meillet, JAs. 1911 (17). 456, MSL 18.20.

[82] But so apparently Petersen, LANG. 11.197 with ftn. where he derives A from **n̥-gnətos*.

[83] So Meillet in Lévi, MSS Remains 377.

haps also in A *sas*, *säṁ*, *ṣa-* 'one' (above, §3, 1, 2). If this explanation of their origin is true the two negative prefixes thus arisen have received an entirely new distribution without reference to the following initial, e.g. *em-pele*, *eñcare*, and **ānewāts*, *anaiwatse* beside the historically correct B *en-eṅka*, *am-plākänte*, etc. This confusion is paralleled to some extent in Irish, where correctly we expect *en-* before dental and velar stops and spirants (becoming *in-* before original *d*, *g*; *ē* before *k*, *t*, *s*) but *an-* (*am-*) before vowels, labials and sonants, e.g. *in-derb*, *in-gnāth*, *ē-coir*, etc., but *an-ecne*, *aim-brit*, *am-lobar*, etc. However we find also *an-dach* 'worthlessness', *an-glan* 'unclean', *an-cride* 'wrong', etc.[84]

[84] Cf. Pedersen, Vgl. Gramm. d. kelt. Sprachen 1.46 ff., 2.7 f.; Thurneysen, Hdb. d. Altir. 493 f.

THE EVIDENCE FOR SCHWA SECUNDUM IN LATIN AND GREEK

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This article[1] was suggested by that of Edgerton in LANG. 10.235 ff., in which he shows the insufficiency of the arguments adduced from Sanskrit for the existence of a schwa secundum in Indo-European times. Since the facts alleged in support of the theory from this language may as well be explained otherwise, the question of the existence of the ə[2] is thrown back upon other languages, notably upon Latin and Greek, the proof from which is supposed to be most conclusive. The writer of this article consequently wishes to point out that the evidence from these also has no compelling power, and that it can be interpreted otherwise more persuasively. He also wishes to show the complete lack of conclusiveness of the a-prioristic generalities used to support the schwa-secundum theory. While he therefore does not take it upon himself to say that he is positive that there was no such thing, he is certain that no decisive evidence for it has as yet been found.

The line of reasoning followed by Hirt, Idg. Gramm. 2.78, is without value in a question which is purely historical. He declares that ə must be assumed as an intermediate step between a full-grade short vowel and the zero grade. But we have the same right to assume intermediate stages for all gradual sound changes, including qualitative vowel changes. Thus we could as well argue that there was extant a stepping-stone from IE *o* to Germanic *a* or from *ā* to *ō*, and yet no one has ever made such an assumption, nor has maintained

[1] The material discussed in this article is drawn from the two principal more recent discussions of the schwa secundum: Hermann Güntert, Indogermanische Ablautsprobleme, Strassburg 1916; Hirt, Indogermanische Grammatik 2.76 ff. These two works are usually referred to merely by the name of the author and the page number.

[2] In accordance with the more recent practice I use the sign ə to designate the schwa secundum under all circumstances, although this deviates partially from Hirt's notation, and completely from that of Güntert.

that the quality of certain unexplained vowels perpetuated such a stage. Furthermore, if we once recognize Hirt's argument as cogent, we must apply the same again and again ad infinitum to assume further intermediate stages between the normal grade and the reduced grade on the one hand, and between the latter and the zero grade on the other, so that we would have on our hands not only a schwa secundum, but a schwa tertium, quartum, quintum, and what not. It is not difficult to see where the fallacy of Hirt's argument lies. Certainly all spontaneous and gradual sound changes presuppose any number of intermediate stages between the old and the new, but we have no right to assume that any one of these stages was crystallized because the process was held up somewhere so as to leave a trace in the language after its completion. On the contrary, the presumption is that there is no trace of any intermediate stage[3] unless we can fix the conditions under which the development was arrested, as opposed to the normal conditions under which it attained its end. If this cannot be done, the assumption of, e.g., the existence of ъ is a reversion to the doctrine of sporadic sound change of the age of Schleicher and Curtius.

The postulate that the existence of ъ can be established only by fixing the conditions under which it develops, implies not that it is sufficient to make a more or less probable guess at conditions under which it might possibly have arisen,[4] but that material must be quoted in actual conformity with the suggestions advanced. However, an examination of the latter shows very clearly that there is either no positive proof, or else the material cited actually conflicts.

One circumstance which might hold out some promise for explaining a number of alleged occurrences of ъ would seem to be the absence of a *i̯, u̯, r, l, m,* or *n* in an ablaut basis, which could, as *i, u, r̥, l̥, m̥,* or *n̥,* become the syllable bearer after the loss of the original strong vowel. The consequent necessity of heaping consonants in combinations impossible or difficult to pronounce might have led to stopping the development just short of the actual dropping of the vowel,[5] and while

[3] In case of IE changes it is self-evident that such stages cannot be established from actual documents, nor would it help if this could be done, since the existence of ъ would presuppose the persistence of such a stage to a later period.

[4] Güntert (128 ff.) makes complicated suggestions, but recognizes that they are no more. The more important of these are discussed below. Others, as the assumption of varying sandhi conditions, or of allegro and lento forms, are so completely incapable of proof that they cannot even be discussed.

[5] Cf. Edgerton, LANG. 10.263 f.; Sturtevant, LANG. 11.184.

the burden of proof would rest upon those who maintain that such a vowel differs from *ə*, yet at least it would seem plausible that there was such a difference. Thus an IE **ghbhono-s* could hardly have been pronounced, and so **ghъbhono-s* is reconstructed to account for Goth. *gibans*, the passive participle of *giban* 'give' with the normal grade from IE **ghebh-*. The only trouble with this line of argument is that nothing that can be reasonably taken as proof for an *ъ* exists under these circumstances, but only forms pointing to the normal grade vowel *e*, as in Goth. *gibans* just quoted. Similarly from the IE **pekᵘ-* the verbal adjective in *-to-*, could not, it is true, have been the unpronouncable **pkᵘtó-s*; but not **pъkᵘtó-s*, but **pekᵘto-s* is the antecedent of Skt. *paktá-ḥ* Gr. πεπτός Lat. *coctus* < **quectos* < **pekᵘ-*. Of course the adherents of the schwa-secundum theory maintain that *e* is here IE *ъ*, although otherwise they assume that *ъ* becomes almost everything else than *e* in the various IE languages. If we reflect how often IE ablaut grades have become redistributed so that weak grades receive the accent and strong grades lose it (cf. e.g. Skt. *gáti-ḥ*, Gr. βάσις, IE **gᵘm̥ti-s*, Skt. *saptá*, Gr. ἑπτά, IE **septm̥* [after **oktōu* > Gr. ὀκτώ] for original **séptm̥*), it seems strange to insist that all these forms pointing to an *e* nevertheless had an IE *ъ* because found in originally unaccented syllables, instead of having suffered an analogical restitution of the *e* after e.g. the present **pekᵘō*, at a time when the weakening of the stem-vowel of what was once **pekᵘtó-s* made the latter hard to pronounce. If the proof of the existence of *ъ* depends on such instances its case is a very weak one indeed, and, unless more convincing conditions of its occurrence can be fixed, one's skepticism is bound to increase rather than decrease.

It is also conceivable that the existence of *ъ* had something to do with accentual conditions, but attempts to establish such connections have been dismal failures and amount merely to the statement of theoretical possibilities rather than to deductions from the material at hand. Statements that the reduced grade preceded the accent and the zero grade followed it[6] conflict with some of the best established and most pervading ablaut schemes, as typified by Skt. *véd-a* 'I know' : *vid-máḥ* 'we know', Gr. πατήρ nom. 'father' : πατρός gen., in which the syllable originally preceding the accent was reduced to

[6] Hirt (194) admits that both zero grade and reduced grade occur before the accent in dissyllabic words, but thinks he can explain the conditions. For longer words he also suggests accentual conditions to account for the divergence. Against such attempts, Güntert 123.

the zero grade and shows no sign of a schwa secundum. To make matters worse, such an assumption conflicts with the theoretical basis for ъ, sc. that it is necessary that there be an intermediate stage between the normal and zero grades. It is obvious that if schwa secundum preceded the accent and the zero grade followed it, the two can in no sense represent successive stages of the same process.

The suggestion of Güntert (121) that ъ arose when secondary accentuation of a syllable prevented the completion of the process of eliminating the unaccented vowel does not fare much better than the preceding. While such an occurrence is conceivable, it is clear that such shifts of accent, necessarily analogical, must have taken place at the time when the original full-grade and now reduced vowel was about to disappear, and could therefore have occurred only occasionally and not on the large scale presupposed by the schwa-secundum theory, so that the very wealth of material cited is a suspicious circumstance against the theory it is meant to prove. Still worse is the obvious fact that actual accentual conditions in Sanskrit and Greek conflict directly with such an assumption, and that too in the very words which are cited as the star witnesses for ъ. Thus Gr. πίσυρες 'four' (only in nom. and acc. pl.), ἵππος 'horse', and ἴθρις· τομίας κριός Hesych. all show the accented *i*, which is supposed to represent IE ъ, in a syllable which was also originally accented; for the weak grade of the second syllable of πίσυρες shows it to have been an unaccented syllable, so that the πί- could only have been IE accented **k̑ᵘó-*, whereas an original vowel *e* with the accent is established for ἵππος by Skt. *áçva-ḥ* and Lat. *equus*, and for ἴθρις by Skt. *vádhri-ḥ*. For the latter the theory becomes still more impossible because in the pair ἴθρις = ἐθρίς the accentuation of both words is just the reverse of what is required by it. In all instances like ἵππος Güntert would have to assume that an original **ék̑ᵘo-s*, both without documentation and knowable cause, had first become **ek̑ᵘó-s*, then *ъk̑ᵘó-s*, and that the accent then reverted to its original position, although no other language offers the slightest support for a construction **ъ́k̑ᵘo-s*.

This lack of coherence of the examples quoted as well as the failure of the various languages to agree as to the conditions under which ъ is supposed to occur, is shown also by the following considerations. In Sanskrit the ъ is sought mainly in the occurrence of *i* and *u*, which are found in antevocalic position before *y* and *v* after heavy syllables as opposed to mere *y* and *v* after short syllables (Sievers' law), e.g. in Ved. *mártiya-ḥ* beside *nā́satya-ḥ*. In Greek it is chiefly the occurrence

of an accented (!) *ι* in place of an expected *ε*, and, more rarely, of an *υ* in place of an expected *ο*, which is supposed to guarantee the *ъ*, although the words cited are otherwise as heterogeneous as can be. In Latin the occurrence of *a*[7] in place of an expected full grade vowel or a zero grade in an equally heterogenous mass of words is cited as proof. In Germanic support for the *ъ* is sought mainly in the passive participles like Goth. *gibans* discussed above, where the full grade vowel representing IE *e* appears for the expected weak grade. In no two languages do the main domains of the supposed occurrence of *ъ* correspond, and also the supposed representation of the *ъ* varies so much both in the different languages and in some individual languages, that additional suspicion is cast upon the whole construction. In Sanskrit, it is claimed, *ъ* is represented by *i*, *u*, or *a*, in Greek by *α*,[8] *ε*, *ι*, or *υ*, in Latin by *a*, in Germanic by *e* or *u*, etc., etc.

When the existence of other IE sounds was established, it was done on the basis of isolated words which were not affected by analogies, and which corresponded completely in the various languages, or at least so nearly completely that the minor differences were easily explained and seen to have been irrelevant. This was particularly true of the vowels, for which ablaut is a complicating factor, so that they are an easy prey to analogy because of their apparent instability. Thus the existence of IE *a* could be proved only by such perfect correspondences as Skt. *ájra-ḥ* Gr. *ἀγρός* Lat. *ager* Goth. *akrs* < IE **aĝro-s*, or Skt. *ájā-mi* Gr. *ἄγω* Lat. *ago* < IE **agō*. Similarly IE *ā* was established by e.g. Skt. *mātā́* Gr. *μήτηρ* Lat. *māter* OHG *muoter* Lith. *motė̀* < IE **mātē(r)*.

In the case of *ъ*, however, there are no such convincing correspondences of individual words, and the evidence of the one morphological category on which reliance was placed, the passive participles like Gr. *πεπτός* or Goth. *gibans*, is spoiled by the fact that no one could possibly have here spotted a vowel different from IE *e* if it were not for the preconceived notion that *e* ought not to occur. There are,

[7] Even if we admit this *a* of many Latin words to have been a weak-grade vowel, this does not mean that it was necessarily a schwa secundum. Since *a* is the regular Latin representative of IE *ə*, it is at least as probable that this was found in the *e*-series under certain conditions, as that there were two weak-grade vowels.

[8] On the representation of *ъ* by Gr. *α* adherents of the schwa-secundum theory vary widely. Güntert (43 ff.) thinks *α* is the spontaneous development of *ъ*, Osthoff (MU 6.212) thought it appeared only after a liquid or nasal, while Hirt (80) doubts Osthoff, and rejects Güntert completely.

however, two instances of alleged *ϑ* in identical roots of two or more languages, and these are supposed to be incontrovertable evidence. The first of these is the Homeric Gr. *πίσυρες* 'four' beside Lat *quattuor*. But whatever is true of the latter, *πίσυρες* cannot possibly be identical. Its single intervocalic *σ* cannot come from *τϝ*, but only from *τ*,[9] so that *-υρ-* is the weak grade of the *-ϝορ-* found in *τέτορες* < **τετϝορες*, cf. Skt. *catvā́r-aḥ*. If *πίσυρες* had an *ϑ*, it must have had two successive weak grade vowels, and yet one of these must have been accented because the nom. pl. case ending *-es* was unaccented both in IE and Greek times. That a sporadic and isolated dialectic form should have suffered analogical extension to the first syllable of a weak grade that occurs nowhere else in Greek, is not to be thought of. Just because *πίσυρες* is old, it must have had a strong initial accented syllable to account for the weak grade of the second. It is clear that the *ι* of *πίσυρες* is the normal grade vowel IE *e*, and the change of *e* to Gr. *ι* is a purely Greek phenomenon, probably a phonetic change due to the assimilative influence of the *σ*, as in other instances of *ι* from IE *e* (see below). Lat. *quattuor* on the other hand has the original strong grade of the second syllable, so that conditions are as different from *πίσυρες* as they can be.

The second bit of supposedly first-class evidence for *ϑ* is Gr. *πίτνημι* 'spread out' beside *πετάννυμι* and Lat. *pateo* Osc. **patensíns**[10] 3. pl. subj. Again only the roots are identical and otherwise they vary widely. Even in their radical part the difference between the *ι* of *πίτνημι* and the *ε* of *πετάννυμι* is not explained by claiming the two divergent vowels as varying representatives of the same ubiquitous *ϑ*, for why and in what circumstances they diverge cannot even be guessed on the basis of these variants. The adjoining consonants are

[9] Cf. Schwyzer 272. A possible suggestion that **-tur-* as well as **kᵘϑ-* arose in oblique cases, as the gen. pl. **kᵘϑturṓm*, and made its way from there to *πίσυρες*, cannot be accepted because *πίσυρ-* is confined to the nominative and accusative plural, where **kᵘét-* is certainly original. It is true, however, that no gen. pl. of 'four' occurs in Homer, but, if **πισυρῶν* had existed, there would have been no reason for avoiding it, whereas *τεσσάρων* or *πεττάρων* would not fit into the meter.

[10] According to the usual interpretation of Osc. **patensíns** it would be derived from **patnō* or **patenō* (so Buck, Gramm. of Osc. and Umbr. 163). It would thus differ in its structure from both Gr. *πίτνημι* and Lat. *pateo*, although one would naturally suspect a close affinity to the latter. Perhaps Osc. **en** is here merely a way of spelling *ē* before *s*, which became possible because *n* before *s* apparently was no longer pronounced. In this case Osc. **patensíns** was identical with Lat. *patērent*.

the same, there is no chance of vowel assimilation in either case, and in both the root preceded a strong accented IE nasal suffix, Gr. *-νῡ-* having taken the place of IE *-neu-* as the strong form corresponding to the weak *-nu-*. As opposed to these nasal suffixes Lat. *pateo* of course ended in a formative descended from the IE *-eio-*. There is therefore no cogent reason for deriving its *a* from an IE *ъ*, because the verbs in *-eio-* most commonly had radical *o*, like Lat. *moneo*. Cf. Brugmann, Gr. 2.3.246 f. Much more probably therefore *pateo* either had retained the *e*-grade **pet-* or had an original *o*, and its *a* is analogical, whereas the *ι* of πίτνημι finds its explanation in the assumption of influence of verbs with present reduplication with *i*. Cf. LANG. 2.14 ff. But even if the structure of Gr. πίτνημι and πίσυρες were more nearly parallel with that of the compared Latin words, these two would comprise a very small fraction of the number of words in which an *ъ* has been scented for no other reason than that the quality of their radical vowels had not been explained otherwise to the satisfaction of those who believe in its existence.

No more successful are attempts to establish *ъ* by claiming that it affords a perfect explanation for certain phonetic phenomena that have been and may as well be explained otherwise. Thus it is maintained that the difference of the representation of the IE syllabic liquids and nasals in pre-vocalic position from that before consonants can only be explained by the assumption that e.g. *r̥* or *n̥* were reconstructed falsely before vowels, and that we should substitute *ъr* and *ъn*. Nevertheless, although we cannot know whether Brugmann was right in setting up *r̥r* with glide consonant e.g. in IE **pr̥ros* 'before' : Skt. *puráḥ purā́*, Av. *para*, Gr. πάρος, Goth. *faur faura*, as opposed to mere *r̥* e.g. in IE **dr̥k̂-* 'see' : Skt. *dr̥ṣṭá-ḥ* 'seen', Gr. δρακεῖν, etc., yet there is no reason why *r̥* before a vowel should not have developed differently than before a consonant. If the existence of *ъ* were otherwise proved, such instances might plausibly be taken as examples, but standing by themselves they cannot even afford a presumption in its favor. Hirt (76 f.) again reasons a-prioristically and accuses his opponents of a mechanical conception of language! In arguing for **sъmós* instead of **sm̥(m)ós* to account for Gr. ἀμός as the intermediate stage from **semós*, he says: 'Wir dürfen doch nicht voraussetzen, dass gr. ἀμός "einer", angeblich aus idg. **sm̥mós*, durch Anfügung von *o* an idg. *sm̥* entstanden sei, sondern es ist ein idg. **semós* anzusetzen. Fiel hier der Vokal aus, so konnte nur ein **smós* entstehen,

vgl. mit anderer Ableitung gr. *μία* "eins" aus **smía* aus **semíja*. Fiel er aber nicht aus, so konnte der Nasal oder die Liquida überhaupt nicht silbisch werden, sondern es konnte der Vokal nur irgendwie geschwächt werden.' He does not see that, no matter whether we assume **sъmós* or **sm̥(m)ós*, we cannot explain the Gr. *ἀμ-* on the basis of pure phonetics. Whatever were the conditions under which the process of the dropping of an unaccented IE vowel may have been arrested, those conditions were not fulfilled in *ἀμός* < IE **semós*. As Gr. *μία*, cited by Hirt himself, shows, these are the very circumstances under which the vowel dropped completely, with the result **sm-*. Whether we reconstruct **sъmós* or **sm̥(m)ós*, the Gr. *α* of *ἀμός* presupposes an early analogical transfer of its root grade, which was developed in ante-consonantal position, e.g. in **sm̥-pló-s* (via **sъm-pló-s* if you like) = Gr. *ἁ-πλό-ς* (cf. Lat. *sim-plex*) from original **sem-pló-s*. Now it is much more persuasive to assume that *ἀμός* represents an IE **sm̥(m)ós* made by adding the suffix *-o-* to *sm̥-* developed in **sm̥-pló-s*, than to manufacture for its benefit a new vowel grade, which itself must have arisen somewhere else under conditions which cannot be fixed.

Among all categories which can as well be explained otherwise, the one which perhaps presents the worst possible case for *ъ* is the one consisting of instances like Goth. *brukans* OHG *gi-brohhan* beside the infinitive Goth. *brikan* OHG *brehhan* with normal root grade. Hirt (81 f.) here assumes IE *ъ* as the source of Germanic *u* on the grounds that *r̥* becomes Germanic *ur* instead of *ru*, and a double representation is impossible. With the latter proposition one may heartily agree, at least where no conditions for the double representation are apparent, but it does seem a strange statement for adherents of the schwa-secundum theory, who assume double representation of IE unaccented *e* on a large scale without being able even to suggest conditions which can be taken seriously. The explanation of Goth. *brukans* for expected **burkans* is one of the most obvious analogies. The pre-Germanic **burkana-s* which represented IE **bhr̥gono-s* must have jarred the speakers of the language alongside of **brekana-n* = Goth. *brikan* OHG *brehhan* (cf. Lat. *frango frēgī*), and the influence of the infinitive and all finite verb forms caused the change of *ur* to *ru*, a case of 'Systemszwang'.

Gr. *αρ* beside *ρα* from IE *r̥* is to be explained analogically. The phonetic development is seen in *θρασύς* 'bold', (cf. Skt. *dhr̥ṣṇú-ḥ* for the *r̥*), but the co-existence of the old abstract noun *θέρσος* with nor-

mal grade beside *θράσος led to the change of the latter to θάρσος.[11] When Hirt (92) assumes a schwa secundum, it is merely one more example of the amazing tendency to think any deviations from the simplest formulation of phonetic laws as far as they concern vowels are explained by calling the refractory vowel a ъ.

One more instance in case of which the ъ is only one of several possibilities, without possessing any discernible advantage, is the interpretation of Sievers' law referred to above. Why *iy* and *uv* seem more intelligible as *ъy* and *ъw*[12] beside *y* and *v* than as due to vocalization of *y* and *w* after a heavy syllable is a mystery, which is dissipated in no way by the assumption of Güntert (128) that even in IE times this ъ before *i̯* or *u̯* plus vowel had become *i* and *u*, e.g. in Skt. *tuvám* 'thou' beside *tvám*, believing that IE **tъ-* had preceded **tu-*, but nevertheless had changed to the latter in pre-ethnic times. For the rest, I content myself with once more referring to the article of Edgerton cited above.

As indicated by Edgerton (264), the 'disturbing *a*-forms' of Latin, i.e. the occurrence of *a* where some other vowel is expected, are 'chiefly responsible for the schwa-secundum theory.' If it can be shown that these are either rhyme words to closely associated words or at least have rhyming roots, or are closely associated otherwise with words which have radical *a*, or are due to ablaut derailment, or are based on etymologies which are far from convincing, the principal support for the theory will be knocked out from under it; for the connecting bond of similar surroundings and other identical conditions which are the necessary basis for any convincing theory concerning a phonetic phenomenon, is conspicuous for its complete absence even in Latin standing by itself.

Want of space will prevent taking up all of the various etymologies mentioned by Güntert, many of which are rejected by Hirt or not deemed sufficiently persuasive to be worth while mentioning. I therefore confine myself mainly to the material accepted by the latter

[11] Hirt (loc. cit.) admits such possibilities, but claims that not all instances can be thus explained. It is difficult to follow him, since accentual variation in every instance led to the continuance of the parallel strong form to serve as a pattern to cause metathesis in the weak grade and thus to bring it into conformity with itself.

[12] Güntert (98) claims for ъ even the case-forms of Skt. *bhrū́-ḥ* gen. *bhruv-áḥ*, although OHG *brāwa* 'eyebrow' < **bhrēu̯ā* shows that the weak grade in its first stage must have had a schwa primum, and that *ū* in *bhrū́-ḥ* came form *əuə*.

in Idg. Gramm. 2.80 f., which may be considered as the nucleus which is considered as being of the greatest weight.

Hirt's first example is Lat. *aper* 'wild boar', Umbr. *abrof*, with *a* opposed to the *e* of OHG *ebur* 'boar'. This *a* has been explained long ago (Skutsch, Vollmöller Jahresber. 5.67) as due to the influence of the rhyming *caper* 'he-goat'. Güntert's objection (49) that the *a* of *aper* is Italic and not specifically Latin, has no weight whatsoever, for *caper*, as shown by Gr. καπρός and Germ. *Haber-geiss*, is itself Indo-European and therefore pre-Italic, and it is absurd to suppose that the influence of associated words to produce rhyme-words could not take place in the Italic period as well as at any other time. On the contrary, the fact that the *a* of *aper* is Italic nullifies Güntert's second objection, that *aper* designated the wild boar only, and could not therefore have been used together with *caper*. Evidently this limitation of Lat. *aper* was secondary, as opposed to Germ. *Eber*, which designates both the wild and tame animal. The possibility of the association between boar and he-goat is proved also, if proof is necessary, by the fact that Gr. καπρός = Lat. *caper* could (secondarily, as Güntert says) be transferred to designate the boar instead of the he-goat. There is therefore no valid reason to doubt the influence of **kapro-s* on **epro-s* to form Italic **apro-s*.

Another instance of two old rhyme-words of which one has an unexpected *a*, is the pair Lat. *pateo* 'am apparent' and *lateo* 'am hidden', which are designated by Ernout-Meillet s.v. *lateo* as being semantic opposites. That in spite of this it should be accidental that they differ only in their initial consonant, is not credible. Instead of relating the *a* of *pateo* to the equally irregular ι of Gr. πίτνημι as coming from IE *ə*, we must assume that at a very early date (at least Italic because of Osc. **patensíns**)[13] some parallel derivative from the root *lat-* imposed its *a* on *pat-* from original **pet-* (or **pot-* ?) as in Gr. πετάννυμι etc. In favor of this is the probability that **pet-* has also had its influence on *lat-*, for the latter corresponds to the Gr. λαθ-, of e.g. the aorist ἔ-λαθον, beside λᾱθ- in λήθω Dor. λᾰθω. Regardless of whether λαθ- comes from **lədh-* or **ləth-*, we are relieved of all difficulties in the correlation of the Gr. *θ* and the Lat. *t* if we assume that *pat-*, to which *lat-* gave its *a*, in turn gave its *t* to the latter in place of the original *dh* or *th*. Thus we find a second indication of the age of the process which resulted in the Latin rhyme words *pateo* and *lateo*.

A third pair of perfect rhyme words in which *ə* is supposed to have

[13] See note 10.

played a part consists of *rapio* and *capio*. The *a* of the latter is of course the ordinary IE *ə* as weak grade of the *ē* of *cēpi*, but the *a* of *rapio* is claimed as *ъ* because it is contrasted with the *e* of OBlg. *rjep* 'take off' and Gr. ἐρέπτομαι 'tear off'. Since *rapio* 'seize' and *capio* 'take' are partial synonyms and largely interchangeable, it is certainly easier to believe that *rapio* drew its *a* from the rhyming *capio*.

The Lat. *lapis*, gen. *lapidis*, 'stone' is rightly connected by Güntert with Gr. λέπας neut. 'bare rock', but since the Greek shows the ultima to have had the weak grade vowel *ə*, we cannot assume that also the first syllable had an IE unaccented vowel, in this case supposedly *ъ*. The Lat. *a* for *e* is due to association with the rhyming *capis*, gen. *capidis*, 'one-handled bowl', an association proved also by the transfer of *lapis*, an original *s*-stem, as shown by λέπας, to the *d*-declension of *capis*. The formal similarity of the two words was assisted by the fact that at least the original material out of which the *capis*, used in sacrificing, was made, must have been stone. Again two perfect rhyme words were the result.

A fifth pair of rhyme words for which *ъ* has been invoked is *crassus* 'fat' and *lassus* 'tired, lazy'. The latter, related to Goth. *lētan*, Engl. *let*, and Germ. *lassen*, certainly came by its *a* < IE *ə* honestly, for it is the ordinary *ə* which is the weak grade of IE *ē*. For *crassus* on the other hand an original *e* is to be assumed because of the zero grade from IE *r̥* in OBlg. *črъstъ* 'solid, massive' and Skt. *kr̥tsná-ḥ* 'entire'. The association between 'fat' and 'lazy' is so obvious that we must assume that *crassus* got its unexpected *a* from *lassus* rather than that the rhyme was accidental and its *a* was IE *ъ*.

What happened obviously in these five pairs of perfect rhyme words must have taken place also in a number of words with rhyming roots only, or words in which a strong semantic association assisted a more superficial formal similarity.

Lat. *flagrāre* 'to flame up, burn' is certainly related to Gr. φλέγειν, but the explanation of its *a* depends upon the assumed age of the change involved. If it really is as old as the schwa-secundum theory requires, it may be explained by the influence of a semantically related root which caused the two to be rhyming roots. The root of OHG *bacchu* 'I bake' < *ƀakkō* < IE **bhəg-nō* is in ablaut with that of Gr. φώγω 'roast', which is shown by φῴζω· καίω EM not to have differed much in meaning from φλέγειν originally. It is therefore probable that this **bhəg-* is responsible for **bhləg-* Lat. *flag-* instead of the original **bhleg-*. If, on the other hand, it is assumed that the *a* of

flagrāre is purely a Latin development, we must explain it by vowel assimilation as due to the *ā* of the following syllable, e.g. *flagrāre* < **flegrāre* or *flagrās* < **flegrās*.

Influence of semantically related roots upon each other does not necessarily create rhyming roots, for such an influence may affect only the vowels and not the final consonants. An obvious old instance of this is the Lat. *magnus*[14] 'large' as opposed to Gr. μέγας and μεγαλο- = Goth. *mikils*. Here the required association with the old IE **məkros* 'long' needs no proof other than mere mention. It is the word which became Gr. μακρός 'long', Lat. *macer* 'lean', and OHG *magar* 'lean', its *ə* being guaranteed by the normal grade **mēk-* in Gr. μῆκος 'length'. It is no objection that the *a* of Lat. *magnus* occurs also in MIr. *maighne* 'large', for this simply puts the process back to the Italo-Keltic period, which was as capable of producing word contaminations as any other period.

Undoubtedly *macer* was also the cause for *a* in place of expected *e* in the synonym *gracentes* (OLat. *crac-*), *gracilus*, *gracilis* 'lean', the zero grade of which is found in Skt. *kṛçá-ḥ* 'lean', Czech. *krs* 'verschrumpfter Baum', beside Lith. *kárszti* 'be old, grow old'. In such instances subsequent suffix variation may well have increased the gap between words originally more similar, e.g. *gracilis* could have taken the place of an older **gracer*, for which assimilation from **grecer* after the older *macer* is an unobjectionable assumption.

If Lat. *catīnus* 'deep vessel, bowl' is related to Gr. κοτύλη 'hollow, cup' and OEngl. *heden* 'Kochgeschirr' (Hirt 81), contamination with *patina* 'flat vessel, plate' is much more probable than derivation from **kʊt-*. That such contamination between the two words actually existed, is proved beyond doubt by the fem. *catīna*[15] πινάκιον (CGl 2.407.61), which certainly received its gender from *patina*.

The *a* of Lat. *frango* 'break' with participle *fractus*[16] 'broken' beside *e*

[14] Brugmann, Gr. 1[1].407, thought of the influence of Italic **mais* = Osc. *mais* on Lat. *magis*, and then on *magnus*, but even **mais* and **megis* were much farther apart phonetically than **meg-* and the **mak-* of **makro-s*. Cf. Osthoff MU 6.224, Güntert 52. In this connection it is interesting that Hitt. *mekkis* 'great' (:Gr. μέγας, Goth. *mikils*, etc.) owes its *kk* < IE *k* to contamination with this **mak-*, cf. Sturtevant, HG 83. Thus both Lat. *magnus* and Hitt. *mekkis* were affected by the same root.

[15] It cannot be determined whether these mutual contaminations also affected the quantity of the *i*, since the two words occur mainly in prose, and inability to prove **catĭna* or **patĭna* may be accidental.

[16] Hirt of course assumes *ʊ* for *frango* and *fractus* as well as for Goth. *brukans*.

in Goth. *brikan* OHG *brehhan* 'break' is due either to secondary ablaut (see below) or to an early contamination with the root **u̯rēg- *u̯rəg-* in Gr. ῥήγνυμι 'break' opposed to the aor. pass. ἐρράγην < *ἐ-ϝραγην, a root shown to be of IE age by Engl. *wreck* and other Germanic words.

Popular etymology is assumed by Walde (s. v.) for *labrum* or *labium* 'lip' with *a*, beside *e* in OEngl. *lepur lippa*, Germ. *Lippe*, Engl. *lip*, etc. While direct derivation from *lambo* 'lick' is impossible because the lip does not lick, it may well be that the *e* became *a* through secondary association with *lambo* due to similarity of sound. Güntert (54) does not understand how these two statements in Walde can be harmonized, but a little reflection on the nature of popular etymology and other much more irrational similar associations should have given the answer. When *asparagus* became *sparrow-grass* in popular English, it was due altogether to the suggestiveness of its sound, and there was no rational connection with the sparrow; or if Germ. *Maul-tier* or *Maul-esel* 'mule', borrowed from the Lat. *mūlus*, is associated with *Maul* 'mouth', there is again no rational basis, and sound alone accounts for it. The change of **leb-* to *lab-* by association with *lambo* 'lick' is therefore a much better explanation of *labrum* than the assumption of an IE *ъ*.

A very probable case of popular etymology is the Lat. *latus* 'side' beside OIr. *leth* 'side'. It was associated with *lātus* 'wide', with consequent assimilation of its vowel quality. Walde s.v. *latus* suggested real affinity, but this is not probable in view of the *e* of *leth*.

A barely possible instance of popular etymology is the Lat. *lacertus* 'muscle of upper arm, upper arm' : Gr. λέκρανα· τοὺς ἀγκῶνας Hesych. If this equation really stands, influence of *lacer*, *lacerāre* on the original **lek-* is more probable than the assumption of an *ъ*, for in the experience of ruffians and boxers it is the devastating effects of the arm which mutilates the opponent. There are, however, almost decisive reasons against this combination, so that Ernout-Meillet s.v. refrain from even mentioning it. The first objection is that λέκρανα and *lacertus* end in suffixes which have nothing in common, and yet are both semantically isolated words which show no reason for suffix assimilation to other words. Still worse is the obvious resemblance of λέκρανα to ὠλέκρανον 'head or point of elbow', which is related to ὠλένη 'elbow'. Clearly, then, λέκρανα is either a mere ms. corruption of ὠλέκρανα, or else is in ablaut with ὀλέκρανα, of which ὠλ- is the lengthened grade, cf. Brugmann, Ber. d. sächs. Ges. 1901.34. It will be better to build nothing whatever on the assumed connection between *lacertus* and λέκρανα.

The case of the comparison of Lat. *caterva* 'band, troop' with OIr. *cethern* 'troop' is similar to that of *lacertus*. If it holds, we may assume that the Irish word suffered contamination by popular etymology, perhaps with *cethir* 'four' (as a quarter of a larger unit in the military organization?). Nevertheless the comparison is definitely rejected in Walde-Hoffmann for a more convincing etymology of *caterva*.

In the discussion of *flagrāre* the question was raised whether the first *a* might have come from *e* through vowel assimilation. Although this is merely a suggestion for this particular word, I am sure that it is the correct ultimate explanation for another much discussed word, sc. Lat. *quattuor* opposed to Gr. τέτταρες. We have seen that the apparent support for *ъ* in Gr. πίσυρες beside Lat. *quattuor* is certainly illusive.[17] It is in fact very dubious to push back the *a* of *quattuor* to any period earlier than that of Latin itself; for Osc. *petora* 'four' (n. pl.) and *petiropert* 'quater' beside Gr. τέσσαρες etc. would lead to the conclusion that also the primitive Italic had **k^{u}et-* and not **k^{u}ъt-*. But as soon as one tries to explain the *a* on the basis of Latin alone, the possibilities are limited. Brugmann, IF 28.370, suggested that it might have come from the ordinal *quartus*, and although perhaps Güntert (48) is right in thinking that *quartus* was a later development from **quortus*, as suggested by the Praenest. *Quorta*, yet this does not mean that the *a* of *quattuor* could not be explained by that of *quartus* if the latter itself can be accounted for. This can be done by assuming influence of *quadrātus*, which is shown to have been an IE word by the completely corresponding Toch. *śtwarāts*. This *quadrātus* arose by vowel assimilation from **quedrātus*, then the original **quortus* became *quartus* after the closely connected *quadrātus*, and, finally, the influence of *quartus* caused the change of **quettuor* to *quattuor*.

Vowel assimilation also accounts for Lat. *castrāre* 'cut off' in place of the expected **cestrāre* < IE **k̑es-*, cf. Skt. *çastrá-m* 'knife', and, for the *e*, e.g. Gr. κεάζω < **κεσ-* 'split', OIr. *ceir* 'spear'.

A number of unexpected occurrences of Latin *a* are due to secondary

[17] Mere mention will suffice for Czech. *čtyři* and Pol. *cztery* 'four' (Osthoff, MU 6.212), for which derivation from **čъtyr-* is very improbable because of the regular OBlg. *četyre*. Cf. Brugmann, IF 28.370, who explains the Czech and Polish forms by dialectic loss of *e*. Güntert (47) counters with the surprising and dogmatic statement that the same change could not have taken place independently in the two languages. One need refer only to the independent and yet parallel development of Umlaut in English and German or to the diphthongization of *ī* and *ū* in both.

ablaut or ablaut derailment. Since the speakers of the language ordinarily had no idea as to whether a long *ē* was the normal grade (*fēcī* beside *facio*) or the lengthened grade (*sēdī* beside *sedeo*), it is a foregone conclusion that in those instances in which *ē* was the lengthened *e*, the speaker, according to the more common pattern, would often substitute *a* for *e* in the forms with short vowel. Thus, supposing that Güntert (55) is right in connecting Lat. *lapit* 'dolore afficit' with Gr. λέπω 'peel' (Wood, Cl. Phil. 2.82), we have only to assume that the perfect of *lapit* was **lēpit*, and the *a* for the *e* automatically followed the proportion *facit* : *fēcit* = *lapit* : **lēpit*. The fact that *lapit* was a rhyme word to *capit* beside *cēpit* suggests that also sound similarity played a part. The creation of an ablaut grade *a* < *ə* was particularly apt to occur in Latin because the shortening of vowels of final syllables had eliminated all lengthened grades in the one place in which they could have been perceived as ear-marks of an inflectional form, and thus be associated with the normal grade in *ĕ*; e.g., *patĕr* : Gr. πατήρ, πατρός, etc.

Nevertheless this facilitating condition was not necessary for ablaut derailment, and we can assume that Lat. *a* stands for *ə* secondarily created beside a lengthened grade *ē* whenever both are in existence somewhere. Thus Lat. *aser*[18] 'blood' exists not only beside Gr. ἔαρ, but also beside ἦαρ· αἷμα Hesych., and *aser* does not have a schwa secundum, but an ordinary schwa primum created beside the long **ēs-* of ἦαρ. Lat. *nactus sum* : Gr. ἐνεγκεῖν 'carry' stands beside Goth. *nēhv* 'near' (cf. Walde-Pokorny 129) and again shows a *ə* in secondary ablaut with long *ē*. The rather unsatisfactory connection of Lat. *trabs* 'beam' with *Trebōnius* and Umbr. *trebeit* 'versatur', OIr. *atreba* 'dwells' would be a similar case, for Güntert and Hirt omit any reference to Osc. **trííbarakavúm** 'build', which shows the lengthened **trēb-*,[19] beside the analogical schwa primum in *trabs*. Lat. *lanx* 'plate' exists not only beside Gr. λέκος ahd λεκάνη, but also beside λήκυθος 'bottle', and again Lat. *a* is IE *ə* in secondary ablaut with *ē*. Possibly also Lat. *frango* with *a* beside *e* in Goth. *brikan* 'break' etc., instead of being due to contamination with another root, as suggested above, received its *a* through ablaut derailment, so that the participle *fractus* beside the perfect *frēgi* took the place of the original **frectos* according to pat-

[18] Cf. Walde-Hofmann s.v. *aser*.

[19] Only the Oscan word is close enough to *trabs* semantically to make the comparison cogent. If the others are eliminated *trab-* is the regular weak grade with *ə* corresponding to the normal **trēb-* in **trííbarakavúm.**

terns like *captus* : *cēpi*, and the *a* then went from *fractus* to the present *frangere*. Finally, for Lat. *sacēna* 'ax of pontifex' and *saxum* 'jagged rock' beside *secāre* 'cut', Brugmann, IF 28.370, called attention to the existence of the parallel **sē(i)k-* presupposed by OBlg. *sěką* 'cut', Lith. *sỹkis* 'blow', and Lat. *sīca* 'dagger', and again the most rational explanation is the assumption of a secondary ablaut grade **sək-* beside **sēk-* < **sēik-*. Güntert (51) can say nothing against this except to make the damaging admission that similar assumptions could be made in all similar cases, and a categorical denial that **sīk-* in *sīca* can be connected with *secāre* and *sacēna*.

A final possibility to account for Lat. *a* in place of expected *ĕ* is that suggested by the laws of Latin vowel weakening in originally unaccented syllables, which, supplemented by many analogies, led to the continuance of an original *a* in the simplex, as opposed to the compounds that showed the *e* which was the product of vowel weakening in closed syllables, as in *patior passus* beside *per-petior per-pessus*. Because of the imitation of such patterns original **gredior* changed to *gradior* as opposed to its compounds like *ingredior*. One may put it into the form of a proportional analogy: *patior* : *-petior* = *gradior* : *-gredior*. That *e* is the original vowel of *gradior* is proved not only by its cognates, cited e.g. by Hirt, Goth. *gribs* 'step' and OBlg. *gręda* 'come', but also by its participle *gressus* instead of **grassus*. Apparently the latter was too close to the abstract *gressus* gen. *gressūs* to allow it to follow the vocalism of the present. At the same time the existence of *gressus* instead of **grassus* shows the futility of resorting to the ever handy *ъ* as an explanation, for there is less reason for an originally weak vowel grade in *gradior* than in *gressus*.

The words of all IE languages with radical nasal have been mentioned above. They form a group which per se can be interpreted as from IE *ṇ(n)* or *m̥(m)* at least as well as from *ъn* and *ъm*, and the explanation adopted is a mere corollary to whatever theory one holds concerning the reduced vowel grade. Only the much discussed *canis* 'dog' for expected **cunis* calls for special consideration. The latter, with *k̑un-* as the weak grade, is opposed to the lengthened *o*-grade **k̑(u)u̯ōn* in the nominatives Skt. *çvā́* and Gr. *κύων* (but gen. *çúnaḥ* *κυνός*, with weak grade). Now Hirt (86) imagined that he found a definitive solution for the *a* of *canis* by assuming that it came from an IE **k̑u̯ъnés* with an IE change of *u̯ъ* to *a*. It will be seen at once that this is no explanation, because it is made purely ad rem. There is no indication that there was such an IE sound change, and the genitives Gr. *κυνός* and Skt. *çúnaḥ* show clearly that IE **k̑un-* results from the

very conditions for which Hirt proposes **kuə̯n-*, not to speak of the gratuitous assumption that there was any IE *k̂uə̯n-* at all. In view also of the failure of any other Latin word to afford a probable presumption in favor of any IE *ə*, it is more probable that here also Latin was the innovator, and that somehow or other an original **k̂un-* became Lat. *can-* through associative interference of other words. The reason why we cannot find a convincing pattern for the *a* is the age of the change, which obliterated the intermediate steps and eliminated the inducing form. One can only suggest a number of possibilities to show how it might have taken place, without feeling certain that the real cause has not been obliterated altogether. Thus **cunis* may have been contaminated with an old *i*-stem derivative from the root of *gannio* 'yelp, bark': e.g. **ganis* 'yelper, barker' i.e. 'dog' + **cunis* = *canis*. Or the *a* may have been due to the influence of *catulus* 'puppy, whelp'. If the forms of *canis* and *catulus* now diverge so widely that this seems dubious, it may be that at an earlier time derivatives which are now lost played the important part, e.g. **cunolos* 'young dog, puppy' would certainly have been close enough to its equivalent **catolos* to result in contamination and the formation of **canolos*, and after the latter **cunis* became *canis*. Much more dubious is a third possibility. H. G. Tucker, Concise Et. Dict. s.v., would derive *canis* from *cano* 'sing', but it is not probable that the resemblance of the Latin words to the IE **k̂un-* is altogether accidental, and there must have been some connection. However, it may have been a case of popular etymology, i.e. secondary association of **cunis* with *cano* and consequent change of *u* to *a*. In this way we would overcome the objection to direct derivation from 'sing', that no one could have thought of the howling of the dog as being singing, even granting that occasionally the term 'sing' is humorously applied to it.

I have ignored all other examples of alleged Lat. *a* < IE *ə* because the etymologies on which these comparisons are based are not probable enough to make them a factor in the decision, and some are admitted to be dubious by Hirt himself, e.g. Lat. *satelles* 'satellite' : Gr. ἕταρος, for which he leans to derivation from the Etruscan, an explanation accepted also in Walde. Similarly he suggests two alternatives for Lat. *grāmen* 'grass'. For us the latter (: Gr. γράω and Skt. *grásati*) is the only one of the two which is worthy of consideration, and this means an IE *a* instead of *ə*.

It thus appears that in every single instance of a certain or probable etymology the assumed IE *ə* as source of Lat. *a* can be explained other-

wise without improbable assumptions, and Latin affords no presumption whatever in favor of the existence of an IE schwa secundum.

The alleged Greek examples can be discussed much more briefly because, with a few exceptions, they can be classified into several distinct groups which show certain common phonetic or morphological conditions, and because they have been freely discussed also from other angles besides that of the possibility of the existence of *ъ*. In Hirt's presentation the first group has 'regelrechtes *ε*' : *πεκτός*, *πετών*, *τεσσάρων*, *δεδόρκαμεν*. As was seen above, the idea that *ъ* is to be sought in the *e* of such words in Greek and elsewhere is merely an improbable deduction from the position of the accent on the syllable following. It is due to a conception of language which is so 'mechanical' that the mental associations which are at the basis of all speech even when it follows established patterns have practically no place in it in actual practice, although usually not denied in theory. In two of the examples, moreover, ablaut conditions contradict the assumption that *ε* was a reduced vowel. In *τεσσάρων* the accent on the penult is due to the law which limits the accent to the last two syllables when the ultima was long. That the original accentuation was **τέσσαρων* is shown on the one hand by the nom. *τέσσαρες*, on the other hand by the weak grade *-αρ-* of the penult, which precludes its being originally accented. It is possible that the very earliest accentuation is represented by **τεσσαρῶν*, but even this would not fit into the hypothesis that *ъ* is found in a syllable immediately preceding the accent. In *δεδόρκαμεν* the *o*-grade suggests that the accentuation may not be older than the law of the three syllables. It was originally **δέ-δορκαμεν*, as in the Skt. middle *dá-dr̥çē*.

In many other instances Güntert, Hirt, and others assume that *ъ* became Gr. *ι*, in fact every unexpected *ι* is thus 'explained' even though other conditions of the change are plainly in evidence. Thus the vowel assimilation *ε-ι* to *ι-ι* (Schwyzer 226) is so clear in many instances that even Güntert (22 ff.) cannot escape the conclusion that the following *ι* played a part, e.g., in *ἴσθι* < **ἐσ-θι*,[20] *λικριφίς* beside *λέχριος*, *χἰλιοι*[21] <

[20] This reconstructed form is of course my own, since others insist on original **z-dhi* = Av. *zdī*. To me the fact that the strong grade *ἐσ-* took the place of the original weak grade **σ-* also in the dual and plural indicative (excepting the 3. pl.) indicates that in all structurally transparent forms the strong *ἐσ-* was adopted. It is no stranger that **ἔσθι* took the place of **z-dhi* than that **s-te* became *ἐστέ*. It is to be observed also that the accent on the penult favors original **es-dhi*, although this is not decisive, since the tendency of verbs to retract their accent could have been at work here just as well as in *ἴσθι* : *οἶδα*.

[21] For *χἰλιοι* Wackernagel, IF 25.329, suggests a phonetic change of *ηλι* to *ιλι*, for which he also finds other evidence.

*χεσλιοι beside dialectic χείλιοι and χήλιοι, Ion. ἰστίη beside ἑστία, ἴθρις beside ἑθρίς and Skt. *vádhri-ḥ*, and, with ι̯, χθιζός < *χθεσδι̯ος : χθές, ῥίζα Lesb. βρίσδα < *μ̥redi̯a : Lat. *rādix*, Goth. *waurts*, etc. It is difficult to see what advantage there is in assuming ἴθρις is assimilated from *μ̥ʊdhri-s* rather than from *μ̥edhri-s*, in spite of the fact that the latter is evidenced by both Skt. *vádhri-ḥ* and Gr. ἑθρίς, and in spite of the accentuation of ἴθρις on the initial syllable, which coincides with the Skt. accent and precludes an original weak grade vowel. Or why is χτίλιοι more intelligible from *ĝhʊs- than from *ĝhes-, which is established not only by Skt. *sa-hásra-m*, but by the parallel Gr. χείλιοι and χήλιοι as well as by the accentuation of all forms compared, including χτίλιοι itself? And how can Güntert explain the existence of two weak grade vowels in his assumed *ĝhʊsli-, even though we have no reason whatever to believe that the accent once rested upon the ultima?

In another group the assimilative effect of a following σ[22] (Schwyzer 351) is clear, a process easily understood phonetically, when we consider how much closer the tongue articulation of *s* is to *i* than to *e*. Thus[23] Hom. πίσυρες <*πέσυρες beside τέσσαρες, ῥίον 'peak of mountain' < *μ̥resom : Lith. *virszùs* 'the upper', διφθέρᾱ < *διψτερᾱ : δέψω, cf. Fick Vergl. WB 14.455, Bechtel Hauptprobl. 113. Either as an alternative explanation or as a contributing factor influence of *s* or *z* may be assumed for χθιζός, ῥίζα, ἴσθι, χτίλιοι.

For the group πίτνημι : πετάννυμι beside πτίπτω, κίρνημι : κεράννυμι beside κιγκρᾱ̃ and πίλναμαι : πελάζω beside πίμπλημι, influence of associated forms with present reduplications in ι were the deciding factor. Analogically, the formally and semantically related κρίμνημι : κρεμάννῡμι, σκίδνημι : σκεδάννῡμι, ὀριγνάομαι : *ὀρέγνῡμι. Cf. LANG. 2.14 ff.[24]

Only a few cases of Gr. ι from IE *e* do not come under the three categories discussed, but such as they are, they are not helped by

[22] It is of no advantage to think of a sonant *z* in some cases, as did Thurneysen, KZ 30.351 ff. The theory has been generally rejected.

[23] I refrain from citing Gr. κρῑθή 'barley' because it is etymologically ambiguous. If it is cognate with Lat. *hordeum* and OHG *gersta* 'barley' it arose somehow from **gherzdā* and would belong here. It may, however, go with OEngl. *grātan* and Engl. *groats* (Uhlenbeck KZ 40.555), and come from IE **ghrīdh-*. In either case Grassmann's Law accounts for Gr. κ- instead of χ-, and there is no reason for definitely adopting the latter alternative, which is less enlightening semantically, and has its own phonetic difficulty in the lack of correspondence of the final consonants in the compared words.

[24] Holger Pedersen, IF 2.293, briefly suggested the influence of the ι of ἵστημι, τίθημι, πίμπλημι, etc., on σκίδνημι beside the aorist ἐσκέδασα.

the doctrine of the schwa secundum. To derive Gr. ἵππος[25] beside Lat. *equos* and Skt. *áçva-ḥ* from IE **ъk̑ṷo-s* is impossible without throwing overboard the fundamental principles of comparison; for the vocalism of the Sanskrit and Latin as well as the accent of the Sanskrit and Greek all combine to establish IE **ék̑ṷo-s*. If ἵππος comes from **ъk̑ṷo-s* (see above) it is necessary to make the impossible assumption that **ék̑ṷo-s* became **ek̑ṷó-s*, then **ъk̑ṷó-s*, and that then the accent shifted back to its original place without the slightest reason for so doing.

In other cases the assumption of an ι beside ε is due to false etymologies. Thus Gr. κρίκε 'es knarrte' (Hirt 80) is not the same word as κρέκω, but it has IE *i*, cf. Boisacq s.v. κριγή.

It thus appears that in all authentic instances of Gr. ι beside ε those who resort to the ъ either have to admit secondary explanations which can as well explain the phenomenon without assistance of the

[25] Of the various explanations offered for the much-discussed ι of ἵππος, two merit serious consideration, without being so convincing that they can be definitely accepted. Brugmann, IF 22.202, suggested that the wandering ἵννος 'colt of mule', which is the Lat. *hinnus*, first infected **ἔκκος* to become ἵκκος, and that the ι went from this to **ἵππος* (still found in compounds like Ἀρίστ-ιππος). Although **ἔκκος* and ἵννος before their contamination were not so much alike as to suggest similarity of sound as a contributing factor, the semantic association was perhaps strong enough to account for the change. The second possibility is that suggested by Meillet, Mem. 9.136 f.: ἵππος < **ἰκϝος* dissimilated from **ὑκϝος*, and the latter with υ like νύξ. However, the assumed change **ek̑ṷ-* to **uk̑ṷ-* is dubious. Of the other alleged examples νύξ (AJP 46.56 f.) is best explained as receiving its υ from a contrasting **λύξ* 'light, daylight': Lat. *lūx*, and the υ of ὄρτυξ and ὄνυξ may be due to suffix variation. The apparent advantage of explaining the rough breathing of ἵππος by the older stage **u-*, which according to the theory must have existed at the time every initial υ became aspirated, is illusive because compounds like Ἀρίστ-ιππος, just mentioned, show the aspiration of ἵππος to have been much younger than that of initial υ. If we accept Meillet's explanation in spite of these difficulties, as does Güntert (25), there is no advantage in assuming an original **ъk̑ṷo-s* instead of **ek̑ṷo-s*. On the contrary, ἵππος, accented like Skt. *áçva-ḥ*, must have had a full grade vowel from IE times. Cf. Holger Pedersen, KZ 38.417.

For the rough breathing of ἵππος I suggest the following explanation, which takes for its starting-point the fact that in Homeric times horses were used only war, and that in the warfare of those days horses were not ridden, but attached to chariots (Gr. ἅρματα), so that ἀφ' ἵππων and ἐφ' ἵππων alternated with ἀφ' ἅρματος and ἐφ' ἅρματος in the sense 'from the chariot' and 'on the chariot'. Occasionally ἵπποι and ἅρματα were even combined into one phrase, as in Λ 198 ἔν θ' ἵπποισι καὶ ἅρμασι. Under these circumstances there is no difficulty in assuming that the rough breathing slipped over to ἵππος from ἅρμα.

schwa-secundum theory, or assume ə where ablaut and accentual conditions make the occurrence of an original weak grade vowel impossible, or at least where there are no positive reasons for assuming its existence, and that in ἵππος, the ι of which is hard to explain satisfactorily otherwise, the assumption of a ə fares even worse. Surely, Greek ι beside *e* offers no plausible support for the existence of IE ə. On the contrary, it furnishes damaging evidence against it because all alleged nominal examples except χθιζός accent the ι. Since ə is assumed as a weak grade vowel, it must have been unaccented when first developed, so that for ι < ə it would be necessary to make the impossible assumption that in every instance except one[26] the accent shifted secondarily to the originally unaccented syllable.

It is assumed by Hirt (98 ff.) and Güntert (32 ff.) that another variety of the schwa secundum is represented in Greek by υ. All of the material cited is dubious. For the analogical υ of νύξ 'night' beside the o of Lat. *nox*, Goth. *nahts*, etc., see note 25. Cases like μύλη 'mill' beside ἀλέω 'grind' < **mḷ-*, or φύλλον 'leaf' beside Lat. *folium* are easier to explain in the old way as containing IE phonetic variants of *ḷ* due to the contiguous sounds, than by the invention of a special ablaut grade for their benefit.

All in all it is clear that the alleged evidence for ə both in Latin and in Greek is either dubious or impossible.[27] It is also clear that its existence is far from proved by the still more dubious evidence from other languages, particularly since Edgerton has shown that the alleged support from Sanskrit is also unreliable. We may conclude that if there was such a thing as a schwa secundum, proof for its existence lies in the future, and that a totally different line of reasoning must be followed to establish it.

[26] This statement ignores two words (ἱστίη and διφθέρᾱ) in case of which accentuation of the ι is prevented by the long ultima. These are consequently of no value in determining the original accentuation of syllables with alleged ə.

I suggest that probably χθιζός was originally accented χθίζος, and shifted its accent to the ultima through analogy, perhaps after πρωϊζός 'on the day before yesterday'. Thus the change ε to ι, in as far as it was a phonetic change (it was analogical in the group πίτνημι), was conditioned on the vowel being accented, which is in line with the musical nature of the Greek accent, since ι has a higher pitch than ε.

[27] I refer only to ə as a distinct phoneme in opposition to ə. That the latter may have been pronounced differently in different surroundings, is self-evident; but, since no diverging development can be proved, there is no evidence for two phonemes instead of one. One may compare the German unaccented *e*, with varying pronunciation, but phonemically, i.e. psychologically, a unit.

MISCELLANEA

A NEAPOLITAN NOTE

Zingarelli[1] correctly gives *pernacchio* as a borrowing from Neapolitan. However, he offers as its source a modern Umbrian *perna* 'gamba, coscia' from the Latin *perna* 'ham'. Of his two definitions of the word, 'scoreggia, suono vocale imitativo per dileggio', only the second is exact for the modern period, since the first is archaic. In Neapolitan the word is masculine, but Panzini[2] gives only a feminine form, *pernacchia.* He glosses it correctly: 'volgare parola: il suono di Barbariccia, trombettiere (Inf., XXI, 139) ma fatto con la bocca. Il superlativo plebeo dei fischi. Voce di origine napoletana. Nel gergo della Guerra la *pistola mitragliatrice.*' The feminine gender evidently is due to analogy.

The *pernacchio* is the so-called 'Bronx cheer', which was undoubtedly borrowed from the Italians, especially those from around Naples, among whom it has long been a vigorous, but vulgar, indication of derision and mockery. The only other Southern Italian dialect in which I find it recorded is Northeastern Calabrian. It is given by Rohlfs[3] as occurring in the commune of Nocara. His entry is *pərnácchiə* 'baia, burla (facendo un pernacchio)'. This form is a borrowing from Neapolitan and is pronounced in the same way.

The oldest form in Neapolitan, however, is *'o vernacchio* [ɣərnákkj̯ə], pl. *'e bbernacchie* [bbərnákkj̯ə], and its original meaning is the same as for *'o pircto* [pírətə], pl. *'e pperete* [ppé̞rətə] < Lat. *pēdĭtum,* which still retains the Latin meaning in Modern Neapolitan. With characteristic Neapolitan delight for the grotesque we find Nicolò Capasso,[4] 1671–1745, professor of law in the University of Naples, writing a sonnet sequence on the matter. Lo Vernacchio[5] is also the title of a disparaging pamphlet written against the Abate Galiani's Del Dialetto Napoletano.

[1] N. Zingarelli, Vocabolario della Lingua Italiana, Milano, 1922.
[2] A. Panzini, Dizionario Moderno, Milano, 1931.
[3] G. Rohlfs, Dizionario Dialettale delle Tre Calabrie (not yet complete).
[4] G. M. Porcelli, Tutti i Poemi in Lingua Napoletana 24, 41–4 (Napoli, 1789).
[5] L. Serio, Lo Vernacchio. The Porcelli Collection 28. 1–45 (Napoli, 1789).

By the end of the eighteenth century *vernacchio* meant primarily 'rumore, che si fa colla bocca simile al peto per disprezzo di alcuno', as indicated in the Vocabolario to the Porcelli collection Tutti i Poemi in Lingua Napoletana. This change from a most vulgar to a less vulgar meaning is not unusual, cf. English *fizzle*. Neither *vernacchio* nor *pireto* are found in Puoti,[6] evidently for reasons of delicacy, but D'Ambra[7] gives the form with both meanings. The form *pernacchio* is found for the first time in Andreoli[8] who gives the less vulgar meaning as the current one. The most modern dictionary[9] at my disposal omits both words. There can be no doubt that *'o pernacchio*, pl. *'e ppernacchie*, owes its first sound to analogy with *pireto*.

Heraeus,[10] in his note to 'vernaculus non vernaclus', states that it is a 'Im Rom. nicht erhaltenes Wort'. I see no difficulty either phonetically or semantically in deriving *vernacchio* from *vernaclu-*. The strengthening of the initial consonant in the plural *'e bbernacchie* shows that the singular form is derived from a neuter noun or adjective. Most probably, the adjective was used currently with *pēdĭtum* and later separately as a noun. The restriction of meaning from 'pertaining to household slaves', to 'a definite vulgar physical manifestation' is quite comprehensible. It was quite natural for *vernacchio* to acquire a secondary meaning, which then became exclusive, since *pireto* expressed adequately the primary meaning.

LOUIS F. SOLANO

GEMINATE CONSONANT CLUSTERS IN MUSKOGEE

In his recent paper, 'The Phonemic Interpretation of Long Consonants' (LANG. 13.1–10), Swadesh gives (8) some illustrative material from Muskogee.[1] As source material on the language he had at that time only Loughridge's Dictionary of Muskokee and English[2]

[6] B. Puoti, Vocabolario domestico Napoletano e Toscano, 2d ed., Napoli, 1850.

[7] R. D'Ambra, Vocabolario Napoletano-Toscano, Napoli, 1873.

[8] R. Andreoli, Vocabolario Napoletano-Italiano, Torino, 1887.

[9] G. Ceraso, Vocabolario Napoletano-Italiano, Napoli. 1910.

[10] W. Heraeus, Die Appendix Probi, Leipzig, 1899.

[1] The Muskogee (sometimes spelled Muskokee) or Creek language is still spoken by approximately four thousand Creek Indians now living in central eastern Oklahoma, mostly within the confines of what was known as the 'Creek Nation' in Indian Territory days.

[2] Loughridge, Rev. R. M., Dictionary of Muskokee and English, last edition, The Westminster Press, Philadelphia, 1914.

and some very brief notes of mine.[3] Loughridge, a missionary to the Creek Indians nearly a century ago, cannot be expected to have given us phonemically written material,[4] and my own notes were entirely too sketchy to establish definitely the phonemic structure of the language, particularly that phase taken up by Swadesh. Recent field-work[5] on the language has thrown new light on the problem of geminate consonant clusters of such a nature as to require a re-interpretation of the facts.

The problem taken up by Swadesh is this: Is that consonant cluster which seems to be [tc] on the phonetic plane to be interpreted as *tc* or as *cc* on the phonemic plane? He concludes, on the basis of the evidence that was available at that time, that the former is the correct interpretation and gives the following as explanation:

> In Muskogee, *t* and *c* are articulated in virtually the same position, so there is nothing in the articulating position to indicate whether the first part of *tc* in *totci·nin* 'three' is *t* or *c*. Furthermore, there is no contrasting *cc* that I have been able to find. However, it is a general feature of Muskogee consonant clusters, including geminate clusters, that they do not involve special variants for either of the component phonemes. If, then, there were a *cc*, one would expect an affricative pronunciation of the first *c* as is normal in syllabic final position, e.g., *icki* 'mother'. The fact that there is no affrication in the first part of *tc* is therefore good pattern indication that it is *t* rather than *c*.

The new evidence, however, seems sufficient to establish the fact that [tc] is after all to be phonemically interpreted as a geminate cluster, *cc*. This evidence is as follows:

(1) Phonetic:

(a) Articulatory position: More careful listening has shown that *t*

[3] In the fall of 1934, while engaged in field-work on the Natchez language (under the auspices of the Committee on Research in American Native Languages), I obtained some brief notes on Muskogee, primarily to be used as a medium through which to translate Natchez into English.

[4] With the help of a number of educated Creeks, Loughridge did, however, work out an alphabet for writing Muskogee which is still used today by many Creeks who write their own language. Even from the phonemic point of view, his dictionary (written in this alphabet) is far superior to much work done in his day along similar lines.

[5] Undertaken in 1936–7. I wish to acknowledge my indebtedness to the Institute of Human Relations of Yale University for financial support in this work.

and *c* are not articulated in the same position,[6] but that the former has the alveolar position while the latter has the palatal position.[7] Although *c* is the only affricate in the language and therefore stands isolated in this respect, it can, however, be classified in all other respects with the stops, *p*, *t*, and *k*. Now if we compare the pronunciation of the groups *tp*, *tt*, and *tk* with the [tc] group, we find that the *t* has the alveolar position in all the groups except the last, where it has the palatal position. That this is not due to assimilation is indicated by the fact that before *y*, which is likewise a palatal, *t* retains its alveolar position. Therefore in its articulatory position the *t* in [tc] is to be compared with *c* rather than with *t*.

(b) Syllabic treatment: All double consonant clusters, including geminates, are ambisyllabic but the treatment of the syllabic final in a geminate cluster differs from the treatment of the syllabic final in a heterophonous cluster. In the case of a heterophonous cluster of stops the syllabic final is aspirated, thus: [tí-ˈní·tʻ-kí·] 'thunder'.[8] In the case of a geminate cluster of stops, however, the second stop is not rearticulated and the syllabic final is therefore not aspirated. On the contrary, the aspiration is inhibited throughout the gemination to be released after the syllabic initial, thus: [ˈnít-tʻá·] 'day'. Now if we compare the syllabic treatment of our [tc] group first with a *tk* group and then with a *tt* group, we find that it is comparable to the latter rather than to the former, thus: [tót-ˈcʻî·nìn] 'three'; phonemically: *tóc'cî·nìn*.

(c) A phonemic group *tc*, which would provide us with a direct contrast to *cc* ([tc]), has not as yet been found to occur in my material (which is quite extensive), and it is quite possible that no such group exists. However, if it did, the *t* in such a group would be adequately distinguished from the pseudo *t* in *cc* ([tc]) by its alveolar articulatory

[6] I was responsible for Swadesh's statement that '*t* and *c* are articulated in virtually the same position'.

[7] This was checked visually as well as auditorially. My informant's reaction could be nicely checked by means of the word *'yící·yí·* 'going back in', where the vowels as well as the consonants are in palatal position. He testifies that in pronouncing this word his tongue 'does not seem to move at all'; in other words, it does not move forward or backward.

[8] Stressed syllables are indicated by a straight up and down stroke placed before the syllabic initial. In addition, each syllable has one of the following four types of tones: (1) high (indicated by an acute), (2) low (indicated by a grave), (3) rising from low to high (indicated by an inverted circumflex), and (4) falling from high to low (indicated by a circumflex).

position and by its aspirated release, since such a group would, of course, be a heterophonous cluster.

(2) Configurational: Omitting *c* for the present, we find that every single consonant of the language may occur geminated,[9] e.g., *'háyyì·* 'hot'; *pów'wánwà* 'our sister (men speaking)'; *'ippócí* 'his son'; *'láffítá* 'to cut'; *ím'má·hì* 'his, her father-in-law'; *'níttá·* 'day'; *'hónnà* 'dress'; *'píllò* 'boat'; *'fóllítá* 'several to go about'; *'císsì* 'rat'; *'pókkò* 'ball'; *'máhhìs* 'he got tall'. Consequently, although such need not necessarily be the case, one would expect that *c* would also occur geminated. Now it has already been shown, on phonetic grounds, that *cc* does occur. On configurational grounds, our conclusion is reinforced by consideration of the fact that, having discovered this, we have filled up the last gap in our set of possible geminate clusters.

(3) Informants' reaction: Further evidence (which would appear to be somewhat dubious by itself, but which seems pertinent when supported by the preceding points) may be adduced from the fact that those two of my informants who are adept at writing their own language (in the Loughridge alphabetic system[10]) are consistent in writing *cc* in all cases for [tc].[11] They do this, moreover, in spite of the fact that Loughridge never writes *cc*, but always *tc*. One informant testifies that some Creeks, however, do write *tc* where he writes *cc*, but it seems more than likely that such Indians are following spelling habits gained from reading Muskogee published literature.[12]

In conclusion, I would like to make this point: On the basis of my own experience, I would say that completely adequate phonetic results are not likely to be obtained by the linguistic field-worker unless, throughout the course of his work, the principles of phonemics are kept constantly in mind. For in this way he will be forced to greater phonetic accuracy than if the contrary procedure is followed, namely, that of recording phonetically without due consideration of the pho-

[9] There are thirteen consonants in Muskogee: *y, w, p, f, m, t, n, l, l, c, s, k,* and *h*.

[10] See note 4.

[11] Interestingly enough, there are other situations wherein these same informants attempt to handle their alphabet (in so far as is possible) more phonemically than it is handled by Loughridge.

[12] Some of these are: Loughridge, op. cit.; Loughridge and Rev. David Winslett, Nakcokv Esyvhiketv, Muskogee Hymns, fifth edition, revised, Presbyterian Board of Christian Education, Philadelphia, 1930; Este Maskoke Etvlwv Emvhakv Empvtakv momet Emvhakv (in English: The Constitution and Laws of the Creek Nation), 192 pp., 1894. The last-named is now rare; the title-page is missing in the only copy I have seen.

nemic problems involved. As a consequence of this fact, it becomes somewhat dangerous to attempt to interpret phonemically (after the fact, as it were) material that was originally recorded from the phonetic point of view only. The blame for this, however, cannot be placed on the phonemicist nor on phonemics. On the contrary, the difficulty arises from this paradox: Whereas the phonemic structure of a language cannot be worked out without accurate phonetic data, accurate phonetic data are not likely to be obtained without consideration of the phonemic structure.

MARY R. HAAS

A MISPRINT CORRECTED

In LANGUAGE 13.317, line 7 read: **cínĭscu* (by dissimilation) > **cíns'cu*.

G. M. B.

BOOK REVIEWS

Indogermanisches Jahrbuch XXI. Pp. 456. Edited for the Indogermanische Gesellschaft by ALBERT DEBRUNNER and WALTER PORZIG. Bibliographie des Jahres 1935. Berlin and Leipsic: Walter de Gruyter and Co., 1937

Once more the Indogermanische Gesellschaft has presented to every linguist an indispensable tool! Once more there arises the pleasing duty to urge upon every linguist the giving of some practical expression of his appreciation! Personal subscriptions, library subscriptions, contributions are all urgently needed for the maintenance and expansion of the work. It is to be hoped that American scholars will do their share in making the sacrifices needed to attain these ends.

The scholars who have unselfishly born the burden of compiling the bibliographies for the various subdivisions may be listed as a slight recognition of their service. General linguistics is divided among W. Brandenstein, P. Meriggi, E. Karg-Gasterstädt, W. Porzig, H. Spehr, Eb. Zwirner. Indo-European linguistics is in the hands of W. Brandenstein, Indo-European antiquities in those of H. Krahe. W. Printz cares for Tocharian and Aryan, V. Inglisian for Armenian, N. Jokl for Albanian. P. Chantraine is in charge of Greek, G. Devoto of Italic, J. Pokorny of Celtic, G. Streitberg of Germanic, E. Fraenkel and O. Hujer of Balto-Slavic, and J. Friedrich of Hittite and other languages of Asia Minor. Certain impending changes in this personnel are announced (414–5); among which the retirement of P. Chantraine will be regretted especially. The frontispiece of the volume is an attractive photograph of Hermann Hirt.

GEORGE MELVILLE BOLLING

An Objective Psychology of Grammar. Pp. 16 + 344. By J. R. KANTOR. Bloomington: Indiana University Publications, 1936.

Once again a non-linguist rushes to the rescue of the sadly misguided science of linguistics. For, according to Kantor, 'the fair-minded observer must agree that hardly a beginning has been made in bringing scientific order into the linguistic chaos.' (3)

The salvation is to be found in the holistic doctrine that every act of

speech, i.e. of 'adjustmental behavior', is an indivisible whole which may on no account be dissected into sounds, words, and sentences. Thus the psychologist 'does away entirely with two departments of speech parts, one dealing with isolated words, the other with word-combinations or sentences. Obviously, the two departments result only from a pre-occupation with word-things'. (197) When Grimm formulated the Germanic sound-shift he dealt with isolated sounds as if they were 'independent thing units'. (175) Kantor, therefore, disapproves of Grimm's law, which he calls 'the mutation theory'. The judgement he passes on Verner and Grassmann is not quite so severe, however; for the conditioned sound changes demonstrated by these scholars are more to his liking since they happen to involve whole words or at least syllables.

The hoariest fallacies are presented as current linguistic tenets; e.g. the existence of truly primitive languages (158), the 'illogical' nature of expressions like *criminal lawyer* (35), a nonsensical remark on Egyptian Arabic (177), and also the conviction that linguists generally look upon phenomena like the Germanic sound-shift as immutable laws of nature, whereas only the psychologist realizes that 'modifications in phonemes are in the last analysis changes in vocal action patterns'. (177) The investigation of historical phonetic change is considered a deplorable waste of time, since it teaches us nothing about actually occurring phenomena, and, moreover, overlooks the fact that no two individuals ever pronounce a word exactly alike, and that consequently there are as many languages as there are speakers. Throughout the book 'the doctors of grammar' are violently berated for employing the historical method.

In short, Kantor proposes to knock away the two main props of scientific investigation: analysis and general inference. 'The task of science', says Eddington, 'is to disclose the scheme of recurrencies in the combined experience of conscious beings. Progress depends on analysing our experience into its simplest elements.' (New Pathways in Science 45, 225) When Marty tried to foist upon linguistics Brentano's system of psychology, he at least offered a complete new terminology of grammar. Kantor offers nothing beyond the suggestion, endlessly repeated on over 300 pages, that acts of speech should be classified according to the situations to which they are adjusted. But surely, if every act of linguistic behavior is unique and unanalysable, so is every given situation. Linguistics is thus reduced to an inactive admiration of the one-ness of the all.

Kantor's rejection of historical comparison and of descriptive analysis naturally leads him to confuse entirely different types of linguistic problems. The following passage is typical. 'The contrast between grammatical and psychological gender is well illustrated by those situations in which there is a lack of coördination between individual and conventional sex references. Because of the sex taboos of polite European society, physicians frequently find individuals lacking necessary speech equipment to explain their sexual difficulties . . . In such cases persons must resort to their own individualized speech patterns. These individualized sex references contrast markedly with the purely social patterns of conventional gender.' Thus grammatical gender has nothing to do with sex or any form of systematic classification; it is based on 'general institutional characteristics' of particular linguistic groups. As to the origins of such characteristics, we must 'admit ignorance of phenomena which have developed before observational facilities became available'. (214) One would like to ask the author what institutional characteristics gave rise, in comparatively recent times, to the system of noun classifiers in modern Chinese. The history of China is not as conveniently unknown as that of the Bantu tribes.

The linguist may also be curious to know why a fixed 'one-to-one relation' is supposed to be a characteristic of all symbols (64 ff.); why linguistic phenomena 'allow room only for active and passive description' so that the 'middle voice constitutes only conventional modes of utterances' (264); or what living Bavarian dialects employ a dual (257). Mistakes and fallacies of this sort are numerous, as is to be expected in the work of an author so scornful of exact scientific methods. Suffice it to say that linguistics will have to do without the aid of the particular brand of psychology expounded in this book.

H. V. VELTEN

De Hettitische *ḫ*, een bijdrage tot de studie van het Indo-Europeesche vocalisme. Pp. xii + 395. By WALTER COUVREUR. (Bibliothèque du Muséon 5.) Leuven, 1937.

Couvreur's new book is an amplification of the pamphlet of forty-three pages on the same subject, which he published two years earlier and which I reviewed in LANG. 12.210–3. As far as I can see, his opinions have not been changed at any point by further study or by the published criticisms of the earlier publication. Consequently I refer to my former review for a sketch of the author's thesis.

The new book contains a very full discussion of the evidence upon which the conclusions are based. Couvreur has worked through the scientific literature with commendable thoroughness, and he clearly has an independent command of the Hittite language. He has carried his etymological studies somewhat further than was necessary for his immediate purpose, and has given us the fullest collection of Hittite etymologies yet published. These are scattered through the book, but they are gathered into a Repertorium van Indo-Europeesche bestanddeelen in den Hettitischen woordenschat (323–30) and a Repertorium van Hettitisch-Tocharische etymologieën (331 f.). If this feature of the book is somewhat less complete and final than the author supposes, that is chiefly due to the fundamental difficulties and uncertainties of etymological research.[1]

Now that we have before us a complete documentation of Couvreur's position, it is possible to form a definite opinion of it. We may say at once that there is no longer room for doubt as to the main point, namely that Hittite *ḫ* (written double between vowels) represents an original laryngeal which gave a preceding or following vowel *a*-color. The evidence on this point is overwhelming, and nearly all students of the question are now agreed about it.

Couvreur holds that no other *a*-vowels than those due to *ḫ* (I should write *x* without intending to indicate the precise sound) existed in Primitive Indo-European, either of the early period before the separation of Hittite or of the later period to be inferred from the comparison of the historic Indo-European languages.[2] Opposed to this is the view of Kurylowicz and Sapir that another laryngeal (Sapir writes *ʔ*), which was lost in the Indo-European languages and is at any rate not written in the Hittite documents, also gave a contiguous vowel *a*-color. The question at issue obviously depends upon etymology, and Couvreur has to reject a considerable number of identifications that

[1] A number of Couvreur's etymologies are discussed below, but I do not attempt either to list his new contributions in this field or to refute what seem to me to be errors.

[2] It is clear from many passages (e.g. 13, 191, 284, 316, 321) that Couvreur virtually accepts the Indo-Hittite hypothesis, but he uses the term 'Indo-Hittite' rarely and only in quotation marks, apparently because he takes Petersen's word for it that this term implies that in case of inconsistency between Hittite and Indo-European evidence the former is always to be preferred. Of course I have never had such an idea. Couvreur frequently contrasts Hittite and the Indo-European languages in a way that would be meaningless without the Indo-Hittite hypothesis.

have been proposed. The acceptance of Kurylowicz' fourth laryngeal seems, for the moment, to depend upon the following etymologies or some of them. I enclose in square brackets items that should in my opinion be eliminated, at least for the present. I omit altogether certain etymologies discussed and rejected by Couvreur, on the ground that they would no longer appeal to anybody.

a-a-ri 'is hot', *a-a-an-za* 'hot': IE *ai-*, *ai-dh-* 'burn, shine' (WP 1.2–4, 4–6). The phonetic value of Hittite initial *a-a-* has never been fully determined, but Götze, MS 31, has pointed out that NINDA *a-ra-an-te-it* (KUB 10.89.1.21) seems to be an error for NINDA *a-a-an-te-it*, and, if so, *a-a-an-* must have dissyllabic value. I suggest that our participle represents IH *ayont-*; if the pre-Hittite loss of *y* between like vowels (HG §107) antedated the change of *o* to *a*, we may read *ayant-* in Hittite (*a-a-* may stand for *aya-* in Akkadian); otherwise we may suppose that in this word some unknown analogy prevented the regular contraction. This is a new etymology, and therefore it is not discussed by Couvreur.

[*alis* 'white'], *alpas* 'cloud': [Lith. *alvas* 'tin'], Lat. *albus* 'white', Gk. ἀλφός 'dull white leprosy'. Couvreur rightly objects (150) to positing any etymology for *alis* on the ground that its meaning is not certain. He cites Ehelolf as authority for the statement that a certain text speaks of the colors of *alis*, which word therefore appears to be a substantive. He objects (106) to connecting *alpas* 'cloud' with *albus* 'white' on the ground that clouds are not thus named; but he admits that clouds sometimes get a name from their dark color, and clouds are more often white than black. Certainly this is better semantics than Couvreur's connection with Gk. λαπαδνός < rather ἀλαπαδνός > 'feeble'. His further connection of these two words with OHG *luft* < IE *leup-* is phonetically impossible.

appa 'afterwards, back, again', *appan* 'behind, after': Skt. *apa* 'away, forth; back', Gk. ἀπό, 'away, from; after; back', Lat. *ab* 'away, from; after', etc. For Couvreur (94) this is semantically impossible, evidently because he doesn't remember that the meanings 'after, back', etc. are citable from Indo-European. In addition to the meanings given above, cf. Gk. ἄψ 'backwards, back again; in return', Lat. *absque* 'without; contrary to', *abrogo* 'repeal'. While the commonest Indo-European meaning of **apo* does not appear in Hittite, the existence of the Hittite meanings in Indo-European proves that they can be derived from the fundamental meaning of the word, whatever that is. Couvreur prefers to connect Hittite *appa* with Gk. ἐπί, ὀπίσσω, ὄπιθεν.

That the meaning of Hittite *appizzis* 'last, later' (apparently a derivative of *appa*) is close to that of Gk. ὀπίσσω has already been noted by Kretschmer, ap. Hrozný, SH 21 fn. 1, and by Lohmann, IF 51.324 f., and the latter suggests that Hittite *appa* has been influenced by the adverb that appears in IE *epi*, *opi*. No doubt it is the final vowel of Hittite *appa* that has prevented others than Couvreur from tracing the word to original **opi*; how he disposes of this difficulty he does not say. For my part, I am not sure that we must ascribe to original **opi* any influence at all upon Hittite *appa*, but there seems to be no escape from connecting it, at least in part, with IE *apo*.

arwa(e)- 'bow down, bow down to': Gk. ἀράομαι 'invoke, pray'. The Greek verb is derived from ἀρά(<*ἀρϝά) 'prayer, curse'. The Hittite verb is shown by its form to be denominative, but the noun from which it is derived is not citable. Couvreur (302) says: '. . . the groundlessness of this comparison is too obvious to require a long refutation.' He then cites the current connection of Gk. ἀρά with Skt. *ā́ryati* 'praises' and Lat. *ōro*, which, however, offers no explanation of the ϝ of Arcad. κάταρϝος 'accursed'. To me the etymology seems certain.

au-, *aus-*, *u-* 'see' : Lat. *aurora*, Gk. ἕως 'dawn'; *uske/a-* 'see' (durative): Skt. *uccháti* 'shines'. Couvreur (152) objects that Hittite does not show the meaning 'shine' and Indo-European does not show the meaning 'see', so, in spite of admitted parallels of the semantic change assumed for Hittite, he rejects the comparison. This is as if one refused to connect Gk. νέμω with Goth. *niman* in spite of admitting the possibility of an interchange of the meanings 'give' and 'take'. Still more frivolous is the objection that the final *s* of the Indo-European root appears only in certain forms of the Hittite word.

awan, an adverb that often accompanies *arha* 'away, from' and *katta* 'down' and rarely *sara* 'up', *u-* 'hither' : Skt. *ava* 'off; down, down from', Lat. *au-* 'away'. Couvreur (153 f.) thinks it unlikely that *awan* is connected with *u-*; he suggests instead a connection with *duwan* 'apart', but such a comparison evidently concerns only the end of the word. Even if the form of *awan* has been modified by the influence of the semantically similar *duwan*, original identity with Skt. *ava* is still probable, since the two words seem to have the same meanings. Couvreur finds that Hittite *u-* 'hither'[3] cannot be semantically connected

[3] In this connection, oddly enough, he reverses the meaning of German *hin* and *her*. He says that Hittite *u-* is virtually equivalent to '*hin* (beweging naar den spreker toe)'.

with IE *au*, which Walde-Pokorny define as 'herab, weg von'. I have suggested, LANG. 7.5, that the original force of the stem was 'down hither', with special reference to the descending course of sun, moon, and stars.[4] But even if it were the whole truth that Hittite *u-* means 'hither' and Lat. *au-* means 'away', the semantic connection would still be perfectly plausible; words for 'here' and 'there' are constantly interchanged in all languages as the point of view shifts.

istanta(e)- 'tarry, delay', *istapp-* 'shut, enclose, cover, store up', midd. 'stop, cease'; *tit(ta)nu-* 'cause to stand, place, appoint, arrange', *tiya/e-* 'take one's stand; approach; come, be (in a place or time); mount': IE *st(h)ā-*. Couvreur (194–9) objects to the connection of *istanta(e)-* and *istapp-* with the IE root, since, in his opinion, Hittite initial *st-* can be written only *ši-it-*. I am inclined to disagree with him (see LANG. 4.1–6); but the point is scarcely worth discussing in the present connection, since Couvreur himself is inclined to accept the identification of *iskalla-* 'tear, split' with Gk. σκάλλω 'hoe', and OIcel. *skilja* 'separate, cut'; of *ispar-* 'spread, cover; trample' with Skt. *sphuráti* 'spurn' (I should prefer to think of Gk. σπείρω 'scatter, strew, sow' as coming closer to the Hittite meaning); and of *ispai-*, *ispiya/e-* 'sate oneself' with Skt. *sphāyati* 'grows fat', apparently on the basis of a prothetic vowel. If these etymologies are defensible in point of phonology, so are the two in question. The semantic objections urged (199 f.) amount to a statement that *istanta(e)-* never means 'stand' and *istapp-* never means 'cause to stand'; but few semantic changes can be easier than 'stand' > 'tarry' and 'cause to stand' > 'shut up' or 'store up'. Furthermore the middle of *istapp-* does have virtually the meaning that Couvreur misses. Couvreur (200 f.) assigns *tittanu-*, *titnu-* to IE *dhē-* 'place'. His attempts to account for the doubled *t* of *tittanu-* from original *dh* are not only unconvincing in themselves but inconsistent with his attitude toward this feature of Hittite phonology everywhere else in the book. First he finds doubling of a consonant before an anaptyctic vowel typical in forms like *uš-ša-ni-ya-* beside *uš-ni-ya-*, *te-ik-ku-uš-ša-nu* beside *te-ik-ku-uš-nu-*, and *aš-ša-nu-* beside *aš-nu-*. It would apparently be more nearly correct to say, at least where the doubled consonant is a stop, that a non-phonetic vowel is sometimes introduced to make possible the double writing (see HG §66 p. 74). The second attempt to justify *tt* in *tittanu-* is a citation from my HG: 'In general neither a vowel sign nor an initial vowel of

[4] I have here omitted all reference to the probable variants of *u-*, namely *we-* and *wa-*, in order to avoid a controversy that does not concern our present topic.

a syllabic sign is allowed to carry a syllable if a different orthography is available.' But why not **ti-ta-nu-*? Couvreur finds it easier to connect the form *ti-it-nu-* with IE *dhē-* than with IE *st(h)ā-*; he identifies the second *t* with the complete zero grade of Skt. *dadhmás*, and thinks that no similar grade is possible for an *ā*-root. The only ground for assuming such a difference seems to be that Couvreur would expect to find *ḫ* in a Hittite form of that sort, but that is precisely the point at issue. Couvreur (201 f.) also rejects Pedersen's (MS 69 f.) brilliant derivation of *tiya/e-* 'take one's stand' from the root which appears in IE as *st(h)ā-*, but I can find no reason given for the rejection.

maklant- 'thin, lean': Lat. *macer* 'thin, lean', Gk. *μακρός* 'long', *μακεδνός* 'tall, slender', *μῆκος*, *μᾶκος* 'length'. Couvreur (223 f.) assumes two IE bases **mēk-* 'thin' and **māk-* 'long'; by assigning the Hittite word to the former he is able to rescue his theory, but at the expense of separating Lat. *macer* and Gk. *μακρός*.

man '*ἄν*; as when, if': Gk. *μήν*, *μᾶν*. I still hold to this identification in spite of Pedersen, MS 59; but Couvreur (203) is right in saying that etymologizing particles is a thankless task.

mema- 'speak': Gk. *μέμνημαι* 'remember'. Couvreur (204 f.) refers to Benveniste, BSL 33.140, who objects to the etymology on the grounds (1) that *mn* should become *mm*, and (2) that words for saying are developed out of words meaning 'shout, cry, groan' and the like, not from words for a mental operation. As to (1), such spellings as *me-im-ma-i* (KUB 12.62.1.13, 14, 2.5; 17.28.2.39, 55) and *me-im-ma-aš* (KUB 14.2.1.10, 3.2.37) are not very rare although much less common than the spelling with single *m*. In general a few double spellings are of more evidential value than many single spellings (cf. below 76). To meet the semantic objection I need only cite Lat. *memorare* 'say'. OIcel. *māl* 'speech', if from **ma-tlo-m* (Pedersen, MS 59) may also be connected. Benveniste prefers to connect the Hittite word with Skt. *mimāti* 'bellows', ChSl. *mъmati* 'stammer'. It is noteworthy that these words also have long stem vowels, and the Slavic *a* cannot represent IE *ē*, but Couvreur seeks to escape the difficulty by combining Gk. *μιμιχμός* 'neighing of horses' (as do Walde-Pokorny). I must ask those who like this etymology to work out the phonology. It is not clear to me.

pai- 'give': Toch. B *ai-* 'give', Gk. *αἶσα* 'fate', Osc. *aiteis* (gen.) 'part'. This attractive etymology, which owes its final form to Pedersen, MS 63, is possibly to be preferred to my connection (LANG. 8.121) of the Hittite word with IE *ei-* 'go' (**bhe + ei-* > **bhēi-* > Hittite *pai-* —

a causative use of an intransitive is, of course, in order), although the latter seems to fit better the identity of conjugation with *dai-* 'place' and *nai-* 'lead, send, conduct, turn'. Couvreur's rejection (158 f.) of Pedersen's etymology is based upon doubts of the analysis of *pai-* and some semantic difficulty which is not precisely stated and which I cannot discover.

[*sakk-*, *sekk-* 'know':Lat. *sāgio* 'perceive'.] Couvreur (205) rightly argues against this etymology of Benveniste's that it requires spelling with a single *k* in Hittite. He might also have cited the ablaut *e* : *a* as a fatal weakness. I still hold to my identification (LANG. 3.161–4, HG 120, etc.) of this word with Gothic *salv* 'he saw', *sēhvum* 'we saw'. The connection with *ὄμμα*, etc., which Couvreur (206) prefers to cite, is perhaps somewhat less certain.

[*uttar* 'affair; word':Gk. *αὐδή* 'voice'.] Couvreur (160–6) makes it very probable that the primary force of *uttar* was 'thing, affair, occurrence', and that would dispose of Kurylowicz' etymology. He also points out that the suffix *tar* appended to a stem with final dental should give Hittite *zt*. Couvreur objects also to Marstrander's connection of *uttar* with Goth. *waíhts* 'affair', on the ground (166 fn. 1) that *kt* survives in *harkta*, *harnikta*, and *lenkta*; but restoration of *kt* for *tt* in a paradigm is, of course, to be expected. The connection of *uttar* with Gothic *waíhts* removes it from further consideration in this connection.

taya/e- 'steal':Skt. (*s*)*tāyús* 'thief', ChSl. *tają* 'I conceal', Gk. *τητάομαι*, Dor. *τᾱτάομαι* 'I lack, am bereft of'. Couvreur suggests (208 f.) two unsatisfactory ways of escaping this etymology. (1) Since only Greek presents clear evidence for the *ā*-vocalism, he stresses the semantic difference between 'steal' and ' deprive of'. (2) Since it is possible to analyze *ta-ya/e-*, he suggests that the word is an extended form of *da-* 'take'.

[*weh-*, *wah-* 'turn':Lat. *vārus* 'bent'.] Couvreur (211) stresses the semantic difficulty of the etymology. More serious objections are the ablaut exhibited by the Hittite stem and the consistent writing of single *ḫ* between vowels.

ya- (*i-ya-at-ta*, *i-ya-at-ta-a-ri*, etc.) 'go':Skt. *yāti* 'goes', Lith. *jóti* 'ride', OIr. *āth* 'ford', Lat *iānua* 'door', *iānitor* 'door-keeper'. Couvreur (101) objects to my interpretation of *i-ya-* as [ja-], but the etymology is quite independent of that point; IE *yā-* 'go' is certainly an extension of **ei-*, and syllabic *i-* (from *ɩi-* or, if one prefer, the by-form required initially in a phrase by Sievers' law) is to be expected beside

it. Couvreur prefers to regard Hittite *iya-* (it is equally possible for one who follows him to read *ya-*) as a thematic present from **ei-*. This is a possible explanation of the forms, although Hittite in general shows fewer thematic presents than Indo-European.

-a, nom.-acc. case ending of many neuters plural: IE *-ā/-ə*. Couvreur (225) implies that I have given two inconsistent interpretations of the Hittite ending, but my theory was that either IH *ā* or *ə* yielded Hittite *a*, and now I would revise this to say that either IH *aː* or *əː* yielded Hittite *a*. He refuses (224–6) to accept either identification because Hittite has no collective *a*-stems. If I understand him correctly he suggests (102, 226) that both the Hittite neut. pl. ending *-a* and the rarer equivalent *-i* are in origin enclitic pronouns. Apparently he separates these endings entirely from the neut. pl. inflection of the Indo-European languages.

[*-as*, gen. sg. ending of *a*-stems: IE *-ās*, gen. sg. ending of *ā*-stems.] Couvreur's objections (211) to this comparison seem to me of no weight, but Pedersen's (Études lituaniennes 23) suggestion, that the Hittite identity of nom. and gen. sg. in the *o*-stems is an archaism, is equally attractive. At the moment I am not sure which theory is to be preferred.

-tta, ending of the 2 sg. pret. of the *hi*-conjugation, *-tti* (<*-tta* by analogy of the *mi*-conjugation forms): Skt. *-tha*, Gk. *-θα*, ending of 2 sg. perf. I discuss this matter in my article in this issue of LANGUAGE.

-na-, present suffix : IE *-nā-*, present suffix. Couvreur (213) records my advocacy of this comparison 'in spite of differences in conjugation and meaning' (HG 244). Since I explain the difference in meaning (formal confusion of *-nā-* and *-neu-* leading to identical meaning in Indo-European) and since Couvreur does not share my opinion about the conjugation of the Hittite *na*-verbs, this quotation does not dispose of the etymology. His only other objection is that his general theory calls for Hittite *-naḫ(ḫ)-* to equate with IE *-nā-*, and this, again, is precisely the point at issue.

-ae/a-, stem final of many denominative verbs: IE *-āye/o-*. (See Sturtevant, LANG. 5.8–14, HG 226–8.) Couvreur promises a discussion of this Hittite suffix at some later time. Until then we know only that he will not connect it with IE *-āye/o-*.

It seems to me that this material decides the question in favor of Kurylowicz. We can now say quite definitely that Indo-Hittite had two laryngeal consonants of *a*-color, *x*, which appears in Hittite as *ḫ* (*ḫḫ* between written vowels, whether these were pronounced or not),

and ʔ, which is lost in Hittite. Whether one or both of these consonants left consonantal traces in any of the historical Indo-European languages, as Sapir thinks, remains to be seen, and there are many other unsettled problems about their history, most of which are not even raised in Couvreur's book.

The author finds (140–3) a small amount of evidence for his thesis that non-ablauting initial *o* of the Indo-European languages is due to a laryngeal consonant (he writes ', I shall write γ) that survives in Hittite as initial *ḫ* in several words, namely: *haras*, gen. *haranas* 'eagle': Gk. ὄρνις 'bird', Goth. *ara* 'eagle'; *hark-* 'be destroyed': Ir. *org* 'ravage, kill'; and *hastai* 'bone': Gk. ὀστέον, Lat. *os* 'bone'.[5] Couvreur lists also (143–6) *hulla-* (*ḫu-u-ul-la-*) 'smite, destroy, annul': Gk. ὄλλυμι 'destroy' and *humanza* (*ḫu-u-ma-an-za*) 'all': Lat. *omnis* 'all'. The reasoning which induced Weidner and Marstrander to read the cuneiform sign *u* as *o* and the sign *ú* as *u* in Hittite texts is restated (145, 250–2), but no attempt is made to account for the development of initial *γo- to *ḫa-* in three Hittite words and to *ḫo-* in two others. This is by far the most serious lapse in method in the book.

For the treatment of medial γ in Hittite Couvreur can find only one etymology (189–94), namely: *lahu-*, *lahuwwa-* 'pour': Gk. λούω 'wash'. It is important for the theory that γ between vowels should be represented by Hittite *ḫ* written singly, and in most instances this word is so spelled. I can, however, cite *la-aḫ-ḫu-u-wa-i* (KUB 9.31.2.9), *la-aḫ-ḫu-tin* (KUB 13.3.2.28), *la-a-aḫ-ḫu-wa-tin* (KUB 13.3.3.2). Even if we separate *lahhus* 'basin, bowl', as Couvreur does, these forms make serious difficulty; for, while Indo-European voiceless mutes are often written single in Hittite (*ši-pa-an-ti* is much more common than *ši-ip-pa-an-ti*), original voiced mutes are seldom if ever written double. But even if the spelling *lahu-*, *lahuwwa-* were consistent, this one word would be an insufficient basis for so far-reaching a theory.

To make matters worse, Couvreur has to reject two excellent etymologies. He cannot accept the identification of Hittite *da-* 'take' with IE *dō-* 'give'. He admits (206 f.) that the meanings 'give' and 'take' may attach to the same word, but he sets up the fantastic demand that those who would connect Hittite *da-* and IE *dō-* must find

[5] Couvreur could not know of Goetze's connection (Mélanges Pedersen 492 fn. 3) of *harssan* 'head' with Gk. ὄρος 'mountain' and Skt. *r̥ṣvás* 'high'. It must be added that this etymology would necessitate separating these words from the root *er-*, *or-* 'move, rise', since Hittite *ar-* 'arrive', *ar-* 'stand', *arai-* 'rise', *arnu-* 'bring', etc., do not have initial *ḫ*.

the meaning 'take' in Indo-European and the meaning 'give' in Hittite. He does not forget to rule out Skt. *ā-dā-* 'take' on the ground that the prefix *ā-* has precisely the function of reversing the meaning of the verb. One must admit the ingenuity of Couvreur's explanation of the likeness in form of *da-* 'take' and *dai-* 'place'; *peda-* 'carry, bring, take away' and *uda-* 'bring' are said to be compounds of *dai-* 'place' with change of conjugation (cf. *penna-* and *unna-* beside the uncompounded *nai-*), and then, we are left to infer, simple *da-* follows the inflection of the compounds. To me it seems much more likely that we have a partial confusion of originally distinct **dō-* (or **dōγ-*) and **dhē-* (or **dhe'-*), as in Latin and other Indo-European languages.

Similarly Couvreur (205) must not connect Hittite *pas-* with IE *pō(i)-* 'drink'. In spite of the attempt to establish a different meaning for the two stems, the fact remains that Hittite *pas-* is either an exact or an approximate synonym for *eku-* 'drink'. It is scarcely ingenuous to urge that Hittite 'shows no trace of *i*' in this very rare word; neither does Latin in many hundreds of occurrences of *bibo*, *pōtus*, *pōculum*, *pōtio*, etc. Again there is no point in objecting to the final *s* in the two citable forms (*pa-a-ši*, *pa-aš-zi*—cf. Friedrich, AOr. 6.374), whereas in Skt. *pāsta* the *s* is an aorist suffix; cf. Lat. *vīso* beside *video*.

The truth is that neither Couvreur nor anyone else has yet succeeded in plausibly connecting Hittite *ḫ* with the assumed *o*-colored laryngeal of Primitive Indo-European or Pre-Indo-European. I am myself inclined to believe that Hittite preserves no traces of such a sound, and I should not be surprised if we finally conclude that there is no satisfactory evidence for any Indo-European *ŏ* due to a neighboring laryngeal. That is, we may after all come to Brugmann's final conclusion (KVG 74) that it is safer to assume only one *o* in Primitive Indo-European < and Indo-Hittite>.

In that case the three words provisionally credited above (76) to Hittite *ḫ* < *γ* may have *ḫ* from antevocalic ', like the words with medial *ḫ* which I discussed in LANG. 12.181–7, 213, would be inconsistent with the view held by nearly all de Saussure's followers that initial *ĕ* in the historic languages implies original initial ', but no one has ever adduced any convincing evidence in favor of this part of the theory. There is no reason to import into Indo-European or Indo-Hittite the Semitic requirement that all words must begin with a consonant.

The book contains many useful observations and suggestions that are not directly connected with the subject. Thus it is noted (177–9, cf. 216, 315) that Hittite representatives of syllabic liquids and nasals

are normally written with a single vowel if the system of writing makes that possible; e.g. *an-za-a-aš* 'us', *kat-ta* 'afterwards, together, down', *kar-di-aš* 'of the heart', *pal-ḫi-iš* 'broad'.

Again, *-war-*, the variant for *-wa*, the particle of direct quotation, which is always used before the enclitic *-as* 'he, she, it', is compared (54 f.) with Southern British *idear of*, etc. It is well known that final *r* tends to be omitted in certain texts, and this may indicate a regular loss of final *r* before an initial consonant of a following word within the same phrase. If so, *alwanzata k-* : *alwanzatar a-* = *-wa k-* : *-waras*.

There are very full indices, and there is a summary in French, which, however, will not relieve scholars of the task of working through 390 pages of Dutch.

E. H. STURTEVANT

An English Pronouncing Dictionary (showing the pronunciation of over 54,000 words in international phonetic transcription). Pp. xxviii + 495. By DANIEL JONES. Fourth edition, revised and enlarged. New York: E. P. Dutton and Company Inc., 1937.

The present edition contains nearly 5000 words which were not in the third edition, along with changes in the pronunciation of about 1000 words; of the total 54000 there are about 15000 which are names of persons and places, and their derivatives. The pronunciation which is given is that which is in use in England, and Dr. Jones is careful to specify variations: *Cirencester* is given in three ways, one used by members of county families, one in the town itself, and the third occasionally in the surrounding country; there is a normal pronunciation for *ferrule*, and another used by those connected with the umbrella trade.

Any American knows that English place-names often are pronounced in America more nearly in accordance with the orthography than they are in England; thus *Newark* is ['nju(:)ək] in England, and therefore in this Dictionary, but it is ['nju:ərk] in the New Jersey city of the name, and ['nju:'ɑ:rk] in the Delaware city. Such omissions we may expect; but it is astonishing to find *Connecticut* given with [-kt-], and [-t-] only as a second pronunciation; I have never heard an American say [-kt-] in this word. When a word is definitely American, my feeling is that American usage should determine the pronunciation—or that a remark about it should be made. This is Dr. Jones's policy with *Roosevelt*, which is given as ['rouzəvelt], followed by ['ru:svelt], with the explanation that the prior pronunciation is that used in the

family of the President of the United States, while the second is that often used in England. I could spend further space on *Chicago, Montana, Cincinnati, Missouri, Utah,* as presented by Dr. Jones, but I refrain, after mentioning the principle which seems to me to be basic.[1] And it does seem to me that the American pronunciations of *clerk* and *figure* might be added, as American usages, to [klɑ:k] and ['figə*], which are as unknown here as ['sænwidʒ]: or else such a pronunciation of *sandwich* is here regarded as sub-standard.

I note also the greater extent to which the recessive accent of English has affected words borrowed from French, as compared with the usage of the educated classes in America: the accent is put on the first syllable of *mêlée, Islam, garage, névé, serac, mirage, chauffeur*—with alternative accentuations on the final syllable of the last two.

Two words I find by accident, whose orthography offends me, *Iolchos* and *Widdicombe. Iolchos* had kappa and not chi in Greek; *Widdicombe* (in Devon) is spelled *Widecombe* by the British postoffice, although I am aware of the great variety in its spellings in the last few centuries. *Vergil* happily appears at the proper place, but so also does *Virgil,* which makes me see red.

Nonetheless I prize this volume, as a reliable source for British pronunciation of many words which are definitely British, and as a record of those (usually) slight variations between the pronunciation which I personally know, and that with which I come into happy contact on those rare journeys to my own ancestral land.

ROLAND G. KENT

Surnames. Pp. xxii + 360. By ERNEST WEEKLEY. Third edition. New York: E. P. Dutton and Co. Inc., 1937.

The present volume is a revision of a work which first came out in 1916, and was followed by a second edition in 1917, reprinted in 1927. This third edition has been changed by the omission of the sections on French and German names, and the substitution of chapters on surnames of Celtic origin (I am glad that they are Celtic and not Keltic) and on those derived from animal names.

[1] [To me it seems that the principle needs qualification. Much depends on (1) whether the dictionary is to be judged as normative or descriptive; and (2) on how far it is to regard dialects, especially American dialects. Spelling pronunciations are to be expected wherever there is lack of oral tradition; and the examples are of interest as showing that it does not matter whether the word crosses the Atlantic from East to West, or from West to East. G. M. B.]

Of necessity the treatment is sketchy, and few names have more than a single sentence devoted to them. Even at that there are many omissions: my own surname is not in the Index of the volume, nor the maiden names of my wife and my mother. Perhaps they are obvious and uninteresting.

What attracts my attention most closely is two sentences in the Preface, page v: 'The author is acutely conscious of the book's imperfections. It is not "scientific", and has little to say about those sacred "Lautgesetze", which, we used to be told, "admit of no exceptions", a pronouncement calculated to excite the hilarity of anyone who has made a first-hand study of surnames.' Such an attitude awakens distrust in the work of one who thus flaunts the only valid basis for a science of linguistics. It is true that surnames behave most astonishingly from the phonetic standpoint; but they are pecularly subject to the working of popular etymology, which transforms many words without regard for phonetic law. Even if *Süssholz* becomes *Seasholes* in German-English Pennsylvania, if *Monigle* is changed to *Mulligan* because the boss misunderstood the laborer's name, if *Vickery* becomes *Victory* (217), linguistic science is not reduced to the chaos which it was before the days of the Junggrammatiker. And similarly the conservatism of some family names, held unchanged by pride of descent, does not upset the principle. Regularity of phonetic development stands firm, however many counter-influences may be in operation, and however many the pieces of evidence which may be lost forever, not to be recovered by us fallible scholars of the present day. Even with the greater liberty which his freedom from the thralldom of regularity confers, Weekley must continually make guesses as to origins, which he can only label as guesses![1]

ROLAND G. KENT

Die deutschen Formen der litauischen Orts- und Personennamen des Memelgebiets. Pp. vi + 154. By WALTER FENZLAU. Halle: Niemeyer, 1936. (Zeitschrift für Mundartforschung Teuthonista, Beiheft 13.)

The term 'Memelgebiet' here includes the stretch of Lithuania extending from Memel on the north, southward and then southeastward along the political boundaries of the country, to Wischwill. The Lithuanian spoken in it is divisible into the dialects of Memel

[1] [The only possible fruit of such 'liberty' is, indeed, to deprive its possessor of the opportunity to do anything but guess. G. M. B.]

(northern and southern varieties), Prökuls, Heydekrug (also in northern and southern varieties), which belong to the Low Lithuanian; and those of Tilsit, Ragnit, and Wischwill, which belong to the Middle Lithuanian. Tilsit and Ragnit, it is true, lie south of the Memel River, and are not included in the territory under discussion; but the natives of the two districts going by these names call themselves *Ragainėnai* and *Tilžėnai*, which justifies their use.

Fenzlau, himself a native of the region, bases these studies on personal investigations. In each town or village he recorded the pronunciation of native speakers of Lithuanian, and also that of speakers of Low German and of speakers of High German—for the Germans there speak two distinct dialects.

After describing his method, he gives the phonetic variations among the several dialects of Lithuanian; then a comparison of the Lithuanian sounds with those used by speakers of Low German and by speakers of High German. All examples, both Lithuanian and German, are given in standard orthography as well as in phonetic notation. The treatment is divided into three chapters: vowels, consonants, endings. The greatest alterations are to be seen in the final syllables, since the German equivalents are commonly assimilated to some type familiar in German names; the most striking is the substitution of *-en* for the Lithuanian plural -(i)ai, as in *Bittehnen* for *Bitėnai* (120). But *Bittehnen*, though an apparent *-n-* stem plural, is not a plural syntactically, for, says Fenzlau, there are no place-names which in German take a plural verb. (It is here to be remembered that in Lithuanian the third person singular and the third plural of the verb are always identical in form.)

The treatise is valuable also as a painstaking record of the differences among the local dialects of Lithuanian with which the author deals.

ROLAND G. KENT

Lithuanian Self-Instruction. Pp. 74. By J. LAUKIS, with the assistance of Rev. C. E. EDWARDS, M.A. For Practical Use by Travelers, Tourists and those who wish to learn Lithuanian. Chicago: 'Lietuva', 3252 Halsted Street, 1908.

Žodynėlis Lietuviškai-Angliškas ir Angliškai-Lietuviškas. Pp. 160. Third enlarged edition. By KIŠENINIS. Chicago: M. J. Tananevicz, 736 West 18th Street, 1927.

These and other books concerned with the Lithuanian language, printed in the last 30 years or so, are called to our attention by the

Lithuanian Daily News, 1739 S. Halsted St., Chicago. The other volumes with which I was previously acquainted, except the Dictionary of Lalis (another publication of the Chicago list), are in German; thus these books would be convenient if adapted to their purposes.

Lithuanian Self-Instruction includes a series of word-lists and of phrases and sentences; in the main they are well chosen, except some of the travel phrases: it is hardly practical to learn the Lithuanian for 'I want to reach the B. and O. depot', and others containing names of American cities. The introductory pages on the pronunciation are quite inadequate; they have no mention of the palatalizing of consonants by front vowels, nor of the change of *a* and *ai* after *i* or *j*, to [ɛ] and [ɛi]. The paradigms at the close of the book fail to include the declensions of the pronouns; even brevity can hardly excuse this. But on the whole the selection of phrases is quite useful.

The Žodynēlis is a pocket dictionary intended for Lithuanians; in both parts (which I shall call L-E and E-L) the English words (and not the Lithuanian) are accompanied by a reasonably phonetic notation. I have sampled its usefulness by examining the entries beginning with E in the E-L part. Here 154 English words are given; these include 10 misspellings: *easeness, eal, effigi, eligant, elidion, elusary, embracment, exampel, exulpation, explation*; and 11 words so unusual as not to deserve inclusion: *effable, elke, elutriation, enstamp, eschar, esker, estuation, eversion, evocation, explorator, eyewink.* The Lithuanian glosses for 18 of these 21 are not to be found in the L-E. Incidentally I noted that the following English words are not in the E-L: *ease, edit, educate, element, employer, end* (verb), *enjoyment, entrance, equate, eradicate, escape* (noun), *establishment, eve, even, evident, exactness, exchange* (verb), *excuse* (noun), *execute, executor, exhort, existing* (adj.), *expand, expectation, expend, experience, expire, explanation, exploit, explore, expose, expressive, extend, exterminate, extinct, extinguish, eye* (verb). The title-page announces that the book contains 18000 words. There are actually 2419 entries in the L-E and 3481 in the E-L. Of 24 Lithuanian words on the title-page, apart from proper names, 15 are not in the L-E. The book is clearly inadequate for any purpose.

ROLAND G. KENT

NOTES AND PERSONALIA

THE BIBLIOGRAPHICAL SOCIETY OF AMERICA was organized in 1904 and incorporated in 1927. It includes in its membership those interested in bibliographical problems and projects of all kinds, as book collectors, librarians, scientific bibliographers, book dealers, and those interested in general bibliography. The Society has meetings once or twice a year, sometimes at Christmas time and usually in conjunction with the annual meeting of the American Library Association. Membership costs $3.00 per year, and members receive the Papers (issued at least once a year, containing papers read at the Society's meetings), and the News Sheet (an informal news organ of the Society, issued quarterly). Libraries may join as institutional members.

The Bibliographical Society of America contemplates expanding the 'Notes and Queries' section of its News Sheet to include as nearly as possible notices of all bibliographies planned or in process of compilation by members of the constituent societies of the American Council of Learned Societies and of other American scholars. The record of published bibliographies is provided in various other ways, but in only a few of our disciplines is any systematic attention given to bibliographies in progress. The duplication of effort in such work is particularly tiresome, whether it be the work of compiling for publication or merely in preparation for some piece of research work. The Bibliographical Society of America, therefore, hopes to render an acceptable service in providing a current record of bibliographical projects.

Notes for publication in the News Sheet may be addressed to Dr. Henry B. Van Hoesen, Secretary of the Bibliographical Society of America, Brown University Library, Providence, Rhode Island, to whom also application for membership should be made.

ANTONIO GARCIA SOLALINDE, Professor of Spanish and Portuguese in the University of Wisconsin, and a Foundation Member of the LINGUISTIC SOCIETY OF AMERICA, died very suddenly of a heart attack on July 13, 1937, at Madison, Wisconsin.

He was born on December 28, 1892, in the city of Toro, Spain, and was educated at Madrid, where he distinguished himself by his multi-

farious activities of scholarly, editorial, and administrative character. In 1922 he came to the United States, and served as Visiting Professor at Columbia University, Michigan, California, and Leland Stanford. He returned to Spain, received his doctorate in letters at Madrid, then came back to America and became a member of the Faculty of the University of Wisconsin, to which he was attached until his death. As a teacher he was in great demand, being called for summer work to Middlebury, Stanford, Chicago, Texas, and Michigan.

In 1930 the University of Wisconsin established a separate department of Spanish and Portuguese, in which Solalinde became the director of graduate studies, and of the Seminary in Spanish Mediaeval Studies, as well as of important committees; and it was through his strong personality and inspiring scholarship that despite the economic depression the new department developed to a position of strength and influence, now extended over the country by the scholars whom it has produced.

Solalinde was the author of many books and articles; his great work was on the General Estoria of Alfonso X. A Guggenheim Fellowship which he received in 1929–30 enabled him to finish his edition of the first part of this work, to classify the manuscripts of the remaining parts, and to initiate his projects of investigation in the subject. He was the recipient of honors from learned organizations and foundations; but, as the memorial resolution of his colleagues at Wisconsin states, 'he did not mention them. With his colleagues he was open and democratic, and he never gave in his society that note of superiority that shrinks the spirit of those who want to learn. That is why his students loved him like a father.'

THE LINGUISTIC PHONOGRAPH RECORDS, prepared by Miles L. Hanley, and included in the list of the Publications of the LINGUISTIC SOCIETY dated November 1, 1937, are now withdrawn from sale, as the supply has been exhausted and no further supplies are at the present time available.

MR. JOHN B. CARROLL is now at the University of Minnesota, as Teaching Assistant in the Department of Psychology.

DR. MARTHA JANE GIBSON is now Professor of English at Talladega College, Talladega, Ala.

MR. EUGENE KLEIN, of Philadelphia, has been elected Corresponding Member of the French Académie de Philatélie, the membership of which is limited to forty in France, with a very few additional members abroad.

DR. FANG-KUEI LI is at Yale University as Visiting Professor of Chinese Linguistics.

DR. DOROTHY PASCHALL has gone to the Texas State College for Women, at Denton, as Instructor in Latin.

DR. HORACE I. POLEMAN is at Yale University, as Sterling Research Fellow in Oriental Studies.

MR. HARRY A. ROSITZKE has gone to the Municipal University of Omaha for the current year, as Assistant Professor of English.

THE REV. LAURISTON L. SCAIFE has gone to St. Paul's School, Concord, N. H., as Teacher of Latin, Greek, and German.

MR. BENJAMIN SCHWARTZ has become Chief of the Oriental Division, of the New York Public Library.

DR. CARL SELMER has been promoted from Assistant Professor of German to Associate Professor of the same subject, at Hunter College.

DR. GEORGE L. TRAGER has been Lecturer in Linguistics at Yale University during the first half of the present academic year.

DR. HARRY V. VELTEN is at the Indiana University as Associate Professor of German.

DR. LYDIA E. WAGNER is now Instructor in Germanic Languages at the University of Nebraska.

THE FOLLOWING NEW MEMBERS FOR 1937 were received into the LINGUISTIC SOCIETY subsequent to the last published list, and up to December 4, 1937: George A. Kennedy, Raven Ioor McDavid Jr., Norman A. McQuown, Sister M. Calixta Garvey, Heinz A. Wieschhoff, R. D. Williams; they are included in the published list of members for 1937, printed in BULLETIN No. 11. The following have been received into the Society as new members for 1938, up to the same date:

Kenneth Morgan Abbott, Dept. of Classical Languages, Ohio State University; 804 Green St., Urbana, Ill.

Myles Dillon, Ph.D., Professor of Gaelic, and Irish History and Literature, University of Wisconsin; Bascom Hall, Madison, Wis.

Frank Joseph Hill, M.D., 863 Bank St., Waterbury, Conn.; *Lithuanian.*

William A. Kozumplik, M.A., Catholic University, Washington, D. C.; *Germanic.*

Andrew Robert Ramey, M.A., Associate Professor of English, University of Oklahoma, Norman, Oklahoma.

Mary Elizabeth Turner, B.A., 3905 Walnut St., Philadelphia, Pa.; *American Indian languages.*

Dorothea C. Woodworth (Mrs. Lewis A.), Assistant Professor of Latin and Greek, University of California at Los Angeles, Los Angeles, Calif.

Intensive courses in the study of the Chinese Language will be offered this year by the American Council Institute of Pacific Relations, from June 27 to August 20, at the University of Michigan, Ann Arbor, Michigan. Instruction will be under the direction of Dr. George A. Kennedy of Yale University. There will be two courses designed to give a practical knowledge of Chinese. One is for beginners, the other for more advanced students.

A limited number of tuition scholarships and grants-in-aid are available for those desiring to enroll for this summer session. Applications should be addressed to Jean W. Kennedy, 80 Howe Street, New Haven, Connecticut, not later than May first.

BOOKS RECEIVED

Under this heading will be acknowledged such works as seem to bear on the advancement of the scientific study of language.

The publicity thus given is regarded as a full return for the presentation of the work. Under no circumstances is it possible to comply with the requests being made by certain publishers for the return of books not reviewed quickly.

Reviews will be published as circumstances permit. Copies of them will be sent to the publishers of the works reviewed.

For further bibliographic information consult the annual list of Exchanges.

Accent, Intonation, Quantité. By LOUIS HJELMSLEV. (Studi Baltici 6. 1–57 [1937].)

Acta Philogica Scandinavica 11. 193–262 (1937).

Die aegyptischen Listen palaestinensischer und syrischer Ortsnamen. Pp. 62. By ANTON JIRKU. Leipzig: Dietrich'sche Verlagsbuchhandlung, 1937. (Klio 25, Beiheft.)

Aegyptus 16. 225–364 (1936); 17. 1–320 (1937).

American Council of Learned Societies, Bulletin, nos. 25, 26 (1936–7).

American Speech 12. 87–244 (1937).

Annals of the Bhandarkar Oriental Research Institute 18. 97–224 (1937).

Anthropos 32. 1–721 (1937).

L'Apprentissage du Langage; les deux premières années. Pp. 288. By ANTOINE GRÉGOIRE. Liége, 1937. (Bibliothèque de la Faculté de Philosophie et Lettres de l'Université de Liége 73.)

Atti del iv Congresso internazionale di Papirologia. Milan, 1936. (Publicazioni di 'Aegyptus'; Serie Scientifica 5.)

Archiv für das Stadium der neueren Sprachen 170. 161–320 (1936); 171. 1–288 (1937).

Archiv für Orientforschung 11. 191–415 (1936); 12. 1–104 (1937).

Archiv Orientální 9. 1–299 (1937).

Arkiv för Nordisk Filologi 52. 1–208 (1936); 53. 1–325 (1937).

Biblica 18. 1–484, 1*–120* (1937).

Bildhaftigkeit im französischen Argot. By IRMGARD SCHULTZ. Giessener Beiträge zur Romanischen Philologie 28 (1936).

Boletim de Filologia 4. 221–406 (1936).

Bolletino delle Publicazioni Italiane Nos. 423–34 (1936–7).

Brut y Brenhinedd; Cotton Cleopatra Version. Pp. xviii + 243.

By JOHN JAY PERRY. Cambridge: Medieval Academy of America, 1937. (Medieval Academy of America, Publication 27.)

Bulletin Hispanique 39. 1–96 (1937).

The Bulletin of the Institute for Research in English Teaching, Nos. 131–8 (1937).

Bulletin of the School of Oriental Studies (University of London) 8. 297–1201; 9. 1–265 (1936–7).

Commentationes 1. 1–43 (1937).

Les Composés de l'Avesta. Pp. xi + 277. By JACQUES DUCHESNE-GUILLEMIN. Liége, 1936. (Bibliothèque de la Faculté de Philosophie et Lettres de l'Université de Liége 74.)

A Contemporary of Shakespeare on Phonetics and on the Pronunciation of English and Latin. Pp. 21. By H. G. FIEDLER. London: Humphrey Milford, 1936.

Diccionari Català-Valencià-Balear. Fasc. 34–38 (cessió-consequència). Begun by ANTONI M^A. ALCOVER, continued by FRANCESC DE B. MOLL. Palma de Mallorca: 1936–7.

Emerita 4. 209–383 (1936).

Englische Studien 71. 321–440; 72. 1–160 (1937).

English Studies 19. 1–240 (1937).

Die Entstehung flektierender Sprachen. Pp. 108. By CARL MEINHOF. Berlin: Dietrich Reimer, 1936.

Études de Linguistique Japonaise. Pp. 161. By AIKITU TANAKADATE, ORESTES PLETNER, HENRI FREI. Paris: Paul Geuthner, 1936. (Bulletin de la Maison Franco-Japonaise 8. 1.)

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SOME GERMANIC DEVELOPMENTS OF IE **ĝeneu AND *ĝen-, **ĝenē

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In Ulfilas' translation of the New Testament, γονυπετῶ in Mark 10.17 is translated *knussjands*, and again in Mark 1.40 *kniwam knussjands*. Since the beginning of research in Germanic linguistics neither the meaning of *knussjan* nor its etymology has been satisfactorily determined. This article is an attempt to establish both, and also to classify several groups of words in the Germanic dialects which from time to time have been wrongly associated both with Goth. *knussjan* and with one another.

At first glance, *knussjands* seems to translate perfectly γονυπετῶν 'falling on the knees', but since the same participle is rendered *kniwam knussjands* it is possible that the translator did not feel that *knussjands* by itself completely conveyed the meaning 'falling on the knees' or 'kneeling'. We should rather assign, then, a more general meaning to the Gothic verb. This position was first taken by Kögel[1] who suggested that *knussjan* has 'einzig und allein die bedeutung "die stellung des bittenden annehmen", so dass dann *kniwam knussjands* heisst "mit den knien bittstellung annehmend" = γονυπετῶν.' This meaning has been accepted by Feist in his Vergleichendes Wörterbuch der Gotischen Sprache (3d ed. Leiden, E. J. Brill, 1936), and others,[2] but we are particularly interested in Kögel's explanation of the development of the verb *knussjan*, especially in his accounting for the troublesome *ss*-suffix. It appears immediately from the meaning he assigns to *knussjan*, that he is endeavoring to dissociate the *knu-* from IE *ĝnu, the reduced form of **ĝeneu 'knee', and that he has in mind such arguments as those of Joh. Schmidt, who would take *knussjan* from the reduced grade of the root mentioned and explain the *-ss-* as 'ein ableitungs-oder compositionselement'.[3] In fact Kögel takes *knussjan*

[1] R. Kögel, in Beiträge zur Geschichte der deutschen Sprache und Literatur 7.178.

[2] See Feist Vgl. Wört.[3] 314.

[3] Joh. Schmidt, in Kuhns Zeitschrift 25.52.

as a regular causative from **knudan* and allies both ON *knoða*, OHG *kneten* to the latter in the same manner as Goth. *trudan* agrees with ON *troða* and OHG *tretan*. He goes on to assert that the preterite participle of the Goth. verb was **knuss* from **knuttás* for **knettás* and that the accent on the *-ta-* suffix was responsible for the reduction of the original *e* to *u*. As further evidence for an original *t* in the root plus the IE suffix **-to-* and for the regular reduction of the strong grade *e* vowel to *u*, he offers OE *hnossian* 'strike' and compares it with OHG *hneotan* 'quassare', the former of which he takes (177) as a regular formation from a participle **hnoss*, standing for **hnut-tá-s*, analogous to the form which he posits as originally responsible for Goth. *knussjan*. Kögel's theory was accepted by Streitberg[4] and by Feist,[5] who felt that an etymology for *knussjan* which endeavored to establish a connection with IE ***ĝeneu* 'knee' would fail utterly to account for the *-ss-*. But Feist, in the first edition of his Etymologisches Wörterbuch, confused OE *hnossian* 'strike', a form quoted by Kögel in connection with *knussjan* (not as a cognate but merely to illustrate his general thesis), with OE *cnossian* and *cnyssan* 'beat, strike' (the latter of which Feist flagrantly misspells *cnyssian*), assuming that Kögel intended to connect etymologically *knussjan* with OE *cnyssan* and *cnossian*. This error of Feist's in interpreting Kögel has led many to assume, without further examination, the connection of *knussjan*, *cnyssan*, and *cnossian* and to draw conclusions from the connection which any careful consideration of the words shows at once to be untenable. That the words are related, though in a way quite different from any hitherto supposed, is the purpose of the first part of this article.

Before we leave those who in general follow Kögel, we should note especially that Hermann Güntert[6] finds in OHG *knetan* 'knead', G *kneten* 'knead', OE *cnedan* 'knead' as against ON *knoða* 'knead' and Goth. *knussjan*, an evidence for schwa secundum, though he, like Kögel, makes no statement as to how he surmounts a seemingly impossible semantic barrier. Hirt[7] follows both Güntert and Feist, for he assumes schwa secundum for ON *knoða* as against OHG *knetan*, OE

[4] W. Streitberg, Indogermanische Forschungen 23.117.

[5] In the first edition of his Etymologisches Wörterbuch der gotischen Sprache; Halle, Niemeyer, 1909.

[6] Hermann Güntert, Indogermanische Ablautprobleme 79; Strassburg, Trübner, 1916.

[7] Hermann Hirt, Indogermanische Grammatik, vol. 2, Der indogermanische Vokalismus 82; Heidelberg, Winter, 1921.

cnedan and for ON *knosa*, OHG *knussan*, OE *cnossian* and *cnyssian* (sic) as against OHG *knisten*. We must notice that though Feist and Güntert are obviously his authorities, he does not include Goth. *knussjan* in the series with *knosa* and *cnossian*.

Prior to a closer examination of the cognates of Goth. *knussjan* and the reaching of any independent conclusion, we must first briefly examine some other explanations of the form, especially Brugmann's which has been frequently accepted. Brugmann[8] in connection with IE *ĝnu beside IE **ĝeneu 'knee' finds *knu in Germanic 'in MHG. *knoche* "Astknorren, Knochen" Ober. Schles. *knutzen* "auf den knien hocken", vgl. got. *kniu*, Stamm *kniwa*.' In his note on the form *knutzen* he says 'Zu *knutjan "knien" war ein *knussus "das Knien" gebildet, wovon got. *knussjan* "auf die Knie fallen".' Theodore von Grienberger,[9] Kluge,[10] and Falk-Torp[11] all agree in supposing a *knussus 'das Knien' showing Germanic *knu from IE *ĝnu to account for *knussjan*. As opposed to all the above views, J. von Fierlinger[12] takes *knussjan* from an IE. *ĝnu-sthí or *ĝnu-sthá 'auf den knien befindlich', assuming that the *-sth-* of the suffix would become *-ss-* in Germanic in unaccented syllables, and offers by way of evidence *kussa-, *kussi- 'kuss' from IE *gustó-, *gustí-.

We have seen, then, various explanations for Goth. *knussjan*, and it will be noted that Feist, presumably on the authority of Kögel, connects OE *cnossian* with the Goth. verb; that Güntert places *knussjan* in ablaut relationship with OE *cnedan*, ON *knoða*, etc.; and that Hirt puts both OE *cnossian* and *cnyssan* in an analogous series with ON *knosa* as against OHG. *knisten*, but fails to include *knussjan*. And it is here that the crux of the problem lies. Kögel assumes that *knussjan* shows a *t* in the root plus a Germanic *-ta-* < IE *-to-suffix which regularly develops to *-ss-*.[13] Hirt assumes an *s* in the root in

[8] Brugmann, Gdr.[2] 2.1.181 and note 1; 445.

[9] Theodore von Grienberger, Untersuchungen zur gotischen Wortkunde 141; Sitz.-Ber. der k. Akad. der Wiss., Philos.-histor. Klasse, Bd. 142; Wien, 1900.

[10] Friederich Kluge, Etymologisches Wörterbuch der deutschen Sprache[11] 312; Leipzig, 1934.

[11] H. S. Falk and Alf Torp, Norwegisch-Dänisches etymologisches Wörterbuch 1.556 under *knæle*; Heidelberg, Winter, 1910.

[12] J. von Fierlinger in Kuhns Zeitschrift 27.440, note 2. But this is impossible on the face of it, since IE *-st-* or *-sth-* always gives Germanic *-st-*, and the etymology he cites as proof has never even been suggested before or since.

[13] For the best account of this development see Hermann Hirt, Handbuch des Urgermanischen, vol. 1 Laut- und Akzentlehre 120-21; Heidelberg, Winter, 1931.

OE *cnyssan* and *cnossian* and shows he is in doubt as to *knussjan*, since he includes it neither in the series for which he posits an original *s* in the root, nor in the one for which he assumes an original *t*. In other words, OE *cnyssan* taken by itself could be the result of Germanic **knus-jan* or **knut-jan*. The former stem would become by West Germanic gemination **knuss-jan*, and then by OE *i*-umlaut, *cnyssan*. The development from Germanic **knut-jan* would presuppose a participial or nominal formation **knut-tá* from which **knuss* would arise, which, plus the causative suffix *-jan*, would result in *cnyssan*, after OE *i*-umlaut had operated. But when we consider the phonetic and semantic equivalent of OE *cnyssan*, namely OE *cnossian*, we see immediately that the first of the above explanations must be eliminated, since *cnossian* too shows the *-ss-*, which we have assumed as the result of gemination in West Germanic, and there is no cause either for gemination or umlaut in *cnossian*, a weak verb of the second class. Therefore we have to assume that whatever the cause for the *-ss-* in both *cnyssan* and *cnossian* it was not the result of an original *s* in the root.

This fact brings us immediately to the possibility of the association of the two OE words with Gothic *knussjan*. Phonetically they agree perfectly, but semantically there appears to be difficulty. As we have seen, *knussjan* has been translated variously as 'to kneel, to assume the position of a suppliant, to be on the knees', while *cnyssan* and *cnossian*[14] are given as meaning 'strike, dash, beat, press, overcome'. But if we assume that the two OE words and the Goth. word are connected, which is almost certain on phonetic grounds alone, there must be a semantic connection. Therefore let us examine two of the earliest occurrences of each of the OE words. In Beowulf, 1326–28 where Hrōðgar is speaking of the dead Æschere, we read, *eaxlgestealla, ðonne wē on orlege | hafelan weredon, þonne hniton fēþan, | eoferas cnysedan* ... This is usually translated '(my) shoulder-companion when we in battle protected (our) heads when the foot-troops crashed together, (and) the boar-crests (of the helmets) smote'. Instead of 'smote' or 'struck together' or any such rendering for *cnysedan*, we see immediately that such a translation as 'fell' or 'fell humbled' would fit the picture better, and would also supply us with a logical result of the clashing together of the armed men. Again in the Seafarer, 6–8, we read ... *þǣr mec oft bigeat | nearo nihtwaco æt nacan stefnan, | þonne hē be clifum cnossað,* ... Toller translates the last half-line 'when it struck the cliffs'.

[14] Bosworth gives only *cnyssan* which he allies to Goth. *knussjan* which he translates as 'to press down'. Toller gives *cnossian* with the meaning 'strike'.

Of course the lines given are not describing a shipwreck, but merely a more or less routine, though rigorous, winter voyage. Realizing this, both Spaeth[15] and Kennedy[16] have translated *cnossad* by 'drove', though 'plunge', which Professor Kennedy suggested to me in conversation, would be more suitable both for keeping the root-meaning of the word and for describing more accurately the picture which the poet undoubtedly intended to convey. Even though the poet was primarily interested in describing a scene, still it is immediately apparent that the idea of 'humbling' is also contained in the verb. This is exemplified best perhaps in such passages as *Gaius Julius Cāsere Brettas mid gefeohte cnysede,*[17] where *cnyssan* should be translated 'humbled, brought to terms' while the secondary semantic extension 'beat, overcome' is easily accounted for. And it is through just such expressions as this, coupled with confusion with other roots of similar sound but quite different meaning, such as *hnossian* 'strike' (cf. *hnītan* 'clash together'), that the originally intransitive verbs *cnyssan, cnossian* developed transitive functions until finally we have *þū cnysest* equalling Lat. *elides*[17] and *cnyssan* used in such a context as *ongan ic wēpan and mīne brēost cnyssan,*[17] 'I began to weep and beat my breast'.

It remains, then, for us to account for the Germanic **knu* in the roots of *knussjan, cnyssan* and *cnossian,* as well as to explain the *-ss-* suffix, for the explanation of Brugmann, the only one that now appears at all possible, is simply an argumentum ad hoc, since it fails to account for OE *cnyssan* and *cnossian.* We must agree with Brugmann, however, in taking the Germanic **knu* as from IE **ĝeneu,* a light dissyllabic base, the zero plus reduced grade (Z + R) of which will give us **knu* in Germanic and is attested in Greek by γνύξ 'on the knees' and γνυπετεῖν 'to fall on the knees' and in Skt. by *pra-jñus* 'on the knees'.[18] Nor does the suffix *-ss-*, which can come only from an IE **-ts-* (now

[15] J. Duncan Spaeth, Old English Poetry 144; Princeton, Princeton University Press, 1927. Spaeth translates the lines quoted, 'Watching at night on the narrow bow, As she *drove* by the rocks. . . . '

[16] Charles W. Kennedy, Old English Elegies 55; Princeton, Princeton University Press, 1936. Kennedy's translation is, 'Bitter the watch from the bow by night As my ship *drove* on within sound of the rocks.'

[17] For the location of this see Bosworth under *cnyssan,* and Toller under *cnyssan* and *cnossian.*

[18] The Zero plus Strong (Z + S) grade will give, of course, Germanic **kneu* 'knee' from which Goth. *kniu,* OE *cnēo(w),* ON. *knē,* etc. all meaning 'knee'. The abbreviations L, S, R, Z are for lengthened grade, strong (normal) grade, reduced grade, and zero grade respectively.

that we have eliminated the possibility of IE *-tt-), longer need trouble us, since in Hittite the word for 'knee' is *genu* or *gēnu* (S + R or L + R) and the instrumental singular is *ganut* < IH **ĝъnéut(s)* showing the R + S grade. The ablative-instrumental ending in IH was **ts*, the zero grade of **tos* which developed in IE to *s* in the ablative except in the *o*-stems where we find *t* as in Skt. *dānāt*, OLat. *dōnōd*. The same sandhi conditions are responsible for the Hittite ablative ending *z* (= *ts*) and the Hittite instrumental in *t*.[19] It was obviously, then, this same IH **ĝъnéut(s)* 'on the knees, with the knees', which had been preserved in Indo-European as **ĝnuts*[20] (Z + R) and which performed much the same function in its time as Greek γνύξ did in its. The verb in Germanic formed from **ĝnuts* was **knussjan* 'to be humbled, to fall, to fall humbled, to be downcast' from which Goth. *knussjan*, OE *cnyssan*, *cnossian*, all with the same meanings. But we should notice that the original force of the IE **ĝnuts* may have been lost very early in Germanic, so that even at the time of the Gothic Bible Ulfilas might have felt it necessary to add *kniwam* in one instance with *knussjands* to be certain that the complete force of γονυπετῶν was rendered.

Having established *knussjan*, *cnyssan* and *cnossian* as coming ultimately from IE ***ĝeneu* 'knee', there remains the task of properly classifying many words in the Germanic dialects which have been associated together and with these words under the general meaning 'to strike, beat, injure'. Needless to say, no etymology which includes these words in such a grouping can be correct, therefore our purpose would best be served if we here attempt a new classification.[21] There existed in IE a **gne(u)t-*, *-d-*; **gne(u)s-*[22] with the general meaning

[19] Edgar H. Sturtevant, A Comparative Grammar of the Hittite Language §§93b, 102, 143c, 196, 197, 197c; Philadelphia, Linguistic Society of America, 1933. This explanation of the Hittite instrumental—that it is from IH R (ъ) + S grade of root plus zero grade *-ts of the IH ъ suffix *-tos—is Sturtevant's. There are, of course, other explanations.

[20] Such 'linguistic fossils' are common enough; compare for example retention of OE *ge-* in NE *handiwork* from OE *handgeweorc*, NE *aware*, OE *ge-wær*, archaic NE *yclept* < OE *ge-clypod*.

[21] The works used for this purpose have been: A. Walde and J. Pokorny, Vergleichendes Wörterbuch der indogermanischen Sprachen, vol. 1.582–3; Berlin and Leipzig, 1930; Franck's Etymologisch Woordenboek des Nederlandsche Taal[3]; Falk-Torp. vol. 1 (noted above fn. 9); F. Holthausen, Altenglisches etymologisches Wörterbuch, Heidelberg, Winter, 1934; W. Wissmann, Nomina postverbalia in den altgermanischen Sprachen, vol. 1, Deverbative *ō-* Verba 83; Göttingen, 1932.

[22] See note 27 below.

'strike, beat, press together'. From the former stem come such forms as OE *cnedan*, ON *knoða* 'to knead'; OE *cnyttan* 'knit'; OE *cnotta*, ON *knútr* 'knot'. From the latter stem come ON *knosa* 'to injure with blows', Dan. *knuse* 'bruise', OHG, MD *cnussen* (also OHG *cnusen*, MD *cnōsen*, *cnōsen*), D *kneuzen*, all meaning 'to push, beat'; OE *cnyllan* 'to strike, knell', MHG *knüllen* 'to strike', from an earlier **knuz-lōn*. Whether either of these roots in IE originally was from **ĝeneu* and whether there is any ultimate semantic connection between the knees and 'striking' and 'beating' is a question which cannot be decided scientifically, though the modern North Scotch dialect form *knuse* (*nuse*) 'to bruise', obviously borrowed from Danish *knuse* 'bruise', means as well, according to Wright's English Dialect Dictionary, 'to press down with the knees; to pommel, beat with the knuckles or fists'. However, that may be, it is no doubt through such forms that OE *cnyssan*, *cnossian* finally came to assume the meaning 'beat, strike'.

There is finally one more word, OE *cnūwian*, to which has been wrongly assigned the very popular meaning 'beat' due to its supposed connection with ON *knýja* (pret. *knúði*) 'to beat, drive up', or, used reflexively 'to struggle, exert one's self'. The OE word occurs twice in the Leechdoms,[23] and these occurrences are the only ones in the entire body of OE literature. It is necessary to quote the sentence immediately preceding the one in which the first instance of the verb occurs in order to see its proper meaning. We read,[24] *Eft celeþonian and wudubindels leaf geaces sure wiđ win gemenge*, which Cockayne renders, 'Again let him mingle with wine celandine and woodbine's leaves and the herb cuckoosour'. Then follows in §14, *Eft to miclum eagece cropleac niþoweard and witmæres wyrt niþoweard cnua on wine læt standan twa niht.* Now obviously the verbs *gemenge* and *cnua* are synonymous and the expressions *wiþ wīn gemenge* and *cnūa on wīne* are exactly parallel. Therefore the correct rendering of the passage would be, 'Again for a severe eye ache, *mix* (not *pound* as given by Cockayne) in wine the nether part of cropleek and the nether part of Whitmar's wort, let it stand two days.' The second occurrence of the verb is identical in form and meaning, for we read, *Gif cneow sar sie cnua beolenan and hemlic beþe mid and lege on*, which we can translate, 'If a knee is sore, mix henbane and hemlock, foment therewith and lay on'. That our meaning for *cnūwian* is the correct one is fur-

[23] Leechdoms, Wortcunning and Starcraft of the Anglo-Saxons (ed. O. Cockayne) 2.32.11, 2.340.15; 3 vols. Rolls Series, London, 1864–66.

[24] Following Cockayne, the quantities of the vowels are not marked.

ther attested by the quite frequent occurrences of the form *gecnūwian* in the Leechdoms (2.18.6, 2.12.19, 2.20.3, 2.94.6, 2.322.26, 2.30.6). Though Bosworth does not list the word, Toller notes the above instances and gives as the meaning 'to pound together'. The first of the above references, however, will serve as well as any to strengthen our conclusion that the true meaning of the word is 'mix, mingle' not 'pound' or 'beat', for we have . . . *genim rudan and wermod, gecnuwa and meng wiþ eced and ele* . . . or, 'take rue and wormwood, *mix* and mingle them with vinegar and oil'.

We are not able, however, to discard on semantic grounds alone the connection with ON *knýja* 'beat', but when we take into consideration OE *cnēowian*[25] 'to have carnal intercourse', OE *cnēow*,[25] ON *kné* 'generation', we realize that it is with these words and their cognates that *cnūwian* belongs. Now Walde-Pokorny 1.582 and Wissmann 110 take *cnēowian* 'to know carnally' and *cnūwian* (wrongly translating it 'pound') from IE **gn-eu* with the basic meaning 'beat', not realizing the correct derivation which is from IE **ĝen-* (beside the heavy dissyllabic base ***ĝenē*) 'generare'.[26] The root also appears in Greek ὁ γόνος and ἡ γονή 'descent, generation'. *Cnēowian* 'to have carnal intercourse', *cnēow*, and *kné* 'generation' must then go back to Gmc. **kneu*, from IE **ĝn-eu*, that is the zero grade of the root plus the *-eu-* extension which in its turn underwent ablaut gradation.[27] Again we may cite the parallel Greek form γονεύς 'father'. *Cnūwian* or *cnuwian* (we have no way of determining the quantity of the vowel) 'mix' shows either the reduced or zero grade of the *-eu-* extension, that is *cnūwian* would be from IE **ĝn-ʒu-* and *cnuwian* from IE **ĝn-u-*.[28]

There can be, then, only one explanation for ON *knýja* 'beat'. Both *knýja* 'beat' and *knúi* 'knuckle' must be derived from IE **ĝnʒu*, the

[25] One should be careful to distinguish this noun and verb from the homophonic OE *cnēowian* 'kneel' and *cnēow* 'knee', which come, of course, from the Z + S grade of IE. ***ĝeneu* 'knee'. See O. B. Schlutter in Hoops' Engl. Studien 43.306 on the meaning of *cnēowian* 'to know carnally'.

[26] I am indebted to my colleague, William M. Austin, who suggested to me that *cnēowian* 'to know carnally' and *cnēow* 'generation' must have this etymology.

[27] This *-eu-*, *-u-* extension is, of course, common in IE, and, in fact, it is only by assuming this *-u-* in the IE root **gne(u)t* that we can place OE *cnedan* 'knead' from IE. **gnet-* (cf. OChSl. *gnetą*, *gnesti* 'drücken') with ON *knoða* 'knead' and OE *cnotta* 'knot' with ON *knútr* 'knot', the latter from IE **gnʒudós*.

[28] It should be noted that OE *cnōsl*, OS *cnōsal*, OHG *knuosal* 'family' all show the Z + S grade of IE ***ĝenō-* 'know'. See Hirt, Der indogermanische Vokalismus 180.

Z + R grade of **ĝeneu 'knee', and the verb in ON must have meant originally 'to beat with the knuckles or fists', just as Danish *kno* or *knoe* 'knuckle' (cf. ON *knúi*) can be compared with a modern Norwegian dialect form *knua* 'beat with the knuckles'.[29] As for the apparent semantic difficulty of deriving a word for 'knuckle' from an IE word for 'knee', we have only to refer again to the North Scotch dialect form *knuse* which means beside 'bruise' also 'to press down with the knees' and 'to beat with the knuckles or fists'.

[29] See Falk-Torp under *kno* and *knuge*.

HITTITE EVIDENCE AGAINST FULL-GRADE *o*

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The familiar doctrine that Primitive Indo-European possessed a full-grade (accented) vowel *o* derives from Ferdinand de Saussure.[1] His thesis was that in addition to *ŏ* in ablaut relation to *ĕ* there was another vowel which appeared in Greek and Italic as *ŏ* and which was due to the influence of a certain lost consonant of undefined character. He had three reasons for this assumption: (1) Greek and Italic present in certain words an *ŏ* that is not observed to alternate with *ĕ*. (2) Greek and Italic *o* in open syllables corresponds to Indo-Iranian *ā*, according to 'Brugmann's law'; but sometimes Graeco-Italic *o* corresponds to Indo-Iranian *a* in open syllables, and de Saussure held that such pairs came from the full-grade, non-ablauting *o*. (3) Armenian usually responds to Graeco-Italic *o* with *o*, but in certain words with *a*, and in these words again de Saussure found full-grade, non-ablauting *o*. A fourth reason was later supplied by Zubaty,[2] who held that in Lithuanian full-grade *ō* appears as *uo* while original *ō* in ablaut relation with *e* or *ē* appears as *o*.

In 1900 Holger Pedersen[3] showed that there is no correlation between the four lines of assumed evidence. Indo-Iranian frequently has *a* in an open syllable beside Graeco-Italic *o* in ablaut with *e* (e.g. Skt. *katarás* : Gk. πότερος). Armenian *a* sometimes corresponds to *o* in ablaut with *e* (e.g. *asr* 'fleece' : Gk. πόκος 'fleece', πέκω 'comb'). Lithuanian *uo* sometimes appears in the *e*-series (e.g. Lith. *súodžiai*, OIcel., OE *sōt* 'soot' : IE *sed-* 'sit'). Armenian *a* never corresponds with Indo-Iranian *a* in an open syllable when this represents Indo-European *o*. Lithuanian *uo* sometimes appears alongside of Armenian *o* (e.g. Lith. *úodžiu* 'smell' : Arm. *hot* 'odor'). There remains, therefore, only the fact that the relatively few languages that distinguish *o*

[1] Mémoire sur le système primitif des voyelles dans les langues indo-européennes 96–116 (1879) = Recueil des publications scientifiques 90–109.

[2] BB 18.241–66 (1892).

[3] KZ 36.86–103.

both from *e* and from *a* not infrequently exhibit *o* in a word-group that nowhere presents an indubitable *e*.

This state of affairs, as presented by Meillet and especially Pedersen, led Brugmann to withdraw his former adherence to de Saussure in this matter; he says (KVG 74):

> Die Ansicht, dass es im Uridg. zwei qualitativ verschiedene *o*-Vokale gegeben habe . . . , steht auf schwachen Füssen.

Hirt[4] justifies his assumption of a full-grade *o* as follows:

> Ausser den zahlreichen Wörtern mit *ȯ*, neben denen solche mit *e* stehen, gibt es aber eine ganze Reihe, neben denen keine mit *e* auftreten. Das kann in einzelnen Fällen auf Zufall beruhen, indem die Worte mit *e* verloren gegangen sind, aber in der Hauptsache dürfte von einem Zufall keine Rede sein, da die Fälle zu zahlreich sind.

He goes on to say that the supposed Indo-Iranian and Armenian evidence for two original *o*-vowels is illusory. Hirt[5] is, however, inclined to accept the doctrine that Lithuanian, with certain exceptions, still distinguishes between full-grade *ō* and *ō* in ablaut with *e* or *ē*.

This feature, like so many others of our reconstructed Primitive Indo-European, has within recent years come to stand in a very different light as a result of newly discovered linguistic material and the more or less general acceptance of the laryngeal hypothesis. Consequently the whole question must presently be reexamined. The purpose of this paper, however, is more modest; I plan merely to examine the additional evidence from Hittite.

The new Hittite material removes a number of the words from the list of those that seem to contain full-grade *o* by giving us forms with full-grade *e* or *a*, and thus compelling us to assign the previously known forms to the *o*-grade. Full-grade in *e* is attested by the following:

Hitt. *nekuz* 'evening', *nekuz* (gen.) *mehur* 'time of evening' : Lat. *nox* 'night'. The full-grade appears also in Hittite verb forms *nekuzi* (act. pres. 3 sg.) and *nekuttat* (midd. pret. 3 sg.). Walter Couvreur[6] has recently discussed this etymology and has satisfactorily explained the phonology involved. He wrongly translates *nekuz* as 'night'; apparently he does not know Sommer's[7] proof that the meaning is

[4] Idg. Gramm. 2.29 (1921).

[5] Idg. Gramm. 2.32 f.

[6] De Hettitische *ḫ*, een bijdrage tot de studie van het Indo-Europeesche vocalisme 171–3 (1937).

[7] Boghazköi-Studien 7.32–6 (1922).

'night-fall' or something similar. He objects particularly to my[8] suggestion that the active verb means 'he goes to bed' or 'he undresses'. Since this suggestion of mine has never been presented as clearly as it should be, I take this opportunity to restate it. Sommer (36) suggested that the middle *nekuttat* (KBo. 5.8.3.19) meant 'es wurde Abend', and when I found the active *nekuzi* in the Yale Hittite tablet = VBoT 24.1.10, I at first[9] translated it 'evening comes', assuming that active and middle were precisely equivalent. Later I saw that Hitt. *nekuz* and *nekumanza* 'naked' might easily be parallel derivatives of the same root; *nekuz* seems to be a verbal noun in the suffix *-t-* from the verbal root *neku-*, and *nekumanza* a derivative in the possessive suffix *-mant-* from the corresponding root noun. The precise meanings to be assumed are necessarily conjectural, but the requirements of the situation seem to be fully met if we suppose that the active verb *neku-* means 'undress, go to bed'. In VBoT 24.1.10 the context provides a suitable subject: 'When he goes to bed, upon the sacrificer . . . '[10] Then the impersonal middle would have to mean 'people undress, people go to bed, it is bed-time' (cf. Lat. *pugnatur* 'people fight, there is fighting'). The verbal noun *nekuz* should mean 'undressing, going to bed, bed-time, evening'; the Indo-European meaning 'night' is an easy development from the point where the Hittite stops. In order to connect the meaning of *nekumanza* we must suppose that the root noun *neku-* meant 'nakedness'. But, whatever one may think of the original meaning of Hitt. *nekuz* and of its connection with *nekumanza*, there is no doubt of its connection with IE *nokt(i)-* 'night'.

Hitt. *nekumanza* 'naked' : Lat. *nūdus*, OIr. *nocht*, Lith. *núogas*, etc. In my opinion these words belong to the same group as those discussed in the preceding paragraph; I list them separately because OIr. *nocht* has hitherto held an independent place in the question of full-grade *o*, and because some who do not accept the connection of Hitt. *nekuz* and *nekumanza* nevertheless accept the connection of *nekumanza* and OIr. *nocht*. Several of the Indo-European words for 'naked' evidently differ altogether from the Hittite word in their formative material. The Indo-Iranian and Greek words, however, seem to be more closely connected; while Hittite contains the suffix *-ment-* in zero grade (IH *négu-mṇt-*), they show the equivalent suffix *-men-* in zero grade plus the thematic vowel. Gk. γυμνός must come

[8] JAOS 52.10 f., HG 122 f.
[9] TAPA 58.7 (1928).
[10] Cf. Sturtevant and Bechtel, Hittite Chrestomathy 106 f.

from **ŋgu-mnós*; how the initial nasal was lost is not clear, but lost it must have been if we are to connect the word with this group at all (see my suggestion in LANG. 6.222). Av. *maγno* (< **naγmas*?) and Skt. *nagnás* seem to imply Indo-Iranian *nъgmnós*, and this may be a contamination of IE *ŋgumnós* with *nъgwmъnós*.

Hitt. *beda-* 'dig' : Lat. *fodio* 'dig'. It seems strange that this Latin word should ever have been thought to contain full-grade *o*, in view of Lith. *bedù* 'dig'. It is to be hoped that the discovery of the Hittite cognate will suffice to convince everyone of what was already clear enough. I have recently suggested[11] that the Hittite word should be connected with IE *bheid-* 'split, cleave'; but all will agree that the etymology now proposed is semantically preferable. The full grade of the roots *bheid-* and *bhedh-* would appear the same in Hittite, and no other grade of *beda-* 'dig' has yet come to light. In my recent article I undertook to prove that we must read the Hittite word *be-da-* rather than *píd-da-*; the new etymology is quite as cogent as the other in this respect, for neither original *d* nor original *dh* can be written double in Hittite.

It is well known that in several words the recognition of a full-grade *o* is tied up with the wide-spread but nevertheless doubtful assumption that in prehistoric Latin *ov* became *av* under certain circumstances. Such pairs as Lat. *lavo* : Gk. λόω 'wash', Lat. *caveo* 'be on one's guard' : Gk. κοέω 'perceive', Lat. *cavus* 'cave' : Gk. κόοι; 'hollows' (Hesych.) may be assigned to the *a/o*-series or they may be assigned to the *o*-series on the assumption that the Latin *av* is secondary.[12]

Probably the strongest piece of evidence in favor of the assumed Latin change is furnished by the pair *lavo* : λόω. Many of the other pairs involve doubtful etymologies, or concern words of doubtful authenticity,[13] while several of them involve difficulties for the exact formulation of the law.[14] It is precisely this clearest case that Hittite definitely assigns to the *a/o*-series.

Hitt. *lah(h)u-*, *lah(h)uwa-* 'pour', *lahhus* 'basin, bowl' : Lat. *lavo* 'wash'. There can no longer be any doubt that Hittite *h* has two different values and that one of these is the representative of an original laryngeal of *a*-color (which I write *x*, following Sapir). The two

[11] LANG. 13.286 f. (1937).

[12] So Brugmann, Grundriss 12.155; Hirt, Idg. Gramm. 2.181.

[13] Cf. Fay, Studies in Honor of Gildersleeve 189–203.

[14] See Kretschmer, KZ 37.274–6; Buck, Comparative Grammar of Greek and Latin 84.

values of *h* initial can be distinguished only by etymology, but the medial *h* from *x* may often be detected by double writing.[15] The Hittite verb for 'pour' is usually written with single *h*, but there are at least three instances of the double writing: *la-aḫ-ḫu-u-wa-i* (KUB 9.31.2.9), *la-a-aḫ-ḫu-wa-tin* (KUB 13.3.3.2), *la-aḫ-ḫu-tin* (KUB 13. 3.2.28). The noun is always written with *hh*. This proves that we are dealing with Indo-Hittite *láxu-* and IE *láw-*;[16] Latin *lavo* therefore contains a full-grade vowel, while Greek λόω must be assigned to the *o*-grade.

This elimination of the strongest evidence for the assumed change of *ov* to *av* in prehistoric Latin, tips the scales in favor of the alternative explanation in the remaining cases. As far as the alternation between Latin *av* and Greek ο(ϝ) is etymologically secure we may confidently ascribe it to the *a/o*-series.

In four Hittite word-groups initial *h* precedes an *a* that seems at first sight capable of being identified with a full-grade *o*.[17]

Hitt. *haras*, gen. *haranas* 'eagle' : Gk. ὄρνις.

Hitt. *hark-* 'be destroyed' : OIr. *orgaim* 'ich schlage, erschlage', Gallic *Orgetorix*.[18]

Hitt. *hastai* : Gk. ὀστέον, Lat. *os* 'bone'.

Hitt. *happar* 'price', *happara(e)-*, *happira(e)-* 'sell', *hap(p)iris* 'town' : Lat. *opus* 'work'. This etymology was proposed by Sapir, loc. cit., when he supposed that the Hittite noun meant 'business transaction'.

[15] The prevailing value of Hittite *h* as representing an *a*-colored laryngeal was originally maintained by Kurylowicz, Symbolae Grammaticae in Honorem Ioannis Rozwadowski 95–104 (1927). This doctrine has been slightly modified by its author in Études indo-européennes 27–76, 253–5, and it has been widely accepted. Reference should be made to an independent treatment by Couvreur, De Hettitische *ḫ*, een bijdrage tot de studie van het Indo-Europeesche vocalisme (1937). The existence of a second value of Hittite *h* has been recognized by Sapir, LANG. 10.276 fn. 4 (1934); Couvreur, *op. cit.* 140–6, 189–94, 252–6; Sturtevant, LANG. 12.186, 210–13, 14.12 f. While there is difference of opinion about this second value of Hitt. *h*, all the scholars named are agreed that the double writing of Hittite *h* indicates an original laryngeal of *a*-color.

[16] Couvreur, De Hettitische *ḫ* 189–94 (1937), did not know of the double *h* in forms of the verb, and so, by denying the connection of *lahhus* 'basin', he was able to equate Hitt. *lahu-* with Gk. λόω on the basis of **loγu-*. This is no longer possible.

[17] Couvreur, De Hettitische *ḫ* 141–3, finds full-grade *o* after the assumed *o*-colored laryngeal in the first three of these. Sapir, LANG. 12.179 and fn. 15, finds the same initial group in the fourth word in our list. The remaining words in which Couvreur finds this initial group do not deserve serious consideration.

[18] Cuny, RHA 2.204 f.

In spite of the subsequent correction of meaning and of the fact that the Hittite noun is an *r/n*-stem and the Latin noun an *s*-stem, the etymology still seems possible, although far from certain.

There are two difficulties in the way of tracing initial *ha-* in these words to original *γo-* (I follow Sapir in writing *γ* for his assumed *o*-colored laryngeal). In the first place Hittite exhibits several words that have no *h*, but that have as strong a claim as any to a full-grade *o*.

Hitt. *ais*, gen. *issas* : Lat. *ōs*, gen. *ōris*, Skt. *āsā* 'mouth', Lith. *úostas* 'mouth of a river'. The discovery of the Hittite word makes it necessary to revise the current opinion that the Indo-European word contains an original *ōu*. Hence we must give up the connection with Skt. *oṣṭhas* 'lip' and the words for 'ear' (Lat. *auris*, etc.); but these etymologies had little in their favor anyway. Hitt. *ais* and Lat. *ōs* both derive from IH *ō(i)s* with lengthened grade *ō*.

Hitt. *para* 'before, forth, henceforth' : Gk. *πρό*, Skt. *pra*, Lat. *prŏ*, Av. *frā*.[19]

Hitt. *pas-* 'swallow' or 'drink' : Gk. *πῶμα* 'a drink', Lat. *pōtiō* 'a drinking, a drink'.[20]

Hitt. *da-* 'take' : Gk. *δίδωμι*, Lith. *dúoti* 'give'.[20]

The second difficulty with tracing Hitt. *h* to an *o*-colored laryngeal is the rather strong, although limited, evidence in favor of interpreting *h* in its second value as representing the glottal stop, that is, the laryngeal that had no influence upon vowel quality.[20a]

weh- (pret. 1 sg. *ú-e-ḫu-un*, midd. pres. 3 sg. *ú-e-ḫa-at-ta*, *ú-e-ḫa-at-ta-ri*, pl. *ú-e-ḫa-an-ta-ri*, *ú-e-ḫa-an-da-ri*) 'turn' : Lat. *viēre* 'weave'. I have long been convinced[21] that this Hittite verb must be connected with the Indo-European roots *wei-*, *wendh-*, *wer-*, *wel-* 'turn', but I have just recently seen the more precise relationship here posited. The laryngeal hypothesis requires us to trace Lat. *viēre* to an ablaut base *weyé'-*, and the presence of the laryngeal is further evidenced by the long vowel of Lat. *vītis* 'grape vine', Lith. *výti* 'turn', and Skt.

[19] On this word see Sturtevant, Mélanges Boisacq.

[20] Couvreur, De Hettitische *ḫ* 205-7 (1937), rejects these etymologies, as he must in order to maintain his thesis; but see Sturtevant, LANG. 14.76 f.

[20a] Since this paper was sent to the printer I have become convinced that Hittite *h* written single between vowels represents not ', but a voiced sound corresponding to *x*; cf. Speiser's Notes on Hurrian Phonology in JAOS 58. I should now employ *γ* instead of ' in reconstructing the Indo-Hittite prototype of *weh-*, *sehur*, *mehur*, etc. It follows that *γ* did not induce *o*-color in a neighboring vowel any more than ' did.

[21] See LANG. 4.161, 12.186.

vītás 'twined, entwined' (zero grade **wi't-* > IE *wīt-*). The Skt. present *váyati* 'weaves' must then be traced to IH *wéyə'-* > IE *wéye-* with the familiar shift to the thematic conjugation. The first full-grade of the same base would be **wéye'-* > IH *wéi'-* > Hitt. *weh-*. This etymology accounts easily for Hittite *h* in full-grade forms before an ending beginning with a vowel (e.g. pret. 1 sg. *wehun*, midd. *wehatta*). Since ' before a consonant was lost, forms like *wehzi* must be analogical. The weak-grade *wah-* (e.g. 3 pl. *wahanzi*, part. *wahanza*) must be due to the influence of such inherited pairs as *es-* : *as-* 'be', *wess-* : *wass-* 'clothe', *ses-* : *sas-* 'sleep'.

Hitt. *sehur* 'urination, urine' : OIcel. *sūrr* 'sour', *saurr* 'male semen', Lat. *sēmen*.[22]

Hitt. *mehur* 'time' : IE *mē-* 'measure';[22] for the semantics, cf. Goth. *mēl* 'time', OIcel. *māl*, OE *mǣl* 'measure, point of time, mealtime', etc.

It is perfectly clear that Hittite loses the glottal stop before consonants (*epzi* 'takes, begins': Skt. *ápnoti* 'obtains', Lat. *coēpi* 'I began'). There is, as far as I know, conclusive evidence for its preservation only in the intervocalic position, although it may also have survived initially before vowels,[23] and between a consonant and a vowel.

If, then, we refuse to derive the initial *h* of Hitt. *haras*, *hark-*, and *hastai* from an *o*-colored laryngeal we must reconstruct the Indo-Hittite archetypes of Gk. ὄρνις, OIr. *orgaim*, and Lat. *os* as *xorn-*, *xorg-*, and *xost-* or as *'orn-*, *'org-*, and *'ost-*. Of the corresponding Hittite words *haras* and *hastai* probably are also *o*-grade forms (IH *xor(n)-* and *xosti-* or *'or(n)-* and *'osti-*), but *harkzi* 'is destroyed' may perfectly well be a full-grade form (IH *xárg-ti* or *'érg-ti*).

Hittite, then, establishes a full-grade in *e* beside Lat. *nox* 'night', OIr. *nocht* 'naked', Lat. *fodio* 'dig', a full-grade *a* beside Gk. λόω 'wash', and probably in all other words that show Lat. *av* beside Gk. ο(ϝ). We must further assume either full-grade *a* or full-grade *e* beside Gk. ὄρνις 'bird', OIr. *orgaim* 'I destroy', Gk. ὀστέον 'bone', and perhaps beside Lat. *opus* 'work'.

This so far reduces the number of apparent instances of full-grade *o* that there remains little plausibility in assuming such a full-grade vowel, unless additional evidence can be adduced from Tocharian or

[22] See Sturtevant, LANG. 12.184 f.

[23] I do not know of any valid reason for accepting the usual dogma of de Saussure's followers that we must assume an initial glottal stop for all words that would otherwise begin with *ĕ* or *ŏ*.

some other language. Hittite has so far presented no evidence for a full-grade in *e* beside any of the so-called heavy bases in which full-grade *ō* has been thought to appear, but such bases are few in number, and the laryngeal hypothesis removes the category of heavy bases from our grammatical system; if IE *dō-* 'give' is a full-grade form, it comes from earlier **doγ-* and thus belongs in the same category as the assumed full-grade IE *low-* 'wash', etc. Consequently there is no reason for treating IE *o* and *ō* separately; both vowels seem to indicate *o*-grade, unless they are of secondary origin.

PHONOLOGICAL SHIFTING IN AMERICAN NORWEGIAN

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Problems connected with the borrowing of words from language to language have occupied the attention of many linguists. In most cases this attention has been focused on languages of the remote past, where opportunity for first-hand phonetic observation has been cut off entirely. In the meanwhile borrowing has been going on within the United States on a scale rarely equalled in history, between English on the one hand and all the immigrant languages on the other. Here it becomes possible to record by modern means and to study by modern methods the phonetic form of each word in each of its incarnations, foreign, American, or hybrid. In the following study of one such dialect the writer wishes to present a concrete instance of the possibilities inherent in this kind of research.

My informant concerning the particular dialect of American Norwegian here discussed is Mr. Odin Anderson, a student at the University of Wisconsin without previous training in linguistics, whose interest had been aroused to the extent that he spent many weary hours preparing a tentatively complete list of the words employed in his Norwegian dialect. That this list is not actually complete is shown by the fact that it contains only some 5500 words (of which about 1300 are English loanwords), which is probably too little for any actual language. In spite of this, it must be regarded as an impressive contribution toward complete knowledge of this very interesting dialect. The gathering of words has reached a point at which new words do not suggest themselves easily, but come to mind only rarely and fortuitously.

This vocabulary represents the speech of a specific family living near the town of Blair in western Wisconsin, in the midst of a solidly Norwegian country community. No living member of this family has ever seen Norway,[1] and my informant is a third generation immigrant. His grandfather and greatgrandfather emigrated from Norway and

[1] Previous to the summer of 1937, when my informant visited Norway.

settled in this locality in 1854, when it was still a wilderness. In spite of the early date of settlement it is only within the past decade that the younger generation has been breaking away from the linguistic tradition of its ancestors. The strength of religious conformity has been a factor in this retention, for the church represents one of the few cultural traditions that were bodily transferred to American soil by these immigrants. In most other fields their Americanization has been rapid and complete, antedating by far the linguistic transition which is only now taking place.

The linguistically dominant members of my informant's family, as well as most of their neighbors, derive their speech from the Norwegian dialect of Solør, a district in southeastern Norway, about 60 kilometers northeast of Oslo and immediately adjacent to the Swedish border. When the speakers of this dialect were transplanted to American soil and subjected to constant and powerful influence from the surrounding American milieu, a more or less gradual transformation of their speech became inevitable. They brought with them the capacity of forming certain linguistic signals with which they had manipulated their old environment. Having been accustomed to spin and weave their own clothes, prepare their own shoes, do their own blacksmithing, they had a wide and detailed terminology in these fields. Being orthodox Lutherans, they were well trained in the distinctions of the faith, though their vocabulary here was not native and local, but inculcated in school and church from Catechism and Explanation. Although their principal occupation both in Norway and in America was farming, it was far from identical in the two countries. In their old home most of them had lived under a self-contained economy, while in Wisconsin they had to market their produce and use the money to purchase factory-made products. They were met by new types of implements, new kinds of barns and houses, new conditions of agriculture, and even some of the old objects seemed unfamiliar in these strange surroundings. All these novel objects and situations demanded new linguistic signals if the proper responses were to be made. In most cases the new signals were chosen from the system already present, namely the English language, and simply added to the store of word-patterns in their minds. At the same time the word clusters centering around the old technical processes and implements were gradually forgotten, and were of course not transmitted to the next generation. In some such fashion there has come about a complete rearrangement of the word material in our American Norwegian dialect, with the

undiscarded Norwegian words and the newly acquired English words together forming new word-clusters just as inseparable and stable as the old ones.

The process here hastily and inadequately sketched is of course a well-known phenomenon, characteristic of all foreign dialects in America, as well as of all languages not in complete isolation. It is generally referred to under the inaccurate metaphor of 'borrowing', and the result is called a 'mixed language', a description which is true only from a historical point of view. These terms obscure the fact that what has really taken place is a shift in structure and emphasis, correlated to a shift in cultural and social form. The basic vocabulary patterns of the language are gradually moving towards English and are thereby easing the eventual transition to the phonetic and morphological patterns of that language.

When, however, the speakers of a language are under such constant influence from another language, it is possible to observe also changes in the phonological pattern analogous to those of the vocabulary. The old distinctions of sound may be broken down, new distinctions may arise, and the result is a new structure which easily accommodates the new vocabulary that constantly streams into the language. To illustrate this progressive shifting of structure I shall describe the history of two sound ranges in the dialect of my Blair informant.

The dialect of Solør in Norway has four distinguishable sounds within the range usually known as 'liquids'. It has a tongue-trilled *r*, an untrilled, palatal retroflex *r* (*ṛ*), a clear dental *l*, and a so-called 'thick' *l*, which is really identical with the untrilled *r* except for a final flap of the tongue.[2] In my informant's speech (as apparently in that of many other Norwegians in America) the flap is lost, so that retroflex *ṛ* and 'thick' *l* become indistinguishable. This is not a significant loss, as the two sounds cannot occur in phonemic opposition to one another.[3] There are accordingly three distinct sounds in the liquid range of my informant's dialect: *r*, *ṛ*, *l*. These sounds are not wholly independent of one another. The *r* cannot occur before dentals, where it alternates with *ṛ* (*stor* 'big', neuter *storṭ*). The *l* does not usually occur in certain positions (e.g. after labial consonants, or back vowels), where it

[2] Amund Larsen, Lydlæren i den solørske Dialekt især i den Forhold til Oldsproget. Vid.-Selsk. Skr. II, Hist-filos. Kl., 1894. No. 4.18 (Kristiania, 1894).

[3] Words ending in 'thick' *l* acquire retroflex *r* instead when a dental (*l*, *n*, *t*, *d*) follows.

generally alternates with 'thick' *l*, or (in my informant's speech) with *ɾ*.[4]

If we compare this scheme with that of the American English liquids, we see certain striking facts. English has only two sounds in this range, *l* and *ɾ*. The *l* is darker than the Solør *l*, but the *ɾ* is untrilled and retroflex so that it approximates the Solør *ɾ* very closely.[5] If we compare the sounds of the two languages on the basis of their physiological (and acoustic) resemblance, their relation might be somewhat as follows:

American English	Solør
	r (trilled)
ɹ̩ (untrilled)	ɹ̩ (untrilled)
l (dark)	
	l (clear)

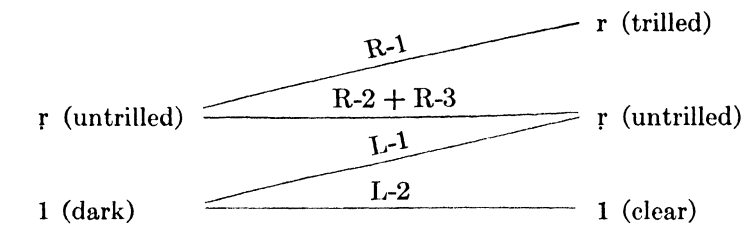
An offhand examination of this diagram might lead one to suppose that any English words adopted into the sound system of Solør would keep the untrilled *ɾ* for English *ɾ* and *l* for English *l*.

But a study of the words in question shows quite a different situation. The words of English origin being used by our third-generation speaker of Solør which contain *r* may be divided into the following groups. In a group which we shall call R-1, English *ɾ* has become Solør trilled *r*. In another group, R-2, *ɾ* has become untrilled *ɾ*, even though it was not situated before *n*, *l*, *t*, or *d*. In a third group, R-3, *ɾ* has also become untrilled *ɾ*; this group includes all words in which the *ɾ* appeared

[4] According to the previously cited work by Amund Larsen (a native of the district and an eminent linguist) *l* becomes cacuminal in the following situations: intervocalic or final, when it is short and follows some other vowel than *i*, *iː*, and *ei* (121–2); before labial and velar consonants (98, 102); before dentals without following consonant (122); after *y* before *j* (123); after labial and velar consonants (104, 106). In all positions exceptions may occur in the case of late loan words: *smal* a., *sal* m., *silke* m., *salme* m., *klær* a., *kløver* m., *støvvel* m.

[5] In pronouncing [ɹ̩] and [l̩] the tongue elevation is almost identical with that of American [r] (retroflex tongue-point and tongue-blade semivowel); cf. Kenyon, American Pronunciation[6] 44, and Larsen 14 and 18. From personal observation I should say that Norwegian [ɹ̩] is tenser than American [r], with more lateral contraction and a larger resonance chamber. The [l̩] is distinguished from [ɹ̩] solely by a slap of the tongue point on the alveolar ridge at the moment of release; as there is no slap before a following dental, the two are not distinguished in this position even in the original dialect. In my informant's speech the slap is entirely lacking, so that his [ɹ̩] and [l̩] are indistinguishable. Several of his transcriptions show confusion of these sounds.

before *n*, *l*, *t*, or *d*. The words containing *l* may be divided as follows: L-1, those in which English *l* became *r* in the positions where this sound occurs for *l* in Solør, and L-2, those in which English *l* became Solør *l* in all positions. To give a clear perspective of the situation, the relationships may be diagrammed as follows:



If we consider words containing the same sounds as constituting 'word-sound' groups, the change occurring here may be described as a breaking up of such groups. The word-sound group R has been split between trilled and untrilled *r*, and the group L between clear *l* and untrilled *r*.

In the history of languages this phenomenon is common enough, but it is usually part of a combinatory sound change. Here there is nothing in the sound or form of the words to suggest that we are dealing with a typical 'sound law'. Instead it becomes necessary to consider the meaning, the frequency, and the probable date of introduction of these words into the dialect. The words in group R-1 and group L-1 are all simple, everyday words which must have become part of the immigrants' vocabulary soon after arrival. They are words like *barrel*, *beer*, *country*, *crackers*, *drag*, *grab*, *radish*, *reap*, *screen*, *street*, *tramp*, *blanket*, *bluff*, *plenty*, *plug* (of tobacco). In groups R-2 and L-2, however, we find words of more recent vintage and less frequent occurrence, such as *airmail*, *ampere*, *arithmetic*, *ballbearing*, *bracelet*, *camera*, *detour*, *protection*, *writing desk*. Some of these may even be said to have an insecure foothold in the dialect, e.g. *writing desk*, which is said to alternate with the semi-Norwegian *skrivadesk*. In groups 1, then, sounds are substituted as if the speakers had been aware of the spelling equivalents, while in the latter groups the words are taken over with their nearest acoustic equivalent.

Group R-3 falls outside this development, in that here the English *r* was prevented from becoming trilled *r* by the following dental; in the system of Solør only untrilled *r* can occur in this position. Hence the words in this group are ambiguous and include old words like

barn, cart, cord, as well as newer ones like *cornstarch* and *department*. There are more words belonging to this group than is immediately apparent. A number of words ending in *r* must obviously have entered the dialect early, and are in all other respects thoroughly assimilated. Examples of these are *fair* (n.), *river, sewer,* and *square* (n.). In *river* the first *r* is trilled, but not the last, so that this would seem to make its classification difficult. The explanation is a striking confirmation of our grouping: these words are all borrowed as masculine nouns, with the ending *-n* in the definite form. They also belong to the type of words that are more common in the definite than in the indefinite form, and in this form the *r* remained necessarily retroflex: [ˈfæ·ṛṇ], [ˈrøvəṛṇ], [ˈsü·ṛṇ], [ˈskvæ·ṛṇ]. By false subtraction a new indefinite was then formed, still retaining the retroflex *r*. This is the only possible explanation of the striking forms which the word *barn* received in this and other dialects. The indefinite form is dissyllabic, and lacks the *n*: [ˈˈba·ṛə], with a regularly formed plural [ˈˈba·ṛər], definite [ˈˈba·ṛa]. All of these forms must have arisen from a dissyllabic definite singular [ˈˈba·ṛˈṇ], 'the barn', in which the *r* was kept untrilled by the following *n*.

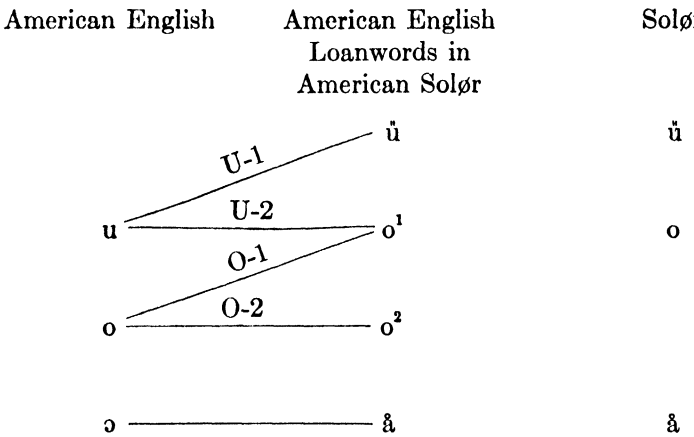
A parallel development can be shown for the long rounded vowels *o* and *u*. The characteristic of the Solør dialect most noticeable to other Norwegians is its treatment of *o, u,* and *y*. *O* is articulated a shade higher than usual even in Eastern Norway, and *u* is fronted and overrounded, so that the *o* and *u* impress other Norwegians as respectively *u* and *y*. *Y* on the other hand is unrounded, so that it becomes identical with general Norwegian *i*. As a result the vowels *o* and *u* are markedly different from their English kinsmen. The relationship may be roughly diagrammed as follows:

American English	Solør
	ü
u	o
o	
	å
ɔ	

American *u* approximates most nearly to Solør *o*, but American *o* is rather higher than Solør *å*.[6]

[6] A comparison of the descriptions in Larsen 12–13, and Kenyon 66, indicate that the American sounds are respectively high, mid, and low back rounded, while the Solør sounds are high central, high back, and low back rounded. This leaves the mid back position unoccupied, where American [o] is imported.

If we now examine the situation in American Solør, we find that in the loanwords borrowed from American English, a new vowel distinction has been introduced, so that instead of two oppositions in this range, there are three.



Here, as with L and R, we can divide the loanwords into two groups, the early groups in which English *o* and *u* became Solør *o* and *u*, in spite of the great dissimilarity of these sounds, and the late groups in which *u* was made into its nearest acoustic equivalent *o*, and the American *o* was introduced bodily into the vowel pattern. The new pattern, accordingly, distinguishes between two *o*'s, the o^1 of the old dialect, and the o^2 of American English. Words from groups 1 are: *coat, vote; deuce, flu, lose, sewer, tools, tube.* Words from groups 2 are: *avenue, bluebird, bootlegger, nephew, perfume; banjo, calico, crowbar, earphone, explode, roadster, swallow.* A striking example of this division is the word *poker*, which has been borrowed in both groups. The game is rendered with Solør *o* [ˈpo¹·ker], and accordingly belongs to group O-1. But the implement, which entered the family vocabulary in recent years when a furnace was installed, is rendered with English *o* [ˈˈpo²·keṛ].

It thus appears that in each of these groups we are dealing with substantially the same phenomenon. We have found a series of criteria by which early and late loans can be distinguished in this particular dialect. By means of these four sounds we can divide the words which contain them into three groups: Group 1, wholly assimilated; Group 2, partly assimilated, and Group 3, ambiguous. It may be

asked: what happens if more than one of these sounds occur in one word? In the overwhelming majority of words their classification agrees; e.g. in Group 1: *r* and *ü* in *spring-tooth*, *r* and *o*[1] in *lawnmower*; and in Group 2: *ṛ* and *u* in *detour*, *proof*, and *prove*. However, the component parts of compounds frequently fall into two different classes. In the word *hardwarestore*, the partly assimilated ["ha·ṛdwæ·ṛ] has combined with the wholly assimilated [stå·r] into a compound in which each part retains its form. As the *w* of *hardware* shows, the four sounds given above are not the only criteria for such division into two groups; in general, the presence of *w* and *dʒ* instead of *v* and *j* are also indicative of words belonging to Group 2.

Armed with these criteria, it becomes possible to look for further distinctions between the two groups. If we segregate the words whose classification is determined as above, and study their development, we soon see that we are dealing with two radically distinct processes. A large part of the vocabulary can be classed as belonging to a presumably earlier stratum of borrowing. In this part it can be shown in detail how the system of the Norwegian language was the determinant in the treatment given English loanwords. In this process the spelling was clearly of secondary importance, for most English words give but little clue to their true pronunciation. As will be shown in greater detail elsewhere, we are dealing here with a genuinely systematic transference, where the habitual pattern of the receiving language was of greater importance than the phonetic similarity of the sounds.

In the second group the English system is seen to be dominant, and has imposed itself on the Norwegian. The phonetic equivalence becomes more exact, and the systematic assimilation less complete. This is clearly due to increasing familiarity with the sound system of English on the part of the speakers. While the earliest immigrants were both unable and unwilling to introduce English sounds into their language, the younger generations have shown no such reluctance. Most of them are probably bilingual, at least to the extent of using English in business and other outside relationships. Why have they then not reintroduced the English sounds into the earlier loanwords, which have here been noted in assimilated form from a member of the third generation? The answer probably is that the third generation is rarely aware of the English origin of most of these. The words of Group 1 act and feel like Norwegian words, and the American Norwegian who tries to pick out the English loanwords is as likely to hit upon a bonafide cognate as an English loan. As a matter of fact, some

of the words here occurring as members of Group 2 are found in other dialects as part of Group 1. Other words vacillate in form between 1 and 2, for instance *tune* appears both as [to·nə] and as [tü·nə], and some unquestionably old words occur in the forms of Group 2. It is very likely that some words have been independently introduced more than once and have not been frequent enough to maintain a traditional Norwegian form.

It is too early to generalize concerning the transitional stages between these two forms. There is a small amount of overlapping between them, and the historical facts surrounding some of the objects referred to will have to be drawn in to give a complete picture of the transition. But the examples given here are sufficient to show that, under the conditions of the Norwegian immigrant group in this country, a gradual transition from language to language takes place which nowhere shows as sharp a break as might be expected from a comparison of the end products, the original Norwegian and the ultimate American. It may be added as a note for further pondering that those features of Norwegian which still cling to many words of Group 2 (e.g. unvoicing of *z*, Norwegian intonation) are precisely those features which characterize the American of the child raised in all-Norwegian communities. This is a continuation into English of the process sketched above: the phonological shift, which may be accomplished by an individual in one generation, but which a community can achieve only in several.

SOME NOTES ON THE INFLECTED GENITIVE IN PRESENT-DAY ENGLISH

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The particular materials examined as a basis for the notes presented in this paper consisted of some three thousand personal letters copied from those in the files of one of the government bureaus in Washington. These letters were made available several years ago through the efforts of the Modern Language Association with the support of the Linguistic Society,[1] and their collecting and copying financed by the National Council of Teachers of English. In this limited body of material it has been possible to record every occurrence in each category examined and by studying the facts quantitatively as well as qualitatively gain some knowledge not only of the kind of variety that exists in actual usage but also something of the relative amounts of that variation. There is much impatience with statistical studies of grammatical constructions and often with just cause. On the other hand, many of the general statements concerning particular phenomena of a language actually express or imply quantitative judgments—judgments of absolute or relative frequency. Most of these depend upon general impressions rather than upon an attempt carefully to calculate the frequency of actual instances in any body of material. Concerning the genitive there are many such quantitative statements based only upon general impressions. The material offered in these notes will assist in checking the validity of three such statements that are frequently repeated.

[1] 'I take pleasure in sending you this official transcript of a minute formally adopted by the Linguistic Society of America, on December 27, 1926, at Cambridge, Mass., on recommendation of the Executive Committee: The Linguistic Society of America, recognizing the scientific value of a survey of the inflections and syntax of American English, hereby expresses its approval of the plans formulated by the Committees of the Modern Language Association and of the National Council of Teachers of English, for this purpose, and joins these organizations in urging the United States Government to make available for such a scientific study correspondence in its possession. Roland G. Kent, Secretary.'

I

The following quotations will serve as examples of the first statement.

> The inflectional genitive is now nearly confined to the possessive meaning, whence it is often called the 'Possessive Case'.[2]
>
> From the fact that the genitive in the majority of cases expresses a relation of possession, it is often called the possessive.[3]
>
> The Personal Pronouns have, properly speaking, no Possessive Case, that is to say, no Possessive Case with the force of a *substantive*. In Anglo-Saxon, when the genitives of these pronouns were used in the *possessive sense*, they were regarded as adjectives and inflected accordingly. As the possessive sense is the only one in which we have retained these forms, and as, when used in this sense, these forms were always regarded as adjectives, they should be regarded as such now; that is, *mine* and *my* are the equivalents not of *mei* but of *meus*, *thine* and *thy* of *tuus*, &c.[4]
>
> In Middle English also the genitive of the personal pronoun came to be used almost exclusively in a possessive sense, the older genitival idea having been lost except in a few phrases. In other words the genitives of the personal pronouns, and the possessive pronouns derived from them became one in use. . . . But as the genitive of nouns is also mainly a possessive case we may still retain the genitive form in its place in the pronoun inflection, putting it in parenthesis to indicate its more restricted use.[5]
>
> In the course of time we witness a gradual development towards greater regularity and precision. The partitive, objective, descriptive and some other functions of the genitive become obsolete; the genitive is invariably put immediately before the word it belongs to; irregular forms disappear, the *s* ending alone surviving as the fittest, so that at last we have one definite ending with one definite function and one definite position.[6]

In other words these quotations represent the very common statement that the inflected genitive is now used with one definite function,

[2] W. H. H. Kelke, An Epitome of English Grammar 77.
[3] H. Poutsma, A Grammar of Late Modern English 2.41.
[4] G. P. Mason, English Grammar 44.
[5] O. F. Emerson, The History of the English Language 320.
[6] Otto Jespersen, Language 351, 352.

the possessive meaning,[7] and that the older uses, the objective genitive and the genitive of description, for example, have not survived. This statement is made not only of the nouns but also (and especially) of pronouns.

In the materials examined for this study,[7a] the inflected genitives of nouns were distributed as follows:

Possessive genitive (liberally interpreted) 40%

Typical examples are:

'lived at his father's home' (8064)
'I am enclosing Mrs. M—'s passport' (8367)
'My son Sam's wife' (9018)

Subjective genitive 23%

Typical examples are:

'in support of his mother's request' (8234)
'since the soldier's enlistment' (8081)
'action regarding the boy's application' (9011)
'I have been under a physician's care' (8278)
'his parents' consent.' (8242)

Genitive of origin[8] 6%

Typical examples are:

'return the General's letter' (6415)
'the mother's affidavit' (8239)
'according to the neighbor's story' (8002)

Objective genitive 17%

Typical examples are:

'He contributed to the family's support' (8283)
'instructions for the boy's release' (8027)
'in regard to my son's discharge from the army' (8046)

[7] This is frequently offered as the 'reason' for the 'rule' that the genitive inflection should be attached only to words referring to persons or to such beings as can really 'possess' things.

[7a] They are cited by the serial number of the letter.

[8] A distinction has been made between the subjective genitive and the genitive of origin in such cases as "the man's resignation" or "the boy's application" on the following basis. When the context showed that the attention centered in the activity—the fact that *the man resigned* or that *the boy applied*—the genitive was called the subjective genitive; when the attention centered upon a resultant thing, as, for example, the *document* in which the man expressed his intention of resigning, or the *letter* in which the application was made, the genitive was called a genitive of origin.

Descriptive genitive (including genitive of measure).... 10%

Typical examples are:

'they found a woman's handkerchief as a clue' (7095)

'the month's time that I have lost' (9027)

'Unfortunately his education did not fit him to teach in a woman's college. He attended first a boys school and then a men's college.' (9436)

Genitive with gerund. (Subjective)

There was but one example of the genitive inflection on a noun before a gerund. (There were 23 examples of the uninflected noun with the gerund.)[8a]

'There can be no objection to Sergeant S—'s making an application for such an appointment.' (7050)

Absolute genitive.

There was but one example of the absolute use of the genitive; in this case the so-called 'double' genitive.

'He was accompanied by a friend of his father's.' (7061)

In respect to the pronouns, in spite of the fact that there were twenty times as many examples, the distribution paralleled that of the nouns to a remarkable degree.

Possessive genitive (liberally interpreted)..... (696)..48%

Typical examples are:

'in driving my car' (9064)

'Which he held in his hand' (9007)

'a sister whose husband is in —' (8196)

'all of my money has been spent' (8224)

Subjective genitive..... (243)..16.8%

Typical examples are:

'Upon my return home' (8251)

'to get along without his care' (8312)

'hearing of his disappearance' (8076)

'subsequent to my departure' (9056)

'my choice of a university' (9050)

[8a] Typical examples are:

'Nothing was said about his mother receiving it' (5104)

'Did you know of the company coming here?' (7553)

'There is no record in this office of any stone being taken' (7051)

'There is no record of this officer having been assigned to this detachment or of his ever having reported here' (7356)

'in her disregard of neutrality' (9017)
'its effect was far-reaching' (9012)

Genitive of origin..... (53).. 4%

Typical examples are
'her attention to our first letter' (8299)
'with reference to your wire' (9000)
'your reports were filed' (9019)
'I sent my petition through the usual channels' (9437)

Objective genitive..... (101).. 7%

Typical examples are:
'In the event of my promotion' (9022)
'he did not contribute to her support' (8240)
'immediately upon its receipt' (8299)
'or have his sentence remitted' (8052)
'the place of my employment is shut down' (8079)

Miscellaneous genitives[9]..... (322)..22%

Examples are:
'he had worked his way out west' (8142)
'he was then on his way to Richmond' (8076)
'when his year of enlistment was up' (8076)
'pray for the best to come his way some day' (8038)
'when my time will come to die' (8026)
'he will have to serve his time' (8288)
'he would do his best' (8152)

Genitive with gerund[10] (Subjective).

[9] In these I find it difficult to describe and classify the precise relation which exists between the genitive and the noun it modifies.

[10] Typical examples of the genitive pronoun before the gerund are:
"in the event of my being assigned to . " (9043)
"correspondence concerning my taking an examination" (9008)
"As to my being on the eligible list" (9042)
"nothing to indicate my having received a citation" (9018)
"the report of his having such a fight" (7094)
"She had given her consent to his entering the army" (8144)
"because of his being a married man" (6416)
"Have you known of his ever being guilty of anything that .. " (7093)
"How did you come to tell him of your having done this?" (7091)
"concerning the matter of your having consulted certain notes that were of assistance to you" (7091)

Typical examples of the pronoun in the dative-accusative form in similar constructions are:
"that led to me being questioned with regard to his efficiency" (7088)

There were 15 examples of the genitive pronoun before a gerund and 16 of the pronoun with the dative-accusative case form.

Absolute genitive.[11]

There were thirteen instances of the absolute form of the genitive pronoun. Typical examples are:

'a former tutor of mine' (9012)
'this communication of mine' (9056)
'this brother of mine' (8051)
'so long as this great nation of ours lives' (8066)

In the light of the frequently repeated general statements quoted above, several matters in these figures deserve special comment.

(1) The inflected genitive is no more confined to the expression of a single function now than it was in the older periods of the language. The possessive meaning is expressed by less than half of the genitives. The subjective genitive, the objective genitive, the genitive of origin, the genitive of description, continue to live in Present-Day English in proportions not strikingly different from those that existed earlier, in spite of the great decrease in the total number of inflected noun genitives.

(2) The genitives of the pronouns are also not limited to expressing the possessive sense. As in the case of the nouns less than half of the instances can be called possessive genitives. Subjective genitives, objective genitives, genitives of origin appear in proportions paralleling those of the various genitives of nouns. Besides these, there are many instances (one fifth of the total, 322 cases) in which the relation between the pronoun and the substantive it modifies is certainly not possession but is exceedingly difficult to classify or describe precisely.

(3) With the gerund the practice in respect to nouns seems to differ from that in respect to pronouns. With nouns, the usage is over-

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"he went away without me knowing" (8040)
"without us knowing anything about it" (8018)
"conditions are such as would justify you taking favorable action" (7089)
"certain things were done without you being consulted" (7092)
"There is no doubt about him being married" (8130)
"I never heard of him working hard" (7087)
"My choice of the University of Nebraska is made because of it being my former school" (9050)

[11] My daughter, in speaking of two sweaters that were used on teddy-bears belonging to her and her brother, used the following; "This blue one is Chucky's teddy-bear's sweater and that red one is *mine's*."

whelmingly the use of the uninflected noun form rather than the genitive; with pronouns, approximately half the instances appeared with the genitive form and half with the dative-accusative form.

II

Two brief quotations will serve as examples of the second of the three statements upon which my material furnishes comment.

> In Modern English there are but a few remnants of what was once the most important of all cases [genitive].[12]

> the [genitive] inflection having been superseded largely by the preposition 'of' . . . This occurs even with the ordinary Possessive meaning.[13]

The common assertion that the inflected genitive has been almost completely replaced by the periphrastic genitive (called also the genitive-equivalent) with *of*, needs some important qualifications in order to represent the facts accurately.

It is true that the comparative figures for the inflected genitive of nouns and the periphrastic genitives with *of* show a steady displacing of the former by the latter beginning in early Old English.[14] In the materials examined here for Present-Day English, of the total of 1327 instances of nouns in these constructions, but 5.2% were the inflected genitive and 94.8% the periphrastic genitive with *of*.[15] In respect to *nouns*, only 5% of inflected genitives remain. In the *pronouns*, however, the situation is entirely different. The genitive inflection[16]

[12] Leon Kellner, Historical Outlines of English Syntax 97.

[13] C. T. Onions, An Advanced English Syntax 94, 96.

[14] The facts are set forth in an unpublished dissertation by Russell Thomas, entitled Syntactical Processes Involved in the Development of the Adnominal Periphrastic Genitive in the English Language. The periphrastic genitive rises to 6.3% in the 12th century, to 31.4% early in the 13th century, and to 84.4% by the end of the 14th century.

[15] Those constructions with *of* were counted as periphrastic genitives for which a parallel could be found using an inflected genitive.

e.g. G. & E.45,1548: Of win and olie fulsum-hed.
Gen.H.155,37: ic sealde him mycelnysse hwaetes and wines.
. . . . . .
Sthn.Passion,5,118: đe children of þe temple.
Bede 4,17: fram þam brođrum þaes mystres.

[16] I include as genitive inflections of pronouns, the forms *my, our, your, his, her, its, their, whose,* as well as the absolute forms *mine, ours, yours, hers, theirs.* These absolute forms and the absolute use of *his* and *its* appeared in these mate-

of the pronouns still persists with practically no displacing by the periphrastic with *of*. Of the 1443 instances of pronouns in these constructions, 97% were the inflected genitive forms and but 3% the pronoun with *of*. In the pronouns, therefore, the genitive forms appear to have successfully resisted the pull of the periphrastic pattern which has almost completely displaced the inflected genitive of nouns.

In respect to this nearly complete wiping out of the inflected genitive of nouns one other matter seems significant. 'In Old English', as Sweet puts it, 'any noun could be made into an adjunct-word by being put into the genitive.[17] Adnominal modification in English has been expressed either by the genitive inflections or by the adjective inflections.[18] During the Middle English period the adjective inflections were completely lost. During this period also the genitive inflections for the singular of nouns became more and more the pattern *-es*. The old genitive plural ending *-a* (or *-ena*) was more slowly displaced by a form in *-s* similar to the nominative and accusative plural. There were many cases in which the historic form of this genitive plural with its developed zero inflection did not yield to the pull of the pattern in *-s*. This was especially true of the genitives of measure following numerals. Many examples still exist in Modern English.

'a five foot pole'

'a three year course'

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rials so seldom, however, that their exclusion or inclusion makes practically no difference in the result. There were but ten instances in all. I cannot argue here the case for preferring to deal with these pronoun forms as genitives. It will be evident from the materials and examples presented above in section I of this paper that these forms of the pronouns cover practically the same range of ideas as do the genitive inflections of the nouns. They are by no means limited to the 'possessive sense'. The facts that these genitives did sometimes take adjective inflections in Middle English, and that there are adjective equivalents for them in Latin or in German seem to me to be beside the point. See also G. O. Curme, Syntax, 526, 527. The adnominal genitive, whether of noun or of pronoun, has always been and still continues to be adjectival in its function. Compare the following statement from W. D. Whitney, in Sanskrit Grammar 98: 'The proper value of the genitive is adjectival; it belongs to and qualifies a noun, designating something relating to the latter in a manner which the nature of the case, or the connection, defines more clearly.'

[17] Henry Sweet, New English Grammar §1996.

[18] For the sake of clearness and brevity I am omitting all discussion of such combinations as *stone wall, cannon ball, sea water* in which the first word has been called a genitive of material. Such combinations however, certainly contributed to the development I am trying to describe. See Jespersen, Modern English Grammar 2. 30 ff.

'an eight day furlough'
'a six month leave of absence'
'a 420 acre farm'

Many of these constructions now also appear with an *-s* ending but wherever a plural is possible they are felt as plurals only, not as genitives. The following examples occur very frequently in my material

'a six months course'
'a ten days leave'
'an eight months old baby' (but 'an eight year old boy')

Such *-s* forms are felt and treated as plurals, not as genitives, and are not usually written with the apostrophe. Only when a clear singular is used, as one emphasized by a numeral, does the genitive sign appear frequently.

'each day's delay' 'the one month's time that I lost'
'a single month's leave'

My point is this. In the use of this noun of measure the pressure has been strongly to suppress the genitive. Wherever a plural occurs, we usually in writing omit any sign of a genitive and feel no difficulty over a substantive modifier with no formal indication, other than position, of its adjunct character. The genitive may even be omitted with a singular. We accept both 'a one month vacation' and 'a one month's vacation'.

This suppression of the genitive form, leaving the adjunct noun without formal mark of its function other than position also occurs frequently in the cases of what would earlier have been genitives of description and of identification.

'the table leg', 'the door handle', 'the chair back'
'the desk drawer', 'the shirt sleeves', 'a lion skin'
'my bachelor degree' (but 'my doctor's degree')
'my business interests' (9000)
'from the hospital gardens' (9002)
'the family quarters' (9030)
'during the summer months' (9058)
'for the company officers' (9021)
'to the city manager' (9014)

The source of this practice seems to have been the zero genitive which persisted. The complete loss of inflection in the adjective certainly cleared the way for its spread. The supplanting of the inflected genitive by the periphrastic form made for such a general decrease in the use of the genitive inflection that the support of a

frequently used pattern was removed. It is to be expected then that the growth of this practice in Modern English should have been great. As a matter of fact, if the materials examined here can be taken as typical of Present-Day English it bulks very large indeed.

There were altogether in these materials 395 instances of these adjunct nouns without formal inflection, which paralleled our genitives of measure, description, and identification. In other words, if we take all the instances of the inflected genitive of nouns, and the periphrastic genitives with *of*, and these particular varieties of adjunct nouns together we have a total of 1722 instances of which 4% are the inflected genitive, 73% the periphrastic genitive with *of*, and 23% these particular adjunct nouns with the genitive inflection suppressed.

In stating, then, the situation concerning the displacing of the genitive inflection in Present-Day English I should call attention to two facts:

(1) The displacing of the genitive inflection by the periphrastic genitive-equivalent with *of* applies to nouns only. The pronouns have successfully resisted the pull of this pattern.

(2) The genitive inflection of nouns has been displaced not only by the periphrastic genitive-equivalent with *of*, but also by the growing use of the adjunct noun of measure, description, and identification, without any formal mark of function except position.

III

The last of the three statements to be commented upon can be represented by the following quotations.

> On account of the loss of its inflections English was forced to employ the concrete prepositions *of* and *to* as the best available forms to make a clear genitive and a clear dative.[19]

> The general movement by which single words have in part taken the place of inflection is the most sweeping and radical change in the history of the Indo-European languages. It is at once the indication and the result of a clearer feeling of concept-relation. Inflection in the main rather suggests than expresses relations. . . . The adverb-preposition is the expression in more distinct form of some element of meaning which was latent in the case-form. It serves therefore as a definition of the meaning of the case-form.[20]

[19] G. O. Curme, Syntax 113.

[20] E. P. Morris, On Principles and Methods in Latin Syntax 102, 103, 104.

After attempting to classify some two thousand instances of the use of *of*, I must vigorously dissent from the view expressed in the quotations just read. It is true that the genitive inflection itself indicates or 'suggests' only the fact that there is some relation between two substantives; the functional distinctions of this relation are gathered from the meanings in the substantives themselves or from the context of the sentence. The genitive inflection represents an extremely vague category covering a great variety of relations. But is the situation markedly different when the word *of* is used instead of the genitive inflexion? All the relations that are now or were formerly suggested for the nouns joined by the genitive inflection are now suggested for the nouns by the function word *of*. The functional distinctions of this relation are gathered not from the word *of* but from the meanings in the substantives themselves and from the context of the sentence. I can find in the *of* no clearer grasp of the relationship between two substantives than I can in the genitive inflection. In fact the case with *of* is even worse, for after all the *of* carries not only all the relationships that words had that were connected with the genitive inflection but, in addition, a great variety of relations that never had and do not now have any connection with the genitive.

Typical examples of the varied uses of *of* are the following:

Non-genitive uses

'His parents lived within a few miles of Baird'. (8060)
'He is of a fine family'. (9039)
'forced to leave college because of the failure' (9036)
'He has been out of employment'. (8283)
'I am convinced of the justice of the claims'. (8234)
'The course consists of practical work in assembly'. (9052)
'teams composed of officers' (9042)
'I am desirous of getting this detail' (9040)
'He is of a rather roving disposition'. (8142)
'The course would be of great benefit'. (9052)
'I will make myself of more value' (9050)
'if the soldier does not meet the payments of the land' (8081)

Genitive uses

Possessive

'the office of the chief' (9050)
'the gravity of the earth' (9037)
'the door of the street car' (8095)

Subjective
'the dip of the compass needle' (9037)
'in the employ of these lumber merchants' (9036)
'they need the help of this young man' (8004)
'at the direction of the President' (9050)
'did not receive the consent of his parents' (8183)
Of origin
'I am enclosing an affidavit of the mother' (8234)
Objective
'Upon receipt of your communication' (8002)
'contributed to the support of his mother' (8142)
'for the discharge of her son' (8234)
'a bona fide statement of facts' (8267)
Descriptive
'an absence of twenty days' (9042)
'a man of fine caracter' (9041)
'for a man of his years' (9039)
'as a house of prostitution' (9036)
Appositive
'the subject of gas engines' (9052)
'to the grade of Major' (9035)
'the months of April and May' (9035)
'promotion to the position of staff sergeant' (8076)
Partitive
'one of the affidavits' (8267)
'eleven days of this leave' (9043)
'for a number of years' (8267)
'While he was at home he never did a stroke of work.' (8144)

In summary then, the materials examined for these notes show that:

(1) The variety of relations covered by the genitive inflection is nearly as rich as it formerly was. The possessive genitive accounts for only about 40% of the instances both in respect to nouns and in respect to pronouns. Subjective genitives, objective genitives, genitives of description, etc. still survive in nearly the proportions they had in the older stages of the language, in spite of the great decrease in the total number of inflected noun genitives.

(2) The inflected genitive forms have been largely displaced, but in nouns only; in pronouns the use of the genitive forms continues with practically no diminution. This displacement in the nouns is not solely the result of the spread of the periphrastic genitive-equivalent

with *of*; it is complicated and aided by the great increase of the use of the noun in these genitival adjunct relations with no formal distinction except word-order.

(3) The use of the function word *of* instead of the genitive inflection is not in any way a clearer genitive or a more precise stating of the relationship that exists between two substantives. Because it carries not only all the relationships expressed by the inflection but also many more that have (and had) no connection with the genitive, it seems a less precise grasping of the relation between the two substantives thus connected.

SOME OBSERVATIONS ON NUZU AKKADIAN

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The Nuzu tablets, excavated by an American expedition, have consumed a good deal of the labor of American Assyriologists in recent years and will continue doing so for some time to come. After the publication of the material has far advanced, the grammar of the new dialect is in progress of reconstruction. The efforts made in this direction have so far been of a merely descriptive kind. It seems time now to apply also the linguistic line of approach to Nuzu Akkadian. Though the registration of the philological facts is the necessary basis for such work, it will in turn help to clarify difficulties which the descriptive grammar is unable to remove.

The observations presented here have arisen from the study of Pfeiffer and Speiser's One Hundred New Selected Nuzi Texts; they will therefore be documented by reference chiefly to that volume. They will deal with certain features of orthography, phonology, morphology and syntax in the order indicated.[1]

1. Orthography (Syllabary)

(a) The sign '*pi*' denotes the syllables *wa*, *wi*, *wu* as in Old Babylonian and Old Assyrian, while *pi* is expressed by '*bi*'[1a]. Concession is made to the younger orthography in as much as internally *ma*, *mi*, *mu* may be used instead of *wa/i/u*[2]. This seems due to a sound-shift

[1] The following abbreviations will be used. N = E. Chiera, Joint Expedition with the Iraq Museum at Nuzi (5 vols.); H V = E. Chiera, Excavations at Nuzi I; H IX = R. H. Pfeiffer, Excavations at Nuzi II; Berkooz = M. Berkooz, The Nuzi Dialect of Akkadian (Language Dissertations 23).

[1a] *w* is rare at the beginning of words, e.g. *wa-al-du* 39:6, 12; *wa-la-az-zu* 53:7. In most cases *w* in this position has disappeared (as in the other dialects); examples *aš-bu* 99:7 etc.; *aš-bá-ak* 10:19; *aš-bá-ku-mi* 60:25 follow the Babylonian practice, but *ur-ka-az-zu* 30:12, 42:14 the Assyrian.

[2] See Berkooz 50 f. It is worth noting that the glide *w* in *Šawuška* = *Šauška* (cf. Friedrich, Anal. Or. 12 129, fn. 5) and in cases like *Ninuwari* = *Ninu(a)-ari* may be spelled with *m*. Such an inorganic *m* must not be regarded as formantic, as L. Oppenheim, WZKM 44.197, does.

from *m* to *w* in intervocalic position[3]; it set the sign '*pi*' free so that it could be used for distinguishing *bi* and *pi*[4]. Using *wa* and *ma* interchangeably looks like a compromise between two orthographies. The older one was possibly kept alive since for the native language of Nuzu (as it is documented by very numerous proper names and a number of technical expressions[5]) the sign *wa* was indispensable.

(b) Special signs for the emphatics are not in use. There are no *ṭi*[6], *ṭu*[7]; *qi*[8], *qu*[9]; *ṣi*[10], *ṣu*[11] such as the contemporary Assyrians and Babylonians have created. The sign '*qa*' exists; but the fact that it is used also for *ga* (transliterate *ga*) and *ka* (transliterate *ka*)[12] proves that primarily it does not denote the emphatic *q* with following *a*.

(c) Voiced and voiceless stops are used indiscriminately. The usual explanation maintains that this is due to the influence of the sub-

[3] The orthodox view asserts on the contrary that intervocalic *w* had become *m* (Ungnad, Bab. Ass. Gram. 2, 14). This necessitates the further assumption that this secondary *m* (together with etymological *m*) later on became *w* again. Since *w*- in initial position in Bab. and Ass. had disappeared at roughly the same time, there was no longer need for the sign *wa*.

[4] The view expressed in the text seems to be shared by Fr. Thureau-Dangin, see his Syllabaire Accadien 34.

[5] See the collection of C. H. Gordon, BASOR 64 23 8.

[6] *ti* is spelled '*ti*', '*di*' or '*te*' in the same word *saṭir*'. With '*ti*': 22:40; 28:14; 32:43, 54:23; 58:47; 61:32; 66:49; 67:31; with '*di*': 63:35; 65:28, 50; with '*te*' 55:48.

[7] For *ṭu* there can be quoted with '*tu*' (rare): *a-na ba-al-ṭú-ti-im-ma* 8:14; with '*du*': *i-pa-aṭ-ṭù-ur* 27:12; *il-ṭù-ru* 59:16; 65:47; with '*tum*': *bal-ṭu*$_4$ 23:11; 30:14. For *ṭa* for which a special sign has never been developed '*ta*' is used: *ḫu-ṭá-ar-ri* 3:60; *ṭá-a-ti* 8:26, 28, 67; *iṭ-ṭá-ba-aḫ-šu-nu-ti* 6:43.

[8] As for *qi*, the observation of Berkooz (10 f.) can be referred to, that '*ki*' serves for *qi* (as for *ki* and *gi*) and '*gi*' for *qe* (as for *ke* and *ge*). It should be added that the same distribution is valid for the Mitanni letter (F. Bork, Mitannisprache 17).

[9] *qu* is expressed by '*ku*': thus in the frequent forms of *leqū*, e.g. *il-qú-ú* 47:4, etc.; 48:36; *te-el-qú-ú* 70:16, 24; *ni-il-qú-ú-ma* 74:19; *i-liq-qú-ú* 51:18, etc.; *li-qú-ú-ma* 84:4; *il-te-qú-ú* 7:47; 21:19; 22:13; 54:7; 74:8; *il-te-qú-ma* 8:7; furthermore *ir-te-qú-šu-ma* 6:51; *šu-qú-ul-ta-šu* 97:2; *il-tar-qú* 1:47.

[10] This sign, used for *zé* in Old Babylonian, is almost entirely absent; see Berkooz 11. In this volume the name *Pa-i-zé-ni* 95:21. For *ṣi* there serves '*zi*': *ú-uṣ-ṣí* 23:15; 30:18; 36:7; 58:40; *ú-še-iṣ-ṣí* 23:18; 63:12; *uš-te-ṣí* 45:8; *i-iṣ-ṣí-du* 88:10; *ma-ḫa-ṣí* 1:34, *mi-iḫ-ṣí* 72:14, 20; *ṣí-di-ta* 8:7, 11; *ṣí-mi-it-tu* 100:13 etc.; *ṣí-mi-it-ti* 8:25; *iṣ-ṣí-el* 75:14; *iṣ-ṣí-e-lu-ma* 75:3.

[11] *ṣu* is represented by '*zu*': *ú-uṣ?-ṣú-ú* 34:25; *ú-še-iṣ-ṣú-ú* 61:28; *ú-še-iṣ-ṣú* 60:18; *it-ta-ṣú-ú* 8:11; *ú-ṣú-ur-ti* 73:12; *pè-ṣú-ú* 100:16; *it-tar-ṣú* 72:12.

[12] Examples, Kramer 68; Gordon, Nouns 9.

stratum[13]. But warning has been given that this unsatisfactory situation may be a heritage from Old Akkadian, i.e. an imperfection of the orthography[14]. This warning seems to be very much in point[15].

(d) As for the sibilants, not only is the emphatic set entirely absent, but also that which is usually read as Samekh plus vowel is quite rare and employed as a mere variant of the set containing *š* plus vowel[16]. Words with etymological Samekh, on the other hand, are written with *'za'*, *'zi'*, *'zu'*[17]. We then have in Nuzu documents:

'ša', *'še'*, *'ši'*, *'šu'* for etymological *š* (partly from *ṯ*).[18]

'za', *'ze'*, *'zi'*, *'zu'* for etymological *z*, *s*, *ṣ*.

A similar picture can be drawn for the signs which contain a sibilant after a vowel. Here we find:

'aš', *'eš'*, *'iš'*, *'uš'* for etymological *š* (partly from *ṯ*)

'az', *'iz'*, *'uz'* for etymological *z*, *s*, *ṣ*,[19] *ṯ*[20]

The four features mentioned force us to infer that the syllabary of the Nuzu documents is independent both of the Middle Assyrian and of the Middle Babylonian syllabary. These both use *'pi'* for *pi* and

[13] Since F. Bork's statement in Die Mitannisprache (1909) 15 f. this has been uncritically repeated time and again; most recently, M. Berkooz 39 ff.

[14] Fr. Thureau-Dangin, Le Syllabaire Accadien v.

[15] Even for Ḫurrian the existence of different sets of stops has been proved by the Ras Shamra documents, cf. J. Friedrich, Anal. Or. 12.130 f.

[16] *'sa'*: *i-SA-as-sí* 67:27; 96:10 (usually *i-ša-as-sí* with the variant *i-sà-as-sí*, e.g. 29:12) of *šasū* 'call'; *SA-ṭi-ir* 58:47 (usually *ša-ṭi-ir* or *šá-ṭi-ir*) of *šaṭāru* 'write'; *SA-dì-in-ni-e* 1:38, etc. seems to be a special case, cf. Sum. s u. u d . d i n . g i g i r ŠL 354, 335. *'si'*: *SI-id-dì* 15:9 (instead of *ši-id-dì*). *'su'*: customary in *sutān* (Berkooz 11), but even here replaced by *šu* (e.g. 67:8).

[17] *'za'* for *sà*: *it-ta-sà-aḫ-ma* 10:21; *sà-a-mu* 98:4; *ri-ik-sà* 23:18; *ir-pí-sà-an-ni-mi* 72:7; *ir-ta-ap-sà-an-ni* 3:55, 60; 8:32. *'zi'* for *sí*: *sí-ḫi* 73:6 etc.; *sí-i-bit-e* 49:26; *sí-is-sí-ik-ta-šu* 32:8; *ra-pa-sí* 3:32; *ri-ik-sí* 44:1; 55:1; 57:11; various forms of *šasū* 'call', such as *i-ša-as-sí* (pass.) and *al-ta-sí-iš* 4:12. *'zu'* for *sú*: *ḫu-us-sú-sí* 69:18; *i-pár-ra-sú* 69:17; *ir-ku-sú(-u)* 44:3; 55:3; *ir-ta-ak-sú* 31:14; *i-ša-as-sú-ú* 16:16; 51:29; *i-ša-sú-ú* 59:7; *il-ta-sú-uš* 71:15.

[18] Instead of *ša* also *šá* sporadically occurs: *šá-ṭì-ir* 61:32; *šá-te-ir* 53:48; *ú-ri-ḫul-šá* 60:22; 61:17.

[19] The use of *'az'* and *'uz'* for *as* and *us* is noteworthy; for classical Old Babylonian employs *'áš'* and *'uš'* with these values. *'az'* for *as*: *ar-ta-ka₄-as* H IX 145:21; *i-ša-as-sí* (pass.). *'uz'* for *us*: *ḫu-us-sú-sí* 69:18.

[20] The rare occurrences of *'áš'* must be mentioned here. I find the sign only in one genuine Akkadian word, namely in *na-ÁŠ-qú* 95:6; the corresponding feminine *na-sí-iq-tu₄* H IX 17:6 shows that *na-ás-qú* must be read as in Old Babylonian. Is this an archaism? The sign, possibly denoting some variety of a sibilant, occurs also in Ḫurrian words, e.g. in *ka₄-ás-ka/i* (cf. Gordon, Nouns 80 f.), interchangeably with *aš* (e.g. *ka₄-aš-ka/u* 64:6, 13). Cf. also the city *a-ka₄-ás* 48:17 (notice *a-ka-a-a-šeᵐᵉᵢ* H IX 34:4).

not for *wa*, are developing special signs for the emphatics, and possess a third set of spirants *sa*, *si*, *su*. The absence of the last mentioned spirants and the preference given to '*qa*' (beside and interchangeably with *ka* and *ga*) in the Nuzu syllabary prevent deriving it even from that of classical Old Babylonian. The facts can be met only on the assumption that it descends directly from an Old Akkadian syllabary. There was a time when '*ša*', '*še*', '*ši*', '*šu*' represented only the Akkadian descendant of primitive Semitic interdental *ṯ* and '*sa*', '*si*', '*su*' (better probably *śa*, *śi*, *śu*) that of primitive Semitic *ś* and *š*. The two varieties coalesced gradually and only upon completion of this process were '*sa*', '*si*', '*su*' set free for a new purpose, namely for differentiating *sa* (with Samekh), etc. from *za* and *ṣa*, all of them previously expressed by the uniform '*za*', etc.[21] The Nuzu syllabary uses '*sa*', etc. as a variety for *ša*, etc., but not in its later value which is already[22] current in Old Babylonian.

The positive characteristic of the Nuzu syllabary, the role of the '*qa*' sign, recurs in the Akkadian of certain Amarna and Boğazköy texts[22] which apparently depend on a syllabary used in Upper Mesopotamia and also in Elamite where *qa* is the usual means of expressing the sequence guttural (*g*, *k*, *q*) plus *a*[23].

2. Phonology

The result obtained by an examination of the syllabary (independence from Babylonian as well as from Assyrian) does not necessarily indicate an analogous linguistic position of the Nuzu dialect. It could however be used to confirm such a conclusion, should linguistic facts point in this direction.

The phonological status of the dialect may be roughly described as follows. The mimation has dropped so that nominal cases now end in free vowels, and the terminative of the verb in *-a* and *-ni* respectively. These final vowels (like those that are inherited) are well preserved[24]. Adjoining vowels are assimilated, the resulting Schleifton being regularly indicated by 'scriptio plena' of the vowel in question[25]. The dialect takes part not only in the Old Babylonian umlaut which is produced by *e* (to a large extent from *a* under the influence of sharp

[21] Fr. Thureau-Dangin, RA 30 93 ff.; cf. A. Goetze, Orient. 6.16 f.

[22] It would be a promising undertaking to investigate the syllabary of the Akkadian texts from the Ḫurri/Mitannian territory.

[23] F. Weissbach, Die Achaemenideninschriften zweiter Art 29.

[24] On the nominal cases which sometimes seem confused see below p. 139 f.

[25] As in Old Babylonian.

laryngeals) and affects both preceding and following *a*'s[26]; it takes part also in the Middle Babylonian umlaut, which is due to a younger *e* (developed from *i* under certain conditions) and influences certain preceding *a*'s only[27]. Furthermore it shares the dissolution of voiced geminates by nasalization[28]; and, with both Middle dialects, the shift of *št* to *lt*[29]. As far as phonology is concerned, Nuzu Akkadian, then, coincides largely with Middle Babylonian.

3. Morphology

(a) Pronoun[30].

The difference between *yāti*, *kāti/a*, *šuāti*, *niāti* (acc.-gen.) and (*ana*) *yāšim*, *kāšim/kāšum*, *šuāšim/šāšim/šāšum*, *nāšim* (dat.), so characteristic for Old Babylonian and present also in Middle Assyrian[31], has no counterpart in Nuzu Akkadian. Here we find for all oblique cases uniformly *yāši*, *kāša*, *šāšu*, *nāšni*[32], a situation similar to that found in the Kassite letters.

With regard to the suffixes, the fact should be stressed that *-šunūti* (less frequently *-šunū*) has the force of both accusative and dative, and that the Old-Babylonian *-šunūšim*, absent also in Old and Middle Assyrian[33], is only rarely found. The material for the two other persons of the plural is very scanty, and, strangely enough, shows just the forms *-kunūši* and *-nāši*[34]. The dative suffix is added to the terminative and thereby recognizable.

[26] A new treatment of this phenomenon is badly needed; the last one was given by P. Haupt, AJP 8.265-91 (1887).

[27] The specific conditions remain to be established. Here belong the numerous present and pi'el forms like *tezerreb*, *iqebber*, *iqerreb*, *uḫemmeṣ*, *umeššer*, etc. It may be added that all this has nothing to do with the Assyrian vowel harmony which is entirely absent (the cases quoted by L. Oppenheim, WZKM 45.43 are to be explained otherwise).

[28] For *gg*, Berkooz 44, for *dd*, ibid. 45, for *bb*, ibid. 49. It may be asked whether *i-na-an-ZA-ar-šu* (present of *naṣāru*) testifies to the nasalization of the peculiar spirant (A. Goetze, Or. 6. 12 ff.) contained in this root.

[29] See Berkooz 62 f.

[30] For this section cf. C. H. Gordon, AJSL 51.1-21.

[31] W. von Soden, ZA NF 6.181 ff. gives the best information about the forms extant in the older dialects of Akkadian.

[32] For this latter (C. H. Gordon 3, fn. 3) *an-na-aš-ni* 5:4 is a new corroboratory example. Cf. furthermore *an-na-ši-in* KBo I 23:10. The *an-na-aš*, attested in the epic dialect (W. von Soden, ZA NF 6.184) seems closely akin.

[33] J. Lewy, Stud. zu den altass. Texten aus Kappadokien 11.

[34] I noted for the second person *i-na-an-din-na-ku-nu-ši* H IX 5:11; for the first *iz-za-ak-ra-an-na-ši* N 324:14 and *il-ta-ad-nu-na-ši* N 360:20.

The *-šu* of the singular appears as *-š* after long vowel[35]; this form of the suffix is known from Old Akkadian, and recurs in the Epic of Creation[36] in the same fashion as in Nuzu.

The general relative is formed by means of the particle *-mē*[37], a phenomenon paralleled from the Mitannian territory (Amarna, Boğazköy), but found neither in Babylonian nor Assyrian.

Notice furthermore *attu-ya* 'mine', etc.[38] (also Kassite and Mitanni Akkadian); *attamannu* 'everyone'[39] (also Middle Assyrian[40]), *allūti* 'the others'[41], *uddu-ya* 'mine alone'[42] (also Neo-Assyrian[43]).

(b) Noun[44].

Singular: All three cases are alive and characterized by the vowels *-u* (nom.), *-a* (acc.), and *-i* (gen.) The use of the genitive is very consistent; nominative and accusative seem frequently confused. The confusion, however, is only apparent. Before judgment is passed, the following phenomena must be considered. (1) Use of *-u* instead of *-i* and vice-versa[45] may cause confusion between nominative and genitive. (2) Use of signs like *'tum'* and *'rum'* with undetermined

[35] Thus after the *-ū* of the 3rd plural: *it-tab-lu-uš* 8:14; *uš-te-ši-bu-uš* 6:53; *idukkū-uš* 74:26; 75:31; *ú-ka₄-lu₄-uš* 24:6; *ú-me-eš-ši-ru-uš* 8:5; *id-du-uš* 68:5; *it-ta-du-uš* 38:34; 41:21; 71:24, 32; 73:33; *it-ta-an-nu-uš* 40:13; *iš-ta-lu-uš* 70:11, 16; 73:9, 18; *il-ta-sú-uš* 71:15. Also after *-i* of tertiae infirmae: *al-ta-sí-iš* 4:12. The protection of an additional element after the verbal form preserves the *u* of the suffix: e.g. *id-du-šu-mi* 71:21; *ni-il-ta-sí-šu-ma* 4:6.

[36] See W. von Soden, ZA NF 6.176 ff.

[37] *mannu-mē* 'whoever', *minum-mē* 'whatever', *immati-mē* 'whenever' pass.; cf. also *i-na a-i-im-me-e u₄-mi* 'on whatever day' 91:10; *a-i-ka₄-am-me-e* 'wherever' N 328:16; 365:7.

[38] Not mentioned by Gordon. *attu-ya* 1:18; N 338:29; 342:15; 359:9; 370:17; *attu-ka* N 321:41. On *ša at-ti-ḫu* N 321:37 see J. Friedrich, ZDMG 91.212.

[39] C. H. Gordon 19 f.

[40] K. F. Müller, Das assyrische Ritual 1.42.

[41] C. H. Gordon 21. Add now 7:49 ff.: 'Ḫašip-abu has collected clothing for the *ubaru* people; the clothing of the others he has returned; the garments of mine alone he retained'; 56:12 ff.: 'the other sons of mine are grown up (and) have taken wives; but thou didst not take a wife.'

[42] For the meaning notice the contrast with *allūti* (see preceding note) and e.g. 10:13 ff.: 'Two sheep Peškilišu has taken; the one belonging to the soldiers they have released; mine alone they have retained'.

[43] Cf. most recently A. Schott, OLZ 1937 col. 362, fn. 1.

[44] For this section cf. C. H. Gordon, Bab. 16.1–153.

[45] *šu-ti-iq* 31:13; *ši-ti-i-iq* 57:9 (both for *šūtuq*); *mi-du-ud* 92:8 (for *muddud*). Cf. Berkooz 36 f.; L. Oppenheim, WZKM 45.43. Such alleged genitives are *kaspi*^{pí} 9:13; *tup-pí* 54:22; 61:30; *ú-ri-ḫul-ši* 62:18. The form of a nominative is encountered in *it-ti* X ^{I}*Ḫa-bi-ru-ú* 40:2; ^{am}*a-bu-ul-ta-nu* 60:47.

vowel; all three cases may look identical[46]. (3) Syntactical rearrangement according to the native language; wherever the accusative (object) precedes the nominative (subject)—sometimes contained in the verb—both object and subject are in the *u*-case[47]; if the object is a plural, the verb too frequently takes the plural form[48]. (4) The predicative noun is in the accusative[49].

Plural: Two cases are distinguishable which in the masculine end in *-ū* (nom.) and *-ē* (obl.) (mostly written plene), with the feminine in *-ātu* (nom.) and *-āti* (obl.). The plural of the adjective normally ends in *-ūtu* and *-ūti*[50], but it sometimes exhibits nominal forms[51]. The agreement between noun and adjective is frequently faulty[52].

[46] *'tum'*: *a-na pa-ni ši-bu-ti an-nu-TUM* 54:2f.; 95:2 (cf. from Old Assyrian I. J. Gelb, AJSL 53 43 f.). *'rum'*: *qāt* (or *aban*) X *tup-šar-RUM* pass. (with *-ri* e.g. N 493:26; 530:24).

[47] Examples are abundant; only a few can be quoted: 1 *en-zu* I*Ḫu-ti-ia a-na ia-ši iddina*^{na} 10:1 f.; *ù kirū an-nu-um-ma* I*Hu-i-ba-a-bu ú-za-ak-ka*$_4$ 21:11 f.; *ù* 1 *amtu* SIG$_5$.GA *na-ás-qú* . . I*Ar-ša-wa a-na* I*E-ḫé-el-te-šup i-na-an-din* 95:6 ff. With circumflexed *-ú*, e.g.: *ù kirū an-nu-ú* I*Tul-pu-un-na-a-a ú-ka*$_4$*-al* 21:14 f.; *ù ša-nu-ú* UDU I*ki-bi-ia il-te-qè* 6:53 f. With the first person of the verb e.g. *6 šiqil kaspu ṣa-ar-pu a-na* I*Peš-ki-ili-šu at-ta-din* 9:2 ff.; *3 imēr ku-nu-šu*^{meš} . . . *a-šar* I*Pu-ḫi-še-en-ni el-te-qè-mi* 62:24 ff. With the third person plural of the verb: *ù ṭup-pu an-nu-ú i-na 3 šanāti*^{meš} *ù i-na 4 šanāti*^{meš} *a-na pa-ni-šu-nu i-ša-as-sú-ú* 51:26 ff. For contradistinction compare the consistent use of the cases in the frequent phrase *šumma eqlu paqirāna irtaši*, or for instance *ù* I*Ki-pa-a-a*$^{am.}$ *mu-ru l na-am-zi-ta a-šar* I*Šúk-ri-ia il-te-qè* 6:16 f.; *ù* I*In-ni-ku ù* I*A-kap-še-en-ni ṭup-pa an-ni-ta il-ṭù-ru* 59:15 f.

[48] See L. Oppenheim, AfO 11.60. In the new volume, e.g. *ša al-lu-ti* TÚG.ḪI.A.MEŠ *ut-te-ir-ru ù ud-du-ia lu-ba-ri ik-ta-la* 7:51 f.; *i-na pa-na-ni-im-ma* I*A-ka*$_4$*-am-mu-uš-ni a-na aš-šu-ti iddin ù 40 šiqil kaspi-ia a-šar mu-ti-ia il-te-qú-ú* 54:5 ff.

[49] X Y Z *3*$^{amelu\,ti}$ *ma-an-za-ad-du-uḫ-li-e ša* I*Ḫa-na-te* 43:10 ff.; *amēlu ana amēli ma-ḫi-iṣ pu-ta* H IX 74:11 f., and frequently in the texts there following.

[50] Cf. C. H. Gordon, Bab. 16.17 f.; also Berkooz 17 f. The formation in *-ānu/i* occurs only sporadically; DINGIR.MEŠ-*ni* (i.e. *ilāni* N 89:10; also in the new volume 47:3?) is offset by DINGIR.MEŠ-*e* (i.e. *ilē* N 352:26); *še-ir-a-ni*^{meš} N 551:14 by UZU.MEŠ-*ra* N 360:20, 25.

[51] *2 ka*$_4$*-lu-mu*^{meš} NITA *ḫu-ra-pu* 66:12 f.; UDU.MEŠ *an-nu-ú* 66:30; *10* SAL.MEŠ *nu-ul-lu-a-ú* 32:15; cf. also (with *-i* instead of *-u*) *4* DINGIR.MEŠ GAL.MEŠ *an-ni-i* 50:19.

[52] See C. H. Gordon, Bab. 16.20. It must however be emphasized that the grammatical agreement becomes much more consistent than Gordon realizes, as soon as the rules established in the text are taken into consideration. In many cases a more correct transliteration of ideograms furnishes better grammatical agreement.

(c) Numerals[53].

For the plural of 'one' *iltēnūti* see ZA NF 6 79f.; the ordinal is *ištēnū* as in the epic dialect[54].

ma-at, *ma-ti* 'hundred'[55] should be compared with Old Babylonian and Old Assyrian *me'at* and Middle Assyrian *me-ti*.

Age is indicated by the nominal form *fu'ulā'u*[56]. Thus in the new volume *rubū* 21 8; 65 13, 16; *tišāu*, *tišū* (cf. *ti-ši* 66 14) is from and for **tušāu* with *i* for *u* (see fn. 45)[57].

(d) Verb[58].

(1) Inflection: The feeling for differences of gender is weak. The specific feminine forms are only occasionally used, and are normally replaced by the corresponding masculine forms[59].

The 1st person plural, besides the prefix *ni-*, takes the suffix *-ū* after the 3rd radical[60].

The 1st singular permansive ends in *-āk*, unless a particle is attached which protects the final *-u* of *-āku*[61].

For plural forms after plural objects see above and note 48.

(2) Stems: All stems are attested, but the nif'al is rather rare.

Pi'el and šaf'el are of the Babylonian type, i.e. they show *u*-vocalism in unprefixed forms (thus the permansives are *purrus* and *šuprus*, and not as in Assyrian *parrus* and *šaprus*).

The *t*-form has generally the force it had in classical Old Babylonian[62]. Its frequency in documents like nos. 1 to 14 of the new volume is justified by their nature. The interchange between *t*-form and *t*-less form in positive and negative pairs is particularly noteworthy[63].

[53] For this section cf. C. H. Gordon, RA 31.53-60.

[54] W. von Soden, ZA NF 7.131.

[55] A. David, RA 25.99 f.

[56] For *šulušū*, *ḫumušāu* see Gordon, l.c.

[57] D. Cross, Movable Property 27, fn. 43, is apparently mistaken when she separates *tiši'u* from this group; cf. H IX 104:12.

[58] For this section cf. S. N. Kramer, AASOR 11.63-119.

[59] Pronoun and adjective exhibit the same feature; it seems that the native language of Nuzu had no specific form for the feminine.

[60] *ni-il-qú-ú-ma* 74:19; *ni-ik-ki-ru-ma* 74:21; *ni-it-tam-ga₅-ru-mi* TCL IX 12:20.

[61] *aš-bá-ak* 10:19; *ti-te-en-na-ak* 29:4. But *ap-la-ku-mi* 62:29; 94:12 and also 67:22; *aš-bá-ku-mi* 60:25; *ḫu-ub-bu-la-ku-mi* 73:5; 96:5; *na-ta-ku-mi* 73:8; *ši-ba-ku-mi* 73:19.

[62] See A. Goetze, JAOS 56.297-334.

[63] Compare, e.g., *attadin* 1:19 with *la addin* 1:20; *elteqē-mi* 6:58 with *la elqē-mi* 6:56. Cf. JAOS 56.313 ff.

(3) Moods. The precative is found relatively seldom; the crasis between the *u* of *lū* and the prefixed vowels is of the Babylonian type; it results in *li-* with 3rd, in *lu-* with 1st persons[64].

The subjunctive is not always found where it would be expected.

(4) Special classes. The primae laryngalis present a puzzling problem; the question as to how the prefix *i-* of the 3rd persons before tautosyllabic sharp laryngeal is treated, is hard to answer. At any rate, the spelling, particularly in the preterite, is not consistent. The present (where correctly spelled with the middle radical doubled) almost regularly presents *i-* before etymological *ḫ* and ', but *e-* before etymological ğ. If this is admitted as true, the Nuzu dialect takes an intermediary position between Babylonian (which exhibits uniform *ī-*) and Assyrian (which exhibits uniform *ē-*)[65].

(e) Prepositions[66].

Characteristic for Nuzu are *ašar* 'with, from', *ina li(e)it* 'by, in'[67], *ina libbi* 'among', *ina* (and *ištu*) *muḫḫi* '(from) upon', *ina pāni* 'before', *itti* (together with', *kī*, *kīma* 'as', *kīmū* 'instead of'.

(f) Conjunctions.

Particularly characteristic are *untu* 'when'[68] and *kīmē* 'that, because'[69].

[64] *lu-ul-lik-mi* 1st sg. 70:7; *li-iq-bi-šu-nu-ti* 3rd sg. 76:13; *li-id-di* 3rd sg. H V 104:16; 106:14; *li-pu-uš-ma* 3rd sg. 23:11. The exception *lu-ša-ka₄-al* 3rd sg. 23:11 may be due to the interchange of *u* and *i* (above fn. 45).

[65] The treatment of this question by Kramer, AASOR 11.79 ff., is not altogether satisfactory. The prefix of the 3rd person is *i-* already in Old Akkadian, so that no *a* at all is involved in this process. It has nothing to do with the Babylonian umlaut nor with the Assyrian vowel harmony which, in their turn, must be strictly kept apart.

[66] Cf. W. von Soden, ZA NF 7.133 ff.

[67] There is no proof whatsoever that the preposition *lit* is a contraction of *liwit* (so also Berkooz 51). Already C. J. Gadd (RA 23.104) has correctly referred to P. Jensen, KB VI 2.5*. The preposition is also Middle Babylonian, see H. Waschow, Bab. Briefe aus der Kassitenzeit 17.

[68] *untu* also in the Amarna tablets (letters from Assyria and Mitanni) and in Boğazköy (see R. Labat, L'Akkadien de Bogh. 226). It seems likely that *untu* is also contained in *šu-un-tu₄* 65:48; possibly 'the year he wrote the tablet (is) the one when he brings the *ašūḫu* wood from the country of Ḫanigalbat'.

[69] As regularly in Amarna and Boğazköy this conjunction is frequently construed with the indicative of the verb.

4. Syntax

I wish to emphasize here only two points:

(1) The negation, in narrative sentences also, is *lā* (not *ul* as in Babylonian)[70].

(2) The particle *-ma* (in Babylonian functioning as sentence connective) is used for emphasis only; the sentence connective is *ū*[71]. Parallels are found in Assyrian and particularly in Old Akkadian.

The results of this brief investigation, which is far from being exhaustive, may be summarized as follows: The Nuzu Akkadian, though close to Babylonian as far as phonology is concerned, exhibits otherwise many un-Babylonian features. They justify the statement that Nuzu Akkadian must be recognized as different from Babylonian, i.e. as a third Akkadian dialect.

[70] E.g. *la a-wa-tù-mi la a-ni-ik-šu-mi* 4:10; *la ni-ku-ul-mi* 5:13; *la ik-ki-ru-šu-nu-ti* 74:9; *la iq-bu-ú-ma* 75:4; *la za-ku* 89:2.

[71] Examples for *ù* in almost every text. The *ù* is frequently put after *-ma*: *ni-i-nu ni-il-qú-ú-ma ù i-na-an-na ni-ik-ki-ru-ma* 74:19 ff.; *ù ṭup-pa-šu šạ* ^{I}*A-bu-uk-ka a-na pa-ni daiānē*^{meš} *uš-te-li-ma ù il-ta-sú-uš* ... 71:13 ff.

MISCELLANEA

ROMANCE *carestĭa 'DEARTH'

Von Wartburg in his FEW, after having turned down the etymologies ἀχαριστία 'ingratitude' and *χρηστεία 'necessity', says: 'Ulrich R 8, 264 hat auf ein part. *CARESTUS, zu *carēre* "entbehren" zurückgehen wollen (nach *monēstus* usw.). Körting macht mit recht dagegen geltend, dass dann *carestía* das einzige auf *-ía* von part. perf. abgeleitete subst. wäre. Doch ist zu bedenken, dass daneben lt. *caristia* "fest der liebe" bestand und dass dieses die betonung von *CARESTIA beeinflussen konnte. Die ablt. von einem part. perf. **carestus* wird auch gestützt durch aligur. *caresto de messi* "missernte" Kr Jahrb. 8, 139.'

Before all, how does von Wartburg know about an accentuation CARISTÍA, *-orum* 'feast of the cognates' in the Latin word which, in the only poetic example given by the ThLL (Ovid: *proxima cognati dixere caristĭă cari*), surely has the stress on the first *i*? The etymology given by the ThLL: 'fort. ex gr. *χαριστίον mutata' is very doubtful, as *χαριστίον does not exist and is only constructed ad hoc. Even if it did exist the result could only be a Lat. *carĭstia* (cf. even βραχίων > *bracchium*). And how should a word meaning a feast of the Romans influence a newly-formed word meaning 'necessity' in Romance?

Further, the OLigur. *caresto de messi* quoted by Salvioni does not mean 'bad harvest', since it is the translation of a Latin *paucissime nuntiorum*: *messi* is not the plural of It. *messe* 'harvest', but of *messo* 'messenger'. *Caresto* means 'mancanza', as Salvioni puts it, 'penury', 'lacking in', not 'disette', 'dearth'. Von Wartburg has in this instance been the victim of a mirage.

The argument drawn from the impossibility of deriving an *-ĭa-* substantive from a past participle is no more valid: (1) because if the past participle had developed into an adjective, no exception can be taken to an *-ĭa-* substantive, cf. Romance **falsía* and **malehabit-ĭa*, and especially Prov. *membradia*, Gasc. *envezadie* from *membrat* 'prudent', *envezat* 'happy', cf. Adams, Word-formation of Provençal 206 (I omit Roum. *avuţie* from *avut* 'rich' (= *habutus*), because possibly Scheludko, Neuph. Mitt. 1932. 82 is right in tracing back the Roum.-*ie*, not to Romance *-ĭa*, but to *-īlia*); consequently, if **carestus* meant

'lacking' 'needy', an *-ĭa-* derivation is obvious, cf. Prov. *mesquinia* 'misery' from *mesqui* 'miserable'; (2) because the OLigur. *caresto* 'mancanza' could as well suggest a substantive or substantivized participle **carestum, -i* (or **carestus -ūs*) and for an *-ĭa-* derivation from a substantive in Provençal (the main area of *carestia*) there are analogies such as *vigoria* (also OCat.; OIt., XIVth cent.) from *vigor*, *fantaumia* from *fantauma*. The OProv. *cridęsta* 'crierie' (beside OProv., OCat. *cridoria*) NProv. *deganèsto* 'querelle, dispute bourgeoise' show the influence of *tempęsta* 'tempête, bruit, vacarme, querelle, dispute'.

So Ulrich's etymology seems to be open to no objection.

LEO SPITZER

SPANISH *cimarrón*

The Shorter Oxford Dictionary explains *maroon* 'fugitive negro slave' as from 'Fr. *marron*, said to be a corruption of Sp. *cimarrón*, wild, untamed'. But Eng. *maroon* is attested earlier (1666) than Fr. *marron* 'fugitive slave' (1701, in Furetière). If there is a connection between Eng. *maroon*, Fr. *marron*, and Sp. *cimarrón*, Spain (or Spanish America) probably gave the word directly to England (or English America).

Sp. *cimarrón* is explained by Gamillscheg, EWF, s.v. *marron* 2 and by Bloch, Dict. étym. s. v. *marron* II, by an OSp. *cimarra* 'thicket' ('Gebüsch', 'fourré'). Gamillscheg adds that this *cimarra* can not, because of the un-Romance ending *-arra*, belong to Latin *cyma*. Evidently not, for this so-called OSp. *cimarra* 'thicket' is a fancy of Lenz, imagined by this scholar in his Diccionario etimolójico de las voces chilenas 188 (1904), s. v. *cimarra*, for the benefit of his etymology. The article (I am cutting) reads as follows:

> **cimárra*, f.—fam.—falta que comete el colejial cuando en vez de ir al colejio se va a divertir en otra parte, esp. se va a pasear en el campo, los bosques o parques; se dice fulano *hace la cimarra* . . .
>
> **cimarrón, a*—adj. i sust. l.—lit.—alzado, bravío, montaraz, se dice de animales domésticos que huyen de la casa i se internan en los bosques . . .
>
> En este sentido la voz es frecuente, tb. aplicada al hombre, en los antiguos cronistas de Indias 'yeguas simarronas' . . . 'puercos cimarrones o salvajes'; 'indio cimarron o bravo' . . . Especialmente se decia de los esclavos negros fujitivos . . . Segun el informe de Olaverria en 1594 los campos de Santiago 'están llenos de vacas i caballos cimarrones . . . '

2.—lit.—silvestre; se dice de plantas . . .

3. fam. rebelde, rehacio al trabajo, perezoso o poco sociable . . .

Etimolojia: No cabe la menor duda de que *cimarron* es puro castellano antiguo, derivado de *cimarra*, cuyo significado primitivo parece haber sido el matorral, el monte nuevo, lo que en Chile se llama 'renoval' (cp. Zerolo). Talvez se ha dicho primitivamente 'ir a la cimarra' por hacerla; cp. el frances *faire l'école buissonnière. Cimarra* es derivado de *cima* (< lat. *cyma* < griego κῦμα) cuyo significado primitivo es el brote, la rama nueva; de ahí la parte mas elevada del árbol, del cerro u otra cosa; metaf. remate, etc. . . .

Por la derivacion compárese la voz *mancarron* de *manco* por caballo malo . . .

One must notice the doubtful tone of Lenz' suggestion 'cuyo significado primitivo parece haber sido el matorral'. In fact no OSp. or Sp. *cimarra* with the meaning 'bush' is to be found, as far as I know. Zerolo in his 'Diccionario Enciclopédico de la Lengua Castellana' gives, not a *cimarra* 'bush' as one might infer from Lenz' context, but *renoval* in its American Sp. meaning to which Lenz refers. Even if *cimarra* existed, one could not understand its genesis: the un-Romance suffix could not be justified (v. Gamillscheg), whereas *renoval* shows the suffix *-al* with the collective meaning M. L. Wagner has pointed out ('Volkstum u. Kultur der Romanen' 3.87: *hayal* 'hayedo', *maizal*, port. *matagal* 'Gestrüpp, Gehölz'). It is strange enough that the German Lenz thought more of the Fr. *faire l'école buissonnière* 'to play truant'[1] than of the German *die Schule schwänzen* and the similar Sp. *hacer la rabona*. It is obvious that *hacer la cimarra* comes from *cimarra* 'simar' (attested by Nebrija [*zamarra, -on*] and Couarrubias [*çamarro -a*], in Sp. Latin of the 10th century *Zamor*, Dozy-Engelmann 365, and by the French text of 1447: *Deux samarres . . . a la faczon d'Espagne.*); cf. REW, 7563a [arab. *sammūr* 'sable']: 'Sp. *zamarra* (> mfrz. *chamarre*; it. *zimarra, cimarra* "langer Rock" (> frz. *simarre* "Schleppkleid"), pg. *sammarra* [read *samarra*] "schleppender Rock".' One should add the Prov. *samarra* 'long vêtement de dessus' of the *Flamenca*, 13th century, as Diez has already done, also Catal. *samarra* (attested since 1403 as 'insignies de consellers', today, as for Cóuarrubias, a garment of fur for shepherds in the mountains, Dicc. Aguiló). From the idea of *schleppen* 'to trail' we come

[1] Cf. Prov. *faire un plantiè* 'to play truant', from *plantiè* 'vigne nouvellement plantée, plant de vigne' (Mistral). But (*se*) *faire vano* 'to play truant', from *vano* 'guilt' (id.) seems to be an analogy to *hacer la cimarra*.

easily to *schwänzen*[2] = *hacer la rabona.* The *i*-variant may have existed also in Spain.

The word *zamarro* is still to be found in a meaning close to the original in Spanish-America, cf. Lemos, Ensayo de lexicografía ecuatoriana: Ecuador *zamarro* 'calzón hecho de piel, que hoy lo usan únicamente los campesinos', 'individuo pesado, necio, rústico, grosero', Venez. 'hombre taimado', Honduras 'pícaro o bribón'; Gagini: Costarrica *zamarear* 'picardear, bellaquear, andar haciendo bribonadas'.

It is probably not the coarse fur of the shepherd, but the solemn garment of senators or councillors (one may think of the *zimarre* of Venetian senators in pictures of Titian or Tintoretto, which are really 'insignia') which has been used as an antiphrastic joke, as it corresponds to the irreverent mood of school-boy slang.

LEO SPITZER

PORTUGUESE, SPANISH *Cisco, Ciscar*

It is strange that Max A. Luria (LANGUAGE 13.315) rejuvenates the etymology of the late Carolina Michaëlis de Vasconcellos (**ciniscŭlus*) by supposing a [*pulvis*]**ciniscus* with *-iscus* as an adjectival suffix; he overlooks the difficulty of the accent, the basic requirement of a good Romance etymology: **cĭnĭ́scus* could not give anything but Span. **cenésco*, Port. **césco*, and no dissimilation destroying the tonic vowel is admissible (cf. *sarracenesco* = *Sarracen-ĭ́scus*).[1]

Yet the good etymology seems obvious to me: the Port. *ciscar* 'to remove from the soil ... twigs and branches which have been set on fire', mentioned by Luria, gives an excellent hint. I add Minho *cisca* 'caruma seca', Galician *ciscar* 'desparramar, echar por el suelo alguna cosa extiendéndola': the meaning 'to cut off the branches' clearly indicates a **cīsicare*, from **cīsus* = *caesus*, a simple verb **cīdere* being extracted from the compounds *occīdere*, *incīdere* like *clūdere* from *occlūdere*, *inclūdere*: cf. the articles of REW 1471: 1. **caesa*, 2. *cisa*; 1472: 1. **caesalia*, 2. *cisalia*; 1474: 1. **caesellum*, 2. *cisellum*; 1475: 1. **caesorium*, 2. *cisorium*. For *s'c* > *sc* compare *rascar*. From 'cut off the branches and burn them' we come easily to 'charcoal-dust',

[2] One may explain either 'to wag the tail' (Engl. *to wag it*) or 'to curtail' (Cp. *rabón* 'cropped' of a dog).

[1] The corrected form of Luria's suggestion (LANGUAGE 14.65): **cĭ́nĭscu* > *cĭ́nĭscu* (by dissimilation) > **cĭ́nīscu* is not any better: the penultima accentuation law in Latin forbids any such thing as a **cĭ́nĭscus*, and a dissimilation of *ī-ĭ́* to *e-ĭ́* (Sp. *vecino*) is more likely than a development in the opposite way, *ĭ*(= *e*)-*ĭ́*(= *e*) > *ī-ĭ́*.

'cinders' and to the words meaning 'little portion, little piece' (Gonzalves Viana, *Apostilas* 1.295). *Cisco* 'uproar, row, hubbub' (*armarse un cisco* 'armarse una cuestion', Besses; Aragon. *mover cisco* 'armar pendencia', Borao) could be explained like German *Staub aufwirbeln* (cf. *meter cisco en una reunión*, Pequ. Larousse), but the frequent meaning 'defecarse' and especially *cisco* 'ventosidad' in the Asturian jargon *Tixileira* (Vocabulario de la Tixileira by Aurelio de Llano Roza de Ampudia, Oviedo, 1924) would fit an onomatopoeic stem **cisc-* 'peer, cagar'[2] (cf. Munthe, Studier i mod. språkvetenskap 9.99: *ciscarse en* = *cagarse en* or *zurrarse*, *jiñarse*, *chincharse en*, the latter also of an onomatopoeic origin). This stem is supported by the **cisc-ulare* (= **cisc-* + *fistulare*) required by Provençal *cisclar* 'to whistle', REW 3333 and 7890, Modern Provençal *giscla* 'siffler (du vent), pleuvoir et venter ensemble', Cat. *xisclar* and must be distinguished from **cisicare*. The vulgar use in Cuba and Mexico of *ciscarse* in the meaning of 'avergonzarse' and the participle *ciscado* 'estonné, espouvanté' (Oudin) derive from a sense 'cagarse', cf. French *avoir la chiasse*. Mexican *ciscar* 'llamar la atencion del que va a ejecutar un acto cualquiera', especially ' . . . de alguno de los jugadores en el momento preciso de ir a hacer éste un lance, con objeto de que lo haga mal' (Darío Rubio) seems to be connected with *mover cisco* but could also go back, together with this latter expression, to a meaning like popular French *une chierie*, in Paris 'chose fâcheuse, tout ce qui est pénible'. Salamanca *ciscar* 'mover, tocar, empujar' (*no andéis ciscando la mesa* . . . , Lamano) must also be a vulgar use of this verb.

At any rate the two stems **cīsicare* and the onomatopoeic **cisc-are* must be separated although it may not always be easy to determine which Romance word belongs to one or the other family.

LEO SPITZER

POLISH INFLUENCE UPON LITHUANIAN

It is a well-known fact that the Lithuanian language has received numerous loanwords from the Polish. The Lithuanians themselves concede this, and are trying hard to purify their language by replacing the loanwords with genuine Lithuanian expressions either old or newly coined. An almost complete collection of Polish and Russian (especially White-Russian) loanwords used in the early Lithuanian

[2] Cf. the Sp. onomatopeic stems *chist-* 'to make noise', *chisp-* 'spark' etc.

texts was published by Pr. Skardžius.[1] Lithuanian consciousness of the Polish influence was especially alive at a time when the Lithuanians were still familiar with Polish in addition to their mother-tongue. In the same degree as the knowledge of Polish is diminishing in Lithuania, the Lithuanians are less conscious of the remnants of earlier Polish influence upon their language. By now most of the earlier Polish loanwords have been eliminated from Lithuanian speech. However, there are still enough left to indicate clearly the way by which Central and Western European civilization and culture reached the majority of the Lithuanian people. Thus, in spite of all democratic and proletarian opposition, the title *ponas* 'mister' (from Polish *pan*) could not be eradicated. Other words have not yet been recognized as being of Polish origin because the borrowing took place centuries ago and the loanwords underwent certain changes. One example of this kind shall be produced here in the form of the Lithuanian adjective *puikùs* 'beautiful, splendid, magnificent, proud, haughty, disdainful' with a great number of derivations, e.g., *puikỹbė* 'pomp, magnificence, splendor, brightness, haughtiness, arrogance', *puikáuti* 'to be proud, to be haughty, to be arrogant', *puikúoti* 'to behave disdainfully', etc. This word with its unusual, though not rare, diphthong *ui* has caused the comparativist much trouble.[2] The reputation of Lithuanian as being the most archaic living Indo-European language blinded the etymologists who were primarily interested in relations reaching back to the Primitive Indo-European period. From the above-mentioned book of Skardžius (182, cf. *puikavoti* and *puikorius*) one sees that the Lithuanian scholars regard *puikùs* as a genuine Lithuanian word.

The answer to the problem of the origin of Lithuanian *puikùs* and its group lies in the early Lithuanian texts of the 16th and 17th centuries. I shall restrict myself in this discussion to Šyrwid's *Punktay sakimu* (*Punkty kazań*; part I: 1629, part II: 1644) since here we have the same text in both Lithuanian and Polish. We find here a Lithuanian feminine noun *puyka* for Polish *pycha* or *hardość* (1.74, 251, 271, 273, 279; 2.12, 164). In addition to this feminine noun we find an adjective *puykas* for Polish *pyszny* (1.273; 2.41) and a verbal noun

[1] Pr. Skardžius, Die slavischen Lehnwörter im Altlitauischen, published in Tauta ir Žodis 7 (Kaunas 1931).

[2] For a discussion of the origin of the Lithuanian diphthong *ui* cf. Būga, Kalba ir senovė 39, 265 7 (Kaunas 1922).

puykavimas (2.180). It is obvious that the borrowing took place in this way: Polish *pycha* was taken over in the form *puyka* as part of the terminology used by the Christian moralists. From the loanword *puyka* all the other words were derived, but later the original borrowing itself went out of usage and only the derivatives survived.

A form of linguistic influence more hidden than outright borrowing is loan-translation, whereby a new expression is formed according to a foreign pattern but with native linguistic material. Such loan-translations appear most frequently in compound form. New formations of this type are not always successful, but very often they do take root. Usually loan-translations take the place of earlier loanwords. While it is easy to recognize a loanword, the detection of loan-translations is not possible without a good knowledge of both (or all) languages in question. The Lithuanian language is immensely rich in forms coined after foreign patterns, and many of these original patterns were Polish. It is one of the many merits of Max Niedermann to have repeatedly (since 1922) called the attention of his colleagues to this interesting complexity of problems.

Here are a few examples to illustrate how Polish expressions were translated into Lithuanian. The Lithuanian word *darbýmetė* 'working-time' theoretically might well have been copied from German *Arbeitszeit*. However, the fact that it occurs already in Šyrwid (1.284, 328) proves that it was copied from Polish *czas roboty* or *czas robotny*. Similarly *bediẽvis* 'a godless person' goes back to Polish *niezbożny* (Šyrwid 1.237), Lithuanian *visagãlis* 'almighty' to Polish *wszechmocny* (Šyrwid 1.175, 210), and Lith. *mielaširdỹstė* 'mercifulness' to Polish *miłosierdzie* (Šyrwid 1.158). While the words mentioned gained ground, maintained themselves, and became permanent parts of the language, there have, of course, been numerous unsuccessful attempts. One example of this type must suffice. Three times we find in *Daukša's Postilė* of 1595 (564–5) the expression *balta galwa* which literally means 'white head'. However, from the context in which it occurs, one should expect it to mean either 'girl' or 'woman'. This interpretation is confirmed by Polish, where we find a compound word *białogłowa* meaning 'woman'. This attempt at word-creation proved futile and we need now some knowledge of Polish in order to understand the passage.

Not only single words, but entire word categories, have been formed after foreign patterns. Thus the Lithuanian verbs in *-auti* very often stand for Polish verbs ending in *-ować*: e.g., Lith. *klebonáuti* 'to be

rector of a parish', *kunigáuti* 'to be priest', *karaliáuti* 'to be king' cf. Polish *proboszczować, królować.*

The Lithuanian language teachers, who know Polish, are very much opposed to a too slavish imitation of Polish patterns. Therefore, they condemn the Lithuanian adverb *neužìlgo* 'soon' because it is an unmistakable copy either of Polish *nezadługo* or of Russian *nezadolgo.* However, the best of them slip. Thus we find the verb *nubúti* 'to forget', a clear imitation of Polish *zabyć* or *zabywać,* in the writings of Professor Balčikonis, the master of Lithuanian style (Brolių Grimmų Pasakos 2.86).

Poland was already an important power and a cultural factor in Europe early in the Middle Ages. At that time all of Central and Western Europe stood under the influence of the Latin language. Latin dominated also the life and culture of the educated Poles and Lithuanians. Its influence reached the Lithuanian language primarily through the channel of Polish, since the educated Lithuanians spoke Polish. The above-mentioned expression, Lith. *neužìlgo,* Polish *nezadługo,* was copied from medieval Latin, where the expressions *non post multum* and *non post multos dies* were very common.[3] The predominance of Latin during the Middle Ages has left a permanent imprint on the vocabulary, word structure, and syntax of the European languages. This is a universally recognized fact. It is not, however, so well known that the Latin influence reached Lithuanian also through the mediation of Polish. The partiality of the best modern Lithuanian authors (e.g., Krėvė-Mickevičius) for the use of synonym-couplets instead of a single word has a striking parallel in medieval style. As an illustration I shall quote but two passages from Vincas Krėvė, Dainavos šalies senų žmonių padavimai (Kaunas 1921): *puikiū ir gražių viẽtų* (7) 'beautiful places' and *kõ tìk nèkalba, kõ nèmini jū̃ skambì dainẽlė* (7) 'ah, what their melodious song does not mention!' In the first example the 'beautiful' is expressed by two synonymous adjectives (*puikių* and *gražių*), and in the second two verbs (*kalba* and *mini*) are used to express the action of 'mentioning'. This same stylistic device is frequent in the Middle Ages. In the Journal of English and Germanic Philology 34.591 (1935), I have shown that this usage prevailed in medieval German prose, e.g., *mit dem heiligen würdigen sacrament, demüetikeit und andaht, betrahten und gedenken, entblöset und beroubet,* etc. Both the modern Lithuanian and the

[3] Cf. W. Schulze, KZ 43.189; A. Deissamnn, KZ 45.60.

medieval German expressions have their source in medieval Latin style. The Polish language served as an intermediary for this transfer from Latin to modern Lithuanian.

Since both Middle High German and Polish were under the influence of medieval Latin, it is conceivable that a knowledge of Polish may put even a student of medieval German into a more advantageous position. Polish syntax has preserved many expressions which in medieval times were also common in German, but are not known in modern German. The Middle High German sentence *diu sol im wesen von rehte bî* (Reinmar; Minnesangs Frühling 153,20) for modern German *er muss diese von rechtswegen haben*, reminds us of the Polish expression *u mnie jest* (= Russian *u menja jest* = 'there is with me') instead of *ja mam* 'I have'. Such an expression would be impossible in modern German. Another example of MHG and Polish conformity is furnished by the MHG adjective *wîz*, the original meaning of which is 'white', but which also occurs with the meaning 'clean'. The latter meaning appears in *Kudrun* 1070,3 *bereiten wîziu kleider den Hartmuotes helden*. This reminds us of Polish *biały* 'white', *białoręki* 'one with white hands', and Lithuanian *baltas, baltarankis*. That Lithuanian *balta diena* 'the bright day' is copied from Polish *biały dzień* is obvious, especially since the Germans do not say *der weisse Tag* but *der helle Tag*. However, in medieval German the day was also called 'white', as can be seen from the 'Spruchgedicht' Salomon und Markolf 853 6 (edited by Walter Hartmann, 1934. Cf. Journal of English and Germanic Philology 36.131-2):

Du spreche, milch si *wizer* dan der *dag*.
Die warheit man nu pruffen mag,
daz der *dag* ist *wizer* me
dan beide milch und ouch der sne.

This poem was written in the Middle Franconian dialect toward the end of the 14th century. Since we know that its unknown author had a medieval Latin work as his source, we are forced to the conclusion that Polish *biały dzień* was formed after a medieval Latin pattern.

There are numerous instances of MHG and modern Lithuanian conformities, the connecting link of which is still missing. However, it is now clear that in most cases this missing link will be found in Polish. A few examples follow:

In the Nibelungenlied (C) 450,2 and (B) 441,2 we find the numeral *vierdehalbiu* 'three and a half', which reminds us of Lithuanian *puske-*

tvirto while the modern German and Polish forms are similar to the English expression; cf. also French *trois et demi*. Apparently Lithuanian here preserves something that was more widely used in the Middle Ages, a construction of which Polish *półtora*, Russian *pultora*, and German *anderthalb* 'one and a half' are modern remnants.

The Germans say *jemanden um etwas beneiden* 'to envy a person (for) a thing'. The MHG construction of this verb was: *jemandem etwas neiden*; cf. Dietmar von Eist's verse *daz nîdent schône frouwen* (Minnesangs Frühling 37,15), a construction required also by Lithuanian *pavydėti* and Polish *zazdrościć*.

Strange as it may appear at first sight that Lithuanian *mylėti* 'to love' should also mean 'to entertain lavishly', especially 'to regale with drinking', a similar meaning, 'generosity, liberality', is also connected with the MHG noun *liebe* in the Nibelungenlied (C) 266,3:

Man sach si tägelîche nu rîten an den Rîn,
die zer hochgezîte gerne wolden sîn,
die *durch der künige liebe* kômen in daz lant.
Man gap dâ genuogen bêdiu ross unt ouch gewant.

'Now one saw them ride to the Rhine every day, people who wanted to take part in the feast and who, attracted by the generosity of the kings, came into the country. They there gave to many of them horses and the equipment'.

The study of the influence of medieval Latin upon Central and Eastern European languages should be a very attractive task for a person well enough equipped linguistically. In such an enterprise Polish would hold almost a key position.

ALFRED SENN

BOOK REVIEWS

La Posizione della Lingua Italiana nel Mondo Neolatino. Pp. 43, with two maps. By Walther von Wartburg. Leipzig: Keller, 1936.

This brochure contains three lectures delivered in Rome in 1936: 'Origine e Vicende dello Spazio della Lingua Italiana'; 'Vom Latein zum Italienischen'; and 'Caratteristica Comparata dell' Italiano e del Francese'. The first two are of an historical nature, with considerable emphasis on political influences and 'substratum' theory. The third is an impressionistic comparison (although liberally documented from Wartburg's great erudition[1]) between French and Italian, intended to demonstrate that French is direct, disciplined, and intellectual, whereas Italian is 'una lingua impetuosa, impulsiva, piena di libertà, di sorprese, di vita spontanea' (42). An effort is made to bring these characteristics into relation with the 'national characters' of the two peoples.

For several reasons one can hardly consider this last lecture as successful. Wartburg's comparison is vitiated from the start by being based on the literary languages. Especially in France the standard language is by no means representative of popular speech[2]; if we were

[1] Which has not prevented, however, some errors in fact as well. On page 5 we have the surprising statement 'per Dante [in the De vulgari Eloquentiâ] il problema storico latino-romanzo non esisteva, perchè il latino non era per lui quello ch'è per noi: una fase anteriore dell' italiano e delle altre lingue neolatine'. As a matter of fact, d'Ovidio long since pointed out (Arch. Gl. It. 2.84) that Dante must have conceived of some kind of historical relation between Latin and Italian, in deriving Italian words from Latin, as in *sì* < *sīc*, etc. Furthermore, as I have already pointed out (Language 12.103), in the phrase *quia magis videtur inniti grammaticae, quae communis est* (De v. E. 1.10.4), although *inniti* 'to rest upon' may mean no more than 'to be similar to', as d'Ovidio thought, it may mean here 'to be derived from', in which case it would be a clear indication of Dante's conceiving an historical relation to exist, and *communis* would indicate 'a common ancestor'.

[2] Wartburg's realization (possibly a trifle exaggerated) of the influence of upper-class French society in shaping the standard language (35) does not seem to have brought with it an equal realization that the distance thus created between upper- and lower-class French speech (which Wartburg also emphasizes [28]) renders the two literary languages useless as bases for comparison, and only confuses his argument throughout.

to compare the speech of Parisian workmen with that of Roman or Florentine workmen, I doubt whether we should find much more 'discipline' or 'intellectuality' in the one than in the other.

Apparently Nyrop's trenchant remarks[3] have not yet exorcised efforts to correlate phonetics and 'national character'; Wartburg's attempt (31 f.) is by no means convincing. Granted the difference in aesthetic impression produced by the accents of the two languages (and even this difference is not very great when French and Italian are contrasted with a language such as Hungarian, with its distinction between long and short vowels and its typewriter-like stress accent), that proves nothing about the 'spiritual' qualities of the speakers.[4] Wartburg's discussion of Italian 'freedom' in word-order as opposed to French 'fixity' (34 f.) is founded upon considerable exaggeration; he speaks as if absolute freedom were preserved in Italian, whereas the subject-verb-order is nearly as fixed in Italian as it is in French, as contrasted with Latin or Sanskrit.

Indeterminate and abstract elements ('Sprachgeist', 'national spirit', 'scienze dello spirito umano'—is not this last somewhat of a contradiction in terms?) are constantly brought into the discussion in all three lectures (18, 29, etc.). At the outset of the third we are told that 'la lingua è piuttosto l'anima e lo spirito d'un popolo incarnati, resi percettibili' (30), and at the end 'le diverse scienze dello spirito umano . . . si riuniranno per tracciare le grandi linee dei caratteri nazionali e del suo svolgimento . . . Quel giorno, toccherà alla linguistica un posto centrale in questo supremo tentativo di capire il senso della vita umana e delle singole sue manifestazioni' (43). Before introducing such elements as 'nation', 'soul of a people', etc., into the discussion, it might have been well to give them valid objective definitions—a task which has never been performed, and which some suspect to be impossible of performance. All that we have here is a mixture of Crocean aesthetics (now dominant in Italian linguistics[5]) with reflections—at a hundred years' distance and through the distorting mirrors of popular misconceptions—of Humboldt's theory of

[3] Grammaire historique de la langue française 1.§§534–7 (Copenhagen, 1914).

[4] In 'national characteristics', if there be such a thing, the Italians seem to me much nearer to the Hungarians than to the French, but the greater similarity hardly extends to the languages! All such comparisons, as Sapir (Language 234) and many others have long since pointed out, are mere rubbish.

[5] Despite the pointed comments of Goidànich, one of the few neo-grammarians in Italy, in Arch. Gl. It. (sez. Goidànich) 20.1–59; 21.59–105.

the identity of 'Volksseele' and 'Sprachgeist', as now incorporated into mystical 'ideological' dogmas.

It is not pleasant to see this type of material coming from a scholar of Wartburg's caliber. I do not believe, however, that such lapses can be excused by a mere 'aliquando dormitat bonus Homerus'. Italian linguistics is at present at a standstill, and is in need of a vigorous revivification. But Crocean aesthetics and a priori political doctrines constitute a serious hindrance to the furtherance of these aims—some consider them, in fact, the cause of the present stagnation. Rigorous application of scientific method—involving, above all, strict objectivity and abstinence from impressionistic or aesthetic judgments—is the only way by which Italian linguistics, like any other, can advance.

ROBERT A. HALL, JR.

Die indogermanischen Nasalpräsentia, ein Versuch zu einer morphologischen Analyse. Pp. 246. By F. B. J. KUIPER. Amsterdam: N. V. Noord-Hollandsche Uitgeversmaatschappij, 1937.

This book is, as the title suggests, a discussion of the IE nasal present stems, but more particularly of those characterized by the nasal infix (Skt. Class VII.), which involve a more serious difficulty than do the stems in *-neu-* and *-nā-*, since they present a feature, namely infixation, which appears to violate the accepted principles of IE morphology.

The first chapter is an account of the views of grammarians with regard to the verbal class in question, from Pāṇini and the Greek grammarians, through the eighteenth century, the early and middle nineteenth century and the period of the Neo-grammarians down to the present time. The second chapter, in which Kuiper deals with the heart of the problem, the actual origin of the nasal-infixing class, is followed in the third by a description of the development of this class in the separate branches of the IE family. Chapter IV. contains a brief account of the semantic character of the nasal verbs; of this aspect of the matter the author abstains from any detailed discussion.

The history of the problem as given in Chapter I. leads the author (33 f.) to lay down three basic assumptions which may be regarded as reasonably certain:

1. The Skt. fifth, seventh, and ninth classes, treated from the point of view of their development, represent a single type (de Saussure).

2. The assumption of infixation in primitive IE is in principle not permissible (Brugmann, etc.).

3. The contradiction which in a sense exists between 1. and 2. can be removed only by the assumption that the *-eu-* in Class V., the *-ā-* in Class IX., and the final consonant of the stem in Class VII., are formative extensions, an assumption which was already suggested by the material itself.

With these doctrines in mind the author proceeds to explain the origin of the nasal-infixing present stems. In Skt. *śināṣṭi* as compared with Av. *syazdaṯ* he sees a variation of two athematic stems made by extension of the same root with *-as-* (in the plural *-s-*), one with a nasal extension before the *-as-*, the other without the nasal. Seeking for similar parallels, where instead of *-s-* we find a stop, he cites (44) Ved. *bhiṣákti abhiṣṇak* (3 sing. impf.) as an example, and after a few pages he goes through a list of roots (as the term is usually understood), from which he abstracts final *-eḱ-*, *-eg-*, *-et-*, *-ed-*, etc. as extensions. The whole subject is treated with careful attention to vowel-gradation, and the theory which explains the nasal present stems as containing further extensions after the nasal element is of course a familiar one; but the author's reconstructions appear to me to call forth the same objection made by Kent in his review of Benveniste's Origine de la Formation des Noms en Indo-Européen (LANGUAGE 13.251), namely, that syllables are made to begin with vowels and end with consonants. Moreover, by his liberal application of this type of analysis he reduces many of the IE roots, or elements containing the essential meaning of the word, to almost nothing. Thus (52) Skt. *vádati*, *vagnúḥ*, *vāghát* (with Lat. *voveo*), Av. *aogədā* (with Gr. *εὔχομαι*), Gr. *εἴρω* are all referred to a single root *u̯* extended with various determinatives. Certain cases in which a root has two variant forms, one ending in a voiced, the other in a voiceless stop, are explained as doublets arising under internal sandhi conditions, where various personal endings were added directly to the athematic stems. This is an ingenious explanation; I believe, however, that the author gives the athematic type of inflection in IE an unduly wide range. The fact that in nearly all IE languages during their known history the thematic forms have constantly increased at the expense of the athematic does not necessarily prove that this process went on to an equally great extent between the last stage of undifferentiated IE and the oldest Sanskrit. In other words I suspect that the thematic type of inflection may have been almost as frequent in late IE as in Vedic. The familiar thematic type represented by Skt. *bhávati* is treated as 'conjunctive' in origin, in which connection we must understand that

modal and indicative uses of the verb were not so clearly distinguished in IE as we find them in the languages actually known. If Kuiper has in mind a comparatively early stage of IE, he may be justified in his theory of the origin of these thematic forms as well as in certain other ideas to which I have taken exception; but the book contains much that is daring and which may fail to convince many of its readers.

On the other hand, it is carefully executed and contains few typographical errors.

JAMES WILSON POULTNEY

One Hundred New Selected Nuzi Texts. Transliterated by ROBERT H. PFEIFFER. With translations and commentary by E. A. SPEISER. Pp. xi + 168. (The Annual of the American Schools of Oriental Research, vol. XVI, 1936).

It is most gratifying to see the publication of the Nuzu documents continued. Their importance for both history and linguistics needs no comment. One must hope that the cooperation of Pfeiffer and Speiser will soon make more of this material accessible to science.

The earlier Nuzu publications contained only copies in the original script. The present volume initiates an innovation in as much as transliterations have replaced the copies, and an interpretation of the published texts has been added. The second feature will be welcomed by everybody.

As to the first, the philologist must insist on having the texts in the original script. Since most of the tablets are damaged or broken, he must see the exact drawings of visible traces, the exact position and the measurement of the gaps, in order to be able to fullfil his duties as an interpreter. Even in perfectly preserved passages, the transliteration may prove misleading by giving preference to one of several possible readings.

The juridical experts, who have frequently stressed the desirability of transliterations, must be reminded that reading and interpretation of a cuneiform document must go together. The intricacies of the cuneiform system make impossible a sound interpretation on the basis of transliterations alone.

The economic question, though often referred to (5), does not exist. The mechanical reproduction of handwritten copies (which can be arranged much more economically than in previous Nuzu volumes) is certainly cheaper than the hopelessly difficult and expensive printing of accurate transliterations. Furthermore, copies are final, transliterations never.

It is very characteristic that Pfeiffer, having chosen to give transliterations, found himself confronted with difficulties as soon as it came to practice. He sees (11) two possible methods: either more or less mechanical transposition by means of the 'normal' Assyrian values, or etymological interpretation which, in the case of ambiguity, chooses the value required by the related dialects. Either method, though not fully satisfactory, is justifiable; but it must be applied consistently. Any mixture results in confusion which impairs the usefulness of the transliteration, particularly to the uninitiated for whom the transliteration takes the place of a primary source. Such a one will be helpless when he finds almost side by side (this is only one example among dozens) *i-ka₄-al-la-šu-nu-ti* (5:26) from *akālu* 'eat' (etymological interpretation), but *i-qa-aš-ša-du* (5:30) from *kašādu* 'meat' (transposition).

Pfeiffer has recognized that the method of etymological interpretation is preferable, but his remarks (11) on 'the outlandish spelling' show that he has as yet no definite opinion on the phonology of the dialect. In the back of his mind there is apparently the feeling that the etymological interpretation must be controlled by an intensive study of the writing habits, of the phonological features, and of the grammar of the dialect concerned. That is just the trouble: a scientifically reliable transliteration is possible only where the language is well known; the knowledge of the language, on the other hand, can come only from texts. This constitutes a vicious circle as far as transliterations are concerned. They are, consequently, always subject to revision.

Apart from this general objection Pfeiffer's readings are reliable[1]. A number of accents have got lost in print[2]; such imperfections are unavoidable, they are nevertheless embarrassing where lack of copies prevents control.

Speiser's translations are generally well done. The use of tenses, etc., sometimes needs improvement. Some syntactical features of the dialect should be investigated to give the interpretations a firmer basis. The increase in our knowledge of the foreign substratum (pantheon,

[1] I noticed only a few real misprints which Pfeiffer was kind enough to confirm: 18.11 read *ibalkatu*^{tu} (text KI.BAL.TUM); 25.4 *ú-še-ri-ib-šu*; 25.27 *ša-at-tu-mar-di* (as in the index of proper names); 40.6 *it-ta-bal-kat-ma*; 63.4 *ra-ma-an*; 79.10 *iš-tù*.

[2] 8.7, 11 *ṣí-di-ta*; 8.25 *ṣí-mi-it-ti*; 9.10 *kaspi*^{pí}; 45.8 *uš-te-ṣí*; 54.22 *tup-pí*; 63.12 *ú-še-iṣ-ṣí*; 71.7 *iš-ta-pár-mi*.

numerals) is particularly gratifying. I agree with Speiser in the opinion that the Ḫurrian nature of this substratum should not be questioned.

ALBRECHT GOETZE

Oriental Manuscripts of the John Frederick Lewis Collection in the Free Library of Philadelphia. Pp. 248 + 48 plates. By MUHAMMED AHMED SIMSAR. Philadelphia: 1937.

The late John Frederick Lewis of Philadelphia bequeathed a collection of Oriental manuscripts, miniatures, and other art objects, to the Free Library of his native city. To our fellow-member Dr. Simsar was entrusted the task of preparing a descriptive catalogue of the manuscripts, which are presented to us in this splendid volume. The 153 manuscripts are in 13 different scripts, the Arabic predominating with 100; they represent 15 different languages, 52 being in Persian, 35 in Arabic, 13 in Ethiopic, 10 each in Turkish and Hebrew. Dr. Simsar is himself at home in Persian, Arabic, and Turkish, and availed himself of skilled aid with regard to certain other languages and alphabets; as a skilled calligraphist and an expert in Oriental art, he is able to draw upon his specialized knowledge in the accounts which he has given, to which he has added many details as to the life and works of the authors, not accessible elsewhere.

The most notable features of the collection are the 21 copies of the Koran, and 10 copies of the Shah-Namah. A number of the other manuscripts are of rare works, some otherwise unknown, and a number by famous calligraphers. Of great linguistic interest is his No. 38, a dictionary of Persian words, dated 1620 and written within the lifetime of its author Surūrī; other works also are of importance to our science.

The value of the volume is increased by the 48 full-page plates giving examples of bindings, scripts, and illustrations, and by complete indexes of the titles of works and of persons' names, occurring in the text.

ROLAND G. KENT

NOTES AND PERSONALIA

THE LINGUISTIC SOCIETY OF AMERICA will hold its Fifteenth Annual Meeting at New York, in conjunction with the conventions of the American Anthropological Association, the Modern Language Association, and the Society for Biblical Literature and Exegesis, on December 27–29, 1938. The other associations hold their sessions on December 28–30, as does also the American Philological Association, which meets at Providence, R. I.

THE LINGUISTIC INSTITUTE will again be held at the University of Michigan, as part of the Summer Session, June 27 to August 20. Inquiries should be directed to Prof. C. C. Fries, Director of the Linguistic Institute, Angell Hall, University of Michigan, Ann Arbor, Mich.

THE LINGUISTIC SOCIETY OF AMERICA will hold a summer meeting for conferences and the reading of papers, at Ann Arbor, during the last week-end of July. Arrangements are in the hands of the Director of the Linguistic Institute and the Secretary of the Society.

THE LINGUISTIC SOCIETY calls attention to the possibility that members may desire to unite in local gatherings for the discussion of scholarly and other matters of common interest. The formation of such clubs will be entirely optional with the local members; but the activities of the clubs, if reported to the Secretary of the Society, will be briefly chronicled in Notes and Personalia.

THE LINGUISTIC SOCIETY OF AMERICA has no political or religious affiliations or prejudices; nothing which is printed in its publications is to be taken as expressing any partisanship of the Society in such matters. It is therefore a matter of regret that some persons have misinterpreted the quotation from a personal letter written about June 1, 1937, and printed in Language 13.260, in connection with a high honor which came to one of our distinguished members. This quotation was printed only because it seemed to the Secretary of the Society, who assembles the material in Notes and Personalia, a fine condemnation of internecine strife.

THE HONORARY MEMBER elected at the Chicago meeting has accepted election. He writes:

Paris, le 12 janvier, 1938. Votre lettre du premier janvier m'arrive ce matin même, et je m'empresse d'y répondre pour vous dire combien je suis touché de l'élection si flatteuse qu'a faite de moi la Linguistic Society of America comme membre d'honneur. Je vous prie de remercier très sincèrement l'Executive Committee et la Society, de la haute distinction dont je suis l'objet. Il va sans dire que je l'accepte avec joie et reconnaissance, comme un nouveau témoignage des excellentes relations qui unissent les savants de nos deux pays.—J. Vendryes, Doyen de la Faculté des Lettres.

ROMANUS F. BUTIN, Professor of Semitic Languages and Literature at the Catholic University of America, and a member of the LINGUISTIC SOCIETY OF AMERICA since 1929, was killed in an automobile accident near Washington on December 8, 1938, in his sixty-eighth year.

A native of France, he came to this country in 1890, and studied at the Marist College and the Catholic University, where he received the degrees of S.T.B. in 1898, S.T.L. in 1900, Ph.D. in 1908. After teaching for some years at the Marist College and at Jefferson College (St. James, La.), he returned to the Catholic University in the Department of Semitic Languages, becoming Professor in 1923; he was also Curator of the Museum of the University.

Dr. Butin's special field of work was the Hebrew language and literature. He was Acting Director of the American School of Oriental Research in Jerusalem, 1926–7. During explorations he discovered early inscribed tablets on Mt. Sinai, by the interpretation of which he threw much light on the early history of the alphabet. He was associated with Prof. Henry Hyvernat in the editing of the Corpus Scriptorum Christianorum Orientalium. He was also a specialist in the history of the French language, in which he directed graduate work at the Catholic University.

He was a member of numerous learned societies, and the author of several volumes on Hebrew and of many articles in the Catholic Encyclopaedia and in scientific journals.

DOROTHY MAY PASCHALL, Instructor in Latin at the Texas State College for Women, Denton, Texas, and a member of the LINGUISTIC SOCIETY OF AMERICA since 1936, died in Dallas, Texas, on February 1, 1938.

She was the daughter of Professor Clarence Paschall of the University

of California, and was born in Oakland, California, in 1908. She received the degree of A.B. from the University of California in 1929, and the M.A. in 1930. The next five years she spent at the University of Chicago, serving as instructor in Latin during part of that time. In 1935 she received the degree of Ph.D. In 1935-36 she was Research Assistant in Linguistics at Chicago, and in 1936-37 she was Sterling Fellow in Linguistics at Yale. Last September she began her work at the Texas State College for Women.

Dr. Paschall impressed all scholars who came to know her with her unusual energy, initiative, and acumen in the investigation of scientific problems. No one doubted that a brilliant career was before her. Her dissertation on The Vocabulary of Mental Deficiency in Roman Comedy is ready for the printer, and it is hoped that it may soon be published. The study of the technical terminology of Roman religion, to which she devoted her last years, had already yielded a paper on The Origin and Semantic Development of Latin *vitium*, which was published in TAPA 67.219-31. Her numerous original ideas on Italic linguistics seemed to insure a steady productivity.

THE FOURTH INTERNATIONAL CONGRESS FOR THE UNITY OF SCIENCE will be held at Girton College, Cambridge, England, July 14-9, 1938. The general theme will be Scientific Language, and the successive days will be devoted to General Addresses; Special Problems of Scientific Language; Linguistics, Behavioristics; Formalization of Special Sciences; Parallel Sessions with Various Themes; Summaries. The registration fee for active members is $5.00. Further information may be secured from the International Institute for the Unity of Science, 267 Obrechtstraat, The Hague, Holland, or from Prof. Charles W. Morris, University of Chicago, Chicago, Ill.

THE SUMMER SEMINAR IN ARABIC AND ISLAMIC STUDIES will be held for a second time at the Graduate College, Princeton University, Princeton, N. J., June 25 to August 6. Courses will be given in the Arabic, Turkish, and Persian languages, as well as on other phases of Arabic and Islamic culture. Information may be secured from Dr. Nabih A. Faris, 58 Mercer Street, Princeton, N. J.

THE SECOND INTERNATIONAL CONGRESS OF ANTHROPOLOGICAL AND ETHNOLOGICAL SCIENCES will be held at Copenhagen, Denmark, August 1-6, 1938. The membership fee is fixed at 30 Danish crowns. Information may be secured from the Secretaries of the Congress, National Museum, 10 Ny Vestergade, Copenhagen.

ROLAND G. KENT, under appointment by the President of the Society, served as official representative of the LINGUISTIC SOCIETY at a joint meeting of the American Philosophical Society with societies and organizations interested in the publication of research, in the Hall of the Philosophical Society, 104 South Fifth Street, Philadelphia, on February 18, at which economies in printing and newer methods of publishing research material were discussed.

RALPH MARCUS has been promoted to be Professor of Semitic Philology at the Jewish Institute of Religion, in New York City.

JAMES W. POULTNEY is Instructor in Classics at the University of Nebraska during the present academic semester.

ALFRED SENN, Professor of Germanic and Comparative Philology at the University of Wisconsin, has accepted a call to the University of Pennsylvania as Professor of Germanic Philology, the duties of which he will assume at the beginning of the academic year 1938–9, in succession to Daniel B. Shumway, who becomes Professor Emeritus.

LYDIA E. WAGNER has just published her doctoral dissertation, The Scientific Interest of Friedrich von Hardenberg (Novalis); orders for copies may be sent to the author, care of Department of Germanic Languages, University of Nebraska, Lincoln, Neb.

LANGUAGE MONOGRAPH NO. 12, The Sounds of Latin, by Roland G. Kent, is needed by the Society for stock. For each copy in reasonable condition, mailed to the Linguistic Society of America, Bennett Hall, University of Pennsylvania, Philadelphia, Pa., the sum of One Dollar will be sent, until a sufficient reserve is built up.

THE FOLLOWING NEW MEMBERS FOR 1938 were received into the LINGUISTIC SOCIETY subsequent to the last published list, and up to February 28, 1938:

Harold Walter Bailey, Ph.D., Professor of Sanskrit, University of Cambridge; Queen's College, Cambridge, England.

Sarah Tarler Bergholz, A.B., 5315 Dorchester Ave., Chicago, Ill.; *American Indian linguistics.*

Pierre C. Delattre, Ph.D., Assistant Professor of French, Wayne University; 702 Glynn Court, Detroit, Mich.; *Old French, experimental phonetics.*

Girdler B. Fitch, Ph.D., Instructor in Romance Languages, Ohio State University, Columbus, Ohio.

Meredith Knox Gardner, Graduate Assistant in German, University of Wisconsin; University Club, Madison, Wis.; *German, semantics.*

J. Homer Herriott, Ph.D., Assistant Professor of Spanish, University of Wisconsin; Bascom Hall, Madison, Wis.

Winfred Philip Lehmann, Research Assistant in German, University of Wisconsin; Ableman, Wis.

Herbert Penzl, Ph.D., Assistant Professor of German, Rockford College, Rockford, Ill.; *English, dialect geography, phonemics.*

Holland DeWitte Roberts, M.A., Assistant Professor of Education and of the Teaching of English, Box 2423, Stanford University, Calif.; *meaning and function of language.*

Sister Mary Sarah Muldowney, Ph.D., Saint Joseph College, 1678 Asylum Avenue, West Hartford, Conn.; *word order in late Latin.*

Eugene Tavenner, Professor of Latin and Greek, Washington University, St. Louis, Mo.

Salomón Narciso Treviño, A.M., Instructor in Spanish, University of Chicago, Chicago, Ill.; *experimental phonetics.*

Edward Jay Upton, P.O. Box 214, Sandy Creek, N. Y.; *Indo-European.*

Robert H. Weidman, B.A., Instructor in French and German, University of Wisconsin; 623 West State St., Milwaukee, Wis.; *Middle High German.*

THE SEMANTIC DEVELOPMENT OF *Verhältnis*

HANS SPERBER

OHIO STATE UNIVERSITY

Any attempt to clarify the development of the German word *Verhältnis* must start from the article, in which the Deutsches Wörterbuch discusses it—rather a voluminous article, but, as will be seen presently, by no means an exhaustive one.

The facts stated by Dr. Wülcker in the DWB are mainly these: *Verhältnis* occurs in Christian Wolf's Mathematisches Lexikon (1716), where it is used as a feminine; the same gender prevails in the earlier writings of Kant, and is to be found in many other authors of the 18th century. During the latter part of that period the word is in widespread use. Wülcker gives an abundant list of passages, in which Herder, Wieland, Goethe, and many other important authors of that period are richly represented.[1]

The mere fact, that the word, for whose previous existence only the scantiest evidence is furnished, appears to be all but omnipresent in the later part of the 18th century, is apt to arouse the suspicions of a critical reader. May it be that the break of about thirty years between Wülcker's quotation from Wolf and the main part of his selections, is due not to real facts, but to our imperfect knowledge of the vocabulary of the earlier part of the 18th century? And if so, may we not expect that the semantic development of *Verhältnis*, which is by no means sufficiently clear from Wülcker's collections, may be elucidated from sources which the DWB has overlooked?

Another objection to Wülcker's article applies to his way of ar-

[1] The DWB also records one passage earlier than Wolf; Paul Melissus(-Schede) uses *Verhältnis* in his translation of the Book of Psalms (1572), where, however, it is used in a sense which is quite different from its later meaning. It signifies 'Verwahrung, Erhaltung', and can hardly be said to be identical with the modern German word. We have to recognize two different derivatives: one based on *sich verhalten* 'to keep oneself in a safe position' (und brochent den Kirchturn von Munoltzheim, . . . wande sü entsoszent, dasz sich der bischof daruf wurde verhalten. Closener, Städtechron. 8, 81); the other from *sich verhalten* 'to be in proportion, to be in a certain ratio to'.

ranging the various meanings in which *Verhältnis* is found. The main headings under which he groups his selections are the following: '1. Zustand, worin sich etwas aufhält, befindet', as f.i. 'in der Zwischenzeit erfahren wir die Verhältnisse des oberen Italiens' (Goethe 43,8); '2. Zustand eines Dinges, soweit er sich aus dem Vergleiche mit, aus der Beziehung zu anderen Dingen ergibt; das Verhalten zu etwas anderem, die Wechselbeziehung überhaupt'. This second group again is divided into three minor sections: 'a) Verhältnis, als Bezeichnung des Verhaltens zweier realer Dinge zueinander (seine Werke sind schöne Herkules, die oft viel zu schmächtige Beine, einen viel zu kleinen Kopf haben, als es das Verhältnis mit der breiten Brust erforderte; Lessing, 5,71), b) von Personen untereinander (so entspann sich nach und nach ein so vertrautes Verhältnis zwischen uns, dass ich gewissermassen die dritte Person in ihrem Bunde wurde; Wieland 38,222), c) als Bezeichnung des Verhaltens abstrakter Dinge zueinander (das Verhältnis unserer Materie zu unserem Geiste ist vielleicht so aufwiegend gegeneinander als die Länge unserer Tage und Nächte; Herder /1820/ 3,11).'

It is only at the very end of this last group of quotations that Wülcker finds a place, and an extremely inconspicuous one, for the use of *Verhältnis* as a mathematical term, which, by his reference to Christian Wolf, he himself has shown to be of a much earlier date than the more general applications of our word.

If this classification is intended to be, not merely a convenient means of presenting a great variety of single occurrences in a synoptical arrangement, but an attempt to show how the different meanings have developed from each other, then it must be said, that this arrangement is open to grave objections. Starting from the widest sense of the word, Wülcker gradually comes down to a very special meaning, its mathematical one. This is hardly in accordance with the general tendencies of semantic changes: while specialization of meaning (Paul's 'Bedeutungseinschränkung') is undoubtedly a comparatively rare process, its opposite (generalization of meaning, 'Bedeutungserweiterung') appears to be far the more frequent.

That this general principle holds good for the history of *Verhältnis*, becomes clear from a fact, which, after the publication of Wülcker's article, has been pointed out by Feldmann, Zeitschrift für deutsche Wortforschung 7.56, and by Piur, Studien zur sprachlichen Würdigung Christian Wolfs, Halle, 1903).

Wolf, according to these authors, was by no means the first to use *Verhältnis* as a mathematical term. It seems to have been coined by

Johann Christian Sturm, who, following the puristic tendencies of his time, attempted to replace the Latin *proportio* by a German word. In the preface of his Sandrechnung oder teutscher Archimedes (Nürnberg, 1667) we find the following paragraph: 'Damit der begierige Leser durch einige scheinende Neuigkeiten etlicher verteutschter Kunstwörter nicht aufgehalten werde, so soll er wissen, dass hierinnen *ein Durchmesser* so viel heisse als sonsten Diameter, *der Halbmesser* so viel als Semidiameter; Proportio ist von uns *ein Verhältnis*, Proportionalia *gleichverhaltende Dinge*, Cylindrus eine *Rund-Seule* verteutschet; wie dann dergleichen Verdolmetschungen in Erfolg des ganzen Werkes sehr viel und hoffentlich deutlich genug werden gegeben werden.' In this case, as in many more, Wolf's terminology is not his own; but it may safely be assumed that his authority and the wide circulation of his writings is responsible for the fact, that *Verhältnis* prevailed over competing words such as, f.i., Schwenter's translation of *proportio* by "Geschick" (Piur, 42). Wolf himself uses *Verhältnis* in many of his writings, and not only in the mathematical ones. But, as far as I can see, it always keeps its mathematical sense, so that its meaning, as far as Wolf is concerned, can not be said to have undergone any generalization; so f.i. in Vernünftige Gedanken vom Gebrauch der Teile des Menschen (third edition 1737, §16): 'Da sich alles (am menschlichen Körper) durch Gesichtslängen ausmessen lässet; so muss auch alles zu der Länge des Gesichtes eine Verhältnis haben, die sich mit Zahlen aussprechen lässet'. This way of using *Verhältnis* is characteristic of the first half of the 18th century. Even where *Verhältnis* appears in non-mathematical writings it does not mean 'Zustand, in dem sich etwas befindet', or any similar generality, but it refers to a ratio, expressible in numbers. The question arises in what way and by what forces did it develop from a strictly scientific term, into an element of the general vocabulary, and how did the rather vague sense which prevails at the end of the century originate from its special meaning. For around 1780 the generalization of meaning is already completed, as may be seen by the way in which it is defined in Adelung's Versuch eines Wörterbuchs der hochdeutschen Mundart: 'Beschaffenheit eines Dinges in Beziehung auf ein anderes oder in Betrachtung eines anderen'. To this definition Adelung makes however a very significant addition: 'Da denn dieses Wort alles das ausdrückt, was die Lateiner durch respectus, relatio und proportio geben, . . . oft aber auch ein blosser Scherwenzel der Katheder-Philosophie, dunkele und verworrede Begriffe darunter zu verstecken'. 'Scherwenzel' is, according to Adelung's own definition, a name for the Jack

in the old deck of cards, which could be used in various games as a Jolly Joker. Thus Adelung's critical observation evidently characterizes *Verhältnis* as a word, whose use at his time was already as vague as it was universal, an expression, which offered its cheap services to anybody who wanted to follow Goethe's sarcastic advice: 'Denn grade wo Begriffe fehlen, da stellt ein Wort zur rechten Zeit sich ein . . . '

It will be necessary to state that in speaking of the 'forces' to which the propagation and the semantic development of the word is due, I do not want this expression to be understood in the strict sense, in which a physicist would use it. I do not, however, feel guilty of using it merely as a stylistic ornament. What I refer to, is the sum of tendencies and conditions, by which a new meaning of a word is originated and finally standardized; and I think that the designation of these factors as 'forces' is justified, because by now it ought to be clear that among the most important causes of semantic changes, as well as of other linguistic innovations, are certain affective tensions which are connected with single words and groups of words. I hope that I have shown in my Bedeutungslehre and other writings, that there is a close connection between the emotional value of a word and its ability to expand its range of meaning. If I am right in this respect, it appears to be a very significant fact that the word *Verhältnis* does not long remain an unemotional mathematical term, but that at an early stage of its development it is already drawn into a sphere of ideas of whose emotional character there cannot be the least doubt. As early as 1708 we find the following characteristic passage in Zivilarchitektur by Goldmann and Sturm (Leonhardt Sturm, son of the above mentioned Johann Christian Sturm): 'Gleich wie das höchste Gemüthe (God) alles mit gewissen Maas, Zahl und Gewichts angeordnet hat, also ist dem menschlichen Gemüthe durch Einscheinung der göttlichen Gnade nichts anmuthigeres, als das Spiel mit den Verhältnissen und ist solches etlicher massen ein Vorschmack des vollkommenen Klanges, dessen sich die seligen Seelen werden in Ewigkeit zu erfreuen haben'.

It may be necessary to state, that Goldmann-Sturm's idea of *Verhältnis* is still a mathematical one, as may be seen from the following: 'Die Symmetria oder Gegeneinandermessung ist eine solche Einteilung der Stücke eines gantzen Gebäudes, dass alles dem Maaße nach wohlgereimt erhalten werde; als wenn die Theile eines gantzen Baues mit dem Bau und die Stücke am Baue mit einander wohl verhaltende zubereitet werden. Daher müssen wir nothwendig handeln von den Verhältnissen, welche aus der Mathematica ursprünglich herrühren.'

But while no real change of meaning can be said to have taken place, it is evident that the emotional value of the word has been increased, not only by its having become a part of the terminology of the creative arts, which, at least for most people, are connected with much stronger feelings than mathematics, but also, and in a still higher degree, by the fact, that *Verhältnis* has become the bearer of a religious idea: by recognizing and appreciating the regular proportions, which in so many provinces of creation—in the structure of the human body, in music, in astronomy and so on—appear to follow unalterable numeric laws, the believing Christian will discover evidence of God's wisdom and thereby a source of confidence and happiness for himself.

I refrain from following *Verhältnis* as a term of the theory of arts through later periods; since, for our present purpose, it is of much greater importance that the religious thoughts, which we meet in Goldmann-Sturm, have remained alive throughout the 18th century, and that in formulating them *Verhältnis* is used frequently and in an altogether typical way. We find it in Gotsched's German edition of Leibniz' Theodicée (1744):

> Aus der Vollkommenheit des höchsten Urhebers folget auch noch, dass nicht nur die Ordnung des ganzen Weltgebäudes die allervollkommenste sey, sondern dass auch ein jeder lebendiger Spiegel, der nach seinem Gesichtspunkte das Weltgebäude vorstellet, das heisst, jede Monas, jeder wesentliche Mittelpunkt, seine Empfindungen und Begierden haben muss, die so gut, als es sich mit den übrigen verträgt, aufs beste geordnet sind. . . . Denn es ist in den Dingen einmal für allemal alles geordnet mit soviel Ordnung und Verhältnis als nur möglich ist, in dem die höchste Weisheit und Güte nicht anders als mit einer vollkommenen Uebereinstimmung wirken kann (777).

Similarly, we find in Spalding's Bestimmung des Menschen (40, 1768): 'Überall Ordnung, Verhältnis, Richtigkeit, Vollkommenheit zu finden und zu lieben, welch ein frohes, erhebendes Gefühl für den menschlichen Geist'. This passage is wanting in the first edition (1748), but instead we read: 'alles ist Ordnung, alles ist Proportion', and, 'so ist es denn etwas, von dem alle Theile der Natur ihre Uebereinstimmungen, ihre Verhältnisse und ihren Reiz haben'. Schulze, Muster der Beredsamkeit, 1755 (225) gives the following quotation from a sermon: 'Ueberall herrscht (in der Natur) Uebereinstimmung, Verhältnis, Ebenmaaß und Gleichheit der Theile untereinander (und) zu dem Ganzen selbst'.

Now, the idea that we live in a well organized world, the best of all possible worlds, is a favorite thought of the Aufklärungszeit; and it is hardly an exaggeration to maintain that the optimistic attitude of this period is mainly due to this idea. Under these circumstances it is easily understood that philosophers and scientists frequently indulge in more or less successful attempts to apply the idea of symmetry and proportion to many fields in which the existence of a mathematical ratio is in no way self-evident. Thus, in discussing the principles of poetic creation, Gottsched (Kritische Dichtkunst[2] 126, Leipzig, 1737) introduces *Verhältnis* into the theory of aesthetics:

> Die Schönheit eines künstlichen Werkes beruht nicht auf einem leeren Dünkel; sondern sie hat ihren festen und notwendigen Grund in der Natur der Dinge. Gott hat alles nach Zahl, Maß und Gewicht geschaffen. Die natürlichen Dinge sind schön: und wenn also die Kunst auch was Schönes hervorbringen will, so muss sie dem Muster der Natur nachahmen. Das genaue Verhältnis, die Ordnung und richtige Abmessung aller Teile, daraus ein Ding besteht, ist die Quelle aller Schönheit . . . Man versuche es doch und berede einen Baumeister, Maler oder Musikverständigen einmal, dass seine architektonischen, perspektivischen und harmonischen Regeln nichts als einen lauteren Eigensinn zum Vater hätten . . . Man wird sich durch dergleichen Einwürfe nur auslachenswürdig machen. Alle diese Künstler, wenn es anders geschickte Leute sind, werden haarklein zu zeigen wissen, was für eine natürliche Notwendigkeit in dem allen steckt und uns den Grund ihrer Regeln in der Empfindung und gesunden Vernunft entdecken. In der Beredsamkeit und Poesie geht es nicht anders. Kann hier gleich das Verhältnis nicht mit Zahlen und Linien ausgedrücket, mit Zirkel und Lineal abgemessen und so handgreiflich gemacht werden als in den anderen Dingen, wo man durch Hilfe der Geometrie alles sehr ins Licht setzen kann: so folgt doch deswegen noch nicht, dass hier alles willkürlich sei. Unsere Gedanken sind so vieler Harmonie, Ordnung, Abmessung und Verhältnis fähig als Figuren und Töne.

It will be readily understood that these statements are closely connected with the philosophical and theological thoughts for whose existence evidence has been given on the preceding pages. Poetry, being based on the imitation of nature, must necessarily follow the same laws of symmetry which dominate creation. In expressing this thought, it is only natural that Gottsched should use the same terms which we

have found in similar connections in philosophical, theological, and architectural writings. Among these terms the word *Verhältnis* holds a prominent place. But in transferring the word to the field of poetic theory it is impossible for its mathematical significance to be kept within its strict mathematical limitations. The use of *Verhältnis* by Gottsched was made possible only by his sacrificing an important part of its mathematical meaning, namely the idea of a ratio expressible by numbers. Thus here, for the first time, we may note a widening of the original semantic range and, as a corollary, a loss of distinctness in the notion conveyed by the word. Gottsched himself, as appears from the above quotation, is aware of the fact that in his usage *Verhältnis* is no longer a precise mathematical word, in other words, that a metaphorical element has been introduced into its meaning.

In later works on the theory of arts, this generalization has made further progress. Meier, f.i., in his Anfangsgründe aller schönen Wissenschaften ([2]1.94, Halle, 1754) gives the following definition:

> ein Verhältnis ist eine Bestimmung, welche in einem Dinge nicht vorgestellt werden kann, wenn man nicht zugleich etwas anders ausser demselben sich vorstellt; folglich muss man, bei einem jedweden Verhältnisse, notwendig vieles mit einem Male sich vorstellen. Wenn also ein Ding in sehr vielen Verhältnissen gegen sehr viele andere Dinge steht, die schön gedacht werden können, so schickt es sich ebendeswegen vortrefflich zum schönen Denken. Zu diesen Verhältnissen gehören alle Aehnlichkeiten, Unähnlichkeiten, Gleichheiten, Ungleichheiten, Proportionen, Disproportionen, wenn etwas als eine Ursache oder als eine Würkung betrachtet wird und hundert andere Verhältnisse, welche man aus der Metaphysik kennen lernen muss.

From this definition it is evident that to Meier the notion of a ratio, for which the word *Verhältnis* was coined originally, forms only a minute part of its range of meaning. According to him, there exists a great variety of relations for which the word can be used. Actually, it has already become that sort of universal expression, whose character of a Jolly Joker is pointed out and censured by Adelung.

Another field in which *Verhältnis* frequently is used in a non-mathematical sense, is the theory of political science. In discussing the nature and the aims of political organization authors of the 18th, as well as of earlier centuries, make ample use of metaphors like *Staatskörper* and *Staatsgebäude*, thereby conveying the idea that a commonwealth is or ought to be an organized unity comparable to the human body or to a

well-constructed edifice. Wherever ideas of this kind are expressed, the word *Verhältnis* offers itself as a most convenient term. F.i., we find it in an essay Der Staat mit einer Pyramide verglichen, by Justus Möser (Patriotische Phantasien 3.53, Berlin 1778): 'Der Staat lässt sich am besten mit einer Pyramide vergleichen, die alsdenn schön ist, wenn sie ihre gehörige Verhältnis hat, unten auf einem guten Grunde ruht und nach der Spitze zu immer dergestalt abnimmt, dass das unterste das oberste völlig, aber auch mit der mindesten Beschwerde trägt'. In the preface to Justi's voluminous work 'Die Grundfeste zu der Macht und Glückseligkeit der Staaten, oder ausführliche Vorstellungen der gesamten Polizeiwissenschaft' (Königsberg und Leipzig, 1760) we find:

> Sie (d.h. die Staatswissenschaft) ist diejenige Wissenschaft, welche zum Gegenstande hat die Wohlfahrt der einzeln Familien mit dem gemeinschaftlichen Besten beständig in einer genauen Uebereinstimmung zu erhalten . . . Ausser der Uebereinstimmung der Wohlfahrt einzeln Familie mit dem gemeinen Besten muss jede bürgerliche Gesellschaft eine gute Ordnung und Zusammenhang in allen ihren Teilen haben, nämlich die verschiedenen Klassen des Volkes, die Regierenden und Gehorchenden müssen in ihren gerechten Verhältnis zueinander stehen. Ein jeder Staat befindet sich auch allemal in einem Verhältnis gegen die übrigen freien Mächte. Sowohl dieses innerliche wie das äusserliche Verhältnis muss beständig so eingerichtet werden, dass die Glückseligkeit des Staates als dessen erster und allgemeiner Endzweck dadurch erreicht wird.

We have seen, so far, that the word *Verhältnis* has by about 1750 ceased to be a purely mathematical term and instead has become the common property of workers in widely different fields. It is found in the terminology of architecture, aesthetics, philosophy, and theology, as well as in other branches of knowledge, and it is to be noted that usually it occurs in the most important and prominent parts of these works, as it offers itself most readily whenever an author wants to make a statement on the proper organization of his subject. Furthermore it is well known that all of these sciences hold a favored position in the opinion of the 18th century. They are interesting, and therefore charged with affective elements, not only to scientists directly concerned with them, but also to the large body of educated persons. It is not difficult to show that these interests have in general been of great importance to the formation of the German vocabulary during the 18th

century. All of them have furnished to modern German a numerous stock of words and phrases, whose history is similar to that of *Verhältnis*, in that expressions which originally have been specific terms of various sciences, have gradually become parts of the common vocabulary, a process of expansion, which, however, was regularly accompanied by the loss of the original neatness of their meaning; in other words, by semantic generalization.

In this way the word *Verhältnis* became, after 1750, what Wülcker's article shows it to be, an all but ubiquitous expression, at least in the speech and writings of educated people. On the other hand, it is a remarkable fact, that toward the end of the 18th century the word was not supposed to be understood by the great masses of the people. Schuler, in his Geschichte der Veränderungen des Geschmacks im Predigen (3.viii; 1794) cites it in a list of expressions of scientific origin from whose use a preacher ought to refrain, as they might prove unintelligible to the less educated part of his audience. It may be worth mentioning that this interesting list contains, along with *Verhältnis*, a considerable number of other words, which today are generally known and understood, f.i., *Begriff*, *Ursache*, *Wirkung*, *Mittel*, *Gegenstand*, *Stoff*, *Zweck*, and *Grund*.

Having seen that the modern use of *Verhältnis* has its roots in many different fields of knowledge, we can hardly decide which of these sciences has contributed most to its expansion. Still, it may be permissible to attach special importance to its use in political science. In discussing the article of DWB, we had to state that passages in which *Verhältnis* refers to human relations form a considerable part of Wülcker's material, a fact in which we can hardly fail to recognize an influence from the terminology of statecraft; for, as far as I can see, it is only here that the word refers to relations between human beings, while elsewhere it carries the notion of connections between inanimate things or between abstract ideas. And I do not think that I am wrong, if I venture to state that the cases in which authors of the later 18th century use *Verhältnis* in speaking of human relations, are far more numerous than any other group. It is true that this could not be proved without an investigation of statistical character. But it may be safely maintained that this statement holds good at least for some of the most important authors of the period, f.i., Goethe, for whom in a certain period of his life it is a favorite word. This has been pointed out by Boucke in his excellent book Wort und Bedeutung in Goethes Sprache (144 ff.):

> Die Terminologie, die auf die Fruchtbarmachung des Wahren zielt, umfasst ausser den Kernbegriffen der Gegenwart und der Wirkung in die Ferne noch einige andere Idiotismen, die auf der Idee der Gemeinschaft beruhen, und sich zu einer Gruppe vereinigen lassen: Verhältnis—Teilnahme—Förderung—Wohlwollen. Sie gehören alle dem Altersstil an, und gewinnen an Prägnanz, je mehr das Verlangen nach Gegenwart dem stillen 'Wirken in die Ferne' weicht und je mehr das unmittelbare persönliche Eingreifen Ersatz und Verstärkung sucht durch das Zusammenwirken der Gleichgesinnten im Dienste der Idee . . . Liest man die Annalen von diesem Gesichtspunkte, so stellen sie sich dar, als eine Geschichte der 'fördernden Verhältnisse'. Auf jeder Seite fast wird von Verhältnissen berichtet, die in alter Weise weiter bestehen oder neu angeknüpft werden . . . Wie handlich und unentbehrlich Goethe das Wort Verhältnis sein musste, ist aus dem vorigen ersichtlich. Abgesehen von der Beliebtheit als reine Zustandsbezeichnung, entwickelt es noch eine besondere Prägnanz als 'gutes Verhältnis; Freundschaft'. Diese Bedeutung liegt auch zu Grunde in den stehenden Wendungen: 'ein Verhältnis gewinnen', . . . 'in ein Verhältnis treten', 'es ergibt sich ein Verhältnis' und ähnliche, die besonders in den biographischen Schriften anzutreffen sind.

It can be easily shown, or rather it is evident from many of the above quotations, that this optimistic tendency in the development of *Verhältnis*, its change from the meaning of 'relation in general' to 'harmonious or beneficial relation' is not limited to Goethe's style. As soon as the representative authors of the Aufklärung had got hold of the word, its meaning was bound to become influenced by the general optimistic attitude of a school of thought, for which the idea, that man, living in the best of worlds, is destined to happiness, was, at once, a gospel and a creed.

It may, however, be necessary to devote a few words to an opposite tendency, which in more modern times has brought about a depreciation of *Verhältnis*. It is well known, that in modern German it frequently refers to extra-matrimonial relations. The beginnings of this development too go back to the 18th century. Since towards the end of this period *Verhältnis* had come to denote human relations of any description, it goes without saying that it could also refer to friendship and love. However, until comparatively late in the 19th century it remains possible to use it without any apprehension of its evoking unpleasant con-

notations. In describing his first meeting with his future wife, Rahel Lewin, Varnhagen von Ense writes: 'indem ich aufzeichnen will, von welchen Umständen und Stimmungen unser beginnendes Verhältnis begleitet war' (Denkwürdigkeiten 2.5). It is evident that he could not possibly have used these words, if he had had the slightest suspicion that the word *Verhältnis* might suggest the idea of illicit love to any of his readers. Still, as early as Goethe's Gross-Kophta (act 2, scene 1) we find a passage where *Verhältnis* clearly alludes to sexual intercourse: 'wenn meine Frau mein Verhältnis zur schönen Nichte erfahren könnte!' As it is evident that in this passage *Verhältnis* is used by way of linguistic camouflage, it may safely be inferred that the change of its meaning to that of 'love affair' is of euphemistic origin.

INITIAL [k] IN GERMAN

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[The High German shift of West Germanic initial [k-], currently defined as a shift to an affricate, can be defined in the phoneme system of the dialects only as a shift to a combination of stop plus related spirant [kx-, kh-, gh-]. This definition enables us to apply an exact criterion to some of the dialects. The shift of [k-] covers a larger territory than appears under the current formulation: it covers all the Upper German (*Apfel*) dialects and in the west extends beyond them.]

It is generally said that the HG dialects have shifted WGic initial [p-, t-, k-] to affricates [pf-, ts-, kx-] : *pound, ten, cold* appear as [pfunt, tse:n, kxalt].[1] The shifted form of [t-] runs north roughly to the Benrath Line (*machen*: *maken*). The shifted form of [p-] covers in the west a smaller area: Spires, Mannheim, Fulda have [p-], as appears on Map 3 of Wrede, Deutscher Sprachatlas, Marburg 1926 ff. In the East Central dialects, [pf-] has largely undergone a further shift to [f-]. The shift of [k-] to [kx-] seems to cover a much smaller territory: Switzerland (except Basle) and some adjoining districts. In most of this area there has been a further shift of [kx-] to [x-]. It is only this latter representation that can be plotted on maps, such as Map 3 of the Atlas. Where [k-] has not been shifted to [kx-] or [x-], it appears as an aspirated fortis

[1] G: German; Gic: Germanic; H: high; M: middle; O: old; W: west.

Author's name and date replace the full citation of a title in the following cases: the study is listed in Mentz, Bibliographie der deutschen Mundartenforschung, Leipzig 1892 (= Sammlung kurzer Grammatiken deutscher Mundarten 2) or in Martin, Bibliographie zur deutschen Mundartforschung und -dichtung in den Jahren 1921–1926, Bonn 1929 (= Teuthonista, Beiheft 2); the study appeared in ZfdMa, Teuthonista, or ZfdMundartforschung before 1938; the study was reviewed in one of these journals or listed in Jahresbericht fgPh in the year of publication or in the next following year. As the studies are mostly short, and the treatment of [k-] easily found, I have not cluttered the text with page references.

Forms in brackets are normalized from the transcriptions of the various authors, or else are normalized OHG; starred forms in brackets are typical G, with unshifted consonants and weakening of unstressed vowels, or else typical pre-G.

stop. As the distinction between affricate and aspirated fortis is merely one of degree, we cannot draw a line between dialects which have shifted WGic [k-] and those which have not. This, the accepted view, will be found, for instance, in Behaghel, Geschichte der deutschen Sprache[5] (= Paul, Grundriss 3) 421 ff., Berlin 1928.

Braune Althochdeutsche Grammatik[3] 75, Halle 1911, stated the matter differently: the shift was [k-] > [kh-] and extended, at any rate in OHG times, over Bavarian and Alemannic, but not into Franconian. This comes nearer the truth, but still falls short of it.

For practical phonetics, from the standpoint of North German, Standard German, or general European pronunciation, the current statement is fair enough, but its value is only 'practical': it is irrelevant to the phoneme system of the HG dialects—and this system is the only relevant frame of reference for a definition of shifted and unshifted consonants.

The term 'affricate' is trivial, since in HG, as perhaps in all Gic speech, a succession of stop plus spirant is spoken always in the affricate manner, with the organs as far as possible in position for the spirant at the moment when the stop is opened. We should say, therefore, that in the HG dialects which shift WGic [p-, t-, k-], these sounds are represented by combinations of a stop and a spirant formed in the same general region of the mouth. Thus, in some dialects, the genitive [ts] *des* is homonymous with the short form of the preposition [ts] *zu*, and a syntactic combination like [t supə] *die Suppe* has the same initial as [tsi:t] *Zeit*; similarly, such a dialect may have [p fyəss] *die Füsze* with the same initial as [pfunt] *Pfund*; see, for instance, Winteler 44 ff. (1876).

A distinction between an affricate [kx-] and an aspirated stop [kh-] can be made in few of the dialects, perhaps only in some of those which have the further shift to [x-]. Some such dialects distinguish [kxy:t] *gekaut*, [k xuə] *die Kuh* from [kholt] *geholt*, [khai] *kein*; compare Winteler 136. The [x-] dialects, however, offer no difficulty: they have shifted WGic [k-] and they are plainly distinguishable from the rest. The distinction between an affricate (as a shifted [k-]) and an aspirated stop (as an unshifted [k-]) is needed under the current formulation precisely for the dialects that do not have the further shift to [x-]. Within these dialects there is no such distinction, and it has been repeatedly observed that, passing from one dialect to another, we can draw no line between dialects that have an 'affricate' and dialects that have an 'unshifted' but 'aspirated' stop; see, for instance, Bohnenberger, ZfdA 45.368

(1913). Whether a dialect has an 'affricate' or an 'aspirated stop' for WGic [k-] is a question irrelevant to the phoneme system of these dialects. It has no meaning, unless, indeed, we decide to set up a precise but arbitrary distinction, in terms of sound waves or articulatory movements, and make mechanical records for each dialect. If the laboratory is equipped to do this, one may still question whether the results would be of any interest.

When we say that WGic [p-, t-, k-] appear in HG dialects which shift them, as combinations of stop plus related spirant (apart, always, from the further shifts to [f-] and [x-]), then the special character of these combinations is determined for each dialect. Most HG dialects conform to Heusler's Law (Heusler 24 [1888]): in clusters of unvoiced sounds (that is, stops and spirants) there is no distinction of lenis and fortis, but uniformly an intermediate sound. Some HG dialects, to be sure, are not covered by Heusler's Law and are capable of such distinctions as [kne:xd] *Knecht*, plural [knexxt] *Knechte*; cf. Nagl 396 (1886). As for clusters in word initial, however, barring, as always, the [x-] dialects, and apart from phenomena of sandhi, only one type seems to occur in any one dialect. Exceptions, real or apparent, will concern us below.

In the phoneme system of our dialects, then, WGic [k-] is represented, apart from further shifting to [x-], quite like [p-] and [t-], by the combination of stop plus related spirant: [kx-, kh-, gh-], quite like [ts-] or [ds-] and [pf-] or [bf-]. The shifted form covers a large area, larger, in the west, than the shifted form of [p-].

A historical accident gives us the evidence for this statement. Over the southerly portion of the HG area, the prefixes [bə-] and [gə-] have lost their vowel before various initial consonants. Hence a form like [pfoktə] *bevogten* (Winteler 54) could be adduced to show that a form like [pfunt] *Pfund* contains phonemically stop plus spirant. Similarly, in a large block of HG dialects a form like [kxaltən, khaltən, ghaltən] *gehalten* has the same initial as a form like [kxalt, khalt, ghalt] *kalt*. Generally [kxain, khain, ghain] *kein* < *[þəh-ain] has the same initial. Hence, apart from the dialects which have [x-], we obtain proof, for a large body of dialects, that WGic [k-] before vowel has gone through the HG consonant shift and is represented by a combination of stop plus spirant.

The shift of [k-] in most dialects has taken place only before vowels. Before [l, r, n] WGic initial [k-] has remained as a stop and has, accordingly, coincided with WGic [g-] : [klain, glain] *klein* like [kla:s, gla-s]

Glas, [kru:t, gru:t] *Kraut* like [kra:s, gra:s] *Gras*, [knext, gnext] *Knecht* like [kna:d, gna:d] *Gnade* < *[gəna:þə] with old loss of prefix vowel. In the same way, unshifted WGic [t-] before [r] has coincided with WGic [d-] : *treten* like *Traum*. Only the [x-] dialects and a southern portion of the [kx-] dialects distinguish between WGic [k-] in the form [x-] or [kx-] and WGic [g-] in the form [k-] or [g-] before [l, r, n]. There is also an intermediate type of dialect in which WGic [kl-, kn-] coincide with WGic [gl-, gn-], but [kr-] (as also the reflex of [gə-r-]) appears as [kxr-], while [gr-] is kept; see Jutz, Teuthonista 6.46 (1929).

Some of the authors who describe our dialects state explicitly that the reflex of [k-] before vowels coincides with that of [gə-h-]; others show by their transcription that the two have coincided; the actual shape of the transcription [kx-, kh-, gh-, k-] does not matter. Now and then, to be sure, an author writes [k-] or [kh-] for WGic [k-] but [gh-] for [gəh-]; this does not always mean that a phonetic distinction is intended. Texts in traditional orthography occasionally yield evidence by writing *k-* in words that have or once upon a time had the structure [gə-h-]. For the loss of vowel in [gə-h-] we cannot use Map 28 of the Atlas, which plots the forms of *ge(brochen)*, since the treatment of the prefix differs before different consonants.

Our citations, then, show that the coincidence of [k-] as in [ghalt] *kalt*, with [gəh-] as in [ghaltən) *gehalten*, extends over the entire area that has lost the vowel of the prefix [gə-] before [h-]. Beyond this, to the north, our criterion fails. Better knowledge of the phonemics of the dialects would doubtless reveal other criteria, and these would show that the shift of WGic [k-] extends farther toward the north. Apparently the full form of [gə-] has been gaining ground at the expense of the shortened form; hence relic words with [gəh-] = [k-] occasionally furnish a clue.

Finally, a word about the origin of our forms. Before the HG consonant shift, the language had the WGic unvoiced stops [p, t, k], the voiced (later unvoiced lenis) stops [b, d, g], and the unvoiced spirants [f, þ, s, h]—or was [þ] already voiced? The shift of initial [p-, t-, k-] consisted in their replacement by a phoneme of the second series plus a phoneme of the third series; apart from conditions peculiar to the cluster and from later changes, this produced phonemically [bf-, ds-, gh-]. The old spellings of *ch-* for WGic [k-], appropriate to both [x-] and [kx-, kh-], cannot guide us to regional pronunciations.[2]

[2] Of the long fortis spirants which replaced postvocalic [p, t, k], only one was a new phoneme: [Z] from [t], as in [eZZan] 'eat'; the other two, [ff] from [p] and

Our listing of references is greatly facilitated by the publication of Map 56 of the Atlas, which gives Wrede's beautiful classification of the G dialects—this in spite of many drawbacks, above all, the omission of Dutch-Flemish, Swiss, Austrian, Yiddish, and the other colonial dialects. In the pamphlet which accompanies the map, we correct two misprints: on page 248, the third line from the bottom (*ββ*. Osthochalemannisch: *mähet*) should be at the foot of the page, below the lines that are numbered 1, 1 and 2, 2; on page 251, after line 8 (aa. Bolchen-Birkenfeld: *fescht*), there has dropped out a line containing something like 'bb. Trier-Koblenz: *fest*'. After these corrections are made, we shall replace the very cumbersome sigla by consecutive numbering of the smallest divisions. Thus, the *ich-Apfel* dialects run from 1 to 29; the *ich-Appel* dialects from 30 to 61; and the *ik* dialects from 62 to 99.[3]

[hh] from [k], coincided, so far as one can see, with sounds already in the language: [offan] 'open' like [heffen] 'heave' < *[haffjan], and [mahho:n] 'make' like [hlahhen] 'laugh' < *[hlahhjan]. New phonemes arose through (later) shortening of these long spirants, [sla:fan] 'sleep' contrasting with [gira:vo] 'reeve' and [bru:xan] 'brook' with [li-han] 'leihen'. In the later development, the spirant of [ts] seems to have resembled the new [Z] rather than the old [s].

[3] Needless to say, we follow the scheme mechanically. However, I have included references to Vienna among Types 1 to 6, even though it is an island of *euch* in the area of *enk*. Wrede's scheme raises a few difficulties of this kind. Thus, among the *ich, Apfel, Kind* dialects (Types 10 to 23), it provides for the combination of *Is, Hus* (Baden type) with *mähet* (Swabian type) for a southern region, Type 14; but this combination occurs also farther toward the north. For the *ik* dialects the scheme is self-contradictory. If one knows the geographic provenience of a dialect to begin with, this does no harm; for an *ik* dialect spoken, say, in Iowa, Wrede's scheme would not work. Doubtless, the discussion of the scheme promised for a forthcoming number of Deutsche Dialektgeographie will clear up such matters. Whoever has tried to classify dialects will appreciate Wrede's skill. The divisions make solid blocks and are based on common forms and on differences recognizable even in the poorest transcription.

Even so, we shall sometimes go wrong in placing a dialect. Strange as it may seem, extensive essays about a dialect sometimes fail to give us such forms as *us* or *you* or the imperative *be*. Powerful and deep-seated inhibitions block the study of language, especially of one's own language. One seeks means of evasion, and these may in time grow conventional. In the nineteenth century, the recording of G dialects was usually replaced by a meagre listing of correspondences with MHG. Today the convention leans rather to geographic discussion of borrowing; here too the information often runs very thin, with sometimes scarcely a speech form to a page. The genuine record of a dialect, such as we have in Roedder's Volkssprache und Wortschatz, New York 1936, is almost or altogether unique. Yet without the protocol, there is little value in scientific pretensions.

References which deal with more than one dialect may appear under several types.

We are concerned, in Wrede's classification, only with the HG or *ich* dialects, Types 1 to 61.

Types 1 to 29 *Apfel* (Upper German) all shift [k-] before vowels, and this appears plainly by our criterion, since here the prefix [gə-] loses its vowel before [h-].

Types 1 to 6 *enk* (Bavarian). Schmeller 1821 did not register our phenomenon; he came close to it in the interesting footnote on page 105. It appears in the studies of Beranek, Die Mundarten von Südmähren, Reichenberg 1936 (= Beiträge zur Kunde sudetendeutscher Mundarten 7): Eichhorn 1928; Gartner 1900; Haasbauer 1924; Hajnal, Az isztiméri német nyelvjárás hangtana, Budapest 1906 (= Magyarországi német nyelvjárások 4); Micko 1930; Nagl 1886; Pfalz 1913; Weigl 1924.

The southernmost of these dialects have the shift also before [l, r, n]: Egger 1909; Gröger 1924; Rudolf 1934; Schatz 1897. Intermediate dialects have the shift before [r], but not before [l, n]: Himmelstosz 1893; Lessiak 1903; Schieszl 1909, 1914; Schöpf 1866 (explicit statement, 297).

In literary sources, the equivalence of [k-] and [gəh-] appears in the conventional spelling of the word *Kälter, Kalter* 'container', representing *Gehälter, Gehalter*: Castelli 1847; Jakob 1929; Mareta 1861; Schmeller-Fromann 1872 (oldest occurrence, 1558). Another such spelling is *keien* 'throw, vex,' representing *geheien*: Hintner 1878; Jakob 1929; Lexer 1862; Mayr 1930; Zaupser 1789. Zaupser has also *kolpet* 'uneven', presumably *geholpicht*. Similarly, dialect texts in conventional spelling; thus, Firmenich, Germaniens Völkerstimmen, vol. 2, Berlin 1846, has 682 *kei'n*, 698 *keit* (Bavaria); 707 *kert* 'gehört', 783 *Koamnusz* 'Geheimnis', 784 *kot* 'gehabt,' 786 *katzt* 'geheizt' (Carinthia); 790 *käu'n* (Lower Austria): vol. 3 (1854) 607 *keiat* 'gehört' (Eger Region).

Types 7 to 29 *euch* (Alemannic-Franconian). For a large body of these dialects, Jutz 1931, 220, states the equivalence of [k-] and [gəh-].

Types 7 to 9 *fescht, Chind* (Upper Alemannic) have [x-] and need not concern us here. An interesting singularity appears in the dialect spoken on the Tann Mountain (Type 9, Rudolf 1934). This dialect has [x-] not only in [xind] *Kind*, but also for [gə̣h-], as in [xolfa] *geholfen* or [xeŋ] *Gehänge*. Presumably the [x-] pronunciation was here borrowed by speakers of [kx-] in whose dialect [k-] and [gə-h] had already coincided.

Types 10, 11 *fescht, Kind, Is, Hüs* (Alsatian types). Here Martin-Lienhart 1899 print [khalt] for both *kalt* and *Gehalt*. Menges, Volksmundart und Volksschule im Elsasz 20, Gebweiler 1893, says: 'Nur

wenn b and g vor h stehen, werden sie zu p und k. Behalten und gehören heiszen mundartlich palte und kere'. Special studies: Enterle, Die Mundarten der Landschaft Freiburg im Breisgau, Bühl 1936 (= Bausteine zur Volkskunde und Religionswissenschaft 14): Heimburger 1888; Henry 1900; Kilian 1935; Lienhart 1891; Mankel 1886; Schwend 1900; Sütterlin 1892.

Types 12 to 14 *fescht, Kind, Is, Hus* (Black Forest type). Ehret 1911 in the phonetic part of his study says (34) that his [k-] represents MHG [k-] before vowels as well as the prefix [gə-] before (he means plus) [h-]; in the historical part (39, 49) he says, however, that [gə-] before (read plus) [h-] appears as a strongly aspirated [kh-], more strongly aspirated than the reflex of [k-]. Heilig 1900, for Bahlingen (97) has [kummə] *kommen* and both [ka:] and [gha:] *gehabt*; for Kenzingen (168, 361) he gives [kumə] and [ka:] as well as [kaldə] *gehalten*, and so for all past particles of verbs in [h-] except [kho:lt] *geholt*. On page 93 he has [kepf] *Köpfe* and [ghalda] *gehalten*. Also Lau 1903 has [kennə] *kennen* but [khaltə] *gehalten*; he has, however, [kin] *kein*. Against this, the usual state of affairs is attested by Dreher, Laut- und Flexionslehre der Mundart von Liggersdorf und Umgebung, dissertation Tübingen 1919; Heilig 1897, 1914; Heusler 1888; Jäger 1903; Ketterer 1930; Kilian 1935; Schlager 1931; Staedele 1927; Wasmer 1915 (he writes [k-] and [gh-] but on page 336 says they are alike); Weik, Lautlehre der Mundart von Rheinbischofsheim, dissertation of Freiburg i. B., Halle 1913. Likewise, for the intermediate southern type, Marte 1910; for the full southern type, Jutz 1925.

Types 15 to 19 *fescht, Kind, Eis mähet* (Swabian). For this group as a whole, Bohnenberger 1928 and Kauffmann 1890 attest [k-] = [gəh-]. Fischer, in his Geographie 66 (1895), has [kx-, kh-] for [k-] and, page 63, [gh-] for [gəh-]; in his Wörterbuch 1904 ff., however, he expressly discusses (3.111) the coincidence of [k-] and [gəh-] and refers to spellings of *k-* for [gəh-], such as *kaim* for *geheim*, and to inverse spellings, such as *Gehebs* for *Kebse* (date, 1350). The special studies confirm this: Bopp, Der Vokalismus des Schwäbischen in der Mundart von Münsingen, dissertation Straszburg 1890; Haag, Die Mundarten des oberen Neckar- und Donaulandes, school program, Reutlingen 1898; Heissl 1935; Keinath 1930; Knupfer 1912; Meisinger, Die Rappenauer Mundart, I. Lautlehre, dissertation Heidelberg 1901; Theodor Müller 1911; Rudolf 1934; Schlager 1931; Veit 1901 (p. 9: [ghed] *gehabt*, [kald] *kalt*, 'trotz des völlig identischen Anlauts der beiden Wörter'; censured by Meisinger ZfdMa 2.283 [1901]); Vogt 1931; Wagner 1889, 1891; Weis-

haupt 1935; Zinser 1933. In traditional orthography, *k-* for [gəh-] appears in Firmenich, vol. 2, 427 *keiht*, 435 *keia*, 442 *keiha*, 469 *kaihr* 'gehöre', 487 *koit*.

For colonial dialects of this type (Type 19) in Transcaucasia, Schirmunski 1928 transcribes both [k-] and [gəh-] as [kh-] but says that the latter has stronger aspiration.

Types 20 to 23 *fescht*, *Kind*, *Eis*, *mähe* (Alemannic-Franconian border type). Braun 1906 has [komə] *kommen*, [kuə] *Kuh*, and [khe:rd] *gehört* for some of his localities, but [ke:rd] *gehört* for others. All the other evidence is for [k-] = [gəh-]: Blumenstock 1911; Boger 1935; Breunig, Die Laute der Mundart von Buchen, school program, Tauberbischofsheim 1891; Dietzel 1908; Knupfer 1912; Lauinger, Lautlehre der Mundart des Dorfes Spessart, Heidelberg dissertation, Leipzig 1929; Mangold 1930; Meisinger 1901; Nohe 1901; Reichert 1914; Roedder, Volkssprache und Wortschatz des badischen Frankenlandes, New York 1936; Sander 1916; F. G. G. Schmidt 1898; Sexauer 1927; Waibel 1932; Wanner 1907, 1908.

For the colonial dialect near Leningrad (Type 22), Ströhm 1926.

Types 24 to 29 *fest* (Upper Franconia, Thuringia). Our criterion applies to only part of the area, since the more northerly districts keep the vowel in [gə-].

Type 24 (Nuremberg): Gebhardt 1907 has, for instance, [gha:ut] *gehauen* and [khu:ərn] *Korn*, but in his phonetic survey says not a word about this. Hans Sachs wrote *Gehalter* with *k-* (Schmeller-Frommann, s. v.) and Hans Folz' printers render the word with *k-* in verse 103 of the poem reprinted by Keller, Fastnachtspiele, vol. 3 (= Bibliothek des Stuttgarter Literarischen Vereins 30 [1853]), p. 1215; the facsimile of a different printing in Hampe, Drucke und Holzschnitte, vol. 2, Straszburg 1899, has the same spelling; and so has a third printing, preserved at Wolfenbüttel (photostat in University of Chicago Library).

Type 25 (Ansbach, Bamberg, Hof): Batz 1912 (Bamberg); Schübel, Versuch einer Charakteristik und Phonetik der Bamberger Mundart von Stadtsteinach, Munich dissertation, Halle 1911.

Type 26 (Vogtland). Our criterion holds only for the southern part: Gerbet 1908; this is confirmed for Schöneck by Hedrich 1891.

Type 27 (Rothenburg, Würzburg, Meiningen, Salzungen) yields evidence for the south: Foerster 1913 (Neustadt); Heilig 1898 (Taubergrund); Lenz 1887 (Handschuhsheim).

Types 28 and 29 give no evidence, since here [gə-] keeps its vowel.

Types 30 to 61, *Appel* dialects (Central German).

Types 30 to 47 *Pund* (Western). The shift of [k-] extends to dialects that have not shifted [p-].

Type 31 *was, fescht, Eis, gebroch* (Western Palatinate) keeps the vowel of [gə-]; however, Christmann 1927 has [kärmsə] 'sich härmen', which is evidently a relic with [gəh-] = [k-]. The colonial dialect in Hungary described by Lindenschmidt, A verbászi német nyelvjárás alaktana, Budapest 1905 (= Magyarországi német nyelvjárások 2) belongs to this type but has preserved the shortened form of the prefix and has [k-] = [gəh-].

Type 32 *gebroche* (Eastern Palatinate) consistently has [k-] = [gəh-] wherever the prefix has lost its vowel: Bertram, Die Mundart der mittleren Vorderpfalz, Erlangen 1937 (= Fränkische Forschungen 7) for the district just west of Spires; Bräutigam 1934 (Mannheim); Freiling 1929 (Odenwald, about Zell); Heeger 1896 (Lingenfeld, Landau, Rheinzabern); Treiber 1931 (Plankstatt); Wenz 1911 (Beerfelden in the southern Odenwald).

The same holds true of colonial dialects in this type: in Hungary, Kräuter, A niczkyfalvai német nyelvjárás hangtana, Budapest 1907 (= Magyarországi német nyelvjárások 5); Heinrich F. Schmidt 1911; in the Banat, Weifert 1935; in the Neva Region in Russia, Ströhm 1926.

Types 33 to 38 *fest* (Hessian) generally retain the vowel of [gə-]; only for the district Fürth, Lindenfels, Reisen (the upper Weschnitz valley) Weber 1908 attests [k-] = [gəh-] for a dialect of Type 35. In Hungary, Schäfer, A kalaznoi német nyelvjárás hangtana, Budapest 1908 (= Magyarországi német nyelvjárások 6), belonging in Type 35 or 36, has [k-] = [gəh-].

Types 48 to 61 *Fund* (Eastern). Our evidence extends only to Type 50 (Southern Upper Saxon), since only here does [gə-] lose its vowel: Gerbet 1900 (Klingenthal); Goepfert 1878 (Freiberg and environs); Hausenblas 1898 (Brüx); 1914 (Brüx, Postelberg). Farther north in Type 50, [gə-] has kept its vowel. For Zwickau, Gerbet 1900 gives the relic form [khak] *Gehäcke*, with initial like that of [khumə] *kommen*. Philipp 1897 says that the lower-class speech of Zwickau drops the vowel of [gə-], but he gives no example of this before [h-].

For colonial dialects, we have [k-] = [gə-] in Weifert 1934 (Weisskirchen, Bela Crkva, in the Banat); this dialect uses [eŋk] for 'you' dative and accusative plural, but belongs in Wrede's Type 50. Similarly, Gréb 1922 (Deutsch-Pilsen). The Zips dialect described by Gröger 1911 belongs in Type 50; it preserves the vowel of [gə-] but has (58) [ka:jə'ra:j] 'vexation', from [gəh-], with the same initial as [kimt] *kommt*.

THE PITFALLS OF POLARITY

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The term 'polarity' was adopted for linguistic purposes by Carl Meinhof, who introduced it in his comparative account of the Hamitic languages,[1] published in 1912. Slow at first in gaining currency, this designation has steadily been increasing in popularity; today it stands for a widely recognized principle said to be operative alike in the Hamitic and Semitic languages.

This is what Meinhof himself has to say on the subject:

> 'If under certain conditions A becomes B, B will become A under the same conditions. I call this process polarity for the following reason. The magnet has a positive pole (A) and a negative pole (B). If the positive pole becomes negative under the influence of a stronger magnet, i.e., if A becomes B, the negative pole will turn positive, B becoming A'.[2]

The author goes on to suggest an explanation for the process. The speakers of the languages involved divide things into two principal categories, such as male and female. A change in classification entails transfer from one category to the other. In Meinhof's own words: 'To put it pointedly,[3] what is not a man is necessarily a woman, and what is not a woman must be a man; tertium non datur'.

This formulation is based on phenomena observed in three out of the seven groups of Hamitic languages with which the book deals. But even in those three Meinhof notes important exceptions to the

[1] Die Sprachen der Hamiten (Abh. des Hamburgischen Kolonialinstituts) 9. Strictly speaking, Meinhof does not seem to have been the first to use polarity in a linguistic sense. It is clear, however, from his elaborate statement that he was unaware of any other secondary application of this term. Moreover, Meinhof suspects the operation of polarity in phonetic and morphological developments, whereas others would use the term in connection with (Greek) syntax and stylistics. Since the advocates of polarity in the Hamito-Semitic field, with which the present discussion is concerned, are all avowed followers of Meinhof, the term will be used here in the sense in which it was independently adopted by him.

[2] Ibid. 19.

[3] drastisch ausgedrückt'.

general rule.[4] The author seeks to strengthen his case by adducing illustrations from the social practices of certain African groups,[5] but such instances can have no direct bearing on linguistic problems.

What Meinhof started out to do was to describe certain contrasted situations observed among several Hamitic languages. The term 'polarity' suggested itself as a convenient designation for such cases. The name implies, however, also a degree of interpretation. Meinhof was thus compelled to find a reason for his linguistic facts, and to do that he had to enlist the aid of psychology. It should be emphasized that the author was fully aware of the subjective nature of his argument. The same cannot be said of many of Meinhof's followers, some of whom have gone so far as to regard polarity as a linguistic law. We can have no serious quarrel with the term if it is understood to be purely descriptive, a convenient label and nothing more. But when one reads and hears with increasing frequency that polarity explains this or that set of facts in Hamitic and Semitic, and realizes at the same time that the facts in question happen to include some of the problems which so far have been eluding all attempts at interpretation, one is prompted to inquire more closely into this linguistic panacea.

Let us disregard for the moment Meinhof's interpretation of polarity, since it is admittedly subjective, and concentrate instead on his definition of the process. This definition requires two sets of linguistic features to have exchanged places, the interchange being interdependent and simultaneous. In other words, A must have started out as B, and B must once have been A; the respective shifts of A to B and of B to A were mutually conditioned; they were also related chronologically. Unless all of the above conditions obtained, the process described as polarity cannot be said to have taken place.

Since a chronological factor is implicit in the definition of polarity, the term can properly be applied only to languages which have been studied historically. Where our knowledge of a given language, or group of languages, is largely empirical, the time element cannot be checked. For this reason it is not advisable to operate with the concept of polarity in the Hamitic languages. The comparative study of Hamitic would have to be much further advanced than it is today before we could speak with confidence of detailed historical processes

[4] For numerous unexplained exceptions in Ful see ibid. 41; in Šilh feminines take 'not infrequently' masculine plurals (ibid. 95); in Somali only collectives to masculine singulars are feminine (ibid. 171).

[5] Ibid. 19–20.

within this group. At all events, it is significant that Meinhof has been able to utilize his theory only for a part of the Hamitic family, although he circumvents more than once the requirements of his own definition. The historical criterion is at no time mentioned, let alone applied.

It is no less significant that in Egyptian, a Hamito-Semitic language for which we have ample historical material,[6] polarity has yet to be demonstrated. A recent attempt to identify the process in Egyptian[7] is likely to embarrass rather than please the advocates of the theory. Confronted with more than two sets of features which he would explain by polarity, the author is forced to set up a 'polar cross',[8] which is a curious contrivance at best. He adds that in its operation polarity seems to resemble the process of analogy. But if analogic change furnishes an explanation which students of language will understand, why does one need to have recourse to a principle which is linguistically obscure? For as an explanation of linguistic processes polarity narrows down to a principle of contrariness; something like 'you have taken my sheep, so I am going to take your sheep'.

It is, however, primarily[9] in the Semitic languages that polarity has been assigned of late a place of honor, not only as a term descriptive of certain contrasted conditions but as the principle behind them. To concentrate only on instances which have become stock examples in works by competent scholars,[10] we find that the operation of polarity is seen in: 1. The 'broken plurals' of Arabic; 2. The plural endings in the Hebrew words for 'fathers' and 'women'; 3. The system of tenses in West Semitic; 4. The syntax of the Semitic numerals from 'three' to 'ten'. If polarity is really responsible for the present conditions in the above four classes, the operation of that principle should be com-

[6] For a convenient summary of the affinities of Egyptian see A. H. Gardiner, Egyptian Grammar 2–3 (1927).

[7] H. Brunner, Das Gesetz der Polarität in der ägyptischen Sprache, Zeitschrift für ägypt. Spr. 72. 139–41 (1936).

[8] Ibid. 140.

[9] Prof. R. G. Kent has called my attention to the fact that the term (in Meinhof's sense) has been making headway also in Indo-European linguistics.

[10] The principal advocates of the theory have been, in addition to Meinhof, W. H. Worrell and G. Bergsträsser. The three sets of examples adduced by Bergsträsser will be discussed below under sections 2–4. Worrell's principal argument is detailed under section 1; his remaining illustrations are covered, for the sake of completeness, in footnote 30. I first expressed my misgivings about Worrell's position in JAOS 49. 181 (1929).

paratively easy to follow, because Semitic furnishes ample material for historical investigations. On the other hand, uniformly negative results will not be wholly satisfactory until adequate explanations are substituted in each case for those which are now current.

1. The most vigorous advocate of polarity as a factor in the development and gender of the so-called broken plurals of Arabic has been W. H. Worrell.[11] As is well known, these forms are original singular abstracts which gradually came to be used in a concrete and collective sense, and hence pass for plurals.[12] Grammatically, the gender of these forms is feminine. According to Worrell 'the broken plurals are feminine, not only because they are abstracts and collectives, but because they must be polaric opposites to their masculine singulars'.[13] In other words, *kilāb* 'dogs' (plur., fem.) is said to be the polaric opposite to *kalb* 'dog' (sing., masc.) in number as well as in gender. The contrast in number ceases to impress, however, when it is realized that *kilāb* is morphologically a singular[14] just as plainly as *kalb*. Nor is the opposition of the genders other than accidental. The normal ending of abstracts and collectives in Semitic is *(a)t*, which came to be the general designation of the feminine gender in all branches of this linguistic family.[15] The analogy of the abstracts and collectives in *-(a)t* is entirely sufficient to account for the gender of those collectives which have no formative suffix: Arabic *kilāb* 'dogs' is feminine by signification because it belongs to a class which is commonly in Semitic, and not infrequently in Arabic itself (e.g., *ǧamā'at-* 'totality'), feminine by form.

The 'broken plurals' are thus in no way abnormal with regard to number or gender. It remains only to examine the differences in form which distinguish these collectives from their respective nomina unitatis. The situation will be clarified if we first cite an illustration involving Semitic languages other than Arabic. Hebrew *śē'ār* 'hair' is a collective to which the nomen unitatis is *śa'ărā* 'single hair'; the

[11] The Formation of Arabic Broken Plurals, Am. Jour. Se. Lang. 41. 179–82 (1925); A Study of Races in the Ancient Near East 61–4 (1927).

[12] W. Wright, Comparative Grammar of the Semitic Languages 148 (1890); Grammar of the Arabic Language[3] 1. 199–234 (1896). A thoroughly satisfactory treatment of the nature of these forms was given by H. Barth, Nominalbildung 417–83 (1889).

[13] Races 64.

[14] It is based on infinitives of the type *qitāl*, cf. Wright, Arabic Grammar 1. 111 no. 26; 202–5.

[15] See Speiser, Studies in Semitic Formatives, JAOS 56. 33–46 (1936).

latter yields the constr. plural *śa'ărôṯ* 'hairs'. *śa'ărā* is 'feminine' on account of its ending (<*-at*), which had become specialized as the designation of the feminine gender, *śē'ār* is 'masculine' because it lacks the one common Semitic suffix of the grammatical gender; it is an 'internal' collective, in that no formative is required to convey the meaning. But in Akkadian the corresponding form is *šārt(u)*, with the abstract-collective ending *-t*; by reason of that ending it is also 'feminine'. In other words, Hebrew could and did make use of the ending *(a)t* to designate a single specimen of the class 'hair', because that ending had not been employed by the collective in question; in Akkadian the same formative was needed to express the collective. Consequently, Semitic **śa'arat* comes to represent opposed grammatical subdivisions in Hebrew and Akkadian respectively.

Now in Arabic (and South Semitic in general) the internal collectives are specialized to a degree not approached elsewhere in Semitic. Whereas in Hebrew these forms are rudimentary[16] and supported for the most part by pleonastic plural endings,[17] the process of forming 'broken plurals' is a living one in Arabic, so much so that modern loanwords are readily assigned to one of the numerous stem-forms of this type.[18] These stem-forms comprise no less than twenty-nine comparatively common classes.[19] It is easy to see, therefore, how plurals (with the idea of individuality wholly suppressed, however,[20]) might be expressed in Arabic by forms that had originally been reserved for abstracts, although the normal Semitic plural formed with the suffix *-ū-* is by no means rare.

[16] Although by no means as rare as is generally supposed; for a recent list of collectives in Hebrew see L. H. Gray, Introduction to Semitic Comparative Linguistics 53 (1934). These forms are represented frequently by 'segholates' of the type *qitl* and *qutl*. A good, but unnoticed, example is *kofĕr* 'villages' (type *qutl*), I Samuel 6.18. An older instance is *sefĕr* 'writing, book' (type *qitl*), which Hebrew borrowed from Akkadian.

[17] Here belong most of the singulars in **-at* which form their plurals with *-îm*; for examples of such forms see H. Bauer and P. Leander, Historische Grammatik der hebräischen Sprache 1.515-6. The segholate plural *qəṭāl-'îm* is most likely of the same origin.

[18] Thus a modern Arabic term for 'Egyptologists' is *barāgiš*, which is based on the name of the German Egyptologist *Brugsch*; see Meinhof, Die Entstehung flektierender Sprachen 55 (1936). I have heard many formations of the type *panākik* < Engl. *pancake*.

[19] This is the number of the more common forms cited by Wright, Arabic Grammar 1. 199.

[20] Ibid. 233.

The number of stem-forms encountered with the 'broken plurals' is exceeded by the number of forms used to express verbal nouns. There are forty-four[21] recognized types of verbal noun which may be formed from the ordinary triradical verb. Since such abstract nouns on the one hand, and collectives on the other, are often related semantically, frequent correspondences in form are perfectly natural. These correspondences merely bear out the theoretical premise that infinitives may be expected to represent abstracts. Thus *qitāl*, a very common type of 'broken plural',[22] occurs also as an infinitive.[23] In Hebrew, an original *ā́* (> *ô*) in the second syllable is evidenced in such pleonastic collectives as *dəḇôr-îm* 'bees' and *śə'ôr-îm* 'barley'.[24]

We now come to the form of the singulars which occur with the 'broken plurals'. From among the wealth of classes of these secondary plurals Worrell has selected eight which he juxtaposes with their respective singulars in order to illustrate the underlying principle of contrast.[25] Since Worrell's groups 5–8 depend on the use of formatives, and thus weaken the argument by introducing an outside element into the discussion, I shall consider for the present only the first four groups. Taking the first example in each group, we get the following picture:

	singular	'broken plural'
a.	*kalb* 'dog'	*kilāb*
b.	*kitāb* 'writing'	*kutub*
c.	*šarīf* 'noble'	*širāf*
d.	*ṣāḥib* 'friend'	*ṣiḥāb*

The difference in number within each group is said to rest 'upon the principle of contrast maintained between two members of a two-class system',[26] the contrast being attained by varying the quantity, character, or position of the vowels in question. But even in this schematic presentation there is apparent a complete disregard of uniformity which violates the 'tertium-non-datur' clause of Meinhof's definition. For if we start out with the 'plural' forms, we find the base *qitāl* in three out of the four groups cited; but the corresponding singulars represent three different bases (*qatl*, *qatīl*, *qātil*). Moreover, in group (b) the singular also shows the form *qitāl*; the plural, however, exhibits

[21] Ibid. 110–2, the list being only 'nearly complete'.
[22] Ibid. 202–5.
[23] Ibid. 111 no. 26.
[24] See note 17.
[25] Races 62–3.
[26] Ibid. 63.

yet another base (*qutul*). In other words, in each of the above four classes *qitāl* furnishes one of the two forms listed, while the corresponding opposites are all different. There is not a trace left of the purely mechanical principle of contrast.

The plain fact is that tabulations of this kind are misleading because they are superficial and incomplete. One of the examples given by Worrell under group (a) is *'abd* 'servant', with *'abīd* as its plural. A non-Semitist is likely to conclude from this statement that no other plural form is used in this particular instance. It so happens, however, that *'abīd* is one out of no less than fifteen 'broken plurals' found with *'abd*.[27] Finally, one singular may have several irregular plurals and a normal plural (in *-ūna*) besides, each form being often specialized for one particular shade of meaning.[28]

It follows that no classification that is based solely on external criteria of form will account for the complex picture which the 'broken plurals' present. We have seen that these forms started out as singulars used in a collective sense. A language will have as many forms of such virtual plurals as it has stems capable of carrying a collective connotation. Arabic happens to have forty-four varieties of verbal nouns which may occur with the primary form of the verb; fully in keeping with this luxurious development is the existence of twenty-nine stems (many of which are identical with the verbal nouns) employed for collectives.

In a final analysis of the situation accidents of semantic change must be taken into account. If we follow Worrell's classification we shall regard the word *kitāb* (group b) as a normal singular. But the word is in reality an infinitive of the verb 'to write' and hence identical morphologically with the *qitāl* 'plurals' found in the other three groups. But whereas abstract forms of the nouns for 'dog', 'noble', and 'friend' came to denote the respective collectives and then plurals proper, *kitāb* was to become specialized in the sense of 'book'; another plural had to be added. From a historical point of view, some nouns started out as collectives and developed their nomina unitatis later on; others substituted collective stems for plurals formed with the aid of a special ending; still others were original abstracts which had acquired a concrete meaning, with the result that a new 'plural' had to be formed from some other abstract stem. In each case the choice of a given secondary stem may have been influenced by a number of factors, such as primary

[27] Wright, Arabic Grammar 1. 225.
[28] Ibid. 225–6.

form, meaning, and analogy. With a wealth of forms to choose from, the contrast in meaning could easily be preserved.[29] The contrast in form was at best a secondary consideration.[30]

2. Another instance of alleged polarity in the use of genders has been seen in the formal contrast between Hebrew *'āḇôṯ* 'fathers' (with fem. pl. ending) and *nāšîm* 'women' (with masc. pl. ending).[31] But 'fathers' and 'women' are not proper opposites, and it can be shown that the two words in question had nothing to do with one another's plural ending. The respective suffixes were acquired independently. *nāšîm* contains an old collective of the word for 'human being' (cf. Arabic *nās*), which had been specialized for 'women', and the plural ending *-îm* which was added pleonastically to strengthen the collective connotation. Similar combinations are found in the Hebrew words for 'bricks', 'bees', 'fig-cakes', 'barley', and a number of others, not one of of which bears the slightest semantic relation to 'fathers'.[32] The norm in all these cases is: collective form + masculine plural ending. Why the corresponding feminine ending was not used for this purpose is open to speculation. It is a fact, at any rate, that the latter suffix (*-āt*) is a secondary development from the singular ending (*-at*).[33]

[29] Barth's statement on the subject, published nearly fifty years ago (Nominalbildung 417–31), is still substantially correct.

[30] Worrell cites two other positive instances of polarity. One is the use of 'the inferiority sign *t*' to distinguish 'the relatively unimportant second person and feminine from the first person and masculine respectively' (AJSL 41.182; Races 64). The fact is that the *t* which is used as a pronominal element in the second person of the verb represents the personal pronoun *'an-tā/tī* 'thou', while the *t* in the feminine forms *qatal-at*, *ta-qtul*, and the like, is based on the feminine ending *-at*. The respective formatives for person and gender are absolutely heterogeneous. The other instance concerns the use of *banāt* 'daughters' in plurals of compounds with *ibn* 'son' when these compounds are used for names of animals and other objects (Races 63–4), but not of persons (Wright, Arabic Grammar 1.196). Thus *ibnu dā'irin* 'son of the lewd', 'young stallion camel' appears in the plural as *banātu dā'irin*, without any regard for the actual sex of the object. But in this case *banāt-* is not properly a feminine plural; it is an original singular collective form of **bin* 'son', viz., **binat-* whose present form is due to the analogy of *banāt-* 'daughters'. Such a use of the collective 'sons' would have its exact parallel in Arabic *'iḫwat* (sing.) 'brothers' and Hebrew *'āḇôṯ* 'fathers', for which see below.

[31] G. Bergsträsser, Einführung in die semitischen Sprachen 15 (1928); Hebbräische Grammatik, Verbum 5 (1929); Meinhof, Enstehung flekt. Sprachen 75; Bauer and Leander, Hebr. Gramm. 1. 515, explain the ending of *'ābôṯ* as due to the analogy of **'immôṯ* 'mothers'.

[32] See note 17, and Bauer and Leander, Hebr. Gramm. 1.515–6.

[33] Speiser, JAOS 56. 41 and the references there given.

The view that *'ābôṯ* contains the feminine plural suffix is due, however, to wrong analysis. **'abŭ*[34] belongs to the small class of proto-Semitic kinship-terms whose stems ended in a vowel; another such term is **'aḫŭ* 'brother'. Now **'aḫŭ* + the abstract ending *-at* (which was to become the common feminine suffix) develops in Hebrew the form *'āḥôṯ* (<**'ăḫātu*) and the value 'sister': The long *ô*-vowel is the result of contraction (of the final stem-vowel and the *a*-vowel of the ending). The absolute form 'sisters' does not occur in Biblical Hebrew; but construct forms of this word presuppose **'ăḥāyôṯ*,[35] from an original **'aḫŭ+at*. Hence the plural for 'fathers' should be **'ăḇāyôṯ*. Our *'ābôṯ* is just as plainly singular in form as *'āḥôṯ* 'sister', not 'sisters'. The failure to regard these two words as morphologically identical must, of course, be ascribed to the ultimate differentiation in number: whereas *'āḥôṯ* means 'sister' (sing.), *'āḇôṯ* stands for 'fathers' (pl.).

I have elsewhere attempted to show how the word for 'father', equipped with an abstract formative, came to have in Hebrew a collective sense.[36] The underlying meaning was probably 'parentage'.[37] A semantic extension of the same basic word in another direction may be seen in Akkadian *abūtu* 'fatherhood, parentage', which develops in the Nuzi dialect the value 'co-parentage', and thence '(adoptive) mother'.[38]

[34] The exact character of the final vowel is unknown since we find it only in contractions with case-endings. These contractions in the masculine forms result in Hebrew in *î* (from the genitive), in Aramaic in *â* (from the accusative). Arabic lost the final vowel of these stems (*'ab*, *'aḫ*). However, various derivative forms with *-w-* favor an original stem-ending *-ŭ*.

[35] Josh. 2.13; Ezek. 16.52; Job 42.11; I Chron. 2.16.

[36] JAOS 56.40.

[37] Arabic does precisely the same thing with *'aḫ* 'brother', whose abstract form is specialized for the plural: *'iḫwat* 'brothers'; cf. C. Brockelmann, Grundriss der vergleichenden Grammatik der semitischen Sprachen 1.427 (1908). In this case, however, the plural ending could not be suspected, as it has been in the parallel Hebrew form *'āḇôṯ*, because the stem-ending of the Arabic word assumed consonantal function, thus preventing contraction.

[38] See Speiser, Annual of the American Schools of Oriental Research 10.11.

The semantic changes exhibited by the respective abstract forms of the two kinship-terms under discussion are illustrated in the following schematic grouping:

Basic Form	Abstract Form
**'abŭ* 'father'	Hebr.: 'fatherhood'; 'fathers' Akk.: 'parentage'; 'mother(hood)'
**'aḫŭ* 'brother'	Hebr.: 'Geschwister'; 'sister' Arab.: 'brotherhood'; 'brothers'.

See also JAOS 56.40.

It takes thus a singularly unilateral view to make polaric opposites out of Hebrew *'ābôṯ* : *nāšîm*. By the same token English forms of the type *foot* : *feet* should also reflect the principle of polarity. Historical data, however, show the English forms to be due to conditioned vowel alternation and subsequent analogic leveling.[39] By considering historical processes the correct perspective is restored. The introduction of polarity as a factor in Hamito-Semitic linguistics is traceable all too frequently to a lack of historical perspective.

3. The verbal system of West Semitic presents numerous problems to the student of comparative Semitic linguistics.[40] After eliminating secondary developments of a phonetic, morphological, and semantic nature we are left with the basic question of the rise of an active afformative conjugation (theme *qatal-tā*). A corollary of this development, in which Akkadian proper did not share, is the difference in the vocalism of the active themes on the one hand and the neuter themes on the other, a difference which gives the impression of premeditated contrast:

	Afformative Conjugation ('Perfect')	Preformative Conjugation ('Imperfect')
Active:	*qatal-tā*	*ta-qti/ul*
Neuter:	*qati/ul-tā*	*ti-qtal*

It is evident that within a given stem there is disagreement between the thematic vowels of the two tenses: *qatal-tā* : *ta-qti/ul* (*a* : *i/u*); but there is complete correspondence between the respective thematic vowels of the active perfect and neuter imperfect: *qatal-tā* : *ti-qtal* (*a* : *a*), and vice versa: *qati/ul-tā* : *ta-qti/ul* (*i/u* : *i/u*). No satisfactory explanation of this contrast in vocalism has yet been offered. Compelled thus to rely in the main on purely mechanical factors, Bergsträsser joined the school of Meinhof and injected wholesale doses of polarity into his reconstruction of the West Semitic verb.[41] Since the original definition of the process was not elastic enough for his needs, Bergsträsser had to lift some of its restrictions; his plea of 'guilty' scarcely minimizes the offense.[42] But even if we grant all of Bergsträsser's premises, the solution will not be a permanent one. It will be at best a stop-gap, because polarity, as used by him, is in the last analysis descriptive and not explanatory.

A more recent attempt to solve the problem under discussion is that

[39] Cf. Sapir, Language 197–203 (1921).

[40] For the latest literature see Bergsträsser, Verbum 9–10, and Driver, Problems of the Hebr. Verb. Syst. 155–7.

[41] Verbum 10–4, 89, 92.

[42] Ibid. 6.

of G. R. Driver. He reproduces much of Bergsträsser's argument,[43] but concludes that polarity may not have been the sole arbiter in the matter of vocalism.[44] Other causes are found in phonetic influences and in the suitability of a given vowel to express the required meaning. Driver's presentation is not made lucid by his adherence to the theory of vowel symbolism.[45]

The following schematic sketch is organized on a strictly historical basis, making use of recognized chronological stages which Bergsträsser himself did much to clarify. It is my contention that the same basic factor, viz., the underlying sentence-type, determined the systematization of the vocalism throughout the history of the Semitic languages. In other words, the development of the active form *qatal-*, which we witness in the second millennium B.C., is the logical and consistent application of a principle that was at work in pro-ethnic times.

a. There are two traceable types of primitive Semitic sentence:[46] the verbal, which employed the actor-action form **ta-q(a)ti/ul-* 'thou -st' (pronominal element + verbal theme), and the nominal, expressed by the equational scheme[47] **qati/ul-tā* 'x (art) thou' (noun + implied verbal copula + pronominal element).[48] Although either type of

[43] Cf. e.g., Driver, Problems 17–20 and Bergsträsser, Verbum 10–4.

[44] Problems 72–3.

[45] I can see no linguistic advantage in statements to the effect that a given form was vocalized 'with the soft or light *i* to express stativity or passivity and the hard or heavy *a* to express activity'; see ibid. 72.

[46] It is not necessary to contribute here to the rather academic speculation as to which particular verbal form (imperative, preterite, infinitive, each of which has its supporters) was the earliest in the language. Nor does it matter whether verbal and nominal forms existed side by side from the beginning, or not. For our present purposes it will suffice to go back to that late, although still prehistoric, period when the two basic sentence-types were fully established.

[47] For the terminology see L. Bloomfield, Language chap. 11 (1933).

[48] Logical considerations compel the use of the second-person forms as paradigmatic in historical discussions, instead of the third-person forms, as is commonly done. In both types of Semitic conjugation (preformative and afformative) third-person forms are demonstrably later than the other two. The same pronominal elements underly in both conjugations the forms for the first and second persons respectively, whereas the third person employs in the preformative conjugation a dissimilar pronominal element, and in the afformative conjugation no pronoun at all. Moreover, in the latter conjugation there are accentual differences between the first and second persons on the one hand, and the third person on the other (cf. Arab. *qatál-tu, qatál-ta,* but *qátala*), which are the indirect result of the disparate origin of the forms in question. Finally, the second person influenced in West Semitic the consonantal afformative of the first (*t* instead of *k*). To start with **qátala* rather than **qatál-tā* would, therefore, be anachronistic and generally disadvantageous.

predication may have exhibited externally several vowel-sequences, one such sequence prevailed demonstrably in both sentence forms:

a-i/u,

since it becomes basic in the two resulting 'conjugations', the preformative, or active (**tá-q(a)ti/ul* > **tá-qti/ul*), and the afformative, or neuter (**qatí/úl-tā*). So far from any contrast in this respect, we have absolute identity instead.[49] The only decisive element was the position of the pronoun, which clearly was conditioned by the sense stress (actor-action as against nominal equation).

b. In crystallizing these forms into a verbal system East Semitic (Akkadian) and West Semitic (Aramaic, Canaanite, Arabic) pursued divergent paths. The Akkadian system is objective and impersonal, the verb thus assuming stative character.[50] The neuter afformative conjugation becomes the 'permansive', and the active preformative verb is established as the 'preterite', which is joined presently by a cognate durative 'present' (*ta-qáta/i/ul*).[51] The closer association of the two original categories results in the addition of neuter forms to preformative themes, with a necessarily neuter or passive sense (*nadin* 'given' to *inádin* 'is, was giving'), and in supplementing afformative themes with verbal forms (*imruṣ* 'suffered' alongside *maruṣ* 'painful'). These extensions were achieved by purely external means. **qati/ul-tā* > *qatlā-ta* added a verbal class on the analogy of *ta-qti/ul*, while the latter acquired a nominalized class after the model of *qati/ul-*. The vocalism of the new forms was borrowed from their respective prototypes. Subsequent individual adjustments are due to phonetic and semantic considerations.

The West Semitic system, on the other hand, bears a personal and subjective character which imparts to the verb a temporal orientation. The neuter conjugation becomes the 'perfect', the active conjugation the 'imperfect'. In such a system neuter forms will soon require means for aspective and temporal differentiation; the afformative class adds thus preformative by-forms. West Semitic accomplishes this extension of the paradigm in a manner quite different from Ak-

[49] Again it would be futile to speculate how and why the vowel-sequence *a-i/u* came to prevail. The fact is that it did.

[50] To use 'stative' as a term descriptive of 'Aktionsarten'; for the contrast between objective and subjective verbal systems see Bergsträsser, Einführung 11 (based on Landsberger, Die Eigenbegrifflichkeit der babylonischen Welt, Islamica 2.355-72[1923]).

[51] For the position of the dialect of Ras Shamra see A. Goetze, The Tenses of Ugaritic. JAOS 58.266-309.

kadian. Corresponding to the psychological contrast between the underlying sentence-types and the consequent syntactical reversal of the position of the pronominal elements, there is here also the internal reversal of the stem-vowels: *qatíl-tā* > *tíqtal-*; *qatúl-tā* > *túqtal-*.[52] The stress-accent, which rests in early West Semitic on the non-final long syllable nearest the end, may have been a contributory factor in this transposition. For with the accent thus determined by position, the accented thematic vowel of the afformative class is transferred to the accented position in the preformative class, while the unaccented vowels exchange positions automatically.

The next step is easy to follow. West Semitic tended to specialize its two neuter forms in that *qátil-* signified a temporary state or condition, while *qátul-* indicated a permanent state or a naturally inherent quality; this is still the situation in Arabic.[53] Accordingly, the new form *túqtal-* developed a passive function and came to be used for passives with transitive themes; on the other hand, *tíqtal-* became the common imperfect to *qátul-* as well as *qátil-*.

At the end of this stage the basic verbal system of West Semitic appears as follows:

	Afformative Conjugation (Perfect)	Preformative Conjugation (Imperfect)
Active:	*x*	*táqti/ul*
Neuter:	*qatíl-tā*	*tíqtal* (> with *qátil-* and *qátul-*)
	qatúl-tā	*túqtal* (> passive with transitives)

c. The final stage in the completion of this pattern was the addition of an active perfect to *táqti/ul*. By the process of analogic back-formation the accented vowel of the imperfect became the thematic vowel of the perfect: *qxtál-tā*. In the choice of the first vowel the analogy of the long-established forms *qatíl-tā*[54] and *qatúl-tā*, where the *a*-vowel is constant, was decisive; *qatál-tā* was the inevitable result.

[52] The possibility of such a reversal shook Bergsträsser's confidence in polarity; cf. Verbum 13. See also F. R. Blake, The Internal Passive in Semitic, JAOS 22. 45–54 (1901), and A. Ungnad, Zeitschr. d. deutsch. morg. Ges. 59.766–8 (1905). The part which sentence-types may have played in these changes has not been considered thus far.

[53] Cf. Wright, Arabic Grammar 1.30; Driver, Problems 12; see also F. R. Blake, The so-called Intransitive Verbal Stems in Hebrew, JAOS 24.145–204 (1903).

[54] It is perhaps pertinent to stress once again the fact that according to the above account the origin of the second *a* of *qatál-tā*, the chief difficulty in previous discussions, was determined automatically. The same principle which led to *tí-qtal-* and *tú-qtal-* in the neuter forms was responsible for *qatál-tā* in the active class.

4. Perhaps the strongest argument which advocates of polarity have used in support of their theory is the disagreement between the gender of the numerals 'three' to 'ten' and that of the things numbered. For 'five sons' the Semite would say **ḫámišat- banī*, i.e., with the numeral in the feminine, while 'five daughters' is **ḫámiš- banāt-*, the numeral lacking the feminine suffix. This syntactical disagreement furnishes the only instance where the conditions are precisely as demanded by Meinhof's theory. Furthermore, it is also the only time that a process linked with polarity[55] is common to Semitic in general instead of affecting but a part of this linguistic group. It follows that if this curious behavior of the numerals was indeed due to polarity, the process was in evidence in primitive Semitic.

Upon closer examination,[56] however, polarity proves to be once again a misleading label. The ending *-at* found with these numerals was originally an abstract formative which had nothing to do with gender; its feminine connotation was demonstrably a later development. **ḫámišat-* meant 'pentad' 'quintet', **'árba'at-* 'quartet', and so on,[57] the digits 'three' to 'ten' having started out as abstract nouns, without any relation to gender. This is made clear by the fact that these forms precede the thing numbered, while 'one' which is expressed adjectivally, follows its noun.[58] Furthermore, Akkadian may express 'the four regions of the world' by *kebrāt erbettim* 'the regions of the quartet', thus emphasizing the character of the numeral as an abstract noun. Originally, **ḫámišat-* must have been capable of numbering both male and female beings.[59]

The present syntactical peculiarity has to be connected with the development of grammatical gender in Semitic. The old abstract and collective ending *-at* had been specialized as the designation of the feminine, and the thoroughgoing dichotomy of genders which followed embraced not only adjectives and such nouns as now employed *-at* for purposes of grammatical differentiation, but also those nouns which had long used this ending as a class formative.[60] Thus 'one daughter'

[55] Cf. Bergsträsser, Einführung 15; Driver, Problems 72.

[56] See JAOS 56.39.

[57] The priority of the ('feminine') forms in *-at* was not disputed by the older grammarians; see the summary in Sven Herner, Syntax der Zahlwörter im Alten Testament 4-12 (1893). A different view is taken by Reckendorf and approved by Brockelmann, Grundriss 2.274–5.

[58] 'Two' is also an adjective, but it frequently follows the analogy of 'three' to 'ten' in behaving like a noun that governs a genitive; cf. Brockelmann 273.

[59] This is still often the case; cf. Herner, op. cit.

[60] See Speiser, JAOS 56.37–41.

became **bín-(a)tu 'áḥad-(a)tu*, alongside **binu 'áḥadu* 'one son'. At the same time, however, the abstract noun **ḫámišat* 'pentad', and the analogous forms for the other digits from 'three' on, although they were now feminines grammatically, could still be used with both **banī* 'sons' and the new form **banāt-* 'daughters', because they appeared in a possessive phrase and not as appositions; the position was the same as in **kull-at- banī* 'the totality of sons, all the sons', and **kull-at- ban-āt-* 'all the daughters'.

In course of time, however, the circumstance that the inherited class formative of the digits in question was the same as the new feminine designation brought about the correlation of such groups as:

$$\begin{matrix} *binu\ 'áḥadu \text{ (masc.)} \\ *bín\text{-}(a)tu\ 'áḥad\text{-}(a)tu \text{ (fem.)} \end{matrix} \quad \text{and} \quad *ḫámiš\text{-}at\text{-} \begin{cases} banī \\ ban\text{-}āt\text{-}. \end{cases}$$

The analogy of the adjectives, and especially of the numeral 'one', introduced dichotomy into the other digits. 'Two' appeared as **ṯínay* and **ṯín(a)tay*, the latter with feminines. It is this regular use of *-(a)t* in the feminine forms for 'one' and 'two' that rules out immediately any possibility of polaric contrast in the numerals. For if the irregular syntax of the digits 'three' to 'ten' is ascribed to polarity, the regular behavior of the two remaining digits would, paradoxically, now have to be explained.

In reality, 'two' was to escape confusion of genders because the formative *-(a)t* was overshadowed in this instance by the final dual ending *-ay*. But with the numerals 'three' to 'ten' dichotomy led to analogic creation based on the pattern

$$*banī : *ḫámiš\text{-}at\text{-}$$
$$= *ban\text{-}āt\text{-} : x,$$

whence the introduction of the form **ḫámiš-*, i.e., without the ending *-at*. There remains only the question why the genders were not brought into agreement after the splitting up of the digits. In other words, why did not **ḫámiš-* go with **banī* and **ḫamiš-at-* with **ban-āt-*? The answer is bound up with the history of the respective forms. We know that both **ḫámiš-at-* and **banī* were old forms, while **ban-āt* is considerably later than the feminine signification of *-at*, from which the plural *-āt* subsequently developed. The neologism **ḫámiš-* was assigned, therefore, to the other neologism **ban-āt-* and not to the ancient masculine form; the two secondary formations were brought together.

To sum up, polarity as a linguistic principle cannot be established for the Semitic languages. It has been linked by some writers with the tendency of Semitic towards dichotomy in number and in gender, but

whereas that dichotomy is consistent and thoroughgoing, polarity could be suspected only in a few isolated instances. Moreover, the favorite examples of polarity turn out to be illustrations of well-established linguistic processes; other examples prove to present no contrast whatever, when viewed historically. But even if a given problem, which polarity has been called in to solve, should happen to elude explanation, that problem will not be a case of polarity merely because it is difficult. This would be a lazy man's way out. At best, polarity may be regarded as a label for certain contrasted linguistic phenomena empirically viewed. Even in this limited sense, however, the designation is dangerous in that it may hamper ultimate solution by conveying a false sense of security. It should be abandoned altogether.

MISCELLANEA

OSCAN NOTES

The Curse of Vibia[1] has been made the subject of a very painstaking and incisive study[2] by R. G. Kent. His article presents the diplomatic evidence with scrupulous care, summarizes the earlier literature, and very cleverly reaches a restoration of the entire text which must be regarded as highly successful—within the limits, of course, that success may be predicated of any reconstruction of a half-lost text in a language as imperfectly known as Oscan. I take this article as the basis for my remarks, and shall present its text with indication of only the larger gaps.

It begins:

(1) **Keri Arentikai manafum pai pui heriam suvam leginum suvam aflukad P[akim Kluvatiium Valaimas puklum inim]** (2) **usurs inim malaks nestrus Pakiui Kluvatiiui Valaimas puklui ant kadum.**

At the close of this sentence scholars have generally seen an infinitive of purpose 'cadere', compounded or adverbially modified by **ant** 'zur Vergeltung'. I suggest that **kadum** 'ill will, hatred', or the like, is the accusative of a noun, whose genitive *cadeis* stands in the Tabula Bantina;[3] and that the prepositional phrase, except for being marginal (metaphorical) in meaning, is much like **viam teremnattens ant púnttram** 'they laid out the road up to the bridge'.[4] I translate: 'To Ceres I have consigned Pacius to her hatred'; practically 'that she may vent her ill will on him'.

Others do not end the sentence at this point, and Kent's text continues:

damia[d pesemad pai poi suvam heriam suvam] (3) **leginum aflukad idik tfei manafum**

The **suvam** at the end of the gap is all that I can accept unhesitatingly; **suvam heriam** is attractive, but the following line shows that its mention is not obligatory. The decision will turn on the question of space,

[1] Buck, Grammar of Oscan and Umbrian[2] 243–6 (Boston, 1928).
[2] Class. Phil. 20.243–67 (1925).
[3] Buck 230–9, esp. 235n.
[4] Buck 239–40.

which cannot be broached until the wording of the preceding is recovered. I am, however, unable to do more than suggest something like the contents of the gap. The subject of **aflukad** (3d person) must be Ceres, while the **tfei** compels us to find within the gap a vocative and some verb in the 2d person. I suggest that Damia[5] was invoked to join with[6] Ceres that the latter might let loose her legion.

In what follows immediately I should differ from the division of sentences made by Kent. First come two sentences in parataxis: **Vibiiai Prebaiam pukulum dadad. Keri Ar[entikai manafum Pakim Kluvatiium] Valaimas puklum inim ulas leginei.** 'May he restore the girl Prebaia to Vibia! (To gain that end) have I consigned Pacius to Ceres and to her legion'. Then **svai neip dadid** to **svai neip** makes a well balanced sentence, the zero-anaphora of **dadid** being all the easier because the phrases are in contrasting position.

In line 7 I should divide the words differently, reading:

nip huntruis nip supruis aisusis putiians pidum putiians uft eisud f[akium nestrus Pakiui Kluvatiui Valaimas puklui].

'By sacrifices neither to the gods below nor to the gods on high shall the kinsmen of Pacius be able to do anything in this (matter)'.[7] The previous reading **ufteis udf[akium** is objectionable, because IE *ud* seems to have left no continuant in Italy. I take **uft** < **opt* to be a doublet of **úp**, comparing **post:** *pos*, **pert:** *per*, **ampt:** *am-*.[8]

For **sakraítir** instead of the expected ***sakraíter** Buck[9] offers two explanations, thus indicating that neither is satisfactory. I suggest that it is merely an instance of writing **i** for **e** before **r**. In Oscan none of the other examples[10] is in a final syllable; but there is no reason for the restriction, which does not hold for Umbrian.[11] In Oscan an ending *-er* is not frequent, and probably this is the explanation for the lack of other examples. Besides, Oscan is a highly standardized language, so that variation in the spelling of the affix *-ter* was likely to occur less frequently than similar fluctuations in the basic syllables of a word.

G. M. Bolling

[5] No vocative of the 1st declension is recorded in Oscan. If the form was like Umbrian *Tursa*, *Serfia*, **Damia[** is entirely satisfactory.

[6] Ignorance of the exact situation in Hell precludes a more precise suggestion; 'bid', 'allow', 'beg' are possibilities.

[7] No doubt the connotation was much like that of English *to do anything about it* 'to interfere'.

[8] See Buck 98–9.

[9] Buck 178; the form occurs in no. 31b4.

[10] Buck 32.

[11] Examples in Buck 32.

OMISSION OF OBJECT PRONOUN IN PORTUGUESE

Affirmative answers are commonly made in Portuguese by the mere repetition of the verb of the question, e.g., *Leu o jornal desta manhã? Li.* 'Did you read this morning's paper? I read.' This was pointed out by L. Spitzer (Zeit. für rom. Phil. 36.683), and H. Sten has recently shown that such repetition often occurs after declarative sentences (Arch. für das Stud. d. neu. Spr. 170.229–231). However, what distinguishes the Portuguese construction from that of other Romance languages is not the repetition of the verb, but the omission of a pronoun object to replace the noun-object of the question or of the preceding statement. It is therefore this omission that requires an explanation.

The explanation is to be found in the contraction of the pronoun with the ending of the verb, the pronoun *o* with the endings *-o* and *-u* and the pronoun *a* with the ending *-a*, these pronouns being commonly enclitics to the verb in Old Portuguese (W. Meyer-Lübke, Zeit. f. rom. Phil. 21.318). Such contractions were fairly common in Old Portuguese, e.g., *E o cide Rui Diaz venceo elrei . . . e prendeo* (J. J. Nunes, Florilégio 47, Lisbon, 1932) 'And the Cid, Rui Diaz, conquered the king and seized him.' Here *prendeo* stands for *prendeo-o*. While in the end these contractions were successfully resisted in the literary language, my theory is that their influence in the colloquial language was such that the pronoun came to be omitted even in combinations where contraction could not take place, for example, between the pronoun *o* and the ending *-a*, and in the plural also.

If the repetition of the verb became 'eine mechanische Wiederholung des Wortklangs der Frage', as Spitzer says, it was because the pronoun had been lost beforehand. And thus a change in syntax is traceable to a phonological cause.

Spitzer has recently (Du Language-echo en Portugais in Boletim de Filologia 5.165–9 [1937]) advanced the theory that this type of answer is a product of 'the primitive, fatalistic attitude of the Portuguese'.

E. B. WILLIAMS

SPANISH *cansar* 'TO WEARY'

Rice is right (LANGUAGE 13.18) in demanding for this verb the same etymon as for It. *cansare* 'to avoid', i.e. *campsare* 'to sail by' (= Gr. *κάμπτω*, aor. *κάμψαι*). He could have quoted Löfstedt, Philolog. Komm. zur Peregr. Aether. 109, who points out that it was in Latin a word borrowed by the sailors from Greek (like *petra*), and he could have

mentioned the general principle that it is wrong to separate Romance homonyms unless there is an especial need for doing so. He might also have said that the insertion of *-n-* in *quassare* 'to shake' > Sp. *cansar* 'to weary' is unparalleled (the examples quoted in Cuervo's dictionary are different: *ensayo* = *exagium* shows the prefix *en-*, *mensage*, etc. a nasal assimilated to a nasal, etc.). But I strongly oppose the etymological principle set forth by Rice 'that meanings unattested in Latin words borrowed from the Greek, but attested in their Greek originals, sometimes best explain the sense-development of their Romance reflexes'. In accordance with this principle Rice reconstructs a Lat. meaning corresponding to the Gr. 'to bend, bow down'. The example Sp. *sima* 'abyss' from Gr. σιμός is not a happy one, although REW[3] (no. 7931) accepts it, because I pointed out in RFE 13.119 the possibility of an etymology **sedīmen* (REW 7784). On the contrary I think we should admit only such meanings of Greek loanwords as are attested in Latin, for otherwise we fall back into the (now fortunately obsolete) reconstructions of a 'grec superflu' (as Sainéan would say).[1] Let us then start from Lat. *campsare de via* (attested in the Peregrinatio with the meaning 'to deviate', 'abbiegen'). Now if we compare the Biblical examples of 13th century Spanish given by Cuervo with the corresponding Latin originals, we find *Non canses de faser orasion* = *ne cesses clamare* (1 Kings 1.7.8); *Non cansaron* = *quorum pietates non defuerunt* (Eccl. 44.10). Thus 'to deviate' becomes before our eyes 'to discontinue the former way' > 'to cease', 'to fail' > 'to get tired'. By this supposition we escape the hypostasis of a Greek meaning hitherto unknown in Latin and conform to the well-known Latin facts: originally a maritime word, like *llegar*, *arriver*, and so many others, *campsare* was used also on the continent for 'to pass by' and later on metaphorically 'to avoid' or 'to discontinue', 'to get weary'.

LEO SPITZER

[1] *chistar* 'to utter a sound' is not to be traced back to Basque *txistu* 'to whistle', as Rice, Hisp. Rev. 6.75, would have it; for the Sp. and the Basque word are both 'elementarverwandt' as 'Schallwörter'. How can Rice without any material prove that the Sp. word is Basque and not the reverse, or that they are not both parallel formations? It is well known that imitative harmony and borrowing of words are not mutually exclusive. Compare the paragraphs on 'Lautmalerei' in my Hugo-Schuchardt-Brevier and especially Schuchardt's review in Literaturblatt, 40.397 of H. Urtel's Zur baskischen Onomatopoesis. There is also a 'basque superflu'.

BOOK REVIEWS

Die hethitisch-akkadische Bilingue des Hattušili I (Labarna II). Pp. XII + 288 with three plates. By FERDINAND SOMMER and ADAM FALKENSTEIN. (Abhandlungen der Bayerischen Akademie der Wissenschaften, Philosophisch-historische Abteilung. Neue Folge. Heft 16). Munich, 1938.

What we know about early Hittite history comes chiefly from three documents ascribed respectively to three great kings, Anittas[1] (about 1950 B.C.), Hattusilis I (about 1800 B.C.), and Telipinus (about 1650 B.C.). The second of these documents is a royal decree in which Labarnas II, later known as Hattusilis, adopts his grandson (?) Mursilis, and designates him heir to the throne. The text is bilingual, Hittite and Akkadian; the Akkadian columns have been broken away for the most part, and there are serious lacunae in the Hittite text also. The editors have done a remarkable job of restoration, so that they print 49 complete or nearly complete lines of Akkadian and practically all of the Hittite text (158 lines in all) except five lines from which only one to three words each could be rescued. Many of the supplements are, of course, uncertain; but the course of the argument is clear almost everywhere. The vigorous personality of the author emerges with startling vividness; we now know Hattusilis I as we know no other Hittite except his successor, five centuries later, Hattusilis III.

The new historical knowledge that results need not occupy us here. Of more interest to linguists is the demonstration that the Akkadian text is a translation of the Hittite; we can no longer doubt that we bave a copy of a document originally composed in Hittite about the beginning of the eighteenth century.

In general Sommer is responsible for the Hittite material in the new book and Falkenstein for the Akkadian, although both authors have paid attention to the entire work. We are warned in the preface not

[1] Sommer, like most other scholars, takes over Hittite proper names in their Akkadian form. To me it seems more satisfactory to name Hittite kings according to their own language. My system of transliteration differs from his in several other respects also.

We need not discuss here the question of the authenticity of the Anittas text that has been raised by Güterbock ZA NF 10.139–45.

to lay too much stress upon the use of the first person singular in discussions of Hittite matters. Nevertheless we may safely conclude that most of the contributions to Hittite grammar are Sommer's unless, as frequently happens, they are explicitly ascribed to Ehelolf or others. These contributions are so numerous and so varied that we can do no more than pick out a few illustrations.

Hitherto we have known the Hittite word for 'heart' in the gen., *kardiyas*, and dat., *kardi*, *karti*, ŠA(G)-*ta*. Sommer (93–6) cites nom.-acc. ŠA(G)-*ir*, sometimes with neuter modifier, and, from a recently discovered fragment, *gi-ir-ši-it* and *gi-ir-me-it* whose meaning may be 'his heart' and 'my heart'. All will be inclined to follow Sommer in interpreting this as evidence for a nom.-acc. *ker* < **kērd*. Sommer finds also evidence for gen. *kardas* and abl. *kardaz* to go with dat. ŠA(G)-*ta*. No doubt the usual declension was *ker*, *kardas*, with a variant *i*-stem or possibly *ya*-stem (*kardiyas*). It follows that *karāz* (masc.) is not the ordinary nom. of the word for 'heart', as I assumed in the second edition of my Glossary, following a suggestion of Ehelolf, ZA NF 9.176 f. Sommer is surely right in connecting *karāz* with the familiar plural *karātes*, *garātes*, which denotes a part of the body. The assumption of the meaning 'interior of the body', pl. 'entrails' seems to satisfy all requirements. Sommer refrains from deciding whether or not the word is ultimately related to *ker* 'heart'.

The discovery (78) of a sentence connective *su* in early Hittite disposes neatly of Petersen's strongest argument (AJP 58.311) against the generally accepted connection of early Hittite *tas* 'et is' with the sentence connective *ta*. Petersen's argument runs: early Hittite *sas*, like *tas*, is used only initially in the sentence, and neither word is accompanied by a sentence connective; but *sas* 'cannot be composed of a particle **sa* with the pronoun *-as* because there is no Hitt. sentence-connecting particle **sa*. Now if an uncompounded *sas* can act as the sole sentence connective, so can an uncompounded *tas*, and the one argument from the origin of the latter as *ta* + *as* falls to the ground'. But now it appears that *sas* represents *su* + *as* just as *nas* represents *nu* + *as*![2]

Sommer (97–100, 232) translates *hattatar* 'Weisheit, Verstand, weiser Rat'; but he proposes an etymological connection with the verb *hatta-* 'strike, hew, carve, make a hole; slaughter'.[3] One may grant that a

[2] Sommer indicates that his discovery has a bearing on Petersen's argument, but he does not go into details.

[3] He shows that the translation 'pierce', which I adopted for my Glossary from Friedrich, ZA NF 3.186 f., is inexact.

semantic bridge between the two can be constructed; but a more natural meaning for a verbal noun from *hatta-* 'carve, make a hole' would be 'writing (on a clay tablet)'. There can be no doubt that this meaning fits the passages even better than Sommer's 'wisdom'. The phrase *hattannas* LUGAL-*us* 'king of *h.*' is an epithet of Nabū, the god of writing, in VBoT 2.16, and of Enki, god of wisdom and the arts, in KBo. 4.1.1.32 f. = KUB 2.2.1.40 f. = 9.33.1.9. In the decree before us Hattusilis says to his nobles (2.56 f.): 'But you who already know my words and my writing (*ud-da-a-ar-mit-it ḫa-at-ta-a-da-mi-it-ta*) indoctrinate my son'. Later on (3.55–8) he says to his heir: 'I have given you my words. Now let them read this tablet to you every month; you will impress my words and my document ([*ud-da-*]*a-ar-me-it ḫa-at-ta*<*-ta*>*-me-it-ta*) upon your heart.' The word in the former passage translated by 'indoctrinate' is *ḫa-at-ta-aḫ-ḫi-eš-ki-te-en*; it is evidently another derivative of the verb *hatta-*. Sommer understands the underlying verb **hattahh-* to mean 'verständig machen, klug machen', and connects it with *hattanza*, in the colophon of a ritual text (KUB 20.74. 6.9), which he thinks may mean 'weiser Mann'.[4] I propose for the verb 'make literate', in the present context 'acquaint with (this) written document', and for *hattanza* 'litteratus'. There remains KUB 24.3.2.18 + 4.1.8, which Sommer translates: 'weiter ist der Menschheit euer (der Götter) *hattatar* verlorengegangen; nun gibt es nichts, was wir recht tun könnten.' Here again the meaning 'writing, written documents' seems appropriate.

It is impossible to give in brief space an adequate notion of the riches that a careful study will discover in the volume. Ready consultation for a specific purpose is made possible by the elaborate indexing that Sommer appends to all his books on Hittite. This time, however, he has not composed the indexes himself; Falkenstein is responsible for those on Akkadian, and Wolfgang Drohla for the others.

EDGAR H. STURTEVANT.

Wortbildung der homerischen Sprache. Pp. xvi + 330. By ERNST RISCH. (Untersuchungen zur indogermanischen Sprach- und Kulturwissenschaft 9.) Berlin and Leipsic: Walter de Gruyter and Co., 1937.

Such a book has been wished for fervently for years.[1] Students of the

[4] In its other occurrence the word is an epithet of a woman who is posing as a midwife; here again it may well mean 'instructed, trained'.

[1] Literally. The earliest expression of the wish that I recall was voiced by Hermann Collitz in one of the early volumes of Bezzenberger's Beiträge. I echoed it as long ago as 1923, Class. Phil. 18.269.

Homeric language have now for the first time a piece of equipment comparable with those which Vedic scholars have enjoyed for decades. Of course it is not all that we need; and perhaps I cannot better testify to my high opinion of the work than by expressing the wish that its author will soon present us with a companion volume on Homeric morphology.

His primary purpose is to catalog the Homeric vocabulary according to the morphemes (other than the inflectional affixes) with which it is built. That these cannot always be analyzed with final certitude is a fact of which the author is fully aware, but he wisely decides (viii) to go ahead and present the facts at least as they seem to be. Consequently, for example, the list of *ρο* and *ρη* stems (65) begins with *ἀρή* (probably [4] a root-noun in *-ē*), *ἀ̔ρή* < **αρϝη* (so explained and listed 156–7), and *βάρβαρος* treated elsewhere (10) as *βάρ-βαρ-ο-*. In all these cases cross-references call attention to the varying possibilities; and such references are given thruout with the greatest liberality. The treatment is similar to that in the corresponding chapter of the Stolz-Leumann[2] grammar, and that is high praise indeed.

My first impression is that the author has attained his primary objective in a highly successful fashion. Continued use may bring some defects to light, but I shall be surprised if they prove such as to impair seriously the value of the tool he has presented to us. Against one danger the author does not seem to be sufficiently on guard—the risk attendant upon taking our texts as they stand without scrutinizing the tradition on which they are based. The most curious trap of the sort I know may be mentioned, tho it does not concern Risch, since a question of inflection (not of word-formation) is involved. A formula *ἀλλ' ἄγεθ', ὡς ἂν ἐγὼ εἴπω, πειθώμεθα πάντες* occurs in both poems. One blindly following Ludwich (and his editions are still the best) must observe that the Iliad has *ἐγὼ εἴπω* but the Odyssey *ἐγὼν εἴπω*. The observation would, however, be an illusion, since the tradition is very strongly for *ἐγὼ εἴπω* thruout.[3] Ludwich has mishandled the Odyssey.

Into such traps Risch falls twice. He speaks (35) of the gen. of *μάντις* as *μάντιος* and *μάντηος*, and bases (117) on the latter form

[2] Risch's work was inspired by Leumann.

[3] Nowhere else is *ἐγώ* used before a word with *ϝ-*, nor is it used instead of *ἐγών* before a vowel because of an intervening caesura. (The tradition is, however, seriously divided at I 167, E 475, the weight of authority being against the rule in the former passage.) The formula *ὡς ἂν ἐγὼ εἴπω* is to be added as a qualification to Monro's statement (Hom. Gram.[2] 370) that the pronoun *ἑο, οἱ, ἑ* is 'the only word in which the original *ϝ* is recognized in the spelling of our texts'.

his explanation of *μαντήϊος*. It is true that our editors read *μάντηος ἀλαοῦ* at *κ* 493, *μ* 267; but they are adopting an emendation of Hermann's. The MSS (with trifling exceptions) have *μάντιος ἀλαοῦ*.[4] Again, the aorist to *καίω* is given (219) as *ἔκηα*. That is a proper description of our texts, but Ludwich's apparatus shows that the MSS support *κη-* in some passages, *κει-* in others. Monro and Meister have shown that such fluctuations between *-η-* and *-ει-* correlate with the non-existence or existence of corresponding forms with *-ε-* in Attic. Now in Attic *ἔκαυσα* did not at once conquer the whole paradigm, and I have shown[5] that the variation between Homeric *κη-* and *κει-* accords well with that of *καυσ-* and *κε-* in Attic.

Risch gives us much more than the description of facts at which he is primarily aiming. I should mention first numerous interesting observations about the semantic groupings of words formed with the same suffix. Valuable also are incidental observations about the uneven distribution of certain forms within the poems. For instance (26) compounds of nouns in *-τηρ* and *-τωρ* are said to be rare and confined to late passages: *ἀμαλλοδετῆρες* Σ 553, 554, *μηλοβοτῆρες* Σ 529, *οἰνοποτῆρες* *ϑ* 456. The same is true of the pseudo-examples *ὁμοκλητήρ* M 273, Ψ 452, *αἰσυμνητήρ* Ω 347, and the juxtapose *πανδαμάτωρ* Ω 5 ∽ *ι* 373. However, feminines implying such compounds must be considered: *λῃϊβοτείρης* *σ* 29 conforms, but *πουλυβοτείρῃ* is found as early as Γ 89 and recurs frequently in the Iliad. The latter may, however, be considered a juxtapose, like *πανδαμάτωρ*. It is, perhaps, worth noting that these compounds (except the juxtapose *πανδαμάτωρ*) end always in *-τηρ*.[6] Again (36), feminines in *-τῦς* are almost twice as frequent (38:20) in the Odyssey as in the Iliad. It would have been worth stating that with allowance for the shortness of the Odyssey the proportion becomes 50:20. Other examples are: (120) almost all examples of the analogical extensions of *-ειος* come from the Odyssey; (142) *ἀνϑεμόεντα* only in late passages B 467, 695, Ψ 885, *γ* 440, *μ* 159, *ω* 275; (237) deverbatives in *-άνω* are more frequent in the Odyssey than in the Iliad; (303) *ἑξῆς* only in the Odyssey.

The author stresses the fact that he is not writing an etymological dictionary, but his book contains numerous incidental references to

[4] Except *πόλις* which has a history of its own, all *ι*-stems make the gen. in *-ιος*, so that *μάντιος* must be allowed to stand. *Μαντήϊον*: *ἱερήϊον*: *χαλκήϊον* = *μαντεύομαι* : *ἱερεύω*: *χαλκεύω*.

[5] Class. Phil. 20.157–9 (1925).

[6] This is all the more noteworthy because of its contrast with forms from *r*- and *n*-stems in which such variations as *πατήρ*:*-πάτωρ*, *φρήν*: *-φρων* are regular.

etymological matters that are well deserving of consideration. The references given in this connection show a wide acquaintance with recent writings on the subject; and yet his reading must be described as spotty. From the list of works most frequently cited one misses at once the name of Hirt.[7] That scholar's work, in spite of some superficial defects, is too stimulating to be simply ignored. America is for Risch almost an unknown land. He cites articles by Buck and Petersen in the 16th and 17th volumes of Classical Philology, but does not know that Greek suffixes were studied at Chicago both before and after these papers, nor that there is anything else in Classical Philology.[8] He is interested in Hittite but makes no mention of Sturtevant, nor of Sapir's articles on *ὀπηδός* and *ἀτύζομαι*. From the publications of the Linguistic Society all that he can glean is that Petersen considers the *ι* of *σκίδνημι*, etc., due to assimilation to the type of *ἵστημι*. Much more could have been got; for instance, from two of our Dissertations: R. N. Albright, The Vedic Declension of the Type *vṛkīs*; G. S. Hopkins, Indo-European **deiwos* and Related Words.

G. M. Bolling

Tabulae Iguvinae. Pp. viii + 447. By G. Devoto. Romae: Typis Regiae Officinae Polygraphicae, 1937.

The Umbrian Tables of Iguvium, giving the ritual of the Atiedian Brotherhood, were found in 1444, and defied interpretation except in small details, until a reasonable start was made by Aufrecht and Kirchhoff, Die umbrischen Sprachdenkmäler, 2 vols., Berlin, 1849–51, followed by Bréal, Les Tables Eugubines, Paris, 1875, and Bücheler, Umbrica, Bonn, 1883. Then came von Planta's Grammatik der oskisch-umbrischen Dialekte, 2 vols., Strassburg, 1892–7, which gave detailed etymological and morphological studies, and an edition of the texts, and Conway's Italic Dialects, 2 vols., Cambridge, 1897, with detailed epigraphic data and brief grammatical treatment. Next came Buck's Grammar of Oscan and Umbrian, Boston, 1904, a briefer grammatical treatment with short commentary on the texts; German translation by Prokosch in 1905, and a second corrected and augmented English edition in 1928. More recent is von Blumenthal's Die iguvinischen Tafeln, Stuttgart, 1931, with many original interpretations (cf. my review in Language 9.214–8). The subject of this review ends the series of collective works; there are of course almost countless shorter articles dealing with the subject.

[7] Nor do I recall any isolated citation of him.

[8] I recall no citation of the American Journal of Philology.

Devoto has given us the most detailed treatment which the Tables have yet received, and he has not shirked the difficulties; he has moreover done much strikingly original work, whether or not we agree with his results, and he endeavors at every point to be clear and precise. But I have one major criticism. Apparently no scholar has ever made and published a translation of the Iguvine Tables into a modern language; and yet the Latin versions which they have given are at many critical places just about as hard to understand as the original Umbrian. It is regrettable, therefore, that Devoto did not (as I urged him in a personal letter) include a translation into Italian (or other modern language); on the other hand, his work has in fact enabled me to make a written translation into English in accordance with his interpretations (though I do not accept them all), and I can understand what it means.

Devoto gives first (5–27) an account of the finding and later fate of the Tablets, which are still at Gubbio, and of the history of their interpretation; he seems to know Buck's Grammar only in the German translation, and not to be aware of the second English edition. In the second section (31–50) he gives excellent, though rather too much reduced, plates of the Tablets, and a precise transliteration. He gives next (51–5) a discussion of the date of the Tablets, his conclusion being that those in the Umbrian alphabet were inscribed between 200 and 120 B.C., and those in the Latin alphabet between 150 and 70, but that all are based on copies inscribed in the third century B.C. The following chapter (56–97) is on the actual writing, with an extremely detailed discussion of all doubtful readings and of all errors or suspected errors.

After this comes (101–36) the text (slightly normalized: *š* for *s* where justified, dots of word-division omitted, errors corrected) and Latin version, followed by the detailed commentary (137–417) and the indexes (421–47). It is the commentary which is the most important, as well as much the longest chapter; it is conveniently divided into numbered sections. To it I now give my attention.

Devoto's method is essentially the combinational; he compares all relevant passages in studying a word or phrase, limiting its meaning as much as possible before turning to etymological connections. He deserves our heartfelt thanks for this procedure, for Umbrian studies have in recent decades suffered from too strict an adherence to the etymological method. But he has at times been led to extremes. For example, in §195 he takes up the meaning of **ampentu** (II.a.20, etc.), and from the succession of ritual acts in V.a.17–20 he decides with his predecessors that it must mean 'slay (the sacrificial victim)'; by comparison with Latin *impendere* he posits a euphemistic meaning 'hang up,

strangle or suffocate (as means of sacrifical slaying), slay'. But Bücheler 33 got the right interpretation when he said *quasi adpenditur deo res sacra* 'the offering is as it were paid to the god'; perhaps this also is a euphemism for the actual word of slaying. Again he seeks to distinguish between the *prosesèto* (§73) and the *supo* (§79), as *exta* 'heart, lungs, etc.' and *viscera* 'entrails' respectively; but my examination of the passages makes me regard *prosesèto* as a name for the *supo* after they have been cut from the victim; every passage can be thus interpreted. He goes astray (§78) in his interpretation of *persaio-*, an adjective applying to the position of the sacrificial victim when it is slain; he takes it not as from *perso-* 'ground' and meaning 'stretched out on the ground', but as from *pers-* 'foot', and meaning 'standing on its feet': he says (213 infra) that when the victim is *persaio-* 'standing on its feet' (*pedarius* is the Latin) the *exta* and the *viscera* could be removed separately, but when the victim was on its back (*supinus*) the *viscera* could not be cut out separately from the *exta*. This is an impossibility.

On the phonological side he not infrequently chooses the less probable alternative or even an impossible way. For *anglaf* 'the singing birds' he reverts (§7) to Huschke, Die iguvinische Tafeln 47, and connects with ἀγγέλους as 'nuntios (sacrificales)', referring to but disregarding Brugmann's **an-klā-* (SbdSAW 1890.205). His reference (302 infra) to my remarks, CP 15.366, on an alternation of **ç** and **i**, is misleading, if not based on a misunderstanding; cf. also §187. Likewise, in his interpretation of **eikvasatis** (III.24; §255), he takes **-sa-** as for **-çia-** from *-kiā-*, despite the fact that this is in the Umbrian alphabet, and cites as collateral support a palatization in (the Latin writing) *iiuvina* for *igu-*: but the developments are not parallel, the script variation is impossible, and the explanation of *iiuvina* was given by Conway, ID 1.405n. Other similar points will be mentioned later.

But Devoto always gives a clear idea of his meaning, and his diagrams are very helpful, notably those of the *templum* (VI.a.8–11) page 163, of the divisions of the ceremony 228, of the altar and its parts 240, of the locations of the 'exterminated' nations (VI.b.54) 275, of the construction of the *feriōn-* or 'litter' on which the victim was put (III.16–20) 382.

I turn now to some specific points, in the order in which they occur in Devoto's commentary; my own comments, if added, are readily distinguished. Page 142, §4: VI.a.1 *dersua* 'in the west' and *mersta* 'in the east', cf. Latin *dorsum* 'back' (the observer faced the east in taking auspices) and Gothic *maúrgins* 'tempus matutinum'; this because the words seem to correspond to **pernaies pusnaes** 'anticis posticis' I.a.2. While this makes excellent sense, the etymological interpretation seems

to me to be far-fetched. 151, §13: VI.a.5 *esmei stahmei stahmitei* 'in hac superficie extensa', denoting the place set aside for observation; but the forms may be dative as well as ablative, and a meaning 'for this established ordinance', coordinate with the preceding datives, would fit the passage equally well. 152, §15: Devoto takes **sestu** II.b.24 (misquoted as **sistu**) as from **sizdo*, **seste** II.b.22 as from **sizder*, *sesust* VI.a.5 as from **sissust* < **sizdust*; in the Umbrian alphabet *zd* is written **st** of necessity, but in the later writing (Latin alphabet) it has, he says, developed phonetically to *ss*, written *s*. This seems only an attempt to avoid assigning some of the forms to the correspondent of Latin *sīdō* and others to that of *sistō*, against which separation there is no good reason. 156, §17 and 188, §52: VI.a.24 *arsie* is taken as an abl. of a substantive meaning 'actio sacra', hence 'in dedicatione', not as a vocative 'sancte', and similarly *arsier frite* 'dedicationis fiducia'; the usual 'sancte', 'sancti fiducia' is out of place, because there follows in each clause an accusative *tio* 'te' with an appositive *Dei Graboue*, and it would be queer to refer to the same deity in 2nd and 3d persons within the same clause.

156, §18: Participles of the type *muieto* to impv. *mugato* VI.a.6–7 are said to have *ē* before the participial suffix; but it is rather *ĕ*, cf. von Planta 2.400–1, Buck §36.3, for a conjugational variation *ā*:*ē* is unknown. 159, §22: VI.a.8 *ocrer peihaner* is taken as a genitive of time within which, not as one of purpose, and similarly other such phrases, despite the apparent dependence on *paca* VI.a.19–20 (174 Devoto takes *paca* as 'rite'). 164, §27 VI.a.12 *ebetrafe*, VI.b.53 *hebetafe* denotes the exit from the city, which is likewise an entrance; for the plurale tantum, cf. Latin *forēs*. 165–8, §§28–31: The interpretations of the place-names forming the city boundaries, VI.a.12–4, is most ingenious and interesting, though they must remain uncertain; particularly *ooserclom* 'the brook', as a diminutive of *Auser*, name of a river, today the *Serchio*. 170, §35: VI.a.18 *esoneir seueir* 'formulis sacrificalibus' is a new interpretation; the usual one is *seuo-* 'totus'. 171–2, §37: VI.a.19 *perca arsmatia* 'togam sacerdotalem', because in VI.b.49 the flamen is to put it on, *anouihimu*, which points to a garment. 172–3, §38: Some of Devoto's difficulties with *habitu*, **hahtu**, etc., would be lightened by reference to my remarks on contamination (*hab-*, *cap-*), TAPA 57.52. 176, §42: VI.a.20 *cehefi dia* 'procedat bis'; but *dia* is quite certainly 'faciat', though Devoto may be right on *cehefi* 'come near', for which he cites Osc. **kahad** as from Blumenthal, IT 79 (who does not mention it), and this meaning fits **ap vuku kukehes** III.20–1.

190–1, §54: Devoto emphasizes an opposition between *sakri-* and

perakni- (later *perakri-*, by influence of *sakri-*) as denoting respectively that which belongs to the sanctuary and that which is fit to be received from outside for use in the sacrifice; he notes the distinction at V.a.6–7 and VI.b.56, where the words occur as asyndetic pairs. The root *ak-* in *perakni-* he finds again in *pusti acnu* (V.b.8 ff.), which he interprets 'secundum moles, modulum, mensuram' §285, while the *per-* means 'distant' as in *peregrē*. 191, §55: VI.a.26 *orer ose* 'huius sacrificii (perfectione)', another genitive of time; **opso-* 'sacrificial work', cf. *opus*. 194, §58: VI.a.26 *arsmor* 'sacerdotum concilia', because this makes a suitable pair with *nerf* in VI.a.32, 'principes sacerdotes'; why then does he (197–9, §62) stick to *castruo* 'capita', instead of the now generally accepted 'fundos' (cf. Buck[2] 364), which makes an excellent pair in *castruo fri* 'fundos fruges'? 200–2, §65–§66: **arvia** 'grana', **ařepes arves** 'tostis granis'; **ař-ep-**, cf. *ad* and Greek ἕψειν 'to boil'. 202, §67: VI.a.56 *tases persnimu* 'tacitus precator' = I.a.6 **kutef pesnimu**. Devoto interprets **kutef** as 'clare', cf. Irish *guth* 'vox', and thinks that there had been a change in the ritual. But it is more natural to retain the conventional **kutef** '*cautens, taking care (that the words be not heard and understood by the bystanders)', which would be the same as '*tacitus*, in low tones'; cf. similar usages in India. 204, §69: *poni*, often with wine as its alternative, must be a beverage known to the priesthood before they came into wine-regions, therefore mead or beer, the old original drink; this seems to me convincing.

Devoto's interpretations of *persondro-* as perhaps the *mola salsa* (§110), *sorso-* as in most passages 'liquid' (could it be 'rubbed with pig's fat'?) and *staflari-* as 'solid' (§111), and *erus* as the parts of the victim not used in the sacrifice but distributed to the people (§117), are most interesting, but would require too much space for discussion here. 257–8, §126: I.b.7 **ukar** is said to be an old form for **oker*, cf. the vowel in *ager*; but this is impossible. **Ukar** is the nominative of an adjective, **okāri-*, used to avoid consonantal variation in a short word: **oker* > **oçer*, hardly identifiable as belonging with the rest of the paradigm: *ocrer ocre ocrem* etc. 258–9, §126: I.b.8 **vaçetum ise** (= **inse**) 'vitium inerit', VI.b.47 *vasetom efust* 'vitium infuerit'; attractive, but ***inse** should yield **inze** or **ize**, cf. Buck §110.1. 271, §138: VI.b.51 *ponisiater*, I.b.15 **puniçate** 'of the *minister potionis*', derivative of *poni-*, cf. §69. 277, §144: VI.b.56 *arsmahamo* 'arrange yourselves in *arsmor* or priestly ranks', cf. §58; good. *Caterahamo* is then 'arrange yourselves into military ranks' (ibid.). 301, §168: VII.a.52 *sehemeniar* gen. 'concilii', from **sem-* 'one'; very doubtful. 303, §170: I.b.45 and II.a.44 **Vuvçis**

'Vovicius', to avoid the difficulty of a retained diphthong; despite which the equation with *Lūcius* still seems to me preferable. **Teteies** 'Tetei', gen. of father's nomen; quite impossible, and I suggest equation with *Tetteiēnus*, a name of Umbrian type. 317, §186: II.a.13 **tra ekvi ne fetu** 'extra collegium non facito', with the word division of the Table; the usual reading is **tra ekvine fetu** 'trans Equinam facito', but the *Equina* remains obscure. Devoto associates **ekvi** with **eikvasatis** III.24, 29 and **eikvasese** V.a.4, 16, 'collegialibus' and 'in collegiis' respectively. It is curious that here only is found the negative with **fetu**, and that the clause is immediately followed by an affirmative **fetu**. 322.14, §190: To speak of pressing the '*soma* semen' among the Hindus, seems to rest on a misunderstanding; the Hindus pressed the stems and not the seed of the plant.

405, §273: V.a.13 **nuřpener** cannot come from the Italic equivalent of Latin **nūdipondiīs*, which would be **nof(e)fo-p-*; Devoto notes this, and suggests an early borrowing of the word from the Romans! 407, §274: V.a.11 **esunesku vepurus** 'cum verbis sacrificalibus', **vepur-** < **u̯eqʷos* 'word', Greek ἔπος; difficult, at least. 407, §275: V.a.12 **ařputrati fratru** 'adventui fratrum', *ad* + root seen in *ebetra*, Latin *baetere* 'go'; but the *-u-* is hard to explain, and the usual 'arbitrio fratrum' is simpler and adequate. Contamination with *putāre* then accounts for the *-u-*, cf. TAPA 57.56. 408, §276: V.a.11 **felsva** 'cenam, dinner'; the sense is good, but Devoto can give no convincing etymology.

There are many other items which I should like to mention, but too much space has already been consumed. I must however list certain misprints, many of which come in the text and translation, and are therefore disturbing:

101, col. 2, line 17: place comma after *boves* instead of after *Grabovio*.
102.2.15: the Latin for **zeřef fetu** is lacking.
104.2.26: the second *auferto* should be *deligito*.
106.1.2: change dot after **perakne** to comma.
107.2.9: insert comma after *porricito*.
108.1.2–3: for **asnatess | evaknis**, read **asnates | sevaknis**.
109.1.8: delete comma after **perakne**.
111.2.10: change *ignis* to *ignem*.
113.2.17 from bottom: delete dot after *auctoratu*.
114.1.4: insert dot after **ukre** to agree with Latin version.
125.1.7 f.b.: insert *pihatu* before *ocre*.
127.1.4 f.b.: insert comma after *couertu*.
128.1.4: insert *cringatro hatu, destrame scapla anouihimu* before *pir* (the Latin version has the translation of this, but divides the paragraphs wrongly).
128.2.20: the Latin translation of *ape desua combifiansiust* is omitted.

129.2.6: the Latin for *com prinuatir* is omitted.
133.1.7 f.b.: change dot after *martier* to comma.
134.2.3: change *libamento* to *libamenti*.
136.1.7: change *atiersier* to *atiersir*.
136.2.9: insert *ita* before *uti*.
234, last line: change *duabus* to *singulis*.
300.7. f.b.: change *firmavissent* to *firmavisse*?
306.6 f.b.: change *praedito* to *praeditum*?
308.8 f.b.: change *praeminet* to *praeeminet*?
316.18: change DIKAMMO- to DIKAMNO-.
349.8: set dot after first FUTU.

Devoto's volume is by far the most important contribution to the study of the Iguvine Tables since Bücheler's Umbrica. Whether or not we agree with Devoto in the interpretation of individual words and phrases, we must hereafter use his book whenever we busy ourselves with these texts, and we must consider his views thoughtfully and at length before we reject them. To him must be paid a tribute of appreciation and gratitude.

ROLAND G. KENT

The Syntax of Castilian Prose: The Sixteenth Century. Pp. xxix + 750. By HAYWARD KENISTON. Chicago: The University of Chicago Press, 1938.

The reviewer of this learned book is confronted with two very discordant feelings: on the one hand the highest admiration for the amount of serious research and abnegation bestowed on a comprehensive work de longue haleine; and, on the other hand, bewilderment at the barren factual results, the absence of any attempt at synthesis, the complete omission of background. The valuable and carefully collected materials displayed in this ponderous volume are for the author sufficient unto themselves, and are in no way utilized to explain the facts: 'I have not tried to present a linguistic philosophy or to interpret the psychology of the Spanish people' (xxiii). It is well known that the rejecters of a 'philosophy' always have one, the philosophy of the rejection of philosophy, i.e. a positivistic philosophy; but our author has not even tried to link the facts. To give one example among hundreds: there is no comprehensive study giving the reason for the use of *á* before direct objects, though the frequency of *á* after certain verbs like *seguir*, *sobrepujar*, *vencer* (2.45) points to a very definite explanation.

This volume is intended to be the pattern for an extensive 'chronological [not historical] survey of the syntax of Castilian prose from the

earliest documents in the vernacular tongue to the present day' (xi), which will be divided into four parts (Middle Ages, Sixteenth Century, Modern Period, Contemporary Period). The decimal system of paragraphing—as in the New English Grammar by Jespersen—is to prevail also in the following volumes, so that the same syntactical construction will have the same paragraph number throughout the four volumes.

One may ask of Keniston: Why is Castilian[1] prose alone considered? If the development of prose is the chief concern of the author he should have delved into the esthetic, rhetorical, etc. principles as Schiaffini did for Italian. On the other hand, if he was interested in the development of language, why did he omit poetry, since obviously many popular features of the language are to be found only in poetry (buffoon scenes, etc.)? If I compare the article by Pla y Cárceles, La Evolucion del tratamiento 'Vuestra-Merced' (RFE 10.245), with the treatment of the polite pronoun of address in our volume, I find missing in Keniston the intermediate forms between *vuestra merced* and the modern *usted* which developed in the 16th century, but are attested only in the vernacular speech of *comedias* in verse. Will it be an accurate chronological survey of this development if perhaps some of the *voa(r)ced(es)*, *voasted*, *vosancé* examples (the first occurs, by the way, in Oudin's grammar written in French in 1597 which was excluded, probably because it does not occur in 'Castilian prose') are found in a later volume because they may be used as archaisms in 17th century prose? Keniston found (39.471) only one example of *re-bien*—in Lope de Rueda!—how many were there in poetry? The author is compelled to confess that 'the texts studied' show only object forms for the personal pronoun after *entre* (6.9) although Bello-Cuervo in note 123 showed the continuity of the construction with the nominative by a quotation from López de Ayala (*entre vos y yo*) and Lope (*entre Fortunio y yo*[2]).

I suppose the volume on the Middle Ages will be even more vitiated by this artificial elimination of poetry. There can be no doubt that medieval prose does not have the linguistic significance of poetry. Can Dante's prose vie with his poetry? Or Don Manuel with el Arcipreste de Hita? And if a great part of the prose works is merely a re-

[1] Can the epithet Castilian be maintained in the two volumes planned to cover the periods 1600 to 1900 and 1900 to the present? Even the Spanish Academy dict. has changed *lengua castellana* to . . . *española*! A. Alonso, Castellano, español, idioma nacional (1938), shows that the name as well as the concept of 'español' was already current in the 16th century.

[2] I do not see how in 2.113 the author can state that the personal pronoun with *entre* has the nominative form in examples like *entre nosotros tres aberiguemos eso*.

working of poetry (the chronicles), why admit the indirect poetry and exclude the direct? And is not the prose of the Celestina (or of the Elicia, included in our volume) highly influenced by poetry? Is it not perhaps more artificial poetry than the simple verses of some romances? Does not Cervantes' prose assume importance precisely because its artistic merits sum up and revive the tradition of a century of spoken language (e.g. the speeches of Sancho, which are also the culmination of the style tradition of the *gracioso*)? Minor prose artists, less inclined to the natural and the vital, did not include such language. Every great literary style is enriched by new affluents of spoken language which it introduces and translates into the written literature, as Thibaudet pointed out for Flaubert's prose. Thus the style of the Celestina is—in opposition to chronology—relatively 'older' than that of Cervantes because natural speech is mostly excluded. How fallacious is the acceptance at face value of mere factual dates! Does not Luis de León's dogmatic or didactic prose, not only from the esthetic point of view, but also as a linguistic monument, betray less of the 'real' Spanish of that time than his mystic lyricism? If the underlying (and wrong!) idea is that prose expression is more natural or genuine than poetry, precisely the reverse is true for the Middle Ages and the Renaissance. Does anything like 'syntax of prose' as opposed to 'syntax of poetry' exist? And furthermore, does anything like 'syntax of a century' exist? Of course I recognize that historians must manufacture such artificial units, but do they not dam up the river of history by subordinating their materials to spiritual entities like 'Middle Ages', 'the Waning of the Middle Ages', 'Renaissance'? Are the years 1500 and 1600 really decisive in Spanish prose? Are the Celestina (1499) and the Quijote (1605) to be excluded in this part? The consequence of this attitude is that, e.g., the use of the imperative as a narrative verbal form or an adverbial phrase does not appear in our volume.[3] Now it may be argued that the juxtaposition of several static pictures may produce history, but does Keniston give us such static pictures of an established syntactic system as Brunot and Damourette-Pichon did for modern French? Is there in Keniston's work a picture of Renaissance prose? One might suggest that by correlating the same paragraphs throughout the four volumes planned one would get a complete history of the various developments. But how, if there is no explanation of the main tendencies? I find it somewhat artificial to include the *vuesa merced* attested by Juan de Valdés while the *vuesasté* of Juan de Luna (1619), quoted

[3] E.g., *Toda la noche, daca el orinal, toma el orinal,* or *en dácame esas pajas.*

by Pla y Cárceles, will not appear until the next volume. The evolution is interrupted by the volume and paragraph system employed; the current of language is sectioned off into separate stagnant ponds. Similarly, the popular demonstrative pronouns *aquellotro, aquestotro* (and their verbal derivatives *aquellotrar*, etc.) are not listed (but *estotro* is) because they occur only in the versified autos, farsas, etc. published by Rouanet.

The distinction between stylistics and syntax as between individual 'behavior' and collective 'mores' seems theoretically correct, but practically syntactical studies must always draw their sap from the life of style. *Nihil est in syntaxi quod non fuerit in stylo*, I wrote some twenty years ago. There may be scholars more attracted by grammatically settled usages, and others, like myself, more friendly to the stylistic root of a construction, but we get no explanation of the grammatical facts if these two schools remain distant and self-sufficient. The treatment of 'stylistic phenomena' grouped in a short last chapter is by no means exhaustive of the really individual variations. The use of *á* before animated direct objects, or the shifts in the polite forms of the pronoun are quite as much a matter of stylistic variation, of individual choice, as the reference of a pronoun to a noun or adjective implied by some other word in the sentence (44.6) or the haplology; and conversely, the types *fue el buldero . . . el mayor echador dellas* [i.e. *bullas*] *que jamas yo vi* and *no ternás de* [*que*] *quejarte* are grammatical habits of the time. The chapter of Pfandl in Spanische Kultur des 16. u. 17. Jahrhunderts on the 'etiquette' of address, by grouping the phenomena under this heading, gives a clearer view of the choice of expression of people in the Spain of the Hapsburgs than do the barren statements in different paragraphs on *tú, vuestra merced, vos*, devoid of psychological link; but even so our author could not entirely dispense with stylistic facts when he states that *vuessa merced* 'is used at times by humble people with a note of more dignity', when (37.863) the fact is mentioned that 'Mendoza's elliptical style tends towards' certain constructions, and when (4.411) we read that 'in lofty literary style' as 'a convention in the plays of the first half of the 16th century' *tú* is used as the regular form of address. There is no justification for the unprecedented exclusion from a work on syntax of problems of word-order, either by virtue of its emotional character or because it is to be treated by a pupil (5); for after all word-order is closely related to the rhythm of the sentence and this rhythm to the psyche of the speaker, as was proved quite independently by Elise Richter, Bally, and Vossler. Knowledge about the use of personal

pronouns as subjects is incomplete if the types *ellos llegan* and *llegan ellos* are not distinguished: we find (5.31) *las cosas no se vienen ellas* on an equal footing with *las grandes maldades, Chrisothemis, ellas son vengadoras de si mesmas.* The fine distinction made by Harri Meier of the different rhythm in *ellos / llegan* and *llegan'ellos* (the first concentrating attention on persons acting, the second on the event), and the relationship of these two constructions to *llegan* without a pronoun have a place in syntax. Granted the emotive nature of phenomena of word-order, does the unconscious philosophy of language of our author tend toward the identification of language with unemotiveness? Does not emotional speech also have its conventionalized grammar, just as it has its sounds, arbitrary in every language, but settled by convention? The German child screams *au*! when the French screams *aïe*! and the American *ouch*! The word-order in German *Lausbub, dreckiger*! is an unusual one, but conventional in scolding. In 5.3 we read that 'a subject pronoun is used emphatically when the subject of the verb is expressed by a noun or by another pronoun'. Does this 'emphatic' speech, which was found worthy of inclusion in the grammar, not have an emotional origin? Now Keniston tells us that only in the position of object pronouns could he find an unemotive and unindividual factor: such statements are of themselves and on principle impossible. If there is a grammaticalization of the word-order in objective pronouns, there must be a similar tendency, perhaps less conspicuous, in the subjective pronouns; further, if word-order generally obeys the dictates of emotion, it does so also in the case of objective pronouns. Thus interesting problems like hyperbaton (*con dilatados truenos y recios*, Solórzano, 1642; even in the 19th century Pereda: *mustios iban y cabizbajos*) and similar phenomena in Old Castilian, pointed out by Anfós Par, ZRPh 45.83, are excluded from Keniston's book.

The author has eliminated consideration of the sentence as such, generally understood to be the main object of syntax ('Satzlehre', a term E. Herzog replaced with the broader one 'Gruppenlehre'): Keniston's work is in its way a morphology ('Formenlehre'), the chapters being devoted to the substantive, adjective, verbs, adverbs, negation, prepositions, conjunctions, interjections—the real syntax, 'the lore of the grouped phenomena', is missing: this explains why the 'nominal clause' does not appear, and why causal, hypothetical, etc. sentences are treated here and there implicitly, under conjunctions for example, whereas Lerch in his Historische französische Syntax devoted one whole volume to the conjunctions, another to the incidental clauses; there is no treat-

ment of direct, indirect speech and 'style indirect libre' (only of tenses in indirect speech). The lexicological prevails over the syntactical to a greater extent than in any book on syntax thus far written. Keniston's is a perfect example of what John Ries long ago called 'Mischsyntax'. Even phonetic variants of a single expression are listed separately in alphabetical order: *nadi, nadie, naide* (40.65), *pardiez, par Dios, por Dios* (43.34), and expressions that can be explained only in a certain context are separately listed: ibid.: *maldita la otra cosa* beside *maldito aquél*—but the first expression occurs in a sentence: *con no haver en la dicha camara . . . maldita la otra cosa que las cebollas . . .* 'nothing but', *maldita cosa* signifies 'nothing' and *otra . . . que* 'but'. And in 40.61 we find under the schematic heading *maldito el* examples like *maldita la gota se perdia*, separated from 40.65 *maldito aquél* (ex. *¿Vos pensáis que los soldados hazen diferencia de[l] viernes al domingo? Maldita aquella*). One must consult the index to link these expressions. The grammar should not only offer the conveniences of a dictionary (alphabetical order), but should explain, interpret, and open vast horizons.

The author has intentionally eliminated bibliographical indications of earlier discussions and interpretations of the several phenomena and plans to print them separately. I think this procedure, though in harmony with his parsimonious explanation of the facts, will unduly increase the size of the work: by such an arrangement a complete Spanish syntax, a most urgent desideratum, would require four volumes on Castilian prose plus at least four volumes on Castilian poetry plus at least one volume on dialect syntax plus one or two with discussions of the phenomena—an enormous waste of space and work and totally uncoördinated. Haas, Bally, Brunot, and Damourette-Pichon could dispense with the scholarly 'literature' on their subject because (1) this 'literature' had been gathered and discussed in historical studies on French syntax, (2) they gave a description of a static system of language. Neither reason is available to Keniston. I cannot see how a bibliographical appendix will help to explain the materials of this book nor how it will add substantially to our knowledge of Spanish syntax. In fact, the word 'syntax' in Keniston's title is a formidable misnomer. A more nearly correct title would be "Statistical Data on Syntax". This work is not a historical, nor, as the author more modestly puts it, a chronological survey of syntax; it is not a 'précis de syntaxe'. Problems of syntax are for Keniston problems of ratio and proportion, but counting is not explaining. This novel of Spanish syntax has no hero, yet

his deeds are elaborately enumerated; the hero, the Spanish people, is encased, imponderable, in a vacuum.

It would be unfair to the author not to consider his 'statistical analysis of grammatical usage', wherein lies 'the importance of the project'. The determination of frequencies in linguistic research is not entirely new. The Swedish school makes wide use of percentage calculations, as does Foulet in his Petite syntaxe de l'ancien français and Marguerite Durand in her book on the disappearance of the feminine in contemporary French—as early as 1898 F. W. Kaeding wrote his Häufigkeitswörterbuch der deutschen Sprache—but as a consistent and systematic approach to syntax the statistical attempt here is new.[4] The author defines the relative frequency of the constructions by two numbers at the end of each paragraph, e.g. 5–9 means that the construction was found in five of the thirty texts (of which 10,000 words were examined), with a total frequency of nine examples. Constructions which were of such assuredly high frequency that they were not counted are indicated by an asterisk. Constructions which were found only in the additional material (of ten books in which the phenomena of low frequency were studied especially) are indicated by a plus sign. The examples found for rare constructions in forty completely studied books are almost all listed. Thus in the paragraph on the subject pronoun *tú* used after prepositions one finds the statistical information (1–1) as to range and frequency of the construction while 7 examples are listed. The proportion 4–4 to 13–20 of a direct object referring to place-names without and with *á* is instructive. It is of course important to learn that the type ¡*amarga de mí*! (25.563) is much more frequent than ¡*yo triste*! (5.67): the figures are, respectively, 6–15 and 1–1. However, I cannot conceal my inborn distrust of statistics which always seem to me numerical metaphors of the statistician's statement: a tautology is involved in all this grouping. When I compare paragraphs 25.446 (a substantivized adjective followed by *de* + a substantive) with 3.521 (a phrase with *de* + a noun replaces a noun in apposition), the figures (6–16 versus 4–10) seem to indicate a notable difference in frequency. In the first paragraph the types *el traydor de Alanio, el traidor del mesonero, el diablo del toro* are quoted, in the second only the first two types. It would appear that the third type, far from increasing the instances, reduces their number! How is that possible? When I add to these paragraphs 3.523 (the type ¡*peccador de mí*!) with the figures 1–1 and

[4] [It would seem to be forgotten at the Hopkins that statistical investigations of syntactic problems were a common occurrence there some fifty years ago. See the early volumes of the AJP and dissertations of the time. GMB]

25.563 (¡*o triste del cortesano*! ¡*amarga de mí*!) with the figures 6–15, nearly equal to those of 25.446, I am puzzled by the meager result for ¡*peccador de mi*! The real problem is to determine the relative frequency of the types *el traydor de Alanio* and *el traidor del mesonero* on the one hand, of ¡*peccador de mi*! and ¡*o triste del cortesano*! on the other. The excellent study of Alf Lombard on Fr. *ma lasse d'âme* could have been consulted for the grouping of the various types. When I read (44.5) that a word used with different meanings is a current phenomenon (marked *), e.g. *comenzó a marchar la compañia sin parar hasta que nos acercamos a la costa y el señor capitan a la mia*, and in the following paragraph that the case of a pronoun referring to an unexpressed noun, is to be found only in the proportion 1–1 (e.g. *fue un buldero . . . el mayor echador dellas* [sc. *bullas*] *que jamas yo vi*), I must express my distrust because the first use is possible only to a speaker who is accustomed to link words on an etymological or pseudo-etymological basis. As the punning attitude is a special case of the etymological instinct, the second construction must, of necessity, be more frequent than the first, and the perspicacity of the observer has not been commensurate with the phenomenon. In fact, the *Poema del Cid* already shows relatively more examples of the first case, cf. Menéndez Pidal, 'Cruz y raya' 39, 1934, and my remarks in Vox romanica 2.201. *Hombre, el hombre; servidor, el hijo de mi padre* are said to be found 1–1 as representatives of the first person, *mi persona* 2–2, but, notes the author, in the latter case the equivalence is not complete, since *mi persona* preserves its connotation of the physical person. But are *hombre* and *el hombre* real equivalents for the pronoun? Is the idea of a generic pronoun (= *uno*) totally absent from the context? Should not the type of statement *a* pro *b* be energetically eliminated from our syntactical terminology?[5] No construction substitutes precisely for another (cf. 543

[5] For example in 39.52 the adverbs are said to be used 'as (with the force of) adjectives' (*crei que cierto fuesse así*). This wording implies that the grammarian expects adjectives; the grammarian's problem, however, is to explain the adverb —which is an *adverb*. It is similarly incorrect to say that 'an adverb is found as an attributive modifier of a noun' in *este juntamente Dios y hombre*: one must take the adverb as such, as belonging not to *Dios y hombre*, but to the whole sentence, and as condensing the sentence: *este* [*ser que es*] *juntamente Dios y hombre* or: *este—juntamente* [¡*fíjese bien*!]—*Dios y hombre*. The author is too much inclined to take for granted that parallel constructions are identical: thus in 44.34 he sees only a slight difference between subjunctive and conditional in parallel clauses. But since the conditional occurs always in the second sentence and since there is a parallel use of indicative and subjunctive with the latter in the second place, there must be a difference in the idea behind the uses. Sorrento (Arch. rom. 11.232) has pointed this out for French and Italian.

'infinitive used INSTEAD of a finite verb', etc.). Furthermore the statistics of our author are sometimes meaningless when compared with those of other scholars, because they are incommensurable: Keniston finds 3–14 as opposed to 1–1 examples of the historical infinitive without *á* versus the infinitive with *á*, the only examples of the latter being found in Teresa de Jesús' 'Vida': Alf Lombard in his book on the Romance historical infinitive gives 4–5 examples from this same book of Teresa's (one of them not quite certain) of the construction with *á* and only 1 without, whereas again Keniston gives 2 examples of the construction without *á* from the same 'Vida', not listed by Lombard. The conclusion to be drawn is that a new and complete registration is necessary. Sometimes the grouping is not arbitrary, but there are several subsections with greater or lesser frequency, the omission of one of which may alter the picture considerably: under 3.31 ('cognate accusative'; by the way, a stylistic phenomenon) are listed the cases *morir mala muerte* (*vivir vida segura*), *las burlas que burlava de mi*, *yr camino*, *dormir el sueño*, *pedir peticiones* with a total of 5–7, whereas the case *van camino de perfeccion*, obviously the same, is listed in another paragraph (41.32) with 6–6. Now the cases *pedir peticiones* and *burlar burlas* may be assumed to be rarer than the others, and the picture would appear different if the examples of *yr camino* were grouped with *yr camino de*. In 39.473 among the cases of a repeated adverb (*paso paso; a poco a poco* is listed also the following construction: 'a conjunction may connect the repeated adverb with the first adverb '(*assi que assi*)—but this is an improper wording for the phenomenon of two adverbs meaning different possibilities in reality 'this way or the other' (cf. German *sowieso* and cases like *della y della parte* in the Poema del Cid: '*de una y otra parte*'). There is no really repeated adverb, and the conjunction is not pleonastic; by excluding such cases the results of the statistical investigation are to a certain extent invalidated. In 43.34 the two cases of asymmetry, indicative and subjunctive and subjunctive and conditional used in parallel sentences, are under one heading. But the second case is probably rarer than the first, which is a feature of general Romance syntax; the statistical figure given at the end of the paragraph (7–10) is arbitrary. Shall we really believe that *estar* in the meaning of position (*el casero ste a la puerta*) and in the meaning of state (with adjectives: *estar alegre*) are both to be found in 30 books, in the proportion 555:282? But the example of the latter construction *me consiente que ninguna falsedad esté oculta* could as well be an example of the meaning 'to stay' (*estuvimos deste manera cinco meses*) which occurs only in 6 books 19

times. The false assumption of such clear-cut distinctions makes statistics illusory. I wonder that Keniston did not question his figures.

Now if we had to summarize all the constructions treated in this book in a comprehensive list of frequency, we should be wrong in assuming that the order obtained in this way (from the constructions with a maximum range and frequency, let us say: 30–273 down to those labeled 1–1) could give us a true picture of the relative frequency of *all* the Spanish constructions—which should be the case if statistics were as easily possible in syntax as they are in phonetics for establishing the frequency of certain vowels and consonants. Particularly insignificant would be the comparison of the 1–1 constructions. I quote from the first paragraphs the subject introduced by *entre*; *a* omitted in comparisons with *como*, comparisons with *de* + *a*, exceptions with *sino* + *á*, singular after impersonal *haber*, a noun in the superlative modified by an adverb of degree, etc. One knows that reliable results in the calculation of probabilities can only be reached from 5 or 6 up. And what inference can we draw from the comparison, for example, of a phenomenon found in 15 books 30 times with one found in 30 books 15 times (the first case is probably of greater significance than the second)? The stylistic intention of one author will shed more light on the syntax of a construction than will the clichés of a hundred. I remember the ironical criticism to which I exposed myself in a juvenile paper in which I listed pages and pages of literary examples for the various shades of the modern Italian *così* only to have Vossler point out a single passage from Pascoli's lyrics which fused and telescoped all my carefully separated 'nuances'—again another argument against the 'pure' grammarian's animosity towards poetry. A further objection is that the statement 'the phenomenon x occurs 30 times in 15 books' without further classification is misleading and fallacious; there should be an indication of the different *genres* of prose writings in which the construction was found: official documents, private letters, historical works, belles lettres, etc. For instance the excellent monograph of Karin Ringenson 'Dies et diurnum' in Studia neophilologica 10 has shown by a statistically individualizing method that the word *jorn* occurs in Provençal poetry under French influence, and later in Provençal prose under troubadour influence, but that the popular word *dia* (used in prose) is employed in poetry in formulae (*noch e dia*) or in rhyme. This case shows the fluctuations between poetry and prose; there are *genres* closer to poetry than others. By eliminating poetry we losé one of the coördinates. Syntax and stylistics must of necessity cul-

minate in history of literature. A 'Syntax of Castilian Prose' (if such a thing exists) can not be divorced from the history of prose writing and the generic figures should be split up into figures based on genres.[6]

What inference is to be drawn from the absence of a construction in our book? As far as I can see the nominal clause (without finite verb or substantivized infinitive) does not occur—I suppose because the collections of proverbs were intentionally omitted, 'as being almost timeless'. But since proverbs do not belong exclusively to one period, do they not belong to every period, is the 'proverbial mood' not a feature of every speaking community? A distortion results from this exclusion of the average standard of a period. And is the question as to the extent of the omission of the verb in proverbs in the different epochs of Spanish prose not a very important stylistic and syntactic problem? And why are the cases of the missing verb not listed together (cf. *dónde bueno*? 14.721)? "Proverb style" betrays a certain propensity for style patterns (cf. Huyzinga, *The Waning of the Middle Ages*). Juan de Valdés saw in the refranes the sound patterns of 16th century oral speech.

I have in vain tried to determine to what public Keniston has addressed his book. He tells us that he started in 1920 with a study of the range of words used in contemporary Spanish and that the American Council of Education in 1924 added new impetus to his investigation. But, if I am not mistaken, the statistical method used for determining the 'basic language x' is a pedagogical service intended to make the teaching of modern languages easier and more fruitful. What benefits obtain from the research of 'basicalities' in older periods of language and in artistic prose? The educationist must avoid teaching the historical grammar of any language, and conversely the historian of language should avoid invading the field of pedagogy.

The real importance of Mr. Keniston's tome lies in the richness of new

[6] The author tells us with his exemplary modesty (xxiv) that some of his Spanish friends expressed the judgment that 'such a work as this is well-nigh futile, since every Spaniard is an individual and therefore every sentence must be studied as a unique whole, comprehensible only in its context'. I agree with these judicious advisers in so far as I think the grammatical attitude takes away the very life from the flowers of speech as the Linnéan classification of plants, to which Keniston refers, withers its subjects. Not only is a botanical attitude imaginable, but also a biological one, and we had, I believe, all too many botanists among our linguists—many phenomena of the so-called historical grammar listed in Meyer-Lübke and Tobler are stylistic facts. Why allow these scattered children to wander about like homeless orphans instead of bringing them together around the warm hearth of stylistics?

examples, of ways of speech, of observations on the use of constructions, and not in the statistical statements. The book can be used as a control together with other syntaxes, not as *the* syntax of the period in question. I cannot help but feel that a syntactical study which explains syntax is more valuable than one which tabulates. It seems to me less important to know how often this or that variant of the negative construction with *maldito* is used than to understand this peculiarly Spanish, very 'Catholic', and thus culturally relevant expression. In our present state of linguistic achievement we need less vast collections of facts than a subtler and more sophisticated approach to the facts themselves. We must realize that the 'facts' in linguistics as in history and the humanities in general are already interpretations. When we collect the facts of the 'French Revolution', we dogmatically assume that a certain period in French history may not only be called such, but, furthermore, is to be seen from this point of view. Real progress will be made when in trying to understand as much as possible of this period we shed our old and traditional opinions and perspectives concerning it. In the same way a syntax which deals with subjunctive or indicative as established units cannot make progress. Our real job is to explain exactly the use of certain expressions and not simply to label them. We try to count phenomena not yet understood—squaring the circle—like counting the number of religious people in a country without knowing the specific religious attitude. Furthermore, I think it characteristic of Romance philology that, precisely because of our broad knowledge of Romance languages and Latin, it leads to a greater understanding of the features of human language in general. It is significant that in the first edition of Meyer-Lübke's Einführung the 'biological' problems, i.e. the general tendencies of human language exemplified by Romance material, take up less space than the 'palaeontological', i.e. the reconstruction of a very definite kind of pre-Romance. The later editions augmented and amplified the first part, and recent works of the same type, if they were to be written by men like Schuchardt, Gilliéron, Bally, Vossler, Jespersen, or Sapir would emphasize still more the biological part. I cannot help thinking regretfully of the time when the greatest of American linguists, Whitney, blamed a mechanical theory of Brugmann (the explanation of Indo-European gender in nouns by mere accidental analogy in the various Indo-European languages) for its tremendous 'devitalization' of human language in general, while today I must accuse Keniston of devitalizing syntax by the extremely mechanical method he employs. The historical grammars of French for a long

time did not reveal anything of the actual people who speak it, their character, psychology, history, culture, literature, art, thought—they might just as well have been grammars of Chinese. In Keniston's book similarly we see nothing of the Spain of Saint Teresa, Luis de Leon, Lazarillo de Tormes, Jorge de Montemayor, Mateo Aleman, Juan de Valdes, Perez de Hita. Their names appear often, only to vanish in the quicksand of numbered paragraphs and statistical figures. Such 'inhumanity' the scholar cannot afford to display. For it seems to me essentially 'inhuman', this attitude of despair not justified by the present state of linguistics, to escape from the living into the agnosticism of statistical research. If Pascal rejoices in finding un homme where he had expected to find a writer, I may be allowed to expect to find behind the grammarian a human being in all his universality. To consider language only as something measurable or calculable seems to me one-sided and inadequate to the very nature of human language. I am traditionalist enough to think that the old mythologems ('Geist', 'Kultur', etc. replacing 'God', etc.) of our Romantic initiators of philology (Herder, Grimm, the Schlegels) are not yet useless: even so skeptical a mind as a statistician's must admit such mythological entities as Castilian ('language'), 16th century ('period'), prose ('stylistic forms'), and history ('history of Spanish prose'). Why then stop halfway and believe in these mythologems and not in Geist and Kultur?

While this review is very skeptical and polemical, I want to make clear my deep respect for Keniston's learning and industry and my regret at being compelled to oppose in a few pages the result of years of patient research.

LEO SPITZER

NOTES AND PERSONALIA

PROFESSOR JACOB WACKERNAGEL, an honorary member of the LINGUISTIC SOCIETY died on May 22, in the eighty-fifth year of his age.

The announcement will bring grief to those who had the privilege of a personal acquaintance with his kind, lovable nature, and a sense of great loss to all interested in philology or in linguistics.

He was a leader in the days when there were giants in the world, and to sketch his life would be to follow the progress of our science from the time of the Junggrammatiker to the present. So much cannot be attempted here. Restriction must be made to the bare mention of three of his works, each incomparable in its kind, anyone of which would have sufficed to render a lesser man famous: the Altindische Grammatik, the Untersuchungen zur homerischen Sprache, and the Vorlesungen über Syntax. Nor can too high praise be given to his briefer writings, a list of which was published in ΑΝΤΙΔΩΡΟΝ, Festschrift Jacob Wackernagel 354–61 (1923). Easy command of a vast material, keenness of insight, clearness of expression, are all in evidence in them; and besides a thoroughness of workmanship and directness of purpose that insists on the relevant while discarding the irrelevant.

A complete republication of these widely scattered writings is earnestly to be desired. There is in them of course some part that is no longer tenable, much more that has passed into the fund of common knowledge, and much that can still be directly instructive. To cite but one item: the Armenian peculiarity of using the augment only in forms that would otherwise be monosyllabic cannot be understood until seen in the light of the wealth of material presented in his article, Wortumfang und Wortform (GöttNachr. 1906.2.147–84). Such gains apart, generations of scholars may with profit read and reread these articles to see how a philologian and a linguist should work.

G. M. B.

LIFE MEMBERSHIP in the LINGUISTIC SOCIETY may, under the provisions of the Constitution, be secured by any Active Member by payment at one time, in addition to the dues of the current year, of the sum of One Hundred Dollars, less half the amount already paid in

annual dues. Thus a member who joined the Society in its first year, 1925, and has paid the dues of 1938, has paid $70 in annual dues, and may in 1938 secure Life Membership by the payment of $100 less $35, or $65.

Certain members have suggested that more would take Life Memberships if it were possible to pay the fee in installments. Although the Constitution does not contemplate such divided payments, the Treasurer is prepared to carry into effect the following arrangement:

1. With the payment of the annual dues or at any later time in the year, a member may notify the Treasurer of his desire to become a Life Member, by sending (in addition to or apart from the annual dues) the sum of $20 or a greater sum which is a multiple of $5.

2. The Treasurer will hold this sum to the credit of the member, adding to it such further sums of $20 or more as the member may thereafter send.

3. The Treasurer will notify the member when the remaining payment amounts to less than $20.

4. When the necessary sum for Life Membership is paid in, the Treasurer will transfer the sum to the Endowment Fund.

5. If the member should fail to pay his annual dues before December 1 of any year, the dues of the year will be charged against the sum already on deposit toward Life Membership.

6. If the member's membership should be terminated by resignation or by death or by disappearance without leaving his new address, the sum on deposit toward Life Membership will be placed in the Endowment Fund of the Society, no part of it being returnable to the member or to his legal representatives.

THE COMMITTEE ON NOMINATION OF OFFICERS FOR 1939 consists of Prof. H. H. Bender, Princeton University, Chairman; Prof. E. A. Speiser, University of Pennsylvania; Prof. Hans Kurath, Brown University. According to the plan outlined in LANGUAGE 6.267–8, members of the Society may send to the Chairman of the Committee their suggestions for nominations for the various offices; these should reach him not later than October 5, in order to receive due consideration.

THE FIRST INTERNATIONAL CONGRESS FOR GREEK AND LATIN EPIGRAPHY is being held at Amsterdam, August 31 to September 4, and the TWENTIETH INTERNATIONAL CONGRESS OF ORIENTALISTS at Brussels, September 5–10. We regret that notices of these meetings did not reach the Society's office in time for mention in the preceding issue of LANGUAGE.

THE CELTIC DIGEST is a new journal, to be issued monthly except during the summer, by the Celtic Society of Columbia University, with Ben Greenwald as Editor. The first issue is dated March, 1938, and contains four large two-column pages, about one page being occupied by the first installment of a story in Irish, taken down by Douglas Hyde (recently elected President of Ireland) from an old man who had received it from his grandfather. Subscription price, $1.00 a year; address, the Celtic Society, Philosophy Hall, Columbia University, New York City.

THE CELTIC FORUM, published at 1 Welland Avenue, Toronto (Canada), has also called itself to our attention by the sending of Vol. 1, No. 3, issued in June, 1935. The issue consists of 32 pages, of which about 4 pages are in Irish. It is scheduled to appear quarterly, beginning with November 30; the price is $1.25 a year in the United States. The Editor is W. J. Edmondston Scott.

GEORGE HERZOG has been appointed Assistant Professor of Anthropology at Columbia University, lecturing on Primitive Languages and Music.

THE FOLLOWING NEW MEMBERS FOR 1938 were received into the LINGUISTIC SOCIETY subsequent to the last published list, and up to May 31, 1938:

Joseph H. D. Allen, Jr., M.A., Assistant Instructor in Romanic Languages, University of Pennsylvania, Philadelphia, Pa.

Denzel R. Carr, Ph.D., Assistant Professor of Linguistics, University of Hawaii, Honolulu, Hawaii; Slavic and Far Eastern languages.

Henry Hare Carter, Ph.D., Instructor in Romance Languages, Northwestern University, Evanston, Ill.

Anna Frieda Gamper, Ph.D., Professor of German, MacMurray College, Jacksonville, Ill.

Curt Rudolf Goedsche, Ph.D., Instructor in German, Northwestern University; Fisk Hall 207, Evanston, Ill.

Richard C. Ledgerwood, Ph.D., Research Assistant in Psychology, Ohio State University, Columbus, Ohio.

Joseph Mersand, Ph.D., Instructor in English, Boys High School; 284 Montauk Ave., Brooklyn, N. Y.

Edward DeLos Myers, Assistant Professor of Linguistics, Trinity College, Hartford, Conn.

John Knight Shryock, Ph.D., 4509 Regent St., Philadelphia, Pa.; Chinese.

BOOKS RECEIVED

Under this heading will be acknowledged such works as seem to bear on the advancement of the scientific study of language.

The publicity thus given is regarded as a full return for the presentation of the work. Under no circumstances is it possible to comply with the requests being made by certain publishers for the return of books not reviewed quickly.

Reviews will be published as circumstances permit. Copies of them will be sent to the publishers of the works reviewed.

For further bibliographic information consult the annual list of Exchanges.

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THE PREHISTORY OF INDO-EUROPEAN *ā*-STEMS

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The 'laryngeal' hypothesis poses a problem concerning the Indo-European *ā*-stem nouns and verbs,[1] for IE *ā* may result from the loss either of a following *x* or of a following *ə*. In the present state of our knowledge a decision between the two possibilities can be reached only on the basis of Hittite evidence; IH *x* appears in Hittite as *ḫ*, usually written double between vowels, while IH *ə* is lost, or at least not written in Hittite. In the case before us the Hittite evidence makes it necessary to assume IH *x* in some words and *ə* in others. The Indo-European *ā*-stems must therefore be divided into two originally distinct classes.

1. Indo-Hittite *ax*-stems

It was the fact that the Indo-European suffixes *-ā-* and *-āye/o-* frequently make denominative verbs from nouns and adjectives other than *ā*-stems that enabled Kurylowicz[2] to identify with them the Hittite verbs in *-ahh-*. In the five lines devoted to this important discovery Kurylowicz mentioned only the verb *idalawwahmi* 'I injure' beside the adjective *idalus* 'bad' and another pair of words that have since been reinterpreted and completely separated from each other. The identification of the Hittite suffix *-ahh-* with Indo-European *-ā-* has, however, been confirmed by much additional material, including *newahh-* 'renew' beside *newas* 'new', which corresponds, sound for sound, with Gk. νεᾶν beside νέος Lat. *novāre* beside *novus*, and OHG *niuwōn* beside *niuwi*.[3]

Hitherto no one has reported anything in Hittite like Gk. τιμή beside τιμᾶν and Lat. *cēna* beside *cēnāre*, but I can now point to two case forms of nouns in *-ahh(a)-* beside verbs in *-ahh-*. The verb *alwanzahh-* 'bewitch' must contain suffix *-ahh-* since we have also *alwanzata(r)*,

[1] The abbreviations used in this paper are those listed in my Hittite Grammar and in the second edition of my Hittite Glossary.

[2] Symb. Gramm. 102 (1927).

[3] Sturtevant, Lang. 7.121.

alwanzessar 'witchcraft', and *alwanzenas*[4] 'bewitched'. An ablative *alwanzahhaz* 'from witchcraft' occurs in VBoT 120.3.4.

The verb *man(n)inkuwahh-* 'make short, approach' stands beside *maninkwanza* 'short, near' and *maninkwess-* 'become short'. These words clearly imply an adjective **maninkus* or **maninkuwas* 'short, near', from which each of them is derived.[5] It is now possible to add a related noun in *-ahh(a)-*, which occurs in KUB 24.9.2.17 f.: SAL ŠU.GI *ša-ra-a da-a-i* 5 NINDA.ḪI.A 1 DUG*KU-KU-UB* KAŠ GIŠGAG GIŠ*kar-ša-ni-*[*ya-aš*][6] (18) *na-aš a-ra-aḫ-za pa-iz-zi ma-ni-in-ku-wa-aḫ-ḫi-iz te-e-kán be-da-a-i*, 'the old woman picks up five loaves, one pitcher of beer, (and) a stick of *karsani*-wood; she goes out (and) in the neighborhood digs herself a hole in the ground.' The new word may be considered an ablatival adverb (with instrumental vocalism—cf. *kez*, *apez*), or better, a dative of place where with the reflexive enclitic *-z*.

Both abl. *alwanzahhaz* and dat. *maninkuwahhi* imply stems ending in *-ahh-* or in *-ahha-*. It is safer to assume *-ahha-* until indubitable stems in *-ahh-* are found,[7] especially because Hittite has few masculine consonant stems except those in *-nt-*. Furthermore there are nouns in *-hha-*, and at least one of these seems to contain a suffix *-(a)hha-*. The noun *senahhas* 'ambuscade' can scarcely be separated altogether from *senas* 'figure (made of clay, wax, tallow, dough, or wool)'. These figures are mentioned repeatedly in ritual texts, and it is clear that they serve as symbolic representations of persons;[8] the primary meaning may perfectly well be 'substitute'. The military word *senahhas* may also mean primarily 'substitute, reserve (to take over the fighting at a given moment)'. If the two words belong together the suffix probably has the same force as in *alwanzahhaz* and *maninkuwahhi*; *senahhas* was probably at first a verbal noun meaning 'substitution'.

[4] Goetze will treat this word in his forthcoming monograph on the ritual of Tunnawi.

[5] Cf. HG 158 f., 231 f., 243.

[6] For the supplement, cf. KBo. 4.2.1.39, 3.33; where it appears that *karsani*-wood is used for making soap.

[7] So far the only known stem in *-h-* is *iskaruh* (nom.-acc. *iš-qa-ru-uḫ*, *iš-ga-ru-uḫ*, dat. *iš-qa-ru-ḫi*, *iš-ga-ru-ḫi*, inst. *iš-qa-ru-ḫi-it*, *iš-ga-ru-ḫi-it*), a vessel into which wine is poured. It may possibly be a vessel with a pointed bottom, to be thrust into the ground, and, if so, it may be derived from *iskar-* 'insert, stick in', on which see Güterbock, ZA NF 10.61. Both the intervening *u* and the consistent writing with single *ḫ* in more than thirty intervocalic occurrences are against finding our suffix *-hh-* in it.

[8] See Friedrich, ZA NF 5.43 fn. 3; Götze, NBr. 77 fn. 3; Güterbock, ZA NF 10.81 fn. 3; Cf. also Goetze, Tunnawi.

The same suffix seems to occur in *menahhanda* 'opposite, against'. I have suggested[9] that *appanda* 'behind, afterwards' and *kattanda* 'down, in' consist of the adverbs (originally acc. sg. of neuter nouns) *appan* and *kattan* with a postposition that differs only in ablaut from Homeric -δε 'to' (cf. Lat. *endo, indu*). To the same group belongs *menahhanda*, and so we may abstract an acc. sg. **menahhan* beside *meni* 'face'. If we assume a stem **mena-* meaning either 'face' or better 'facing, opposite', then **menahha-* was in form a close parallel to *senahhas*. Its primary meaning may have been 'oppositio'.

The tendency of Hittite to extend the *a*-declension (corresponding to the Indo-European *o*-declension) at the expense of the consonant stems[10] relieves us of any necessity to assume a suffix *-axo-* for Indo-Hittite, and it would scarcely be possible to derive Indo-European *ā*-stems from such a source. On the other hand an Indo-Hittite declension in *-ax*, *-axm*, *-axos*, *-axai*,[11] etc. would yield the Indo-European forms commonly assumed. Only the nominative, accusative, and vocative call for comment. In the nominative we might expect either final *-s* (φύλαξ) or lengthened grade (ῥήτωρ) or both (*pēs*). Since we have no proof of any such modification of the bare stem, it is natural to think of a neuter nominative, and this would harmonize with the facts: (1) that the *ax*-nouns beside *ax*-verbs are apparently action nouns (pp. 239–40), and (2) that their stem is identical with that of the *ax*-verbs. Whether the transfer to the personal gender evidenced by the accusative (Hitt. *-ahhan* = IE *-ām*) occurred in Indo-Hittite times or separately in pre-Indo-European and pre-Hittite is not clear.[11a] The Indo-European nominative and vocative forms can readily be derived from a final *-ax*. This would yield IE *-ā* before an initial consonant and *-ă* before an

[9] Mélanges Boisacq, vol. 2, to appear soon.

[10] Goetze, Mél. Pedersen 488–95.

[11] With the ending *-ai* from *-ei* on account of the preceding *x*.

[11a] Since I wrote the above it has occurred to me that *maḫḫan* 'when, as, in the meantime' is perhaps the accusative of a noun with suffix *-ḫḫ-* from the stem that occurs in *man* 'how? (see Sommer, Die hethitisch-akkadische Bilingue des Ḫattušili I 75f.), how, when, as' and in *masis*, *masiwanza*, *masiyanza* 'quantus?, quantus'. Hittite clearly had a second interrogative-indefinite-relative stem, *ma-*. If we may identify *man* with the *o*-grade form of Gk. μέν and *maḫḫan* with Gk. μήν, we have at least a reasonable explanation of the vocalism of Dor. μᾶν. IH *me-* with noun suffix *-x-* would yield an accusative *máxm̥* (Hitt. *maḫḫan*), and we may assume that before an initial vowel this would take the form *máxm*, whence Gk. μᾶν. If this etymology is correct, the transfer from neuter to personal gender belongs to Indo-Hittite. Compare now Pedersen, Hittitisch und die anderen indo-europäischen Sprachen 67–72.

initial vowel. Homeric Greek preserves both endings in nominative function as well as in vocative function (νεφεληλερέτα, ἱππηλάτα, etc.). Later Greek and also Church Slavic (*ženo* 'woman') and Umbrian (if *Tursa* and the like were actually confined to that usage) show a secondary specialization of the shorter forms as vocatives. It is no longer necessary to ascribe the Latin short *a* in the nominative solely to the iambic law; Umbrian shows that primitive Italic retained both *-ā* and *-ă*.

In at least one word Hittite seems to present a noun suffix *-hha-* without a preceding vowel. In two passages of the code (§65 and §119) we find an acc. sg. *an-na-nu-uḫ-ḫa-an* designating a domestic animal of relatively high value. Walther (HC) plausibly connected the word with the causative *annanu-* 'cause to perform, train' and tentatively translated it 'trained'. Since the suffix *-ahha-* is otherwise known only in substantives, we must assume that this is one of the rather numerous adjectives developed from the descriptive genitive.[12] The original type of phrase must have contained *annanuhhas*, gen. sg. of a noun meaning 'training' or the like, and an animal 'of training' may have been either a 'trained' animal or an animal used to train others.

At any rate *annanuhhas* beside *annanu-* suggests that the suffix may originally have had an initial consonant. Now *senahhas* beside *senas* and also the Hittite verbs in *-ahh-* derived from *a*-stem adjectives easily admit a similar analysis; *newahh-* 'renew' is more naturally taken as *newa-* + *-hh-* than as *new-* + *-ahh-*. We must translate this into Indo-Hittite as **newa-x-* from pre-Indo-Hittite **newe-x*, since **newo-x-* would have remained unchanged.[13] Similarly we can most easily derive the Indo-European *ā*-stems beside *o*-stems (τομή : τομός) from the stem-form in *-e-* and suffix *-x-*. We thus find a solution for the old riddle of the loss of the stem vowel before suffixal *-ā-*. The Hittite suffix *-ahh-* (*nakkiyahh-* : *nakkis*) and also IE *-ā-* appended to other than *o*-stems (Lat. *iūdicāre* : *iūdex*) got their initial vowel in the same way as Lat. *-ānus* for *-nus* (*Rōmā-nus* but *urb-ānus*).[14]

[12] On the latter see HG 166 and references. Götze, AOr. 2.160 f., has called attention to an adjective of this origin. Cf. also Sturtevant, HG 153.

[13] Sturtevant, LANG. 14.107 f.

[14] Just as Hittite *-ahhas* corresponds with IE *-ā-* < IH *-ax-*, Hitt. *-uhhas* implies IE *-ū-* from IH *-ux-*, and it is noteworthy that Skt. *tanūṣ* 'body' is readily derived from IH *tъnu-* 'thin' with suffixal *-x-*. The gen. sg. *tanuvas* comes quite regularly from IH *tъnu-xos*. It is not necessary however, to ascribe all IE *ū*-stems and *ī*-stems to the suffix *-x-*; any 'laryngeal' would yield the recorded forms, and Hitt. *iskaruh* (see above p. 240, fn. 7) may indicate a suffix *-γ-*. I no longer interpret a single intervocalic *h* as ', as I did in LANG. 12.186 f., 14.109 f.

Whereas the Indo-European nouns in *-ā-* and verbs in *-ā(ye)-* show a bewildering variety of meanings, the Hittite verbs in *-ahh-* commonly have a very definite force; they are normally derived from adjectives and mean 'put (something) into the state described by the adjective'. The following are the clearest cases: *arawwahh-* 'set free' (*arawwis*), *katterahh-* 'make lower' (*katteras*), *kunnahh-* 'make right' (*kunnas*), *lazziyahh-* 'make favorable' (*lazzis*), *marsahh-* 'make bad' (*marsas*), *nakkiyahh-* 'make heavy' (*nakkis*), *newahh-* 'renew' (*newas*), *sarazziyahh-* 'make superior' (*sarazzis*), *suppiyahh-* 'make holy' (*suppis*), *dannattahh-* 'make empty' (*dannattas*), *tepawahh-* 'make small' (*tepus*), *walkessarahh-* 'make expert' (*walkessaras*), *tan pedassahh-* 'make of second rank' (*tan pedas*). We have already seen (p. 240) that beside *maninkuwahh-* 'make short' we may infer an adjective meaning 'short' partly on the basis of *maninkwess-* 'become short'. Similarly we have *hattuless-* 'become well' beside *hattulahh-* 'make well', *duddumess-* 'become deaf' beside *duddymiyahh-* 'make deaf'. Again, just as the adjective at the basis of *maninkuwahh-* is represented by *maninkwanza* (p. 240), so we have *armahh-* 'impregnate'(?) beside *armanza* 'pregnant', *daswahh-* 'make blind' beside *daswanza* 'blind'. Several verbs in *-ahh-* of similar force to those already listed are accompanied by substantives of suitable form and meaning; whether these stems had adjectival usage also is unknown and not very important. They are: *kutruwahh-* 'make (one) a witness' beside *kutrus* 'witness', *siuniyahh-* 'make divine, make insane' beside *siunis* 'god', *paprahh-* 'make unclean' beside *papra* 'uncleanness' or the like,[15] ERUM-*nahh-* 'enslave' beside ERUM 'slave'. The semantic history of several verbs in *-ahh-* is not yet clear: *allappahh-* 'spit' beside *allapa* 'spittle', *ishiullahh-* 'draw up (an army), instruct (a person), prepare' beside *ishiul* 'obligation, contract, treaty', *idalawwahh-* 'injure' beside *idalus* 'bad', *sakiyahh-* 'give an omen' beside *sakais* 'omen', *sanezziyahh-* 'sate oneself' beside *sanezzis* 'sweet, excellent, best', *watarnahh-* 'communicate, command' of unknown derivation. Perhaps if we had fuller knowledge these words also would be found to fit into the pattern of the majority.

We have, however, found (pp. 242 f.) one instance of a deverbative suffix *-hha-* (*annanuhha-*), and it is quite possible that some of the verbs in *-ahh-* were derived from other verbs, or from nouns but not with the factitive meaning. We can only say that the factitive value of the suffix was normal in Hittite.

And it is common also in the Indo-European *ā*-stem verbs. Par-

[15] This word occurs only in KUB 7.53.3.10. In his forthcoming Tunnawi Goetze reads: *pa-ap-ra<-ta>*.

ticularly in Latin the same relationship as in Hittite between adjective and denominative verb is familiar. A few examples must serve for the many that might be given: Lat. *acerbāre* 'make bitter' (*acerbus*), *caecāre* 'make blind' (*caecus*), *foedāre* 'make ugly' (*foedus*), *gravāre* 'make heavy' (*gravis*), *opācāre* 'make shady' (*opācus*), *pūblicāre* 'make public' (*pūblicus*), Gk. *φοιβᾶν* 'make bright' (*φοῖβος*), *κωφᾶν* 'make blunt' (*κωφός*), *ἀτιμᾶν* 'dishonor' (*ἄτιμος*), Goth. *galeikōn* 'make like' (*galeiks* 'like'), OHG *fullōn*, OE *fullon* 'fill' (OHG *fol*, OE *ful*); ChSl. *gotovayą* 'prepare' (*gotovъ* 'ready'). It is perhaps significant that in Indo-Iranian, where there are no verb-forms of the type *τίμᾱμι*, there seem to be no clear instances of this semantic feature; Skt. *priyāyate*, cited by Whitney[16] as meaning 'holds dear', probably means 'is dear to'.

We have found reason to believe that in Indo-Hittite derivative nouns related on the one hand to adjectives and on the other to the verbs in -(*a*)*x*- were consonant stems; their stems seem, in fact, to have been identical with the stems of the verbs. It seems likely, then, that their relationship to the verbs was the same as that of the root-nouns to the root-class verbs. If so it is impossible to decide whether the verbal or the substantival usage was the more primitive; one may as well derive Gk. *τιμή* from *τιμᾶν* as the reverse. The important fact for our present purpose is that we must assume for Indo-Hittite sets of three words, each consisting of an adjective with a derivative noun and a derivative verb in -(*a*)*x*-. It is possible to pick out a few such sets in the historical languages, although there is no reason to suppose that these particular sets were inherited. Examples are: Hitt. **maninkus* or **maninkwas* (see above, p. 240) 'short, near': *maninkuwahhi* (see above, p. 240) 'near at hand' (originally **maninkuwahhas* 'abbreviatio'), *maninkuwahh-* 'make short, approach'; Gk. *αἴτιος* 'blameworthy' : *αἰτία* 'accusation', *αἰτιᾶσθαι* 'accuse'; Lat. *offēnsus* 'struck' : *offēnsa* 'a striking', *offēnsāre* 'strike'. In the set *profugus* 'fugitive' : *fuga* 'flight', *fugāre* 'put to flight', the derivative noun does not present the causative meaning; perhaps we should assume a prehistoric change of meaning; but possibly *fuga* is a back formation like *pugna*. More plausible is the set *ἄτιμος* 'dishonored' : *τιμή* 'honor', *τιμᾶν* 'honor'; for, although *τιμή* usually means 'the state of being honored', the active meaning survives in *τιμή* 'a gift of honor'.

We have already noticed (pp. 240, 241, 242) that Hittite *senahhas*, *menahha-*, and *annanuhhas* are readily understood as verbal nouns. Their history is not certain enough to add to the strength of our case, but at any rate they seem to fit the conclusion.

[16] Sanskrit Grammar p. 388 §1059 b.

2. Indo-Hittite *a:*-stems

Hittite possesses in addition to the verbs in *-ahh-* two other large groups of denominatives. The verbs in *-(i)ya/e-* correspond in part to the Indo-European verbs in *-(i)yo/e-* and in part to those in *-eyo/e-*; we need not discuss them here. There remain the numerous verbs in *-a(e)-*,[17] like *handa(e)-* 'establish, fix' beside *handas* 'firm', *assuwa(e)-* 'be good' beside *assus* 'good'. The outstanding feature of their conjugation is that, with certain exceptions, the stem ends in *-ae-*[18] where Indo-European thematic verbs have *-e-* (3 sg. *ḫa-at-ra-a-iz-zi*) and *a* where the thematic vowel should be *-o-* (3 pl. *ḫa-at-ra-a-an-zi*). The conclusion is inescapable that we have here some class of thematic denominatives. Since Hitt. *a* cannot correspond to IE *e* or *i*, and since the type of Gk. δηλόω is probably a specifically Greek innovation, it seems clear that we have before us the Hittite analogue of the verbs in *-āye/o-*. It is agreed by all that pre-Hittite **-ayanti* (< **-ayonti*) would become *-anzi*. The precise pronunciation and prehistory of the *e*-forms need not detain us at present; suffice it to say that *-aezzi* can easily be derived from a pre-Hittite *-ayeti*. The conclusion is strengthened by two striking resemblances to the IE *āyo*-verbs. (1) Before the suffix a stem-final *-a-* or *-i-* is lost as is the stem-final *-o-* or *-i-* in Lat. *armō* and *illūstrō* while a stem-final *-u-* is retained as in Lat *flūctuō*. (2) The meaning of Hittite verbs in *-a(e)-* is as various as that of Indo-European verbs in *-āyo/e-*.[19] Etymological confirmation comes from the very probable connection of Hitt. *arwa(e)-* 'bow down to' with Gk.

[17] Götze, Madd. 81–100; Sturtevant, Lang. 5.8–14, HG 226–8. W. Petersen, Lang. 9.291–7, maintained that these verbs correspond with the Indo-European *ā*-verbs (type Aeol. τίμαμι). A sufficient answer is contained in the first part of this paper.

[18] Since there are no separate signs for *ez* and *et* it is usually necessary to write *-ai-* or to insert a pleonastic *e* (*mu-ka-e-it-te-en*—KBo.3.43.2.9). In the pret. 2 sg. we read *ḫa-at-ra-a-eš* (14.1.1.76) and *ták-šu-la-a-eš* (14.1.1.75) beside *pít-da-iš* (14.1.1.3). This 2 sg. form is used as a 3 sg. in *ḫa-at-ra-a-eš* (KBo. 3.4.3.79; KUB 6.41.3.50). It is impossible to decide whether we have the same transfer or a heteroclitic form of the *hi*-conjugation in 3 sg. *ḫa-at-ra-iš* (KBo. 4.4.2.34) and *pít-da-a-iš* (KUB 6.41.1.41, 49). In the imper. 2 sg. we find consistently *ḫa-at-ra-a-i*, *tar-kum-ma-i*, etc.; but in view of the constant writing of final *-i* for inherited *-e* in this form of all thematic verbs except those in *-ya/e-*, and also in the pres. 3 sg. of consonant stems of the *ḫi*-conjugation, it seems likely that final *-e* regularly became *-i*. Cf. Sturtevant, Lang. 14.15.

[19] Examples in HG 226 f. In line 15 on p. 227 read: 'provide a witness', instead of: 'make one a witness'.

ἀράομαι 'invoke, pray'.[20] The lack of *ḫḫ* in the Hittite *-a(e)-* verbs and the quality of the initial vowel of IE *-āyo/e-* makes it necessary to set up IH *-aːyo/e-*.

The close connection of the Indo-European verbs in *-āyo/e-* with nouns in *-ā-* suggests that Indo-Hittite may have had nouns in *-aː-* as well as nouns in *-ax-*, and Hittite proves that this is true. The Hittite neut. pl. nom.-acc. regularly ends in *-a*, and this must be identified with the Indo-European ending *-ā/ə*.[21] Once more the lack of *hh* in Hittite and the *-ā* in Indo-European require IH *-aː*.

Evidently the Indo-Hittite suffix *-aː-* formed collectives from *o*-stems (Lat. *locus* : *loca*), and so we may tentatively ascribe its initial vowel to the primitive noun; *-e-ː-* > IH *-aː-* > IE *-ā-*. If this conjecture is correct the origin of the type must antedate the ablaut changes; for IE *-ə* (in the neut. pl. and fem. sg.) must represent IH *-ɔː*, which could result only from *-eː* in the unaccented position. There seems, however, no reason against assuming so early an origin for our collectives. At any rate it seems certain that in Indo-Hittite and also in Indo-European collectives in *-aː-* > *-ā-* existed beside individualizing nouns with the thematic vowel *-e-*. That explains why the *āyo*-verbs in Indo-European and the *a(e)*-verbs in Hittite show about the same range of meaning as the *eyo*-verbs and the *iya*-verbs respectively.[22]

Summary

We may, then, conclude that Indo-Hittite had a set of verbal nouns in suffix *-ax-* (probably of neuter gender) and a set of non-thematic factitive verbs in the same suffix. Both nouns and verbs were normally formed from adjectives.

There were also collective nouns in suffix *-aː-* formed from individualizing nouns. From the collective nouns, as well as from the underlying

[20] I have recently discussed this etymology in Lang. 14.71. In HG 226 I also identified *tarma(e)-* with Lat. *termino*, but, since we have learned that the Hittite word means 'blockade', that seems less plausible. The proposed connection of *mita(e)-* 'fix, determine' with Lat. *mētor* 'measure' must be given up.

[21] Sturtevant, HG 177 f., Lang. 14.75. The 'laryngeal' theory requires some correction of the former passage; if *ː* survived in Indo-Hittite, the *-a* in Hittite cannot come from IH *ə*, but only from *-aː* or *-ɔː*.

[22] For Hittite, see Sturtevant, HG 225–8. The second sentence in §309 should read: A common meaning is 'be that which the primitive noun or adjective denotes'.

individualizing nouns denominative verbs were formed with the suffix *-yo/e-*.

We have found reason to suppose that the initial vowel of each suffix originated in the thematic vowel *-e-* of underlying adjectives or substantives. There is no reason to suppose that either the suffix *-x-* or the suffix *-ʾ-* was originally restricted to use with *e/o*-stems; and there is no reason to suppose that either was originally confined to the respective values for which we find abundant evidence. In fact Hittite seems to show the suffix *-x-* in one or two deverbative nouns with *u* immediately before the suffix.

GLOTTALIZED CONTINUANTS IN NAVAHO, NOOTKA, AND KWAKIUTL

(WITH A NOTE ON INDO-EUROPEAN)

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1. It is well known that a very large number of American Indian languages number among their phonemes glottalized stops and affricates (e.g. *ṗ, ṭ, ḳ, q̇, ḳʷ, q̇ʷ, ċ, č̣*). Examples of such languages or groups of languages are Dakota, Winnebago, Ponca and other Siouan languages; Tonkawa; Chitimacha; Kootenay; Salish languages; Sahaptin languages; Chinookan; Tlingit; Haida; Tsimshian; Kwakiutl and Nootka[1]; Chimakuan; Athapaskan languages (e.g. Navaho;[1]) Alsea; Siuslaw; Coos; Takelma; Karok; Shasta; Achumawi and Atsugewi; Pomo; Chimariko; Yana; Klamath and Modoc; Kalapuya; Yana; Yurok; Yuki; Wintun; Yokuts; Washo; Keres; Tanoan languages; Kiowa; Mayan languages; Quechua. In most of these languages the glottalized consonants are fortes, as in Chinookan and Athapaskan; in others, as in Chitimacha and Taos (Tanoan),[1a] they are lenes. In the overwhelming majority of cases the glottal release is posterior to the oral release, precisely as in Georgian and other Caucasic languages. These glottalized consonants are sometimes known as 'ejectives'. In some languages, however, the oral and glottal releases are synchronous. An example of a language having glottalized consonants of this type is Southern Paiute (Shoshonean), where they are not true phonemes, however, as they may always be analyzed into stop (or affricate) + ʔ.

[1] The Navaho forms cited in this paper are from the author's field notes. The Nootka forms are quoted from Sapir and Swadesh, Nootka Texts (William Dwight Whitney Linguistic Series, Yale University, 1938). The Kwakiutl forms are quoted from F. Boas' writings on Kwakiutl, particularly: Kwakiutl (Handbook of American Indian Languages, 1.423–557, Bureau of American Ethnology, Bulletin 40, 1910); Ethnology of the Kwakiutl, Part 2 (35th Annual Report of the Bureau of American Ethnology, 1921), Vocabulary (pp. 1389–1466); A Revised List of Kwakiutl Suffixes (International Journal of American Linguistics 3.117–131 [1924]); Notes on the Kwakiutl Vocabulary (ibid. 6.163–78 [1931]).

[1a] Fide Morris Swadesh and George L. Trager respectively.

2. Less common than glottalized stops and affricates are glottalized continuants. Voiceless spirants with glottal affection are well attested in Tlingit, where we have the series: *ś*, *x̀*, *x̀ʷ*, *x̧̀*, *x̧̀ʷ*, which must be carefully distinguished from the parallel series of glottalized stops and affricates: *c̀*, *k̀*, *k̀ʷ*, *q̀*, *q̀ʷ*. In Chasta Costa (Athapaskan) *x̧̀* is the regular reflex of Athapaskan *k̀*, whose more common dialectic reflexes are *k̀* (e.g. Navaho), or *q̀* (e.g. Hupa).

Of greater frequency, it would seem, than glottalized voiceless spirants are glottalized sonorant consonants (*y*, *w*, *m*, *n*, *ɒ*, *l*): *ẏ*, *ẇ*, *m̀*, *ǹ*, *ɒ̀*, *l̀*. In these consonants the glottal closure is synchronous with the momentarily voiceless initial phase of the continuants, its release being immediately followed by the voiced phase of the continuant. A pronunciation ʔ + *y*, for instance, is always resisted by the native's ear as incorrect, particularly as such clusters may occur as well. In Nootka, for instance, *ẏaṅa* 'child' cannot be syllabified *ẏaʔ-na*, as there are no syllables ending in ʔ. The *ǹ* is a true phoneme, beginning its syllable, and a syllable can only begin with a single consonant. In Navaho the same phoneme, *ǹ*, must be carefully distinguished from the cluster *ʔn*; the former occurs, e.g., in *xàná·ǹàʔ*[2] 'he has crawled out back again', the latter in *xàʔnà·* 'across'. A few indications of the occurrence of these sounds may be welcome. Haida possesses *ẏ*, *ẇ*, *m̀*, *ǹ*, *ɒ̀*, and *l̀*; *ẏ* and *ẇ* are actually more common phonemes than *y* and *w*, though these are not absent. Yokuts, Tsimshian, and Kwakiutl have *ẏ*, *ẇ*, *m̀*, *ǹ*, and *l̀*. Nootka has *ẏ*, *ẇ*, *m̀*, and *ǹ*. Navaho possesses *ǹ* and *m̀* and, very rarely, *ẏ*; these can only occur as stem initials, never as word initials.

3. Examples of these phonemes as initials in Nootka are: *ẏaṁa* 'salal-berries'; *ẏa·q* 'long' (contrast *yaq-ʔi·tq* 'he who is'); *ẏowa·-ƛ* 'filled with surprise, grateful' (contrast *yoxʷa·* 'heating up, giving off hot air'); *ẇa·-* 'ashamed, bashful' (contrast *wa·* 'to say'); *ẇaẇal* 'the temples' (contrast *wał-šiƛ* 'to go home'); *ẇi·-ya·* 'to split into thin slabs' (contrast *wi·-ẏa* 'never'); *ẇic-a·* 'to nod one's head' (contrast *wica-* 'well to do'); *m̀a-* 'holding in the mouth' (contrast *ma-* 'to dwell'); *m̀a·t-il* 'captive' (contrast *mat-* 'to fly'); *m̀o·q* 'throwing off sparks from fire-drill twirler' (contrast *moq-* 'having liquid in the mouth'); *ǹa·pi* 'there is moonlight' (contrast *na·p-a·* 'to get coiled up'); *ǹaš-* 'to look' (contrast *naš-ok* 'strong, firm'); *ǹixʷ-* 'salmon roe, kidneys' (contrast *nixʷ-ak* 'cheap'); *ǹoč-šiƛ* 'to cook food by steaming' (contrast *noč-i·* 'mountain'). Non-initial examples are: *ƛoł-i·ẏoł* 'well-throated, having a good voice'

[3] ˊ, high tone; ˋ, low tone.

(contrast *ʔo·-yoqʷa* 'doing to him'); *-ẃi*, *-ẃi·* 'mark of . . . ' (contrast *-wi*, *-wi·* 'first'); *-m̀a*, *-m̀a·* 'as far as . . . ' (contrast *-ma·ʔaλ* 'intending to . . . '); *ʔo·-ǹaqi·ł* 'to find it' (contrast *ʔo·-naqa* 'using it as bait').

4. It is obvious, from the behavior of *ẏ*, *ẃ*, *m̀*, and *ǹ* in Nootka, that these are true phonemes, sharply distinguishable from the non-glottalized *y*, *w*, *m*, and *n* and etymologically irreducible, at least in the first instance, to ʔ + sonorant consonant (or sonorant consonant + ʔ). So singular are these consonants, however, that it is tempting to seek evidence accounting for their origin, though in a purely descriptive treatment of the phonemes of a language no such evidence need be considered. We may turn for a moment to Navaho, for neither *ǹ*, *m̀*, nor *ẏ* is an original Athapaskan phoneme and a comparative study of Athapaskan sounds must account for their appearance in Navaho. Fortunately this is an easy task. It is true that there are, or seem to be, a few stems in which an initial *ǹ* must be accepted as an unanalyzable phoneme, e.g. *-ǹé·h* (ipf.), *-ǹà*ʔ (pf.) 'to crawl'. But in other cases an apparently irresolvable *ǹ* can be plausibly shown to result from a contraction of *d* + *n*. Thus, the stem *-ǹìʔ* of *ʔì·ǹìʔ* 'thunder' and the stem *-ǹí* of *dìǹí* 'he moans' are best explained as resulting from an earlier **-d-nìʔ* and **-d-ní* respectively. The *-d-* of **-d-nìʔ* is reduced from the *di-* of the verb *di-. .-ní* 'to say', whose stem (*-ní* 'to make a sound') is identical with the *-ní* which underlies the *-ǹí* of 'to moan' and closely related to the *-nìʔ* underlying the *-ǹìʔ* of 'thunder'. That this is the case can be shown by the testimony of related languages, in which we either have actual combinations of *d* + *n* or the proper reflexes of *-dn-* in the words for 'thunder'. In Navaho *dìǹí* 'he moans' the prefix *di-* is the same sound-referring element which we have in *di-. .-ní* 'to say' and in countless other verbs having to do with sound, word, or speech, while the *-d-* of earlier **-d-ní* is a self-referring element; *dìǹí* is, therefore, etymologically something like 'he says, makes a sound, with reference to himself', i.e. 'he makes a sound without purpose of communication'. It will be seen that the glottal element of *ǹ* in *-ǹìʔ* and *-ǹí*, while going back to the same stopped consonant, is not derivable from the same morphological element. In other words, we are dealing here with a purely mechanical morphophonemic emergent, *ǹ*, due to the coalescence of two originally distinct phonemes, *d* and *n*, of whatever etymological value. Owing to the great semantic distinctiveness of 'thunder' and 'to moan' as compared with 'to say' and the lack of obvious paradigmatic relationship of *ʔì·ǹìʔ* and *dìǹí* to *di-. .-ní*, it is safe to assume that the analyses that we have given, however clear to

the dissecting linguist, have not the 'configurative pressure' that would justify our considering the phoneme *ṅ* as merely a resultant of *d* + *n*. If such an interpretation was at one time possible, it is probably no longer the case from a purely descriptive point of view. *-ṅé·h* (*-ṅàʔ*), *-ṅìʔ*, *-ṅí*, and other stems of this sort are best listed, for descriptive purposes, as possessing a distinct phoneme, *ṅ*, whose remoter history has no compelling relevance for its placement in the scheme of Navaho phonemes.

There are, however, two important considerations which weaken the force of the negative argument. All other consonantal phonemes (aside from the rare *ṁ* and *ẏ*, of which a word later on), 32 in number, not only occur as stem initials but as word initials, while *ṅ*, *ṁ*, and *ẏ* can only occur as non-initial stem initials. This at once suggests, and quite aside from historical or morphological considerations, that *ṅ*, *ṁ*, and *ẏ* have a secondary, derivative, phonemic status. This surmise is borne out by the second and more important consideration, that all cases of *ṁ* and *ẏ* (there are only a few) and the great majority of cases of *ṅ*, a common phoneme, are not as isolable or as relatively isolable as those we have just spoken of but occur in a clear-cut morphophonemic relation to *m*, *y*, and *n* respectively. To make this clear, we shall have to go into certain details of Navaho morphology. An important feature of the structure of Navaho verb-paradigms is the appearance of what may be called a *d*-effect at certain points in the total configuration. There are chiefly two such points: the first person dual-plural subjective pronoun prefix (*-ì·-* < **-ì·d-*) and the mediopassive forms (characterized by an old element **-d-*). For these two groups of forms it is necessary to modify either the stem initial or the form of a consonantal prefix immediately preceding the stem or, very frequently, both. Thus, a *d*-modified stem initial, even when such modification is only implicit (in cases where the earlier **-d-* disappeared without leaving an overt trace, e.g. before stops and affricates), requires that the perfective prefix *-z-* in the third person of a very large 'class' of verbs (the most numerous of the four 'classes') take the form characteristic of the other three classes, namely *-s-*; thus, *yì-dè·-z-ʔą́* (pf. stem *-ʔą́* of ipf. *-ʔà·h* 'to handle the round object') 'he has gone off with it (an object of a certain semantic classification)', but passive *dè·-s-ɬą́* 'it has been gone off with' < **dè·-z-d-ʔą́*. Similarly, *yì-dè·-z-ką́* (pf. stem *-ką́* of ipf. *-kà·h* 'to handle the container with its contents') 'he has gone off with it (an object of another semantic classification)', but *dè·-s-ką́* 'it has been gone off with' < **dè·-z-d-ką́*. Again, *dì·ɬà·h*

'we 2 go off with the round object' < *d-ì·d-ʔà·h : dìšʔà·h 'I go off with it'; while in dì·kà·h 'we 2 go off with the container with its contents': diškà·h 'I go off with it' there is no overt difference between d-effect and its absence. It may be remarked that in certain Athapaskan dialects, e.g. Hupa, the theoretical *-d- of Navaho actually appears as a syllable of type -di-. The table of d-modified stem initials, aside from irregularities which do not interest us, is as follows:

Primary consonant	d-modified
ʔ	ṭ
s, z	ʒ
š, ž	ž
γ	g; sometimes ʒ
y	ʒ; rarely ẏ
m	ṁ
n	ṅ
ł, l	λ

In the forms analogous to those which require a change of ʔ to ṭ, n becomes ṅ. Hence, analogously to yìde·zʔą́, dè·sṭą́, dìšʔà·h and dì·ṭà·h, we have yìdè·znìl (pf. stem -nìl of ipf. -ní·ł 'to handle the group of objects') 'he has gone off with them', dè·sṅìl 'they have been gone off with', dìšní·ł 'I go off with them', dì·ṅí·ł 'we 2 go off with them'. Again, within the paradigm of the verb di-. . .-ní 'to say', the stem -ní, pf. -nì·d, must be modified to -ṅí, -ṅì·d, e.g. dì·ṅí 'we 2 say' (homonymous with dì·ṅí 'we 2 moan' but referable to dìšní 'I say', not to dìšṅí 'I moan'), hòdò·ṅì·d 'it has been said' (contrast dí·nì·d 'I have said').

5. In Navaho ẏ only occurs in morphophonemic alternation with y in the stem -yói, e.g. hòñšyói 'I have bravery, am able to endure', hònì·ẏói 'we 2 have bravery'; ṭó· ʔòhdyói 'they are in great number', ṭó· ʔòhònì·ẏói 'we are in great number'. We should have expected *-ʒói in the d-modified forms of -yói but, for reasons which seem totally obscure at present, -yói here follows the analogy of stems with initial n and m. ṁ is the regular d-modified form of m, not a common phoneme, and occurs almost exclusively as its morphophonemic pendant. Examples are nànì·ṁàʔì· 'we 2 are vagabonds': nàñšmàʔì· 'I am a vagabond, roam about like a coyote', a denominative verb based on màʔì· 'coyote'; nè·ṁàs 'we 2 have got round': némàs 'I have got round', nè·zmàs 'he has got round'.[3]

[3] I owe these forms of the verb 'to get round' to Father Berard Haile, O.F.M.

It would seem, therefore, that the class of glottalized voiced continuants in Navaho, consisting of *ṅ*, *ṁ*, and *ẏ*, of which the two latter are but sparsely represented, arose in the first place as secondary phonemes, owing to the coalescence of an old *d* with an immediately following *n* or *m*, irregularly also with an immediately following *y*. The processes *-dn-*, *-dm-* > *-ṅ-*, *-ṁ-* may be considered true phonetic laws but *-dy-* > *-ẏ-* contravenes all known analogies, which suggest *-ẓ-* as the regular phonetic development. We shall therefore infer that *ẏ* arose, not by the operation of a normal phonetic law, but by a peculiar type of morphophonemic analogy. Perhaps analogies of this sort have played a greater part in linguistic history than is generally suspected. In Navaho we have the interesting spectacle of a pecular class of phonemes 'on the make', as it were. Their functional dependence on the more common, non-glottalized, forms of the voiced continuants (*n*, *m*, *y*) is still entirely clear, yet cases like *-ṅé·h* 'to crawl', which cannot easily be traced to an earlier **-d-né·h*, though that is almost certainly what it does go back to, already show that there is a tendency for the glottalized voiced continuants to establish for themselves a more independent position in the configuration of Navaho phonemes.

6. We must now return to Nootka. This language is at the opposite extreme from Navaho, for its glottalized voiced continuants show little sign of a relationship to the corresponding non-glottalized voiced continuants. The glottalized varieties have obviously been completely independent phonemes for a very long time and comparison with Kwakiutl, a remotely related language,[3a] confirms this, for in both languages these consonants occupy the same position. Among the cognates are examples illustrating them, e.g. Nootka *ṅop-* 'one' : Kwakiutl *ṅəm-* 'one' (syllabically final *m* becoming *p* in Nootka; before this *p* original *ə* labialized to *o*, phonetically open *u*). As no obvious biconsonantal prototype can be suggested for either Nootka or Kwakiutl *ṅ-* in this word and in similar words, we must ascribe the phoneme *ṅ*, and with it also *ẏ*, *ẇ*, *ṁ*, and *ḷ*, to the early Wakashan[4] period.

There are, however, certain processes which strongly suggest that

[3a] The degree of genetic relationship of Nootka and Kwakiutl is hardly greater than that of, say, Russian and German.

[4] Wakashan is the term employed by Americanists for a linguistic group which includes two main branches: Kwakiutl, consisting of Kwakiutl proper, Bella Bella, and Kitamat; and Nootka, consisting of Nootka proper (also known as Aht), Nitinat, and Makah. This synthesis was first established by Boas.

Nootka *ẏ*, *ẇ*, *ṁ*, and *ṅ* are of secondary origin, at least in part. These processes are shared by Nootka and Kwakiutl, so that the emergence of the glottalized voiced continuants is at least as old as the Wakashan period. One of the most important of these processes is the 'hardening' of consonants when they are immediately followed by suffixes with inherently 'hardening' power. In the main the 'hardening' process consists of glottalization. Thus, in Kwakiutl *p*, *t*, *k*^{w}, *c*, *q*, *l*, *x*^{w}, to single out a number of consonants, are 'hardened' to *ṗ*, *ṭ*, *k̇*^{w}, *ċ*, *q̇*, *ḷ*, *ẇ* respectively; *q̇*, undoubtedly a Wakashan phoneme, develops to Nootka *ʕ*, a laryngeated ('strangulated') glottal stop, phonemically distinct from ʔ, while -*ḷ*- develops to Nootka -*ẏ*-. The other of these 'hardened' consonants remain in Nootka. Thus, to give only a few Nootka examples, *hap*- 'hair' + '-*itoł* [R] 'dreaming of . . .'[5] yields *hahaṗitoł* 'dreaming of hair'; *hopt*- 'in hiding' + '-*a·ʔa* 'on the rocks' yields *hopṭa·ʔa* 'in hiding on the rocky (shore)'; *ʕi·k*^{w}- 'pair of brothers' + '-*akχi* 'at the rear, last' yields *ʕi·k̇*^{w}*akχi* 'the two brothers (are) at the rear, come last'; *ṭoc* 'large sea-egg' + absolutive suffix '-*op* yields *ṭoċop*; *čikit-q*- stem abstracted (because of current relation of Nootka -*t-q*- : -*n*-) from *čikinis*, borrowed from English 'chickens', + '-*is* 'eating . . .' yields *čikitʕis* 'eating chicken'; *hoł*- 'dancing' + '-*as* 'outdoors, in the village' yields *hoẏas* 'dancing in the village'; *ʔox*^{w}- 'paddle' (absolutive *ʔox*^{w}*a·p*) + '-*aḥs* 'in a vessel' yields *ʔoẇaḥs* 'a paddle (is) in the canoe'. In part, therefore, non-initial *ẏ*, *ẇ*, *ṁ*, and *ṅ*, like other non-initial glottalized consonants, can be shown to go back to the 'hardening' of glottalized continuants, such as -*ł*- and -*x*^{w}-. The chief types of Nootka 'hardening' to these consonants are:

-*n*-	'hardened' to	-*ṅ*-
-*m*-	" "	-*ṁ*-
-*ł*-	" "	-*ẏ*-, sometimes -*ẇ*-
-*s*-	" "	-*ẏ*-
-*š*-	" "	-*ẏ*-
-*x*^{w}-	" "	-*ẇ*-
-*ḥ*- (rarely)	" "	-*ẇ*-

Nootka *š* goes back, in the main, to Wakashan *x̣* (voiceless prepalatal spirant); -*ḥ*- (laryngeated *h*, similar to Arabic *ḥ*) is developed from -*x̣*- (voiceless velar spirant) or its labialized form, -*x̣*^{w}-, 'hardening' of *ḥ* to -*ẇ*- always implying an underlying Wakashan -*x̣*^{w}-. Of the four

[5] '- is a symbol to indicate the 'hardening' effect of a suffix. R indicates that the suffix causes reduplication of stem; L, that the suffix causes lengthening of stem vowel.

glottalized voiced continuants, therefore, two (*ṅ* and *ṁ*) may be direct resultants of a glottalizing process dependent on an initial peculiarity of a suffixed element, and two (*ẏ* and *ẇ*) may result, more indirectly, from glottalization, voicing and loss of spirantal friction or shift in articulatory position, of voiceless spirants or their prototypes (*ł*, *s*, **x̣*, *x*^{w}, **x̣*^{w}), dependent on the same initial peculiarity of a suffixed element. No doubt the three etymological sources for *ẏ* and the two (or three) etymological sources for *ẇ* were originally reflected in five distinct glottalized phonemes (perhaps *-ł̇-*, *-ż-*, *-γ̇-*, *-γ̇*^{w}*-*, *-γ̇*^{w}*-*). This is confirmed by the fact that in Kwakiutl the 'hardened' forms of *-ł-*, *-s-*, and *x̣* are respectively *-ł̇-*, *-ć-*, and *-ṅ-*, the last of which suggests that 'hardened' Wakashan *-γ̇-* was a different phoneme from the primary *ẏ* shared by Kwakiutl and Nootka.

What this 'initial peculiarity of a suffixed element' was we cannot be sure of at present. That it was not the simple presence of a glottal stop is likely, for there are several suffixes which begin with ʔ that remains unabsorbed by the preceding consonant, such as *-ʔato* 'to fall off, come off', *-ʔokt* 'obtained by . . .', *-ʔał* 'aware of . . .'; thus *his-* 'to hit' + *-ʔokt* yields *his-ʔokt* 'obtained by violence', not **hiẏokt* (contrast *hiẏo·ƛ* 'to hit on the rocks' < *his-* + *'-o·ƛ* 'on the rocks', momentaneous aspect). There may have been a weak consonant, say *-h-*, following the ʔ in the case of 'hardening' suffixes, which had the effect of throwing the ʔ back on the preceding syllable, with resultant glottal absorption or 'hardening', while the *h* was left to begin the following syllable. Later, when nearly all but analogical *h*'s (chiefly in reduplication, e.g. *hihis-*, distributive form of *his-*) disappeared in non-initial position, the syllabic division was shifted and the appearance created of simple glottal absorption. If this view is correct, *hisʔokt* derives from an early *his-ʔokt*, but *ƛoƛoẏi·ḥ* 'fishing for herrings' < *ƛos-* 'herring' (absolutive *ƛos-mit*) + *'-i·ḥ* [R] 'hunting, collecting . . .' derives from **ƛoƛos-ʔhi·x̣*$^{(w)}$ > **-ƛośhi·x̣*$^{(w)}$ > **-ƛoż i·x̣*$^{(w)}$ > Nootka *-ƛoẏi·ḥ*; the *-ʔ-* of the suffix would be responsible for the glottalization of the emergent *-ẏ-*, the former *-h-* for the 'softening' (see below) of the glottalized spirant (*-ś-* ?) lying back of the present *-ẏ-*. Be this as it may, it can be shown from a comparison of Kwakiutl and Nootka elements that consonant + ʔ does not yield a 'hardened' consonant; in other words, that the difference between retained ʔ and 'hardening' of preceding consonant is not simply a matter of difference of chronology, the 'hardening' process being due to an early, Wakashan, glottalizing, but the mechanical preservation of ʔ to a later Nootka process.

An example of preserved consonant + ʔ that must go back to the Wakashan period is Kwakiutl *-x̣ʔənx̣* '... year', Nootka *-qʔičḥ*, Wakashan **-qʔəḵx̣* (voiceless stopped consonants are spirantized before certain consonants in Kwakiutl; for earlier Kwakiutl **-x̣ʔəx̣x̣* > *-x̣ʔənx̣*, cf. 'hardening' of *-x̣-* to *-n̓-* above).

7. The theory that Nootka and Kwakiutl 'hardening' are due to the pooling of two historically distinct processes, glottalization and 'softening', receives some weight from the fact that there is an independent but related 'softening' effect exerted by several suffixes on the immediately preceding consonant. The 'softening' effect is very clear in Kwakiutl, which possesses three distinct series of stops and affricates ('intermediate' or voiceless lenis, aspirated voiceless fortis, and glottalized: e.g. *b, p, p̓*). The 'softening' suffixes change aspirated voiceless stops and affricates to their corresponding intermediates (e.g. *t* to *d*, *c* to *ʒ*). As Nootka has pooled the old intermediates and aspirates in a single voiceless fortis series (unaspirated before vowels, aspirated as syllabic finals), the old 'softening' process is visible only with spirants and here only in considerably diminished range. Those suffixes (like *'-il*[6] 'in the house' and *'-is* 'on the beach') which have the 'softening' effect change *l* to *y* (or *w*), *s* and *š* to *y*, and x^w and *ḥ* (if < *$x̣^w$) to *w*; e.g. *-ol* 'place of ...' + *'-is* yields *-owis* '... place on the beach', *ʔi·ḥ-* 'big' + *'-a-čiλ* (inceptive) yields *ʔi·wačiλ* 'to get big', *-mal* 'moving about' + *'-il* 'in the house' yields **-mayil* > **-mail* > *-mi·l* 'moving about in the house' (older *ai* and *au* become monophthongized to Nootka *i·* and *o·* respectively). It cannot be argued that 'softening' is simply due to the mechanical lenition of an immediately following smooth vowel, for in both Kwakiutl and Nootka a very large number of suffixes which begin with a vowel leave the preceding consonant unaffected. Both 'softening' and 'hardening', therefore, with their parallel effects, must have a phonological feature in common which goes back to the Wakashan period and this feature is most likely to have been a weak consonant that has now disappeared. Now *h* is a very common initial consonant in both Kwakiutl and Nootka but is rare in other positions. As we have seen, it occurs postvocalically in reduplicated forms, where its presence is readily explained as due to analogy (an irregular **hi·s-* 'several hit' < *hihis-*, distributive of *his-*, could hardly have withstood analogical restoration under the pressure of thousands of regular forms like *mimis-* 'several smell', distributive of *mis-*). Significantly enough, present intervocalic *h* shows a tendency itself to 'soften' and

[6] '- indicates 'softening'.

palatalize to *-y-* after *i* in certain very common stems, e.g. *hiyił* < **hihił*, distributive of *hił* 'at that place, there'; *hiyiq-* and *hihiq-* 'various things', distributive of *hiq-* 'all'. 'Hardening' is not a process opposed to 'softening', as originally conceived by Boas for Kwakiutl, but a 'glottalized softening'. 'Lenition' (due to former *-h-* ?) and 'glottalized lenition' (due to former *-ʔh-* ?) would seem to be the linguistically preferable terms.

It may be pointed out that it is at least conceivable that the *'-i-* of *'-ił* and *'-is*, the two most common 'softening' suffixes of Nootka (and Kwakiutl: *'-i·ł*, *'-i·s*) is an old demonstrative stem **hi*, which obviously occurs in an important series of local and referential stems: *hita-*, *hin-*, *hina-* empty stem or peg for attachment of semantically significant suffixes; *his-*, *hist-*, *hisa-*, *hista-*, *hił* 'at that place, there' (referential). The last series of stems is formally parallel to *ya·s-*, *ya·st-*, *ya·ł*, *ya·* 'there, that' and to *yi·s-*, *yi·st-*, *yi·ł*, *yi·* 'yonder', enabling us to isolate **hi* without difficulty. The meanings 'in the house' (often also 'inside' without reference to human abode) and 'on the beach' (originally, as can be shown, 'on a level stretch') are clumsy renderings of a more generalized type of orientation.

8. 'Hardening' is a process that had worked itself out long before the dialectic Nootka period. We cannot directly prove the presence of an old *-ʔ-* to account for it, because the *-ʔ-* which appears before a 'hardening' suffix when the monosyllabic stem ends in a vowel (e.g. *ƛa-* 'stick-like object standing up' + *'-a·ʔa* 'on the rocks' yields *ƛaʔa·ʔa* '[tree, stick] standing up on the rocky place') is found also with 'softening' suffixes under the same conditions (e.g. *ƛa-* + *'-is* 'on the beach' yields *ƛeʔis* 'standing up on the beach', umlauted < **ƛaʔis*); this glottal stop, while it may be a survival of the phoneme that actually caused the 'hardening', can also be a mere hiatus-filler, for a syllable must begin with a consonant and vocalic contraction cannot take place between an initial monosyllabic stem ending in a vowel and an initial vowel of a suffix that 'hardens', 'softens' or begins with inherent *-ʔ-*, though such contraction does take place in subsequent syllables or when the suffix begins with a smooth vowel that has no disturbing effect on a preceding consonant (e.g. not only *ƛa-* + *'-a·ʔa* = *ƛaʔa·ʔa*, *ƛa-* + *'-is* = **ƛaʔis*, but also *ƛa-* + *-ʔi·-ʔa* 'to get to be on the rocks' = **ƛaʔi·ʔa* 'stick gets to be standing on the rocks' > *ƛeʔi·ʔa*; contrast *ƛa-* + *-a·s* 'on a surface' = *ƛa·s* '[stick] standing on [it]' and distributive *ƛaƛa·ʔa* 'several trees standing on the rocks'). What probably happened is that original forms of type **ƛa-ʔi-* remained, that forms of type **ƛa-a·s* con-

tracted early, and that original forms of type *ƛ*a-ʔha·ʔa* (hardening') lost their *-h-* and thus leveled with the first type in this particular category of cases. The fourth original type, e.g. *ƛ*a-his* ('softening') also lost its *-h-* and yielded at first dissyllabic *ƛ*ais*, analogically remodeled to *ƛ*aʔis*, instead of eventually monophthongizing to *ƛ*i·s*. Hence resulted a leveling, in forms involving retained -ʔ-, between 'hardening' and 'softening'.[7]

9. We have a few interesting cases in Nootka of forms in -ʔ- + vowel + voiced continuant which alternate with glottalized voiced continuant, suggesting that this class of consonants could also arise from an absorption of a ʔ by a following consonant when the intervening vowel had dropped out. Such cases are mere survivals of what was probably at one time an active process. Thus, the stem *koʔoq-* 'to berate, vituperate', probably monophthongized from earlier **koʔauq-*, alternates with *kowaq-*; in other words, an underlying **koʔawaq-* either reduces to **koʔawq-* (with loss of second *-a-*) > *koʔoq-* or to **koʔwaq-* (with loss of first *-a-*) > *kowaq-*, syllabically final ʔ being impossible in Nootka, though very frequent in Kwakiutl. Much clearer than this example is the obviously archaic alternation of *ʔeʔim* 'at first, immediately', umlauted from **ʔaʔim*, with *ʔamа-* 'at a proximate time (immediately before or immediately after), immediately, at first, for the first time'. In order to understand these forms, it is necessary to know that syllabically final *m* and *n* have a light *i*-murmur release, that they go back to original *m*, *n* + vowel (*a*, *i*, or *o*; original syllabically final *m* and *n* become *p* and *t*), and that original *a* preceding such elements (m^i, n^i) regularly thins to *i*. After a non-initial consonant *-ama* (*-ami*, *-amo*) and *-ana* (*-ani*, *-ano*), except under conditions which we need not attempt to define here, develop to *-in* (i.e.

[7] That -ʔ- of *ƛeʔis* is merely analogical seems also to be indicated by the fact that a number of smooth-vowel suffixes, which neither 'harden' nor 'soften' preceding consonants, also insert -ʔ- after a final vowel of the syllable. Thus, *ča-* 'island', *ča-* 'stream', and *ča-* 'water' form absolutive (or durative) *čaʔak*, *čaʔak*, *čaʔak* (cf. indifferent effect of *-ak* in such words as *kimt-ak* 'long pole extending from end to end', *kʷas-ak* 'dead limb, twig') distributive *čača·k*, *čača·k*, *čača·k*. On the other hand, the 'softening', as well as 'hardening', in Kwakiutl of original *-y-*, *-w-*, *-n-*, *-m-*, *-l-* to *-ẏ-*, *-ẇ-*, *-ṅ-*, *-ṁ-*, *-l̇-* (e.g. *han-* 'hollow vessel is somewhere' + '*-i·l* yields *haṅi·l* 'kettle on floor'; see Boas, Kwakiutl, Handbook of American Indian Languages 1.430, 473) might be interpreted to mean that 'softening' too is the result of an old absorption of a glottal stop, though it seems much simpler to assume that in Kwakiutl types *-nʔh-* and *-nh-* leveled to *-ṅ-*. Possible confirmation of this will be pointed out below, when Kwakiutl initial glottalized voiced continuants are discussed.

*in*ⁱ) and *-im* (i.e. *-im*ⁱ) respectively, whether in final or non-final position (e.g. *-ła·*, *-ła* 'having . . . as name', durative, forms momentaneous *-ła·noλ*, alternating with **-łanoλ* > *-łinλ*, i.e. *-łinⁱλ*; similarly, *-ma* 'thing', when combined with *-ẇana-*, *-ẇinⁱ* 'in the middle', forms *-ẇanimⁱ* 'thing in the middle'). This means that *ʔeʔim* is not only closely related in meaning to *ʔaṁa-* but, in all likelihood, goes back to a form, **ʔaʔama*, that is originally nothing but a phonetic variant of *ʔaṁa-*. It is quite possible that **ʔaʔama* was originally the independent form, as which (*ʔeʔim*) it is still used, while *ʔaṁa-* was always used as the base for derivations, as which alone it is found today. Present cases of *ʔeʔim-* with derivational suffixes (which, if 'hardening' or 'softening,' require an inserted -ʔ- after *m*, in other words *ʔeʔimⁱ-ʔ-*) are perhaps merely analogical, as derivations are often secondarily based on absolutive or durative forms rather than on the proper 'combining forms'. This interpretation of the relation between *ʔeʔim* and *ʔaṁa-* seems simpler than to consider the former a reduplicated form of *ʔaṁa-*, for *ʔeʔim* is not, as a matter of fact, the distributive of *ʔaṁa-*. Both of these stems are, then, divergent forms of an inherently reduplicated **ʔaʔama-*, which we cannot analyze further at present. Presumably, when the final *a* of this stem was non-final, the second *a* was elided (original stress patterns: **ʔaʔáma* but **ʔàʔamá-* ? but stress is not functional in Nootka today), the resulting **ʔaʔma-* yielding *ʔaṁa-*.

10. Far more numerous than such isolated cases are those in which *-ẏ-*, *-ẇ-*, *-ṁ-*, *-ṅ-* alternate with *-y-*, *-w-*, *-m-*, *-n-* without a trace of preceding -ʔ-. Cases in point are: *-ṅok*, momentaneous *-ṅokʷiλ* : *-nkʷ-*[R] 'at, on, of the hand' (e.g. *λiλis-ṅok* 'white-handed', *ya·ya·k-ṅok* 'sore-handed': *sosi-nkoλ* 'to get hold of by the hand' < **soso-nokʷi-λ*, *titinkom* 'hand-wiper' < **titi-nokʷi-ma* 'thing for wiping the hands', *kʷikʷi-nk-so* 'hand' with obscure stem *kʷi-* and suffix *-so*, possibly also in *hołi-nk* 'benumbed of hand')[7a]; *-ṅi*, *-ṅi·* : *-n* 'to come' (e.g. *ʔo-ṅi·* 'that one comes' : *hini-n* 'to come' < empty stem *hina-* + **-ni*); *-ṅiq-*, *-ṅi·q-* 'down a slope' : *-nq-* (e.g. *po-ṅi·q-saλ* 'several run down a slope to the beach' : *hiti-nq-is* 'down a slope on the beach' > 'the beach' < empty stem *hita-* + **-niq-* + '*-is*); *-coẇat*[L] 'on . . . side, on the . . . side' : *-co·t* 'on . . . side' (= theoretical **-cowt* ?); *-ẇiłta*

[7a] There is a survival of *-nkʷ-* 'at the hand' [R] in Kwakiutl *ċənċənkʷ-a* 'to wash the hands', with secondary stem *ċənkʷ-*; cf. Kw. *ċo·xʷ-* 'to wash', N. *ċo-*. This seems to imply that the Nootka alternation of glottalized and non-glottalized *n* is of Wakashan age.

'out of a canoe' : *-ołta* (= theoretical **-włta*; e.g. *ẗaq-wiłta* 'to get directly out of the canoe' : *hinołta* 'to come out of the canoe' < empty stem *hina-* + **-włta*); *naẏaq-* 'baby', absolutive *naẏaq-ak* : *na·ni·q-a* 'lulling to sleep' < reduplicated **na·naiq-* = **na·nayq-*, probably a denominative verb based on an older form of stem underlying *naẏaq-*.

It is fairly obvious that in cases of this sort we are dealing with divergent developments of a single element under differing phonetic conditions. To understand these developments we must take account of the fact that the sequence vowel + ʔ + vowel, when the first vowel is in the second or a following syllable of the word, contracts to a long vowel, which may then be secondarily shortened. The rules of contraction are quite complex as to detail and a few examples must suffice: *ma-* 'to dwell' + *'-as* 'on the ground' yields *maʔas* 'tribe', but its plural, with reduplication and lengthening *t*-infix, is *ma·tma·s* 'tribes'; *ƛa-* + *'-akƛi* 'at the rear' yields *ƛaʔakƛi* 'the (stick) is standing at the rear', but *ḥayo* 'ten' + *'-akƛi* yields *ḥaya·kƛi* 'having ten at the rear', distributive *ḥahayakƛi*; *ti-* 'boulder' + *'-akƛi* = *tiʔakƛi* 'boulder at the rear', but *-'akƛi* + inherently possessive *'-at* = *'-akƛat*, e.g. *ya·k̓ʷakƛat* 'having one's buttocks sore'. If, now, we examine cases like postconsonantal *-n̓okʷ-* : postvocalic *-nkʷ-*, we shall be led to surmise that these divergent forms are independent reflexes of a basic *-ʔvnokʷ-* (in which v means *a*, *i*, or *o*); that *-ʔv-* contracted with preceding *a*, *i* or *o* to *-v·-*, whence frequently shortened *-v-*, which united with following *-no-* to *-in-*, i.e. *-inⁱ-*; and that, on the other hand, when v was itself elided, the ʔ, inasmuch as it could not be absorbed by the preceding syllable-ending consonant, coalesced with the following *n* to *n̓*. Thus, **ƛi̓ƛis̓ʔanokʷ* (assuming that v = *a*) > **ƛi̓ƛis̓ʔnokʷ* > *ƛi̓ƛisn̓ok*, but **titiʔanokʷima* > **tita·nokʷima*, shortened (because of reduplicated form of word?) to **titanokʷima* > *titinⁱkomⁱ*. Similarly, an old **naʔayaq-* > **naʔyaq-* > *naẏaq-*, but *na·naʔayaq-* > **na·na·yaq-* > **na·nayq-* > *na·ni·q-*. In other words, the alternations *n* : *n̓*, *m* : *m̓*, *y* : *ẏ*, *w* : *ẇ*, in non-initial position, are correlates of such syllabic alternations as *maʔas* : *-ma·s*. If we may generalize from these cases and from those due to 'hardening', it would appear that many, eventually perhaps all, examples of glottalized voiced continuants in Nootka in medial position are due to glottal absorption, the ʔ responsible for this process either preceding or following the continuant (e.g. *-ʔn-* > *-n̓-*; *-nʔh-* > *-n̓-*).

11. A careful scrutiny of the Kwakiutl and Nootka lexical materials would undoubtedly yield confirmatory comparative evidence.

One striking instance is Kwakiutl '-*aʔano·*[8] 'rope, line' (e.g. *səq-aʔano·* 'harpoon line') : Nootka *-aṅo-ł*, *-a·ṅo-ł* [L] 'all along, on a long thing' (durative), *-aṅo·-ƛ*, *-a·ṅo·-ƛ* (momentaneous), often *-aṅo·-*, *-a·ṅo·-* in combinations (e.g. *hi·n-a·ṅo-ḥsim* 'whaling spear' < *hin-* empty stem + *-a·ṅo·-* [L] + '*-aḥs* 'in the canoe' + *-im* 'thing'). Here the **-aʔano·-*, **-a·ʔano·-* required by theory to explain Nootka *-aṅo·-*, *-a·ṅo·-* is directly given by its Kwakiutl cognate, *-aʔano·*. We do not know enough about Wakashan phonology as yet to explain why absorption took place in Nootka but not in Kwakiutl. Apparently contraction processes of various sorts were more far-reaching in Nootka than in Kwakiutl.

Nevertheless, there seem to be a few examples in Kwakiutl too of glottalized voiced continuants in medial position resulting from a coalescence of ʔ with following *y*, *w*, *m*, *n*, *l*. Thus *ʔaʔams* 'bad luck, defiled' : *ʔaṁe·ła* 'to spoil, to make a mistake' < **ʔaʔams-* + '*-ła* (*-y-*, 'softened' from *-s-*, vocalized to *-e·-* before consonant); *ʔaẏo·s-əla* (durative) 'to understand' < **ʔaʔayo·s-* with primary reduplication : regularly reduplicated *ʔaʔayo·ċ-a* 'to try to understand' < unreduplicated base *ʔayo·s-* + '*-a* [R] 'to endeavor to . . .'; *ʔaẏaso·* 'hand' with primary reduplication[9] < **ʔaʔayaso·* (cf. Nootka *kʷikʷinkso* above); *ʔaẇał-iłɛ·la* 'to walk about searching for something' < **ʔaʔawał-* (reduplicated from **ʔawał-* ?; cf. reduced form of stem in *ʔo·ł-əla* 'to turn a corner' < **ʔauł-*) + *-iłɛ·la* 'about' (this suffix, though not regularly reduplicating, seems to favor reduplicated forms, e.g. *do·dəqʷiłɛ·la* 'to look about' : *do·qʷ-* 'to see'; note weakening of *do·qʷ-* to *-dəqʷ-* as of **ʔawał-* to **-ʔwał-*); *ʔaṅa·ḳ* 'enough', reduplicated from **ʔaʔana·-ḳ* ? (cf. Nootka *ʔana* 'only' ?).

In view of the possibilities of dialectic development of glottalized voiced continuants, it is not surprising that it sometimes happens that Nootka has what seems to be a permanent glottalization where Kwakiutl shows the more archaic alternation. A case in point is Nootka *ḥaṅaḥ* 'naked', corresponding to Kwakiutl *x̣aṅa·-la* 'naked', which point to a Wakashan **x̣aṅax̣*, **x̣aṅa·-*. So far as Nootka is concerned, the *ṅ* is a primary phoneme, but Kwakiutl *x̣ən-x̣ʔi·d* (momentaneous) 'to undress' suggests that here too *ṅ* is secondary.

In Kwakiutl an enormous number of secondary cases of *ẏ* and *ẇ*

[8] Boas writes *-aanō* (op. cit. 511) but all cases of Kwakiutl v- and -vv$_1$- are to be interpreted as ʔv- and -vʔv$_1$-. Smooth vowel initials and combinations of vowel and smooth vowel are impossible in Kwakiutl and Nootka.

[9] Such primary reduplications occur with other Kwakiutl nouns for paired body-parts, e.g. eye, ear, foot, kidney.

arise when the vowels *e·* (*ɛ·* < *aya*) and *o·* (*ɔ·* < *awa*) are resolved into *ay* and *aw* respectively and these heterosyllabic groups are then 'hardened' to *aẏ* and *aẇ*. Thus, *ʔa·ẇaqe·ʔ* 'the place between, inside' < *ʔo·*- stem of location + '-*aq*- 'among' + noun-forming -*e·ʔ*; *na·naqaẇa* 'to try to meet' < *na·qo·* 'to meet' + '-*a* [R] 'to endeavor to . . .'; *ca·caẏa* 'to try to draw water' < *cɛ·*(< **cay-a*) 'to draw water' + '-*a* [R]. Of greater interest to us is a group of cases, equally numerous, in which *e·ʔ* and *o·ʔ* are resolved before vowels into *aẏ* and *aẇ*. Thus, *nɔ·qaẏas* 'his mind' < *nɔ·qe·ʔ* 'mind' + -*as* 'his'; *λəẇe·s* 'and his' < *λo·ʔ* 'and' + -*e·s* 'his'; *ʔaẏəlkʷ* 'attendants' < **ʔe·ʔəlkʷ*, reduplicated plural of *ʔəlkʷ* 'attendant'. And, further, secondarily labialized guttural and velar spirants (*xʷ*, *x̣ʷ*) develop *ẇ* because of immediately following *ʔ*, e.g. *bo·xẇi·d* 'to leave' < *bo·*- + momentaneous -*x̣ʔi·d*.

12. It is hardly to be expected that we should have reflexes initially of an old alternation in Wakashan between glottalized voiced continuant and *ʔ* + vowel + voiced continuant, for both Nootka and Kwakiutl are non-prefixing languages and, as these alternations, if present, could not easily develop systematic morphophonemic significance (except perhaps in connection with reduplication), they would inevitably tend to be ironed out by analogy. Nevertheless there are a few cases which are suggestive, though obscure. Nootka *ẏaq*- 'long' may be interpreted as reduced from an old *ʔaya*- 'much' + **-q*- 'in length (?)' (this hypothetical -*q*- is perhaps preserved in Nootka -*q*-*ʔičḥ* '. . . year, for . . . many years' : -*ʔi·čḥ*, '-*ičḥ*, '-*i·čḥ* 'season of . . .'). Much clearer is dialectic (Ucluelet and N. Nootka) *ṅi·c*, *ṅi·c*-*ʔis* 'short', which is synonymous with Tsishaath[10] *ʔa·ne*-*ʔis*. -*ʔis* is diminutive; *ʔa·ne*- is umlauted from *ʔa·na*-, based on *ʔana* 'only that; thus much, thus many', 'short' in effect meaning 'diminutively thus much (in length)' Now the older form of -*ʔis* is *-*ʔic*, as is shown by the compounded -*ʔiċ*-*aλ* (diminutive + '-*aλ* 'now, then') and this older form is preserved in *ṅi·c* < **ṅaic* < **ʔana*-*ʔic*, a parallel form to early **ʔa·na*-*ʔic*; *ṅi·c*-*ʔis* is probably a later pleonastic form, with double diminutive. A further example is *ṅama*- 'only', probably related to *ʔana*- (see above). The element -*ma*- is probably a variant of -*ṁa*, -*ṁa·* '. . . far off, as far as . . . ; . . . in quantity, degree', an original **ʔana*-*ʔama*- 'only to that degree', after contracting to **ʔanama*- (see above), still further reducing to *ṅama*-. The verb *ẇaqʔo·* (durative and momentaneous) 'to go to a feast in response to an invitation', combining form *ẇaqʔo*-*q*-, is probably reduced from an irregularly

[10] Which we take as our basic Nootka dialect.

reduplicated *ʔ*awa-q-ʔauq-*, *ʔ*awaqʔaw-a*. If this analysis is allowed, an etymology is suggested which may or may not be true. With reduplicated *ẇa-..-ʔo·* compare *ẇa-čk-* (*ẇa-šk-*, *ẇi-nčk-*) 'living beings bunched together' and with reduplicating *-q-* (..*-q-*) compare *-q* [R] 'traveling in ... vehicle, canoe'; in other words, *ẇaqʔo·* would properly mean 'to come (as invited guest) in a canoe, crowded with one's people', which fits the cultural requirements.

Turning to Kwakiutl, we have two striking examples of *ẇ-* alternating with *ʔ-* + vowel + *-w-*. These are *ẇa·la-s* 'large' (sg.) : *ʔawo·* 'great' (pl.) (also with suffixes, e.g. *ẇa·la-ʔas* 'distance' but *ʔawo·-ẓəm* 'great tribes'); and *ẇa-* 'size, measure' (sg.) : *ʔawa-*, *ʔawɔ·-* (pl.) (only with suffixes, e.g. *ẇa·-x̣a-ċo·* 'measure inside' : *awɔ·-x̣a-ċo·* 'measures inside'). These common and important words seem to point to original **ʔawá·la-* : **ʔáwaw* and **ʔawá-* : **ʔáwa-*, **ʔáwawa-* respectively. A probable example of *ṁ-* : *ʔam-* in Kwakiutl is *ʔama-* 'small' (pl.), also an extended form, apparently, *ʔamay-* (*ʔame·x̣ʔid* 'to become small' < **ʔamai-*; *ʔamaʔinx̣-e·ʔ* 'youngest child' = *amaẏ-ənx̣-e·ʔ* 'the youngest in season, time'?; *ʔama·ẏace·* 'fifth child') : Koskimo (Kw. dialect) *-ṁən* 'young of an animal' (= N. *-ṁit*, *-ṁi·t* 'son of ...'), Kw. *-ṁəne·x̣ʷ* 'small' (pl.) (= N. *-ṁinⁱḥ* plural suffix; *-ḥ*, *-i·ḥ* is a common plural suffix in Nootka, Kw. *-e·x̣ʷ* probably a survival of this element, hence N. *-ṁinⁱḥ* probably extended in meaning from 'small ones').

13. Still further examples of the probable emergence of initial glottalized voiced continuants appear when we compare Kwakiutl and Nootka. There are enough examples of the correspondence in the two languages of these phonemes in initial position to make it reasonably certain that the glottalized voiced continuants had become phonemic in character in the Wakashan period or, at the least, that *ʔ* + *y*, *w*, *m*, *n*, *l* were allowable initial consonant clusters at that time; e.g., besides Nootka *ṅop-* 'one' : Kwakiutl *ṅəm-*, already quoted, may be noted : N. *ẏak-* 'in view, peering out; having one's neck stretched' < **ẏaxʷ-* (perhaps related to suffixed *-ẏo·č* [L] 'extending out, in view' < **-ẏaxʷ-ḳ*) : Kw. *ẏəxʷ-a* 'land looms up'; N. *ẏas-*, *ẏas-x̣-*, *ẏas-x̣ʷ-* 'opened out, having the legs spread out' : Kw. *ẏɩł-* 'to spread the legs' (perhaps < **ẏəy-ł-*, 'softened' from **ẏəs-*); N. *ṁina·ł-i* 'fishing bank' : Kw. *ṁəna·la* 'fish gather at mouth of river'; N. *ṁokʷ-* 'stone' : Kw. *ṁəkʷ-* 'a round thing is somewhere' (e.g. *ṁəkʷ-ayind* 'to put [a stone] on top'); N. *ṁačk-* 'having the jaws closed' < **ṁaḳx̣ʷ-* : Kw. *ṁəkʷ-*, *ṁəḳ-əx̣ɔ·* 'to choke'; N. *ṁoš-* 'closed', *ṁoš* 'fish weir' < **ṁəxʷ-x̣* :

Kw. *ṁəẇ-a* 'salmon weir' < **ṁəxʷ-* + *'-a* 'on the rocks'; N. *ṁaẇ-a·* 'delivering, taking a thing to its destination' : Kw. *ṁaxʷ-* 'to carry property'; N. *ṁay-ink-šiλ* (momentaneous) 'dancers join in completing a circle' (*-ink* 'together') < **ṁal-*[11] : Kw. *ṁəl-* 'to plait a rope, to take a turn on a trail'; N. *ṁoqʷ-* 'phosphorescent, glowing' < **ṁəl-qʷ-* : Kw. *ṁəl-* 'white'; N. *ṅa·-s* 'daylight' : Kw. *ṅa·-la* 'day, light'; N. *ṅoːaq-* < **ṅəq̇ʷ-aq-* (for *'-aq-* cf. *'-aqλ* 'in, into') 'to swallow' : Kw. *ṅaqʷ-* 'to swallow'.

But by no means all cases of *ẏ-*, *ẇ-*, *ṁ-*, *ṅ-*, *l̇-* which appear in these languages can be as archaic as the examples that we have just given presumably are. Some of them are secondary or point to an old Wakashan alternation of type *ẏ-* : *ʔay-*. Such are: N. *ṁisk-* 'dull, without power' < **ṁəsxʷ-* < **ʔməsxʷ-* : Kw. *ʔaʔo·ms* 'man of ordinary power' < reduplicated **ʔaʔaums* (< **ʔaʔaməsxʷ* ?); N. *ṁa-* 'holding in the teeth, in the mouth', *ṁač-* 'to close the teeth', *ṁačk-* 'having the jaws closed', Kw. *ṁəkʷ-* 'to choke' (see above) : Kw. *ʔam-* 'closed up, tight'; N. *ṅoš-* 'to distribute property in a potlatch (after the more important gifts have been made to chiefs)' < **ʔnəxʷ-x̣-* : Kw. *ʔane·xʷ-sʔɔ·-ʔe·* 'what is left over' (cf. *-sʔɔ·-la* 'deserted'); N. *ṅeʔiλ-* 'lit up, light' < **ʔnaʔ-* : Kw. *ʔanʔq-a* 'to light fire, charcoal' (this alternation is likely to be old, cf. Kw. *ṅaqŭ-la* 'light' and N. *ṅa·-s* above); perhaps also N. *ʔana, ṅa-ma-* 'only' (see above) : Kw. *ṅa·xŭ-la* 'alone' (related to Kw. *ṅa·xʷ-* 'all' ?).

14. There are a number of Kwakiutl words in *ha-* which look as if they were irregularly reduplicated forms. As this *ha-* occurs rather more frequently before *ẏ, ẇ, ṁ, ṅ, l̇* than pure chance would render likely, in view of the relative infrequency of these phonemes, we are confronted by the possibility that another source of the glottalized voiced continuants in Kwakiutl is *h* + voiced continuant. Early **hya-* would yield *ẏa-*, according to this theory, and early reduplicated **hahya-* would yield *haẏa-*; later on *ẏa-* would of course reduplicate to *ẏaẏa-*. Examples in point may be: *haẏəxʷ-ano·ma* 'to come to dance' < **hahyəxʷ-* (see below for Nootka confirmation) : *ẏəxʷ-* 'to dance'; *haẏano·* 'round-headed club' < **hahyano·* ?; *haẏamo·t* 'mark, sign' < **hahya-mo·t* (*-mo·t, -mu·t, -mut* 'remains of . . .' reduplicates, e.g. *x̣ʷa·x̣ul-mu·t* 'what is left over from cutting salmon' : *x̣ʷa·λ-a* 'to cut salmon', hence *haẏa-mo·t* is self-defined as redupli-

[11] There is no properly phonemic *l* in Nootka. Wakashan *l* almost certainly developed to Nootka *y*; cf. 'hardening' of Nootka *l* to *ẏ* (*ẇ*), 'softening' to *y* (*w*), as against Kwakiutl *l̇* and *l* respectively.

cated); *hawi·nał-əla* 'to frighten away' (plur.) < **hahwi·nał-əla* : *wi·nał-əla* (sing.) [read *wi·nałəla* ?]; *hamo·* 'pigeon' < **hahmo·* (for similar reduplicated animal name cf. *ho·mho·m* 'blue grouse'); *haməlq̆ŭ-la* 'to remind' < **hahməlq̆ʷ-* [read *haməlq̆ŭ-* ?] : *məlq̆ŭ-la* 'to remember'; *hanakʷ-e·la* 'to do quickly' < **hahnakʷ-* = **hah-nakʷ-* ? (cf. *ha-* 'quickly'); *hanas-x̣aw-e·ʔ* 'collar-bone of porpoise' < **hahnas-x̣aw-* (*-x̣aw-* = *-x̣o·* 'neck' ? cf. *ʔo·-x̣a·w-e·ʔ* 'neck'); *hałła-ba-la* 'quickly' < **hahla-* (*hala-*, *hala·-* of many derivatives, e.g. *hala·-q̣a* 'go away!', *hala-ḳa·la* 'to tell to hurry', *hala·-x̣ʷa* 'to eat quickly', probably represents the unreduplicated form); *hałła·-la* 'to hesitate' < **hahla·-* (probably based on an old **hala·-*, **hala-* > N. *haya·-ʔak*, *hayi-mⁱḥ* 'to be ignorant'). An example of *hay-* : *ẏ-* (<*hy-*) is *hayo·t* 'rival' (< *haya-* 'to go along' [?] in *haya·qa* 'to pass', *haye·q̣i* 'to imitate', *hayo·łła·la* 'to bring out of woods' + *-o·t* 'fellow') : *ẏa-q̣as* 'woman friend' < **hya-* (reduced from *haya-*) + *-q̣as* 'woman'. Not all of these examples are certain but several are highly suggestive. The cluster **-hm̓-* would naturally become *-m̓-*; an example is *he·m̓a-ʔo·mas* 'kinds of food' < plural-reduplicated **he·hm̓a-* : *ham̓(a)-* 'to eat'.

There are also cases in Kwakiutl in which *ha-* is a formative element (reduplicating syllable?) before non-glottalized voiced continuant (e.g. *hamanxŭ-łał* 'smiling dance' : *manxʷ-* 'to smile'; *hawa·x̣ʔ-əla* 'to beg, to pray' : *wa·x̣ʔ-* 'to have mercy'; perhaps also *hawa·ḳas* 'great, dreadful', *hamane·kʷ-a* 'to be dazed', *hamase·lalis* 'grebe', *hane·naxʷ* 'to desire to go in company'). Some of these may be cases of early (aspirate) or late (glottal) dissimilation, e.g. *wa·x̣ʔ-* < **w̓a·x̣ʔ-* < **hwa·x̣ʔ-*?; *hane·naxʷ* < **hahnaihnaxʷ*? Cases of *hay-* (e.g. *hayałilaq̣as* 'invisible spirit', *hayałcama* 'to keep secret', *haya·x̓o·-la* 'to warn' : *ya·x̓ɔ·* 'take care') are likely to be dissimilated from **hah-* (cf. Nootka *hiyił* for **hihił*, distributive of *hił* 'there').

15. In Nootka **hy-*, **hw-*, **hm-*, **hn-* did not become glottalized voiced continuants but simply dropped the *h-*. This is not only suggested by the absence of forms of type *hay-* which could be explained as developed from type *hahy-* but is directly indicated by the verbs *weʔič* 'sleeping' (sg.), momentaneous *weʔičo-x̓* : *ho·ʔič* 'sleeping' (pl.) < **waʔiḳo* : **hawʔiḳo* < **hwaʔiḳo* : reduplicated **hahw(a)ʔiḳo*. This interpretation, curiously enough, is confirmed by the baby word for 'sleep', *ho·š*, which reconstructs to a **hauẋ* that must have been the old simplification, in baby talk, of **hawaʔiḳo*, the prototype of reduced **hwaʔiḳo*. There are also cases of Kwakiutl glottalized voiced continuant which seem to be in relation to Nootka forms with *h-*,

e.g. Kw. *ẏəxʷ-* 'to dance', *haẏəxʷ-ano·ma* 'to come to dance' (see above) : N. *ho·ya·ł* 'dancing', an irregular reduplicated durative, < **hohyaxł* < **həxʷhyax(ʷ)-la* (there are several such cases of N. durative *-ł* < voiceless spirant + Wakashan durative *-la*, preserved in Kwakiutl but lost as such in Nootka), from which was then abstracted the stem form **həxʷł-* > *hoł-* (e.g. *hoł-ma·s* 'going from house to house dancing'); Kw. *ẏa* 'oh!', *ẏo·* (call from a distance), *ẏɛ·* (exclamation of disgust) < **hya*, **hyawa*, **hyaya*, reduced from exclamations of type **haya*, which represents a well-nigh universal pattern : N. *hay* (shout used in various rituals; interjection to attract attention), *ha·yi* 'I told you so!' (if this is correct, N. *ẏo·ẏo·-waʔa·ł* 'welcoming one heartily', which looks like an iterative of *-wa* 'to say . . .', hence < 'to keep saying *ẏo·*', is based on a ceremonial *ẏo·* borrowed from Kwakiutl *ẏɔ·*); Kw. *ẃŭn-* 'to hide' < **hwun-* < **hwəmn-* : N. *hopt-* 'in hiding' < **homn-* < **hwəmn-*; Kw. *ẃat-* 'kelp' < **hwat-* : N. *hos-min*, *hoc-smin* 'kelp' < **hot-smin* < **hwət-sma-*; Kw. *ẃa·l-* 'to stop' < **hwa·l-* : N. *hawi-ł-*, *hawi·-*, momentaneous *hawi·-ʎ* 'to stop' < **hawa·y-* < **hawa·ł-*.

Aside from cases such as these, in which *h* was preserved before a vowel or before an *o* which resulted from *w* + reduced vowel, *h* was bound to disappear, as in *weʔič* above. Thus, we may deduce from correspondences of type Kw. *ẏ* : N. *y* an older (possibly Wakashan) *hy*. Examples are: Kw. *ẃe·-* 'how, where' < **ẃay-* < **hway-* ('softened' from **hwas-* ?) : N. *wa·s-*, *wa·s-t-*, *wa·s-a-*, *wa·s-i* 'where?', *wa·y-aq-* 'which?' < **hwa·s-* ('softened' to **hwa·y-*); Kw. *ẃc·ḳ-* 'to carry long, stiff thing (firewood, pole) on shoulder' < **hwaiḳ-* : N. *wača-* 'piled up firewood' < **hwaḳa-*; Kw. *ẃas-* 'dog' < **hwas-* : N. *wa·win* 'hunting deer in the manner of wolves, employing wolf howls to scare out the deer', contracted < **hwa-hwa-ʔin* (**hwahwaẏ-in* ?) (originally 'making the sound of dogs') < **hwa(s)-* + *'-in* [R] 'making the sound of . . .'; Kw. *m̀aʔo·s-* 'to work' < **hmaʔau-s-* : N. *mamo-* 'working' < **hmahmaw-*. This type of correspondence applies also in medial position, e.g. Kw. *haṅakʷ-e·la* 'to do quickly' < **hahnakʷ-* (see above) : N. *nawit* 'to do, finish, quickly' < **hnaxʷ-* (**hnakʷ-* ?) + *'-it*; Kw. *x̣aẃe·* 'loon' : N. *ḥa·wi* 'small variety of loon' < **x̣ahwi*. An original *hn-* can also be inferred for Nootka when it has *n-* corresponding to Kwakiutl *han-*, e.g. N. *naʔo·-*, *naʔo-* 'accompanying, following' < **hnaʔaw-*('softened' from **hnaʔaxʷ-* ?) : Kw. *hane·naxʷ* 'to desire to go in company' (see above); N. *ni-* 'hollow object, container', *ni-čiʎ* 'to carry in pack-basket' < **hni-* : Kw. *han-* 'an open vessel is somewhere' (sg.), *han-x̣ʎa·-la* 'kettle'.

As syllabically final ʔ or glottalized consonant is not possible in Nootka, certain cases of initial glottalized voiced continuant in Nootka are likely to be due to absorption of such a glottal element. A clear case is N. *ṁo-* 'burning; setting on fire' < **ṁəl-* < **məʔl-* or **məlʔ-* : Kw. *məl̓-* 'to light a fire', *mal̓-eˑʔ* 'torch'. There are, however, also a number of cases of glottalized voiced continuant in Nootka which correspond to an unglottalized consonant in Kwakiutl, e.g. N. *ṅowi-*, *ṅowi-c-*, *ṅowiˑ-qso* 'father' (*ṅowiˑ-* 'softened' from **ṅowas-* ?) : Kw. *noˑmas* 'old man' (for N. *-w-* : Kw. *-m-* cf. perhaps also N. *hawa-* 'to eat' : Kw. *hamṅ(a)-* 'to eat' alternating with *ham-* in certain derivatives); N. *ṅiƛ-* 'supine', momentaneous *ṅiƛ-šiƛ* 'to lean, fall, back' : Kw. *nəƛ-a* 'to lie on back'; N. *ẏoˑ-qʷaˑ* 'likewise' (probably compounded of a demonstrative *ẏoˑ-* and *qʷaˑ* 'thus, such, so') : Kw. *yuˑ* 'that near thee'. The reason for this divergence is far from clear. In at least one case Nootka *ṅ-* may go back to *ˀn-* (ˀ is a laryngeated glottal stop, regularly equivalent to Kwakiutl *q̓*): *ṅi-* (durative *ṅi-yaˑ*), *ṅiq-* (durative *ṅiq-aˑ*) 'to sew' < **ˀni-*, **ˀniq-* < **q̓ni-*, **q̓niq-* : Kw. *q̓ən-*, *q̓an-* 'to sew' (this relation would seem to be parallel to that of N. *ni-* : Kw. *han-*, see above). Other, Kwakiutl, examples of alternation between glottalized and unglottalized consonants, in initial position, are: *moˑ-* 'to pile up', *maˑw-a* 'to move', *moˑ-x̣s* 'to load canoe' (cf. N. *maw-iqs* 'covered box' < 'box for storage of goods') : *ṁəw-eˑs* 'heap on beach' (but plur. *məx̣məw-eˑs*), *ṁəmw-aˑla* 'cargo of canoe' (cf. probably also Kw. *ṁaxw-* 'to carry property' : N. *ṁawa-* above); *ẏoˑ-ẏa* 'cold wind' : *yoˑxʷ-* 'wind', absolute *yɔˑla* < **yaw-ala*, *yaˑw-ap-a* 'to set sail' (cf. N. *yo-*, durative *yoʔi*, 'wind blowing', *yox-ł-* 'to get blown by the wind'); *wŭl-* 'to stop, to arrest' : *waˑl-* 'to stop' (see above); *ṁəns-* 'to measure' : *məny-ayo* 'measure' < *məns-* + '*-ayo* 'instrument of . . .'.

16. The last example takes us back to our hypothesis (see 7 and note 7) that 'softening' in Kwakiutl and Nootka is due to the effect left behind by a former *-h-*. In that case an older **hməns-hayo* could have dissimilated to **mənshayo* > **mənzayo* > *mənyayo*. Again, there are many cases of instrumental and passive *-ẏoˑ*, *-ẏo* in Kwakiutl instead of normal '*-ayo*, '*-aˑyo*, e.g. Koskimo *nəl-ẏoˑ* 'song' < 'singing instrument'[12] (instead of expected **nəl̓aˑyo* < *nəl-* 'to sing' + '*-aˑyo*), *cɛˑla-ẏo* 'dipper', *təq̣o-ẏo* 'blown off by steam', Koskimo *łəm-ẏo* 'rope', *noˑ-ẏo* 'medicine put near back of pregnant woman' (: *nəxʷ-* 'near'); but also *q̓ən-yo* 'thread' < 'instrument of sewing' (**q̓ən-ẏo* dissimi-

[12] In Nootka too various types of songs are named with *-ẏak*, *-čak* 'instrument of . . .'.

lated to *q̇ənyo* ?), *degəm-yo* 'towel for face'. To Kw. '*-a·yo*, '*-ayo*, *-ẏo·*, *-ẏo* probably corresponds N. *-yo*, *-yo·* (after consonants), *-čo*, *-čo·* (after vowels) 'having been . . . -ed'. These various forms are best reconciled on the basis of a Wakashan **-hayo*, **-ha·yo*, alternating, with loss of *-a-*, with **-hyo·*, **-hyo* > Kw. *-ẏo·*, *-ẏo*, N. *-yo·*, *-yo*; N. *-čo·*, *-čo* < **-q̣o·*, **-q̣o* < **-q̣-hyo·*, **-q̣-hyo* with intercalated *-q̣-*, frequent in both Kwakiutl and Nootka (as *-č-*). But the Nootka instrumental, corresponding to Kwakiutl normal '*-ayo*, '*-a·yo*, is *-ẏak* (after consonants), *-čak* (after vowels) < **-ẏo-ʔak*, **-čo-ʔak* (< **-ḳo-* < **-q̣-ẏo-*). It looks, therefore, as though Wakashan, like Nootka, distinguished an instrumental **-ʔayo*, **-ʔyo* (**-ʔhayo*, **-ʔhyo* ?) from a passive **-hayo*, **-hyo* and that the two formations merged in Kwakiutl because of the leveling of **-ʔyo* and **-hyo* to *-ẏo* and because of the leveling of **-ʔhayo* and **-hayo* in certain cases (e.g. **-n-ʔhayo* and **-n-hayo* > *-ṅ-ayo*). In this way would also be explained why a few important instrumental nouns, likely to be archaic formations, have 'hardening' '*-ayo* instead of the regular and far more common '*-ayo*, e.g. *ẗəmẏ-ayo* 'baton' (*ẗəms-* 'to beat time'), *se·ẇ-ayo* 'paddle' (*se·x̣ʷ-* 'to paddle').

17. We see, then, that these rather curious phonemes can be shown to go back to coalescences of ʔ or *h* with following or preceding *y*, *w*, *m*, *n*, *l*, also to coalescences of other consonants, such as *d*, with following voiced continuants. The details naturally differ for the different languages but all the cases here considered have this in common, that a relatively weak consonantal phoneme, instead of disappearing entirely when in a cluster with a sonorant consonant, is absorbed by the latter, so that new consonantal phonemes emerge. These new phonemes, characterized by glottal affection, tend at first to be in a morphophonemic relation with the simple sonorant consonants. In time, however, they tend to take on the status of isolated phonemes. It would be interesting to analyze the status of these consonants in Haida, Tsimshian, Yokuts, and other American Indian languages in which they occur to see if there too there is reason to think that they are phonemic emergents due to absorption. Essentially, consonants of this sort are on a par with the nasalized vowels of so many languages, the *n* or *m* originally following a vowel becoming absorbed in the vowel, whence a new set of phonemes results. The French nasalized vowels are, of course, the most familiar example. The methodologically interesting point is suggested by cases of this sort that if a language has two sets of phonemes, one of which, B, can be reasonably defined as identical with the other, A, except for a definite qualitative plus

which linguistic experience shows to be relatively infrequent, then the set B may be *suspected*, certainly not *assumed*, to have emerged from some type of absorption in the set A of, or from modification of the set A by, a phoneme (or group of phonemes) having something of the character of this qualitative plus. Entirely new phonemic categories, such as nasalization, glottalization, aspiration, rounding, palatalization, laryngealizing, emphasis, tonal distinctions, may thus arise as absorption products.

For the rest of this paper we should like to show how the phenomena we have considered for Navaho and, more particularly, for Nootka and Kwakiutl, may help us to understand certain Indo-European phonological developments, specifically in the prehistory of Greek. Assuming that Indo-European possessed four 'laryngeal' consonants, namely ' (a glottal stop followed by *e*-timbre of full grade vowel in its primary form), ˀ (another glottal phoneme followed by *a*-timbre of full grade vowel in its primary form), *x* (presumably a voiceless velar spirant = *ḫ*-, -*ḫḫ*- of Hittite), and *γ* (presumably a voiced velar spirant, Arabic 'ghain', = *ḫ*-, -*ḫ*- of Hittite), we have not the right to take it for granted that when a vowel dropped out between such a preceding 'laryngeal' and a following consonant, the laryngeal necessarily disappeared without a trace. To do this is to project back into the earliest period the feeling for vocalic syncope that developed in the later stages of many Indo-European dialects. If, for instance, Greek ἀμέλγω, ἀμολγή, with its 'prothetic' ἀ-, leads us, in accordance with the laryngeal hypothesis, to posit a base **xamelĝ-* (or rather **xamelAĝ-*[13]) or **ˀamelĝ-* (or rather **ˀamelAĝ-*), we must not implicitly abandon this same hypothesis when we deal with reduction products and allow the *x*- or ˀ- to disappear with the -*a*-. The fact that we have forms in *m*- (e.g. OIr. *bligim* < **mligim*, Goth. *miluks*, Toch. A *malke*) in the later dialects is far from justifying our dealing with an ablaut **xamel-* : **mel-* for the earliest period. We must either believe in our 'laryngeals' and speak of an ablaut **xamel-* : **xmel-* or discard them and deal only with the conventional **amel-* : **mel-*. It may well be that in the vast majority of cases such Indo-European sequences as **xm̥-*, **'w-*, **ˀy-*,

[13] We use '*A*' to indicate any one of the four laryngeal consonants when it is desired to speak in general terms or when we have reason to suspect a laryngeal consonant (in this case chiefly because of the 'Stosston' of Lithuanian *mélžu*) but have not enough evidence to determine which one is involved. Similarly, it is convenient often to use '*A̯*' for a laryngeal which must have been either ˀ or *x*, say because of the *a*-timbre of the following vowel, but between which we cannot decide. This is, as a matter of fact, a frequent contingency.

and *γl- simply became *m-*, *w-*, *y-*, and *l-*, or their appropriate reflexes, but we have no more right to assume this than to assume that, at a later period, the treatment of pre-Greek *sm-* was identical with that of *m-*. 'Losses' of elements are frequently fallacious because of our tendency not to look closely enough into the possibilities of absorption phenomena.

If, now, we posit an IE series *'y-*, *'w-*, *'m-*, *'n-*, *'l-*, *'r-*, a series *ʔy-*, *ʔw-*, *ʔm-*, *ʔn-*, *ʔl-*, *ʔr-*, and a series *xy-*, *xw-*, *xm-*, *xn-*, *xl-*, *xr-*, we have to inquire what happened to these clusters (initially and in other positions) in each of the main branches of IE. In the end it will undoubtedly prove far more economical of effort to assume little or nothing in the way of sweeping reductions of these, to us, uncomfortable clusters and to keep our eyes open for distinctive reflexes of them in the IE dialects than to oversimplify our task by assuming radical reductions in the IE period. Hittite *xwantes* 'winds' (*ḫu-u-wa-an-te-eš*) should warn us. Applying this principle to the Greek reflexes of this very base (IE **xawe'-*, conventionally **wē-*, 'to blow'), we are helped forward at once to a valuable hypothesis with regard to early Greek phonology. An old **xawe'-* : **xwe'-*, we may surmise, need not have developed an early Greek **awē-* : **wē-*, for there is no reason for certainty that a prevocalic *x-* would behave the same way as a *x-* caught in a consonantal cluster *xw-*. The actual course of the development may very well have been something like this:

1. **xawe'-* or **xəwe'-*[14] (tautosyllabic) : **xwe'-*
2. **hawe'-* (or **həwe'-*) : **hwe'-*
3. **hawe'-* (or **həwe'-*) : **ẇe'-*
4. **hawē-* (or **həwē-*) : **ẇē-*
5. **awē-* (or **əwe-*) : **ẇē-*
6. **awē-* : **hē-*

By *ẇ* is meant a *w* which had absorbed the preceding aspiration, therefore probably a voiceless *w*, but we do not claim to be able to say whether such a *ẇ* (stages 3–5) was a true phoneme or merely a consonant cluster. When it was in regular morphophonemic relation to *h* it may have been felt as a cluster, like *sk-* or *tl-*, but when many such

[14] 'ə' should be used by those who hold to the laryngeal hypothesis only for a real schwa, a simple murmur vowel reduced from a full grade short vowel. The so-called 'schwa indogermanicum', for which 'ə' is in general use, will have to yield to the series *ə'*, *əʔ*, *əx*, *əγ*.

irregular relations as *aw-* : *ṷ-* had developed, it is more likely to have constituted a true phoneme. The Greek forms which actualize this construction are, needless to say, ἄη-σι 'it blows' : αἴ-ν-ω 'I winnow' < **ṷə-n-yṓ* < IE **xwə(')-n-yó-A*.

Similarly, while it is easy to dispose of Greek ἕλκω, ὁλκός, ὁλκή, ὁλκάς by assuming an IE **selk-* and comparing with Latin *sulcus*, AS *sulh* 'plow', sound method requires that we refuse to disconnect ἕλκω from its uncomfortable Greek relatives. Just as we insisted on keeping ἄησι and αἴνω together, thereby arriving at a valuable hypothesis in regard to the treatment of IE *xw-* in early Greek, so here we may not disconnect ἕλκω from ἄλοξ, αὔλαξ, ὤλαξ, Hom. ὤλξ, 'furrow', Laconian εὐλάκᾱ 'plow'. When we deal with this series, we are driven to posit **Ạawelk-* as our base and the difficulty of equating ἕλκω with the obvious Lithuanian parallel *velkù* (OCS *vlěkǫ*) disappears. Greek *he-* : Lithuanian *ve-*, in other words, means IE **xwe-* or **ˀwe-* or **'we-* and the Greek words for 'plow' narrow the choice down, in spite of difficulties of detail, to an *a*-timbred laryngeal, hence base **Ạawelk-* = **xawelk-* or **ˀawelk-*. Again, ἕ-τερος, which may be etymologically distinct from its dialectic variant ἄ-τερος, is a difficult word and no satisfactory etymology has been given. If we define it as '(one or) the other' and connect with ἠέ 'or', Latin *aut-*, *ve-*, Skr. *vā*, we arrive at a base **Ạawe-*; **Ạwé-teros* (**xwe-* or **ˀwe-*) yields **hwé-teros* or **ˀwe-teros*, either of which, via voiceless *w*, results in *hé-teros*. The Latin ablaut *au-* : *-ve* is then seen to be in significant relation to the Greek *h-*. In this way, too, we are now free to hold to Gk. ἕσπερος, ἑσπέρᾱ : Latin *vesper* and Gk. ἑστίᾱ : Latin *Vesta* without difficulty. We cannot at present decide if IE *xw-*, *ˀw-* and *'w-* fell together in the pre-Greek period into, say, *hw-* or *'w-* or voiceless *w*, or simplified to *xw-* and *'w-* whence later *hw-*, or kept apart for a very long time. All we can say now is that the reflexes of IE *xw-*, *ˀw-* and *'w-* seem to be different from that of *w-* in Greek as we know it.

The same result is reached when we investigate the Greek reflexes of so-called IE *y-* (*i̯-*). Current theory states that this phoneme regularly yields Greek *h-* (e.g. ὅς : Skr. *yás* < IE **yó-s*) but that there are a number of strays (e.g. ζυγόν : Skr. *yugám*; ζέω : Skr. *yásāmi*; ζωστός : Lith. *jū́stas*) in which we have an 'irregular' ζ- instead of the 'regular' *h-*. Why should a voiced continuant (*y*) branch into a voiceless continuant (*h*) and a voiced affricate (*dz*) when nearly all the relevant analogies in Greek indicate that Greek *h* is of voiceless origin (e.g. < *s*) and that voiced consonants before vowels keep their

voice unless unvoiced by voiceless consonants that precede them (e.g. *dy-* > *dz-*; but *sw-* > *hw-* > *h-*)? It is only statistical evidence that leads to the assumption that *y-* > *h-* is the type development and *y-* > *dz-* the aberrant one. It would seem far more natural to suppose that *y-* regularly yielded *dz-* (cf. Lat. *mājor* > Italian *maggiore, jocus* > *giuoco*) but that a reflex *h-* is due to an unvoicing of the old *y-* by some voiceless consonant that has disappeared as such. The laryngeal hypothesis implies the possibility that, as with IE *w-*, we have here two distinct prototypes: IE *y-* and IE *'y-*, *ːy-*, *xy-*, *y'-*, *yː-*, *yx-*, the former of which led to *ζ-*, while clusters of the latter type eventually led to *h-*, perhaps via two distinct phonemes, *ẏ-* and *ẏ-*. In examples like *ζυγόν* and *ζέω* all the available evidence points to simple IE *y-*; in other words, there is nothing to show that IE **yewg-* and **yes-* are reduced from bases of type **'eyewg-* (**ːayewg-*, **xayewg-*) and **'eyes-* (**ːayes-*, **xayes-*). In Gk. *ὅς* : Skr. *yás*, however, we are not dealing with a primary **yó-s* but with a reduced form of the base **'eye/o-* (cf. Skr. *ayá-m*, Lat. *ea* < **'eya-Ạ*), hence properly **'yó-s*. Again, in Gk. *ἅγιος*: Skr. *yaj-*, it cannot be an original **yaĝ-* that we have to posit, for a full-grade *a* is itself defined by the laryngeal hypothesis as colored from *e* by a preceding *ː* or *x*, hence we must posit **yẠaĝ-*, i.e. **yːaĝ-* or **yxaĝ-*. At what time the *y-* of this base was glottally affected or unvoiced to *ẏ-* we do not know. Possibly there were IE forms of type **ẏaĝ-* < **yːaĝ-* long before later Greek ones of type **ẏo-* developed from **'yo-* (incidentally, now note an easy explanation of why Vedic Skr. *yaj-* reduces, not to *ij-*, but to *ījˉ* < **iːĝ-* or **ixĝ-* = theoretical **yːĝ-* or **yxĝ-*). Significantly enough, there seem to be no primary examples of Gk. *ζα-* < IE **ya-*. There was no IE **ya-* (there might, of course, be a **yaː-* or **yax-* > later **yā-*) but only **ye-*, **yo-*, **yə-*.

With *r-*, too, we can now see why our Greek reflexes are partly *er-* (e.g. *ἐρυθρός* : Skr. *rudhiráḥ*), partly *hr-* (*ῥ-*). A smooth *r-* developed a true prothetic vowel *e-* (not to be confused with the large class of pseudo-prothetic vowels which are the remnants of laryngeal + vowel, generally reduced, which originally began the base, e.g. Gk. *ὄνομα* : Lat. *nōmen* < **γənəγ-mən* : **γneγ-mən*, hence palatalized *ñ-* in Toch.A *ñom*), while the series *'r-*, *ːr-*, *xr-* eventuated in *ῥ-*, possibly via two distinct phonemes *ṙ-* and *ṙ-*. In this way it is easy to understand why preserved initial *r* always takes the rough breathing in Greek. This characteristic can hardly have been a spontaneous development of early Greek *r-* but may be due to the fact that the vast majority of

cases of initial *r-* in Greek as we have it are reduced from initials of type *'er-*, *ʔar-*, *xar-*, plus a later group of cases of type *sr-*. A case in point is ῥύζειν 'to growl, snarl' : Hom. ἐρυγόντα 'bellowing' < **'ruĝ-* : **'eruĝ-*.

Thus, the Greek history of IE *w-*, *y-*, *r-*, which, as generally presented, is full of unsolved problems, becomes a symmetrical and phonetically intelligible series of events from the standpoint of the laryngeal hypothesis:

IE	Pre-Greek	Greek
we-	*we-*	*we-* > *e-* (secondarily *ewe-* > *ee-*)
'we-, *ʔwe-*, *xwe-*	*ẇe-*, *w̥e-*	*he-*
ye-	*ye-*	*dze-* (perhaps via *dye-*)
'ye-, *ʔye-*, *xye-*	*ẏe-*, *ʿe-*	*he-*
re-	*re-*	*ere-*
're-, *ʔre-*, *xre*	*ṙe-*, *ṙe-*	*hre-*

It would be difficult to present the known facts in a simpler light. There will still be numerous refractory problems of detail, but it is not a bad test of the validity of a theory (the laryngeal hypothesis) which uses entirely different evidence for its establishment that it incidentally seems to set the house in order for the difficult phonology of IE '*w-*', '*y-*', and '*r-*' in Greek.

Undoubtedly there are still other types of absorption of the laryngeals in Greek, and elsewhere. It is very likely, for example, that Greek at one time had a true phoneme *ŋ* (not merely the assimilated *ŋ* of *-ŋg-*, *-ŋk-*, *-ŋkh-*, written -γγ-, -γκ-, -γχ-), for otherwise it would be hard to understand why γ was chosen to represent this sound instead of ν. If Greek γ were always of conditional origin, as in ἔγγαιος 'in the earth' < *én-gaios*, it would have been almost unavoidably written *n*, e.g. *ἔνγαιος. That the *ŋ* was regularly indicated by -γ-, an intrinsically poor symbol for the purpose, indicates that it may, like the parallel ν and μ, also have been a free phoneme. It is therefore possible that quite a number of words that are now considered as having a stopped *g* were in the earlier period pronounced with *ŋ*. If, further, we ask how such a phoneme could arise in Greek, we have not far to seek for an answer. Either IE *x* or IE γ might be expected to be absorbed in a preceding or following *m* or *n*, yielding *ŋ*. Just which, if any, of the clusters *xm*, *xn*, γ*m*, γ*n*, *mx*, *nx*, *m*γ, *n*γ actually yielded *ŋ*, and under what conditions, is a matter for detailed research. Two cases may be given for illustrative purposes.

The IE words for 'naked' are known to be difficult. Starting with Lith. *nū́gas*, we know at once, if we apply the principles of the laryngeal hypothesis, that there must have been a laryngeal consonant before the *-g-*, IE *-gʷ-*. This can only have been *-γ-*, the laryngeal which united with a preceding *e* to form a secondary *ō* in many (but not all) IE dialects, an altogether different entity from the lengthened grade of *o*. We have, then, **néγgʷos* as the primary form. Other dialects used various suffixes, with shift of stress and zero grade in the first syllable. One of these derivatives was **nγəgʷnós* > pre-Gk. **ŋəgʷnós* > **ŋugʷnós* > **ŋubnós* > *ŋumnós*, probably the early pronunciation of γυμνός. Another form of the *-nó-* derivative was, apparently, **nγegʷnós* > **nogʷnós* in those dialects (by no means all) in which IE *γe-* fell together with *o-*, hence Skr. *nagnáḥ*, Avestan *maγna-* (perhaps the curious divergence between Indic *n-* and Iranian *m-* is due to the fact that each represents an independent development of a *ŋ-* which either developed independently in Indo-Iranian or goes back, with Greek *γ-*, to an IE *ŋ-* < *nγ-*). The persistent *o*-timbre of the IE word for 'night' (Lat. *noct-*), undoubtedly, as Sturtevant has pointed out, related to 'naked', and the *υ* of Gk. *νυκτ-* are both of them characteristic traces of the *γ*-laryngeal (cf., e.g., Gk. ἀν-ώνυμος). We now understand why Hittite has *e* (not *a* < IE *o*) in the related words: *neku-mant-s* 'naked', *nekut-s* 'bed-time' (read *nekᵘ-mant-s*, *nekᵘt-s* ?), for IE *γe* did not yield Hittite *a* < *o*. Hit. *nekut-* : Lat. *noct-* is therefore not an example of *e* : *o* ablaut, something otherwise unknown for this group of words, but represents independent reflexes of IE **nγegʷ-t-* 'bed-time' > 'night'. Here, then, the assumption of an IE *nγ-* cluster (or, perhaps less likely, its early reflex, *ŋ-*, in which case Hit. *neku-* is < IE **ŋegʷ-* < **nγegʷ-*), so far from merely explaining Greek γυμνός, throws all the related IE forms into a more intelligible focus.

We may return to **xamelAĝ-* (or **ʔamelAĝ-*) 'to milk'. In most IE dialects its reduced parallels, **xmelAĝ-* (or **ʔmelAĝ-*) lost the initial laryngeal. But the form **xm(ə)ləAĝ-t* (or *-d*) 'milk' seems to have yielded **ŋ(ə)ləAk-t* in that dialect or dialect group which, for Greek, yielded **ŋalakt* > γάλα, γάλακτ-, and, for Latin, **ŋlakt* > *lac*, *lact-*. It is not necessary to suppose, should it later appear that IE *xm-* regularly yielded Latin *m-*, as is probable, that *lact-* is a proper Italic form. It may be a cultural loan-word from an IE dialect in which the indicated course of development was regular, as indeed may the Greek word. All we need to note is that, granted a base **xamelAĝ-* (rather than **ʔamelAĝ-* ?) on the basis of Gk. ἀμέλγω, ἀμολγή, we have a right to expect forms in **xmeleAĝ-* > **ŋeleAĝ-*.

SOME VERBAL CATEGORIES OF HOPI

B. L. WHORF

In the earlier stages of work on the Hopi language I had the pleasant feeling of being in familiar linguistic territory. Here, wondrous to relate, was an exotic language cut very much on the pattern of Indo-European; a language with clearly distinct nouns, verbs, and adjectives, with voices, aspects, tense-moods, and no outré categories, no gender-like classes based on shape of objects, no pronouns referring to tribal status, presence, absence, visibility, or invisibility.

But in course of time I found it was not all such plain sailing. The sentences I made up and submitted to my Hopi informant were usually wrong. At first the language seemed merely to be irregular. Later I found it was quite regular, in terms of its own patterns. After long study and continual scrapping of my pre-conceived ideas the true patterning emerged at last. I found the experience highly illuminating, not only in regard to Hopi but as bearing on the whole subject of grammatical categories and concepts. It happens that Hopi categories are just enough like Indo-European ones to give at first a deceptive impression of identity marred with distressing irregularities, and just enough different to afford, after they have been correctly determined, a new viewpoint toward the, on the whole, similar distinctions made in many modern and ancient Indo-European tongues. It was to me almost as enlightening to see English from the entirely new angle necessitated in order to translate it into Hopi as it was to discover the meanings of the Hopi forms themselves. This was notably true for the four types of verbal category herein discussed.

It will be well to outline first the following general distinctions:

(1) OVERT CATEGORY; one marked by a morpheme which appears in every sentence containing the category, vs. COVERT CATEGORY; not marked in sentences in general, but requiring a distinctive treatment in certain types of sentence, e.g. English genders.

(2) WORD CATEGORY; a category (overt or covert or mixed) which delimits one of a primary hierarchy of word classes each of limited membership (not coterminous with entire vocabulary), e.g. the familiar 'parts of speech' of Indo-European and many other languages, vs.

MODULUS CATEGORY; one which modifies, either any word of the vocabulary, or any word already allocated to a delimited class, e.g. voices, aspects, cases.

(3) SPECIFIC CATEGORY; an individual class of any of the above types, e.g. passive voice, durative aspect, vs. GENERIC CATEGORY; a higher hierarchy formed by grouping classes of similar or complementary types, e.g. voice, aspect.

The categories treated in this article are all of the overt and the modulus types, but it should be stated that covert categories and word categories are also of great importance in Hopi grammar. Failure to define such classes would at once give the grammar a very irregular appearance. The generic categories here treated belong to the verb system, and have been designated ASSERTION, MODE, STATUS, and MODALITY.

ASSERTION

Hopi verbs have three assertions: REPORTIVE (zero-form), EXPECTIVE (suffix *-ni*), NOMIC (suffix *-ŋʷï*). These translate, more or less, the English tenses[1]. But they do not refer to time or duration. They distinguish three different kinds of information. Assertion, in other words, is a classification that refers the statement to one of three distinct realms of validity. The reportive is simply a reporting statement, telling of the historical actuality of a certain situation, e.g. Eng. *he ran, he is running, I see it.*[2]

[1] In 'The Punctual and Segmentative Aspects of Verbs in Hopi', LANGUAGE 12. (1936), I referred to the assertions as tenses and called them factual or present-past, future, and generalized or usitative.

[2] There is no distinction in the reportive between past and present, for both are equally accomplished fact. What we call present tense (not counting our present form which corresponds to the nomic) is from the Hopi standpoint simply a report to others concerning a situation shared with them, this report being either redundant information, or used to call attention to, or tell about some fragment of the situation not fully shared. Thus to the Hopi 'he is running' need not be different from 'he was running', for if both the speaker and listener can see the runner then the 'is' of the former sentence means merely that the listener can see for himself what he is being told; he is being given redundant information, and this is the only difference from the latter sentence. Now the Hopi listener senses no lack in Hopi grammar for its not telling him that the information is redundant, when he can see for himself that it is. If the speaker can see the runner but the listener cannot, then the information is not redundant, but the situation in that case is one of rapidly relaying the information, which rules out the distinctive past meaning of 'was', and again the Hopi find our tense distinction irrelevant.

The expective declares an expectation or anticipation of a situation. It is translated by the English future, or by 'is going to', or by 'begins to', for an attitude may still be one of expectant anticipation, rather than one of reporting a fait accompli, while the action is already beginning. Since the assertions have nothing to do with time as such, an expectant attitude may be projected into an account of past events, giving the translations 'was going to', 'began to'. A clue to meanings otherwise obscure resides in the rendering 'his doing it is (or was) expected'. Thus, addition of the concursive mode suffix *-kaŋ* 'while' forms an expective concursive in *-nikaŋ*, but to translate here by the English future 'while he will do it' would be quite incorrect. The form means 'before he does it', i.e. 'while his doing it is expected'.[3]

The nomic does not declare any particular situation, but offers the statement as a general truth, e.g. English: *she writes poetry, he smokes only cigars, rain comes from the clouds, certain dinosaurs laid eggs in sand.* The three assertion categories are mutually exclusive.

Mode

Mode, in Hopi, is the generic category of the system by which is denoted the nature of the mingled discreteness and connection between a sentence (clause) and the sentence (clause) which follows or precedes it. The INDEPENDENT mode (zero-form) implies that the sentence is detached from others, though it is possible to relate such sentences by paratactic connectives like our 'and'. But the Hopi show a great liking for hypotactic constructions. These employ six mutually exclusive DEPENDENT MODES, denoted by suffixes placed after the assertion suffix. Each mode denotes a basic type of relatedness involving both linkage and discreteness, or disparity. With the further

[3] The orthography used for Hopi is phonemic and employs the American Anthropological Association symbols generally used for American Indian languages, with the following minor variations: *k* is somewhat fronted, and before *a* and *ɛ* sounds like k^{y}, *c* is the affricate *ts*, *ʔ* is glottal stop, *v* is bilabial and when syllable-final unvoiced, *r* is retroflex, untrilled, and slightly spirantal, and when syllable-final unvoiced, small capitals denote voiceless consonants which are separate phonemes, a dot under a vowel denotes the short, and lack of a mark the medium grade of the Hopi three-quantity vowel system, ′ ('high accent') denotes raised pitch and fairly strong stress and on monosyllabic words is not marked but to be understood, ˋ ('low accent') denotes a lower level of pitch and weak stress and is marked where it occurs on monosyllabic words. The mode suffixes and most of the particles have special pausal forms when they stand at the end of sentences; these are not given except for the suffix *-qö̱ʔ*, which is used in an example.

addition of qualifying particles, these modes can distinguish a great number of possible relationships, much as in Greek the basic system of three oblique cases can be further developed by a large number of prepositions. However, the mode relationships are not case relations, nor are the modes defective forms like IE infinitives and gerunds, but full verb paradigms.

The names, suffixes, and types of discreteness-connection are as follows: CONDITIONAL (*-ɛ?* eclipsing final vowel of base), condition needed to justify a non-reportive (expective or nomic) assertion in the other clause (Eng. *when, if*) (the mode clause is logically in this same assertion, though it does not bear any assertion suffix); CORRELATIVE (*-qạY*), explanatory justification of the statement of the other clause (Eng. *because, since, as, for*, gerund construction); CONCURSIVE (*kaŋ, -kakaŋ*), parallel contemporaneous occurrence (Eng. *while, as, and*); SEQUENTIAL (*-t*), sequence in time (Eng. *after, ... and then ...*); AGENTIVE (*-qa*), qualification of a person or thing in one clause as the subject in the other, the mode clause (Eng. *who, which*, though not Eng. *whom*); TRANSRELATIVE (*-q, -qö?*), general relatedness bridging a difference of subject in the two clauses (no Eng. equivalent). Each mode refers to a certain kind of disparity or contrast, as well as of connection, between the two clauses; and a separateness of subjects or performers in the two clauses is itself one basic type of disparity on a level with the other basic types, and necessitates the transrelative mode in one of the clauses. Hence all the other modes refer to conditions in which the subject of both clauses is the same, i.e. clause-contrast is based on other factors than disparity of subjects. If it is desired to add to the general notion of the transrelative mode an expression of factors like those which are basic in the other modes, this can be done by detached particles. The subject of the transrelative in many constructions and of the agentive in some is in the objective case. That of all other modes is in the nominative. There follow some illustrations of the uses of the modes. In these examples and henceforth throughout this paper the mode suffix is hyphenated to assist in identifying the dependent verb.

Conditional: *ni'm-ɛ? mï·'nat tïwạ'ni* 'when he goes home he will see the river' (expective); *ni'm-ɛ? mï·'nat tïwạ'ŋʷï* 'when he goes home he sees the river' (nomic).

Correlative: *ni'ma-qạ'Y mï·'nat tị̈'wa* 'because he went home he saw the river'; *ni'ma mï·'nat tïwạ'ni-qạ'Y* 'he went home in order to see the

river', i.e. 'he went home because his seeing the river was expected, looked forward to (by him'); *pạ'Ŋqa'W-qaY ya'w mï·'nat tị̈'wa* 'he said that he saw the river' (by his saying, to quote, he saw the river').

Concursive: *ni'ma-ka'ŋ mï·'nat tị̈'wa* 'as (or while) he went home he saw the river'; *wïni'ma-ka'ŋ ta·'wla'wï* 'he danced and sang' (at the same time).

Sequential: *ki·y ʔaw pitị̈'-t mï·'nat tị̈'wa* 'after (or, when) he arrived at his home he saw the river'; *wïni'ma'-t pị̈'ʔ ta·'wla'wï* 'he danced and then he sang'.

Agentive: *ta·'qa ni'ma'-qa mï·'nat tị̈'wa* 'the man who went home saw the river'; *nïʔ tị̈'wa ta·'qat ni'ma-qa'-t* 'I saw the man who went home' (the agentive takes noun cases and here is in objective case, as likewise its subject).[4]

Transrelative: *ni'ma-q ta·'qa ʔaw pitị̈'ni* 'when he goes home a man will meet ('arrive to') him'; *ni'ma-q mọ'ŋʷi ʔaw pitị̈'ŋʷï* 'when he goes home the chief meets him' (nomic); *ti'yo wïni'ma-q ʔo·'viy ma·'na ta·'wla'wï* 'since the boy danced, therefore the girl sang'; *pạ'Ŋqa W-q ya'w ma·'na ni'ma* 'he said that the girl went home'; *na·t ta·'wlaW-q ma·'na wïni'ma* 'while he sang the girl danced'; *pạ'nis wïni'ma'-q pị̈'ʔ ma·'na ta·'wla'wï* 'right after he (or, as soon as he had) danced the girl sang'; *ʔạ'son ni'ma-q mọ'ŋʷi ʔaw pitị̈'ni* 'after he goes home the chief will meet him'.

Our relative clause with relative pronoun object is transrelative in Hopi, since there is disparity of subject; e.g. *ta·'qat nïʔ tïwạ'-q ni'ma* 'the man whom I saw went home' ('man', objective dependent on 'my seeing', 'he went home'). Hopi treats 'man' as the object of the seeing clause, while the subject of the going-home clause is 'he' expressed in the verb. English makes very little distinction between this construction and the one expressed in Hopi by the agentive, often using the same connective, 'that' or 'which', for both, so that bewildering changes of construction may result, in translating a number of outwardly similar relative clauses into Hopi. These changes however are perfectly transparent to a Hopi; even a bilingual Hopi when given the two propositions dressed in the same garb of English form, 'the man that I saw went home' and 'the man that saw me went home' will instantly react with two completely dissimilar patterns: *ta·'qat nïʔ tïwạ'-q ni'ma*, and *ta·'qa nïy tïwa''qa ni'ma*. Hopi also produces transrelative forms

[4] This objective-case agentive clause is the one exception to the rule that disparity of subject requires the transrelative.

that translate our relative pronoun object of a preposition; and here the remoteness from our pattern is extreme, e.g. *yamạ'kpit ʔạŋ wa·'yma-q lɛ''pɛ* 'the bridge on which he was walking collapsed' ('bridge [objective] on-it being his walking, it collapsed'). Most bewildering of all to the usual Indo-European view is the manner in which Hopi combines modes with modalities according to a systematic logic of its own, of which more presently.

Lest the omission of 'imperative' from the modes seem peculiar, I may say that it belongs to a category of INJUNCTIVES (imperative, semi-imperative, optative, vetative), defective paradigms that are neither modes or modalities.

STATUS

The status categories have been named AFFIRMATIVE (zero-form, declarative sentence), NEGATIVE, INTERROGATIVE, INDEFINITIVE. The negative is formed in the reportive and nomic by the particle *qa`* 'not' before the verb, in the expective by *sọ'ʔon* before the verb. The interrogative is the status of asking a question for a yes-or-no answer. It is formed by the particle *pï`* as the first word in the sentence, e.g. *pï` ma·'na tï'yot tị̈'wa* 'did the girl see the boy?' There is no different intonation from any other type of sentence. The reply-forms are *ʔowi·^y* 'yes', *qạ'ʔe* or *qaʔɛ·^y* 'no', *pi·'hi·^y*, an unanalyzable word meaning 'I don't know'. The indefinitive is the status of the sentence containing an 'interrogative' (better, indefinitive) pronoun, adverb, or verb (e.g. the verb 'do what'). It need not be necessarily a true question, because such interrogative words are also indefinites (i.e. the words 'what?' and 'something' are the same); or, better said, the meaning of the word is an indefinite suggestion that implies also a more or less inquisitive attitude 'something—I wonder what?' The Hopi sentence *hi·'mï pɛ'wiʔ* 'something is coming' could be rendered psychologically as 'an I-wonder-what is coming'. Whether this is to be interpreted as a request for information or merely as an interesting remark will depend on the context, which may include the general behavior of the speaker.

MODALITY

Modality in Hopi is in rough terms the sort of thing that is referred to by the subjunctive and some other moods of IE languages. But of course we ought not to rest content with this 'sort of' sort of definition.

I would say that the modalities of Hopi are moduli of moduli, that is, they are methods of further modifying and amplifying the three-assertion system that distinguishes three basic realms of validity, so that in effect many more than three realms and sub-realms of validity are distinguished—again, much as prepositional relationships in Greek amplify a basic system of case relationships. Modalities are to ASSERTIONS what the particles *na·t*, *ʔa'son*, etc. in our mode examples are to MODES. They are denoted by particles designated MODALIZERS. The word-category of modalizers is more abundant in morphemes than the category of modality strictly considered. While there are many particles of the modalizer type, it seems necessary to distinguish eight (perhaps more) as mutually coordinated to the point of forming a system of eight modalities, to which on schematic grounds we may add as ninth the INDICATIVE modality or zero-form. The line between the modalities and the lexical use of other modalizers is not sharp, yet on the whole the modalities are a set of mutually exclusive forms (with certain exceptions), while the other modalizers are used more freely along with each other or with the modality modalizers, occur in less frequent and less formally patterned uses, and are perhaps most conveniently treated as lexical items (see examples at end of this paper). The modalities have been named: INDICATIVE, QUOTATIVE, INHIBITIVE, POTENTIAL, INDETERMINATE, ADVISORY, CONCESSIVE, NECESSITATIVE, IMPOTENTIAL.

QUOTATIVE: modalizer *ya'w*. The assertion becomes the assertion of a linguistic report. In telling a folk-tale *ya'w* is used like the Eng. 'so' or 'and so' at the beginning of almost every long sentence; it there means 'so' in the sense of 'according to the story'. In the simple independent sentence it adds the idea 'they say', 'on dit que'. It is placed immediately after a direct quotation, the last word in which then receives high stress on the last syllable, this combination being equivalent to spoken quotation marks. It is used in indirect discourse, the verb of saying, hearing, etc. being in correlative or transrelative mode (see examples under these modes). Contrast *nïʔ navọ't-q ya'w mïṭ'nï* 'I heard that he fell down' (by verbal report) with *nïʔ navọ't-q mïṭ'nï* 'I heard him fall down' (heard the sound of his fall). However the quotative cannot imply the confirmation nor concede the truth of the report, which is the function of the concessive modality (*kïṭr*), hence *nïʔ navọ't-q kïṭr mïṭ'nï* 'I heard (of the fact) that he fell down' (which is conceded to have actually happened).

INHIBITIVE: modalizer *kïrhị'n.* This means that the subject is blocked or prevented from producing the effect specified by the verb, with complete lack of implication about the cause of this condition, as to whether it lie in the ability of the subject or externally, etc. It is translated simply by 'cannot'.

POTENTIAL: This formulation strikes the English-speaker at first as topsy-turvy, if not positively weird. It is translated by 'can', but is simply the negative of the inhibitive, donated by *kïrhị'n qa`.* Yet analysis shows the form to be remarkably logical. By this means the Hopi produce a perfectly neutral, potential 'can' that does not merely refer to personal ability, but denotes that the way is entirely open for the subject to turn potentiality into action if he chooses. (For the 'can' of personal or technical ability, 'knowing how', there is a definite verb, used with the expective correlative of the action-verb.) But how could such a neutral potential 'can' be better expressed than by a negative form which declares simply the absence of all inhibitive or frustrative checks between the subject and action?

INDETERMINATE: modalizer *sẹn*; indicates uncertainty, corresponding to 'perhaps, possibly, maybe', or in the expective to 'may', e.g. *ni'm-ɛ? sẹn mọ'ŋʷit ʔaw pitị'ni* 'when he goes home he may meet the chief' (and again he may not). The uncertainty is like that of a balancing between about equal positive and negative probabilities; hence, e.g. *nï? ʔaw tï'viŋta`-q sẹn ni'mạ`ni* 'I asked him if (whether) he were about to go home' (transrelative construction). Here at first *sẹn* seems to play the role of Eng. *if*, but it is not so. It answers only to the indeterminacy expressed by the *if*, whereas the linking and relating function of the *if* is performed by the transrelative mode relation; *sɛn* itself is quite unable to effect any linkage.

ADVISORY: modalizer *kɛ.* It denotes an uncertainty like *sẹn*, but stresses slightly the possibility of the positive rather than the negative outcome. If this positive possibility is being asserted in the presence of a somewhat opposing attitude, or a dread of such an outcome, it gives the feeling of our 'might', 'may nevertheless', or in the reportive 'might have done so'. Thus, *ta·'qa tïw-ẹ'? kɛ wa·'ya'ni* 'when the man sees it he may run away' (his possible running away is the thing to be kept in mind). Hence the sentence containing *kɛ* has an advisory character, since it does not merely adumbrate an uncertainty, but calls attention to one possible outcome thereof. Logically enough, our 'may not' is *kɛ qa`*, not *sẹn qa`*, e.g. *ni'm-ɛ? kɛ qa` tïwạ'ni* 'when he goes home he may not see it'.

CONCESSIVE: modalizer *kïr*. It denotes that the assertion is given validity as a concept rather than validity as an objective experience,[5] e.g. 'it is conceded, granted, inferred from the evidence at hand, assumed, considered as known', etc. In the independent sentence it may be rendered 'it seems that', 'evidently', 'apparently', or merely 'so', e.g. *kïr mọ'ŋʷi ni'ma* 'so the chief went home' (I gather). It is in complex sentences that its subtle importance stands out. Consider the transrelative pattern; *nï? tïwạ'-q pa'la* 'I see that it is red' (by my seeing it is red'), *nï? tïwạ'-q rïpi'pita`* 'I see that it sparkles'. The Hopi refuse to use this pattern as it stands for, e.g. 'I see that it is new', which demands the concessive in the clause expressing newness, i.e. *nï? tïwạ'-q kïr pï·'hï* ('by my seeing it is inferentially new'). In other words newness is not a visual sensation like redness or light; it is not seen directly, but is inferred or assumed, '*kïr*', from seen data. To us this seems like psychological analysis, but to the Hopi it is a clear and practical distinction. The ordinary English conditional *if*-construction requires in Hopi that the conditional or transrelative mode which represents the linking function of our *if* be also concessive to represent the hypothecating function of the *if*; otherwise the mode would be translated 'when' and not 'if'. Thus, *kïr ni'm-ɛ? mï·'nat tïwạ'ni* 'if he goes home he will see the river' ('assuming that he goes home etc.'). The 'contrary-to-fact' *if* is another matter (*?ạs*, see below).

NECESSITATIVE: modalizer *sọ'?on qa`*. It means 'necessarily, naturally, inevitably', and seems queer from the IE standpoint in being simply a double negative, which in Hopi always makes a positive. It

[5] We may not here read 'sensory' for 'objective', for experiences which psychology would place at the level of percepts rather than sensations, or even at the level of simple concepts, do not require the concessive, though they are treated differently from sensations. They are the percepts (or simple concepts) of seeing an action or phenomenon of a type having a lexical name (verb), and are put in the indicative transrelative, while it is now the verb of seeing that is in the independent mode, e.g., *nï? tị̃'wa wa`'ya'-qö?* 'I saw him run away', *nï? tị̃'wa cị̃'rot mị̃'?a-qö`?* 'I saw him shoot the bird'. It is a remarkable fact that the Hopi seem to recognize, in their language, a distinction of four types of received information which correspond roughly to gradings made by psychology: (1) sensory, e.g. 'I see that it is red', 'I hear him fall', sensing-verb in transrelative, information-verb in indicative; (2) perceptual-conceptual, e.g. 'I see him fall', sensing-verb in independent, information-verb in transrelative; (3) overt linguistic, e.g. 'I heard that he fell' (unconceded), sensing-verb in transrelative, information-verb in quotative; (4) pure conceptual, e.g. 'I see that it is new', 'I heard that he fell' (a conceded truth), sensing-verb in transrelative, information-verb in concessive.

is a combination of *qa*ˋ 'not' and *sọ'ʔon* 'expective not' and thus means that there can be no expectancy of a negative. It often translates Eng. 'must' and 'have to' but is not tinged with any notions of compulsion, duty, or obligation, being entirely neutral and abstract. It is often used in the conclusion of conditional statements to indicate a necessary consequence, e.g. *kïr ni'm-ɛʔ sọ'ʔon qa*ˋ *mï·'nat tïwạ'ni* 'if he goes home he will see the river' (as a necessary consequence).

IMPOTENTIAL: modalizer *ʔạs*. This modality is very difficult to express in terms of our ways of thinking. It indicates what I might call teleological ineffectiveness. We go part way along the road with the Hopi by recognizing forms of assertion, like 'may' and 'can', that are on a different plane from the bare 'does' and 'does not', and as compared with these more rudimentary assertions have a status that combines the qualities of affirmative and negative, of reality and unreality. They are on a middle ground between these opposites, even though formally cast in an affirmative pattern. But all our middle-ground expressions refer to the realm of latency; the reason for the statement's dual positive-negative character is that its truth is that of a latency, of which the manifestation belongs to the future. The Hopi also have middle-ground expressions of this sort, as we have seen. But they have, further, a middle-ground expression in which the dual positive-negative character is not a matter of latency, but is ascribed to events that have already happened. Moreover an expression referring to something that never happened at all can be assigned to this same realm of quasi-reality along with references to actual happenings. The criterion that fastens this particular stigma, as it were, of quasi-reality upon the subject matter of discourse, is ineffectiveness in terms of the purpose, goal, drive, need, function, etc. (a variety of concepts in our own ideology are here applicable) that originally formed the grounds for the action. If a Hopi is reporting a train of events in which a man ran away from his pursuers but was eventually captured by them, he will use the impotential, and say *ta·'qa ʔạs wa·'ya* 'the man ran away' (implying that 'ran away' cannot here be held to mean 'escaped'). If the man ran away and escaped, the statement would be simply *ta·'qa wa·'ya*. *Nïʔ ma'qto* is 'I went hunting'; *nïʔ ʔạs ma'qto* is the same except that it implies that I came back empty-handed or practically so. We could convey such information by saying 'well, I went hunting!' in a disgusted tone, but the Hopi sentence is not really comparable to this. It is a quite unemotional statement; *ʔạs* is not an expression of affect but is intellectual, and would be used whether the speaker be unmoved, displeased, or pleased; e.g. *cö·'viw ʔạs wa·'ya* 'the

deer fled' (but I caught him just the same). My name for this modality, 'impotential', refers to the connotation of impotence that it gives to the statement of actions and attempts. In the expective it changes the translation from 'will' to 'tries to', e.g. *ma·'na ʔaṣ wïni'maṇ'ni* 'the girl tries to dance' (but does not for the present succeed). However expective impotential does not imply that later attempts will not succeed. When the expective refers to the past of narration, its impotential refers to a frustrated attempt at some event that did not actually happen, e.g. *ʔaṣ wa·'ya'ni* 'he tried to escape', of a prisoner who failed to escape. When the impotential is expective in a dependent mode (i.e. when it is conditional or transrelative and the other clause is expective) the reality is further attenuated to a never-realized theoretical past possibility. Thus our contrary-to-fact condition is impotential in Hopi, e.g. *ʔaṣ ni'm-ɛʔ soʔ'ʔon qa' mï·'nat tïwaṇ'ni* 'if he had gone home he would have seen the river' ('when in not-to-be-realized capacity of going home he was in necessitous expectancy of seeing the river'). Here the concessive may be added to the impotential, especially if the mode is transrelative, e.g. *kïr ʔaṣ ni'ma-q soʔ'ʔon qa' moʔ'ŋʷi ʔaw pitïṭ'ni* 'if he had gone home the chief would have met him'. Our 'although', 'but', 'yet', etc. indicate a tension of some sort between two conflicting tendencies. The Hopi unerringly discriminate whether one of these tendencies has actually aborted the other (the impotential meaning) or whether the opposition is of the sort to be indicated by *kɛ*, *sɛn*, or some other means. Thus, in concursive, *nïʔ ʔaṣ qatï'-kaŋ ma·'ŋïʔi* 'although I was sitting I felt tired'; in transrelative, *ʔaṣ wa·'ya'-q nïʔ ŋïṭ'ʔa* 'although he fled I caught him'. On the other hand *ʔaṣ* would be wrong in 'although he was running he was singing' since neither action has aborted the purpose of the other; here a Hopi would say simply *wa'riki'w-kaŋ ta·'wla'wï* 'while he was running he was singing', or perhaps add to this an element denoting mild surprise. In the sequential form *ʔaṣ pitïṭ'-t qa' wïni'ma* 'although he had arrived he did not dance', *ʔaṣ* implies that the function of coming was to dance; he might as well not have come.[6]

The disparity of pattern from Indo-European appears in that while all these modalities resemble the IE subjunctive, not one aligns with it. The Hopi interpret our subjunctive in various ways according to a

[6] The realm of the might-as-well-not-have-been is, in a nutshell, that middle ground between positive and negative which the impotential represents. What we call the might-have-been is to the Hopi simply a part of this realm. It is the expectancy (potency, tendency, possibility, wish) that might as well not have been.

recognition of relationships of which we are not linguistically conscious. Thus in 'if I were king', 'were' from the Hopi viewpoint is impotential; in 'to see if he were brave', 'were' is indeterminate; in 'though he be stubborn', 'be' is advisory; in 'if he be right', 'be' is concessive. Or is the pattern so very different from Indo-European after all? It remains a fact that the Uto-Aztecan languages in general, and Hopi especially, are for American languages unusually reminiscent of IE in their type of grammar. Could it be possible that in ancient forms of IE, perhaps in Hittite, patterns of syntactic construction may exist that would lend themselves to an analysis following somewhat the Hopi outline?

Other Modalizers

There are a number of modalizers having uses less definitely formal than those of the modality system. Their wide range of expression is shown by the following samples:

ʔï'ra: memory, recollection—'according to recollection it is so'.

ʔi'snïntị̀q: probability, reasonable expectation, justifiable assumption or hope—'supposedly'—and in the expective is like our 'if all goes well'.

na'wïs: obligation to voluntary action without compulsion; differs from our 'ought to, should' by greater promise that the action will be performed; may sometimes be rendered 'has to' but according to my informant corresponds to our expression 'can't very well refuse to'.

pɛ·v: 'almost, nearly', *pɛ·v kɛ* 'possibly almost', 'almost may'.

pi̓: acceptance of conditions as they are or must be, undeniable fact, inevitability; sometimes corresponds to English word-stress, e.g. *pạm pitị̈'ni* 'he will arrive', *pi̓ pạm pitị̈'ni 'he* will arrive', 'he, at least, will arrive'. It also corresponds to 'after all', and still more to our shrug of the shoulders, while *pay pi̓* 'already, *pi̓*' indicates philosophic resignation to unchangeable reality—*pay pi̓ wa·'ya* 'he ran away, and that's that'. Also common are the combinations with negative (*pi̓ qa̓*) and necessitative (*sọ'ʔon pi̓ qa̓*).

ta'tam: necessity to which one resigns oneself with a self-sacrificing feeling; 'must', or 'may as well' imbued with this feeling; implies that the subject is sacrificing his own interests or preferences.

tị̈r: intention without clear resolve, vaguer than 'wants to' or 'intends to'; more like 'is (was) thinking of —ing', 'would', 'would like to'.

The extent to which 'modal feeling' is diversified and the *finesse* of its application differ markedly in different languages, but probably few languages have gone so far into these fields as Hopi.

MISCELLANEA

FRENCH *chef-d'oeuvre*

In FEW von Wartburg lists this word under the entry *caput* (3a) 'Führer'. Thus *chef-d'oeuvre* attested since 1260 . . . 'ouvrage que tout aspirant à la maîtrise d' un métier doit présenter à un jury', i.e. 'masterpiece' (in the original sense, not of an excellent piece of work, showing the skill of a master, but of a piece of work by which a craftsman gains the rank of 'master' as in Dutch *meesterstuk*, Germ. *meisterstück*) appears on an equal footing with OF *chief* 'celui qui est à la tête de qch.', *chef d'hôtel, chef de famille, chef de cuisine*. But it is easy to see: (1) that this *chef* 'leader' (Lat. *caput rei publicae*, etc.) appears only in designation of persons (for Gers *kat de pe* 'gros orteil' is a justifiable exception, the big toe being really a 'leader'); (2) that the idea of 'master' is in other cases of names of objects expressed by an adjectivized *chef-*, for example, *chef-lieu*, OF *chief cens* 'premier cens et capital . . . ', OPr. *capfeu* 'fief principal'; (3) that the idea of 'piece' as in Eng. *masterpiece*, Germ. *meisterstück* is to be expected in the French word.

In fact, *chef d'oeuvre* has to be listed under v. Wartburg's paragraph 1b 'Stück', i. e. 'piece' (cf. dialect forms with the meaning 'pièce de bétail', 'échantillon', 'portion', and especially OPr. *non . . . cap* 'ne . . . point', litt. 'not . . . a piece, a bit'). This meaning 'piece' derives evidently from 'head', for example OF *cap d'ail* = MF *tête d'ail*, objects (and human beings!) being counted by the emerging part of them, their heads (Lat. *grex xxv capitum*, Poschiavo *ka da pan* 'piece of bread'): OF *chef* is used with *ail, estains, napes, tooilles*. It is very difficult to distinguish the meanings 'head' > 'piece' and 'end' ('bout') (cf. FEW, *caput*, 2b).

Von Wartburg has overlooked my article in Aufsätze zur roman. Syntax u. Stilistik 1918 no 3, where I tried to prove this, in opposition to the Dict. gén., to Bernitt, 'Lat. *caput* und **capum*', and to Meyer-Lübke, Rom-Gr. 3.219, by the first work in which *chef-d'oeuvre* is attested: in Etienne de Boileau's Livre des mestiers (1260) one of the passages (not the one quoted everywhere) reads as follows:

> Et qui portera peletrie au marchié de Paris, de tant come il en vendra, de tant rendra son paage, et l'autre emportera tout quite

arriers. Et se peletrie vient de foire, et ele passe parmi Paris, et ele vait outre, toute s'aquitera et quanqu'il i aura de cuirien cru des charretes, pour que de sauvagine soit, si doit doner .IIII. den.—V. Se troussiaus n'est entreliés de cordes, li premiers trousiaus donra .IIII. den., et tout li autre trousiau cordé après .II. den., *de sauvagine*, sanz péleterie faite; et autresi d'aigniaus et de tout privé, *s'en chief d'uevre n'est* .IIII. den.; *de chief sauvage* .I. den., de privé obole. *Chief d'uevre de* .II. piaus ne doit noient, uevre de testes ne de ventresches de connins ne de lievres ne doit nient.

If we compare the parallel OPr. passage quoted by Levy, s. v. *obra*: 'Se passa pel pont de Tarn una pessa de coire . . *que no sia en hobra*, deu pagar mealha . . . Per eissa manieira una pessa de latho, *que no sia en hobra* . . . Lato *en obra*—I. basci de latho deu mealha . . . e una conqua I. denier' (Cost. pont Albi), we see that OF *s'en chief d'oeuvre n'est* is equal to OPr. *que no sia en obra*, the latter being translated by Levy 'bearbeitet'. There is a different duty on a finished piece of work than on a *sauvage*, an unmanufactured one. Thus, *chief d'oeuvre* means 'a piece of work which shapes the raw material', a manufactured piece of work' (and because of its use as a test of mastership, it has assumed (later, in the 16th century, v. W.) the sense of 'excellent work'.

In *chef d'oeuvre*, there is no place for a 'Führer' (of objects!), but simply for a 'head' in the technique of counting, a 'piece'.

I find *chef d'oeuvre* in the sense of 'finished piece of work', not of 'masterpiece' as late as Hardy, Mariamne:

Mariamne (speaking of Herod who should be killed for his crimes):

Las! helas! viendra point ce deplorable jour,
Qu'vn Alcide t'immole, O Busire, à ton tour!
Que ie preste *au chef-d'oeuure*, & le coeur & la dextre,
Ouy, la fin des meurtriers sanglante aussi doit estre. (II, 1)

Herode: Tandis ie conuaincray d'vne honte decente
Mon empoisonneresse, & feray dans demain
Imposer *au chef-d'oeuure* vne derniere main. (III)

The *chef-d'oeuvre* to be performed in these passages is clearly the killing of Herod and Mariamne respectively.

In the passage of Ambroise Paré quoted by Littré: 'Et pour ce, sire, estant ceci *un chef d'ouvre, et l'amas de tous les travaux*, d'un de vos serviteurs et sujets', Littré translates 'somme', i.e. a finished piece of work consisting of different parts.

A masterpiece is finished, thus this meaning derives logically from 'head' > 'piece'.

LEO SPITZER

NEAPOLITAN *pernacchia*

Solano has overlooked (LANGUAGE 14.60):

(1) That M. L. Wagner has dealt with the word in Volkstum und Kultur der Romanen 6.7. He traces the Neapolitan gesture, which has been carefully described by Andrea de Torio, La mimica degli Antichi investigata nel gestire napoletano (1832), back to antiquity: in Petronius' Satyricon, ch. 117: 'nec contentus maledictis tollebat subinde altius pedem et strepitu obsceno simul atque odore viam implebat. ridebat contumaciam Giton et singulos crepitus eius pari clamore prosequebatur'.

(2) That I explained in ZRPh 44.220, and in Volkstum und Kultur der Romanen 6.274 French *tromper*, in Old French 'to mock', then, 'to cheat', by analogy with the Neapolitan *pernacchia*: originally = *sonner de la trompe*, or, as Dante puts it, *far del cul trombetta*, then 'to mock' by imitation of this indecent sound.

(3) That Nunzio Maccarrone gave the same etymological explanation in *Arch. glott.* 27.68 (1935): *vernaculum* 'atto da schiavo', quoted Martial (10.3 *vernaculorum* dicta 'dicta procacia, contumeliosa, scurrilia', Forcellini, cf. Tacitus, Hist. 3.32 *vernile dictum*), and noted the phonetic shift *v-* > *p-* in Southern Italian dialects (Neap. *varacchio* > *paracchio*), so that the explanation of a blend with *peditare* is superfluous.

(4) That there is no need to see in the lengthening of the initial consonant in the plural *e bbernacchie* an indication of derivation from a neutre singular, since the plural *e bbernacchie* is regular according to Schuchardt, Rom. 3.25 (Neap. *le ffiglie* 'le figlie'—*li figlie* 'i figli'). From the neuter plural *vernacula* (dicta, acta) a feminine singular *b-(p-)ernacchia* has been formed which has the regular plural *e bbernacchie*.

LEO SPITZER

CORRECTIONS

C. D. Chrétien writes that a recomputation of the data published by A. L. Kroeber and himself in their Quantitative Classification of Indo-European Languages, LANGUAGE 13.83–103 (1937), has led to some corrections. Read in Table III: Sanskrit-Greek 0.15; Slavic-Greek −0.23; Slavic-Sanskrit 0.19*; Baltic-Iranian 0.11*; Baltic-Sanskrit −0.02; Germanic-Armenian −0.19*; Germanic-Slavic −0.10*. In Table IV read: Sanskrit-Greek 0.21; Baltic-Sanskrit −0.03; Germanic-Baltic 0.42*. The starred variations are too small to possess any significance, the others bring the authors' conclusions still more in line with non-statistical judgments.

In LANGUAGE 14.220,13 read 'younger' instead of 'older'.

BOOK REVIEWS

Hittitisch und die anderen indoeuropäischen Sprachen. Pp. 227. By HOLGER PEDERSEN. (Det. Kgl. Danske Videnskabernes Selskab. Historisk-filologiske Meddelelser. 25.2.) Copenhagen, 1938.

Pedersen tells in his introduction (12) that his book is intended to bring nearer to its solution the question whether Hittite is just another Indo-European language or whether the previously known Indo-European languages form a unit (eine Einheit) as against Hittite. After reviewing the morphology and phonology of the language, he finds (190 f.) for the former alternative. The book is therefore in large part an argument against the Indo-Hittite hypothesis. I shall attempt a reply elsewhere.

The incidental discussion of morphology and phonology is, however, much more thorough than would be necessary for this argument. Pedersen has given us virtually a new essay on the comparative grammar of the Hittite language, and this, I think, will be the book's chief claim upon the attention of scholars. As is well known, my Comparative Grammar of the Hittite Language (Philadelphia, 1933) has proved unsatisfactory to many, especially those who prefer to cling to old doctrines until the new ones are established beyond any possibility of doubt.[1] They may turn to Pedersen in the hope of finding a more conservative treatment, but I fear they will be disappointed. Pedersen has always been distinguished for his original and challenging ideas, and so he is here. The following are some of the points on which he has something new to offer.

He rejects (13–18, 35 f.) Meillet's theory that the lack of the feminine gender is an archaistic feature of Hittite. He is confident that he can find traces of the former presence of this gender in Hittite in neuter plurals in *-a*, in the variation between the stems *parku-* 'pure' and *danku-* 'black' and equivalent *parkui-* and *dankui-* (cf. Lat. *tenuis* beside Gk. *ταυν-*), and in some other Hittite *i*-stems.

The instrumental ending *-(e)t* he connects (22–4) with the Lat. ablative in *-d*, but he separates it completely (24 f.) from the Hittite

[1] This is not intended as a claim that all the opinions in my book deserve acceptance; many of them I no longer hold myself.

ablative in -(*a*)*z*(*a*). This he thinks (ibid.) may contain the reduced grade of the adverbial ending *-tos*; but on the last page of the book he holds rather that the ending is a postposition identical with the <problematical> preverb *ze-* 'across'.

He follows (27 f.) Forrer in distinguishing dative and locative in early Hittite, but he calls the forms in *-a*, which tend to denote end of motion, datives, and the forms in *-e* locatives. The ending *-a* he derives from IE *-ōi*.

The type *memiyas*, gen. *memiyanas* (41 f.) 'word' may be compared to formations like Lat. *imāgō*, *vertīgō*, *ferrūgō*; IE *-ō* > Hitt. *-a*, and the usual masc. nom. sg. ending has been added.

The gen. pl. ending *-enzan* of certain pronouns (56 f.) contains an inherited ending *-ēm*, which may have been extended by a pluralizing *-s* and then contaminated with the ending *-an* < **-ōm*.

The Hittite *ḫi*-conjugation (80–100, 115–21, 122–5) is derived in part from the Indo-European perfect and in part from thematic presents. Pedersen, unlike Couvreur,[2] recognizes two classes of thematic verbs in Prim. Indo-European, one like the Sanskrit *a*-conjugation, with endings **-m*(*i*), **-s*(*i*), **-t*(*i*) in the singular active, and the other like the Greek *ω*-verbs, with **-oH*, **-e*(*ï*), **-e*(*ï*). Hittite thematic verbs of the '*m*-conjunction' are, for example, the verbs in suffix *-ske/a-* (*da-aš-ki-mi*, *da-aš-ki-ši*, *da-aš-ki-iz-zi*); while the thematic verbs of the *H*-conjugation may be illustrated by the verbs in suffix *-na-* (*tar-na-aḫ-ḫi*, *tar-na-at-ti*, *tar-na-a-i*). Pedersen argues his thesis with great skill, and it must be admitted that he provides a neat explanation for a number of hitherto troublesome details. If I am still unconvinced, it is largely because there is no trace of vowel alternation in the Hittite *hi*-conjugation verbs in *-a-*, and because it seems unlikely that Prim. Indo-European (I should say Indo-Hittite) *-eï* remained uncontracted and yielded Hittite *-aï*.

Original long diphthongs are said (170) to have developed in the same way as the short diphthongs, to Hittite *e* or *u*. Where the texts present *a-*(*a-i-*)*i*, *a-*(*a-i-*)*e*, *a-*(*a-u-*)*u* (88, 113, 166, 170) Pedersen assumes dissyllabic pronunciation in Hittite, and he regards the frequent tautological *a*, *i*, or *u* as an indication of just this. The theory seems to me to lead to some unsatisfactory inferences. For example, the retention for many centuries of dissyllabic *-eï* in the 3 sg. of the thematic *H*-conjugation seems inconsistent with general Indo-European habits of

[2] Mélanges Cumont = Annuaire de l'Institut de Philologie et d'Histoire Orientales et Slaves 4.551–73; cf. Sturtevant and Bechtel, LANG. 14.10–19.

speech, and the assumed dissimilation of this to Hittite *-aï* is in startling contrast to the assimilation of vowels in successive syllables that Pedersen elsewhere assumes. Similarly *pa-a-i-mi* 'I go' (129) is here derived from prefix *pe* + **imi* 'I go'. It is surely more satisfactory to assume Pre-Hittite or Indo-Hittite *bhe-eimi* (or *bhe-' eimi*) > **bhēimi* > *paimi*. It would seem to me almost inevitable that Hitt. *a-i-iš* 'mouth' should be equated with Lat. *ōs* on the basis of **ōis*, but Pedersen apparently reads *aïs* and sets up IE *ayes* (*a* = Brugmann's *ə*?).

Pedersen operates with only two Indo-European laryngeals (179–90) as against Couvreur's three and Kurylowicz' four. He writes H_1 for a laryngeal that has no effect upon vowel quality and H_2 for one that converts an immediately preceding or following *e* to *a*. He holds that in Hittite both phonemes yield *ḫ* (*ḫḫ* between vowels) except that both are lost before consonants with compensatory lengthening, that both yield *a* between consonants, and that both are lost after a stop with doubling (or lengthening) of the stop. Pedersen regards IE *ŏ* and the corresponding Hitt. *a* as due to ablaut. He does not assume an initial laryngeal in Indo-European where Hittite has an initial vowel. This is a remarkably simple and attractive form of the laryngeal hypothesis, but in my opinion it neglects a good part of the available evidence.

There are some excellent etymological suggestions in the book, notably these. The defective pronoun *asi* 'is, ea, eum, id' (usually nom. masc.) is explained (59–61) as the anaphoric pronoun *as* with the particle seen in Gk. *οὐτοσί*, etc., while *uni* (prevailingly acc. masc.) is the corresponding acc. (cf. *kas, kun*; *apas, apun*). The remaining one of these defective anaphoric pronouns, *eni* (61–3), is compared with Lat. *ēn*, Gk. *ἦν, ἠνί δέ*. The adjective *kallar* 'bad' (particularly with reference to the supernatural) is compared (46) with Ir. *galar* 'disease'.

These few details, however, give no satisfactory idea of the stimulating originality and constant reasonableness of Pedersen's treatment. The book must lie constantly at the elbow of students of Indo-European comparative grammar.

E. H. STURTEVANT

Varro On the Latin Language; with an English translation. Volume I: Books V–VII. Pp. l + 367. By ROLAND G. KENT. Cambridge: Harvard University Press; London: William Heinemann, 1938. (Loeb Classical Library.)

A valuable, laborious piece of work, and done very well.

Varro is an author who has fallen into undeserved neglect. One indication is the fact that the present translation is the first made into

English, while the only others (one in French, one in Italian) date from the middle of the last century. Another indication may be seen in the scarcity of writings about Varro, especially in the English-speaking world. From America, Kent has listed[1] in addition to his own recent (1936) article (an outgrowth of this edition) in the TAPA, only a paper by Fay (1914); while England is represented only by articles of H. Nettleship (1886), of Robinson Ellis (1891, 1901), and of W. G. Foat (1915). This compares poorly with the nearly 130 items from the continent, chiefly from Germany; but even of these only 16 are of post-war vintage, and the bulk belong to the 19th century. The reason is not far to seek: the undue stress put upon literary values especially in England and in this country. Of course Varro is indispensible as the source for many facts relating to the antiquities of Rome, but the philologian feels that he can skim these off as needed; the linguist too has the same feeling about the facts of immediate value to him (chiefly descriptions of the situations in which words were used), while scorning the attempts at explanation as pre-scientific. So sustained attention is not given to the author and his works. I would suggest, however, that it would be an excellent thing for every linguist to read in Varro at least once a year (oftener, if tempted to be a scoff-law) until he is ready to exclaim: But for Rasmus Rask there go I![2]

The present volume consists of an introduction, the text with critical apparatus, and a translation with brief commentary.

Translation is a matter about which there can be no profitable argument, a thing about which one can express only his likes and dislikes. A type of translation, at present fashionable, can be well exemplified by the rendering of a sentence in the opening section (5.1): 'Of these six <books> I have already composed three, and have addressed them to Septumius; in them I treat of the branch of learning which is called Etymology. The considerations which might be raised against it, I

[1] The list is, to be sure, selective; but papers not chosen do not need to be considered here.

[2] Cf. Bloomfield, Language 346: 'It is not useless to look back at those times. Now that we have a method which brings order into the confusion of linguistic resemblances and throws some light on the nature of linguistic relationship, we are likely to forget how chaotic are the results of linguistic change when one has no key to their classification.'

For Rask's place in the development of our science, cf. Holger Pedersen's introduction to the splendid centennial reprinting of his works by the Dansk Sprog- og Litteraturselskab under the editorship of Louis Hjelmslev (Copenhagen, 1932).

have put in the first book; those adduced in its favour, in the second; those merely describing it, in the third.' For the close: *quae contra eam dicerentur, volumine primo, quae pro ea, secundo, quae de ea, tertio.* I should much prefer: 'what might be said against it, in the first book; what for it, in the second; what about it, in the third.' Kent likes the type of translation exemplified; but, fortunately I should say, realizes (xlv) that it is not suited to his present purpose, where it is 'necessary to follow the Latin with some degree of closeness'. He is thus forced into a mode of translation which I can enjoy and admire. In particular I would call attention to the way in which by keeping and glossing Latin words Varro's etymologies are made clear in their new dress. Quotation of a single illustration (5.101) will suffice: *Volpes* 'fox', as Aelius used to say, because it *volat* 'flies' with its *pedes* 'feet'.[3]

The commentary brings brief answers to the philological questions that arise. Correct etymologies are passed in silence, others are marked; generally, in such patterns as: 'Wrong', 'Such-and-such words belong together, but have no connection with so-and-so', 'The Latin word is cognate with the Greek, not borrowed from it'. An attempt to supply correct etymologies is seldom made, and more frequent efforts of this sort would have been entirely out of place. However, I am afraid many users of the book will be completely mystified as to why they should not accept Varro's suggestions. For their benefit it would have been well to have added to the introduction—long as it already is—a section contrasting Varro's method with that of modern scholars, and indicating say half a dozen books in which the question could be pursued further.[4]

This seems to me the one important gap in the introduction, which in addition to the material to be expected contains a very detailed bibliography, and a careful and valuable discussion of the points of orthography and morphology in which it may be presumed—more or less securely—that Varro differed from what we have come to regard as standard Latin.

The text rests essentially upon a single wretched Ms. (F) of the 11th century. In rescuing from its mass of corruptions the original wording Kent has worked diligently and wisely, and has, thru a judicious use of his predecessors and a number of good suggestions of his own, reached

[3] Here a note pointing out that Varro is identifying *-pes* and *pes* would not have been superfluous.

[4] I should have chosen familiar works by Bloomfield, Holger Pedersen, Kent, Buck, Walde-Hofmann, Ernout-Meillet.

a text that is excellent in substance. Unfortunately, I think, he has declined to give it a Varronian garb. He argues against such a procedure (xvii f.), but to my mind unconvincingly: 'we should do constant violence to the best manuscript tradition, without any assurance that we were in all respects restoring Varro's own spelling.' In this I can see nothing but a refusal to budge, unless there be final certitude about all details. Granted that there is no hope of producing the text that Varro actually wrote, we would have at any rate a text that he might have written; while one that clings to F (the product of an unknown number of blunders and modernizations) must always be one that Varro could not have written, and that would have troubled him continuously. The change of fleas would be a gain.[5]

The critical apparatus gives (with the aid of certain typographical devices in the text) a picture of the gradual development of the text from F to Kent's own edition that is unusually clear and easy to survey. The gap is that none of the 'almost countless emendations' which have not met with the editor's approval is recorded. This material must lie ready to Kent's hand, and I think that a publication of it (perhaps with brief commentary) as a sequel to his edition would be highly desirable.[6]

I realize fully that most of the criticisms here made will not commend themselves to many scholars, and that following them would have produced a work unsuited to the purposes of the series for which it was intended. I would sum up my general estimate by parodying a famous saying of Solon's: Kent has given us, if not the best possible edition, at least the best edition the Loeb Library could be expected to accept.

I add notes on a few passages.

5.1: Since Varro is fond of the phrase *rebus vocabula imponere* (cf.

[5] Of course a text such as I suggest would entail a critical apparatus of a type entirely different from that which the editor desired to present.

[6] To illustrate the value at times of the unmentioned emendations I may refer here to Stroux' treatment (ΑΝΤΙΔΩΡΟΝ, Festschrift Jacob Wackernagel 309–25) of 5.90, 177, 180; 6.67 (which suggests to me the possibility of reading in 6.78: *proprio nomine dicitur facere—dictum facere a facie—qui rei quam facit imponit faciem*); to M. Leumann's reconstruction of 7.66 in Glotta 11.186–7 (for the etymology involved cf. E. Leumann, ib. 12.148); and to Blumenthal's emendation, IF 53.120, of *sola vel* in 5.68 to *Sol* <*vel a s*>*avel* instead of to *Sol vel*. Compare also 7.17,29 at which Stroux gives good support to earlier suggestions.

Kent writes that he is playing with the idea of a variorum edition, and very generously gives me information about some passages in which I am interested. This I shall use below.

the opening of this book *quemadmodum vocabula essent imposita rebus in lingua Latina*, and 7.109, 110; 8.1, 10) it is probably better to read: *In his ad te scribam a qui* (*quibus*, F) *rebus vocabula imposita sint in lingua Latina.*

5.22 n.: As differences of quantity are not systematically represented in Latin orthography, Varro's disregard of them seems a natural corollary to his confusion of speech and writing. Does he disregard the difference between short and long *i*, which would appear to him as a difference between EI and I?

5.48: I should read: *Eidem regioni adtributa Subura* <.....> *quod sub muro terreo Carinarum.* The last clause gives an etymology, *Subura* < *sub muro.* Since the etymology is not Varro's own, the authority for it must have dropped out. The gap contained something like: *ita dicta, ut quidam dicunt.* Then *in eo* refers to the *murus*, and no neuter pronouns referring to *Subura* are left. L. Spengel suggested the deletion of *quod*; Christ its correction to *quae.* Canal[7] translates: ....fu ascritta....., perch' è di sotto..... which makes Varro speak rather illogically.

5.55: Rather than struggle with the syntax of *a quo tribus appellata Titiensium, Ramnium, Lucerum* I should read *appellatae.* So Augustinus from B; other editors have held to the more difficult reading of F.

5.61: I translate: 'and fire is male, because semen is in it.' Similarly Canal.

5.68: In dealing with Varro it seems to me necessary to draw a distinction. He will make up artificial forms, corresponding in a way to our pre-Latin forms, that we are free to set aside, as we set aside modern constructions in which we do not believe. But, when he speaks of a form as in actual usage, we must be prepared either to believe him, or to correct his copyists. Here, according to F, Varro tells us that the moon was called (by implication actually called) *Diviana*, which I find incredible. Canal accepted it, etymologizing *dis* + *via* 'Two-Ways'. This I find still more incredible.[8] As Stroux 324 notes, Varro calmly repeats himself in the seventh book, and fortunately he returns to this etymology in 7.16 in such a way as to permit the restoration of his text.

[7] Only after the completion of his work did Kent succeed in securing a copy of Canal, which he believes to be the only one in the country.

[8] **Bivia* would seem to be the obvious construction, and would serve an ancient etymologist (*littera commutata*) just as well. Or he could have worked along the line followed by Isidore (ap. Spengel): *Dianam quasi Duanam, quod die et nocte luna appareat.*

Read: *et hinc quod luna in altitudinem et latitudinem < et longitudinem> simul it, <Trivia> Diana apellata* 'and as a consequence to this (moon = Diana), because the moon goes three ways, Diana is called Trivia.'[9]

5.73: I should read: *aut quod <a> Sabinis acceptus ubi (ibi,* F) *est Mamers.* The *a* was read by Augustinus and by Canal.

5.79: Kent does well to follow Stroux in reading *ab <luere> lutra*; but it would have been still better to read *ab <luendo> lutra,* thus getting what is by all odds the most frequent method of citing verbs. For the same reason read in 6.81: *dictum cerno a cere<and>o, id est a creando.*

5.95: 'Hence they called it a *peculatus* "peculation" from the state in the beginning when a fine was imposed in *pecus* "cattle", and there was a collection into the state treasury of what had been diverted.' How can a fine or the recovery of property stolen from the state be a peculation from the state? There must be a lacuna for which I suggest something like: *Hinc peculatum publicum primo <appellarunt non ut nunc furtum publicum, sed id quod dicimus publicationem >, ut cum,* etc. This I paraphrase: A phrase formed from this word (*peculium*)[10] *peculatus publicus* they used not as at present in the meaning *furtum publicum* 'theft of state property', but for what we call *publicatio* 'taking of property by the state', for instance when a fine was levied, or there was a forced return to the treasury of property that had been diverted.' Varro has finished with his etymology of *pecus,* and is appending, as frequently, a note about Roman antiquities, this time one descriptive of a change in legal terminology.[11]

5.98: Unless *cornua* is an unrecorded euphemism, there must follow a lacuna in which it was stated that the *aries* had its testicles intact.

5.109: In this and the preceding sections Varro is sketching the

[9] Explanation of the astronomy underlying this section would have been most welcome. According to my (no doubt imperfect) understanding the moon, like other heavenly bodies, seems to rise and to move from east to west, but in her case there is more easily observable also a snaky motion in reference to the ecliptic. It is of this that Varro speaks immediately below: *quod haec* (the moon, not Proserpina) *ut serpens modo in dexteram modo in sinisteram partem late movetur.* This is why there is occasion to speak of the three (not two) movements of the moon.

[10] We should now say more precisely from **peculum,* the form that underlies *peculium.*

[11] What he tells is not surprising. The adjective *publicus* designates the performer of the act as in *cura sacrorum publica. Peculatus publicus* came (first in the acc. no doubt) under the influence of *furtum publicum* in which *publicum* was the genitive plural. Festus speaks of Varro's equating the two phrases.

development of cooking: first *puls*—with it *pulmentum* (*caseus*) which the shepherds lacked at first—apples and like fruits eaten raw—then vegetables cooked in a pot—*hinc ad pecudis carnem perventum est*—first roasted, then boiled, then cooked *ex iure*. Translating 'From here we go on to domestic animals as meat for the table' misses the point of the arrangement.

6.18: The last sentence is hopelessly corrupt. F reads: *Cur hoc, togata praetexta data eis Apollinaribus Ludis docuit populum.* To change *togata* to *toga* (M, Laetus, Kent) is a step in the wrong direction: the Latin is readable, but the contents seem incomprehensible. Evidently there was a reference for further information to some play, just as the immediately preceding discussion of the Poplifugia ends with *de quibus rebus Antiquitatum Libri plura referunt.*[12]

6.59: I take the meaning to be, not that *nimium novum* was the proper superlative of *novus*, but that the use of *novissimum* = *extremum* was 'too new' in the opinion of these old purists. Canal's understanding is the same.

6.76: Since, as Stroux 321 has seen, Varro in *ornamentum, quod, sicut olim, ornamenta scaenici plerique dicunt* is appealing to the usage of the *scaenici* for confirmation of his view that *ornamenta* was once a plurale tantum, the word must here mean not 'play-actors' (Kent, Canal), but 'scenic poets'.

6.83: A large lacuna before the last sentence is to be assumed. Varro is arranging his material according to the five senses with the transitions clearly marked. Here, however, hearing and smell are telescoped. Spengel (xli n.) recognized a loss, tho his efforts to correct the damage do not seem successful. The change of a letter refers to *odor*:*olor*, and the note should be deleted.

7.32: The meaning seems to be that in Varro's day at least some people in spite of puristic objections quoted the proverb in archaic form: *canes caninam non est*, and ought not to be censured for so doing. I should therefore read *canes* both here and in 31.

7.40: In the preceding section Varro has cited two attempts to

[12] Beyond this one can only guess. Probably *togata* has come in from the margin; either as a would-be correction of *praetexta*, or (as my colleague Kenneth Abbott suggests) through a gloss *toga* that supplied the wrong noun for *praetexta*. The name of the author and the dating of the play are missing. Possibly for the latter *<prim>eis Apollinaribus ludis* might be considered. Spengel (lxviii) approves the reading of F without discussion of its difficulties, remarking 'fefellit omnes editores Laetus'.

explain the adjective in *lucam bovem* 'elephant': as derived from *Libycus*, or from *Lucanus*. He now clears the ground for a third etymology of his own. The opening words dispose of the first. *Si ab Libya dictae essent Lucae, fortasse an pantherae quoque et leones non Africae*[13] *bestiae dicerentur, sed Lucae.* What follows must dispose of the second; and, if Varro used the fullest form of expression,[14] would run something like: *neque, <si ab Lucania dictae essent Lucae, dicerentur> ursi potius Lucani quam Luci.* Canal saw the corruption and sought to cure it by adding at the end *<si ab Lucanis Luci>* which would need improvement to *Lucae.* Either error is well within the range of what F could do, and the choice must be made on grounds of style.

Kent, like Ernout-Meillet, follows Meister in deriving this adjective from Oscan *Loucans, Loucano, Loucanom.* It does not seem to me at all probable that this adjective, should be borrowed when *Lucanus, -a, -um* was available; and still less so, that, if borrowing took place, it should start from the one divergent form, nom. sing. masc. *Loucans,* especially when the borrowed phrase contained a feminine. Nor can I see how **Lucās* < *Loucans* could start an inflection *Lucus, -a, -um*; rather **Lucās, *Lucātis* should be expected. I am inclined to believe that the source is a phrase λευκαὶ βόες picked up from prisoners in the campaign against Pyrrhus. It would be an instance of soldier-wit playing with the double meaning of the phrase: (1) 'glittering cows', from their trappings; (2) 'white cows' obviously inappropriate, and therefore amusing.

7.55: Kent's etymology *congerro* < *congerere* will not hold, cf. Ernout-Meillet, and Walde, s.v. *gerrae*; and also my remarks, LANG. 12.221 (1936), tho they are not as novel as I supposed.

7.74: It does not seem possible to derive *triōnēs* from *terō*. Morphology (Stolz-Leumann 239) would lead us to expect **terōnēs*; then too, on this derivation, the word is supposed to come from threshing, while Varro says it was used *maxime cum arant terram.* I suggest that it was cut from a compound such as **aratro-trahōnēs*, after its weakening.

7.84: The word to be etymologized needs to be stated. Read: *quae <scortum> dicta a <scorto, id est> pelle.* The obsolescence of *scortum* 'skin' is an interesting example of how an innocent word may

[13] *Libycae* would be expected, but Varro is probably alluding to the phrase of some poet.

[14] Zero anaphora was of course possible. For the term cf. Bloomfield 251–2.

be killed by a taboo homonymn.[15] Rustic *pellicula* is not 'euphemism', but due to an effort to get a more novel, hence more witty and effective expression.[16]

G. M. Bolling

French Sports Neologisms. Pp. viii + 84. By Armstead Otey Grubb. (University of Pennsylvania Dissertation.) Philadelphia: privately printed, 1937.

The author has gathered diligently from books and pamphlets written by athletes, from newspapers and from sports journals, those technical terms of athletics which he properly names neologisms; among them are foreign words (mostly English), native French words used in foreign meanings (mostly taken from English), and derivative formations. Of such he lists, with citations of occurrence for the less usual, about 1000 words which are not to be found at all, or not in these meanings, in the regular French dictionaries. His remarks on the prevalence of English terms are illuminating; for instance he notes that in sports introduced from England, Anglicisms are abundant, while in fencing, a sport indigenous and highly developed in France itself, there are practically none.

His list is a valuable contribution to lexicology, and not the least valuable feature is the Appendix (79–84), in which the terms are grouped by the sports to which they belong.

Roland G. Kent

Paleographical Edition and Study of the Language of a Portion of Codex Alcobacensis 200. Pp. x + 98. By Henry Hare Carter. (Publication of the University of Pennsylvania Series in Romanic Languages and Literatures, No. 28.) Philadelphia, 1938.

This is a University of Pennsylvania doctoral dissertation, in which the author treats a portion of the Codex Alcobacensis 200 of the Biblioteca Nacional of Lisbon; the portion of the text which is here edited (folio 195 Ro. to 219 Vo.) is a unit in itself, comprising a dialogue between a Christian theologian and a pagan philosopher. No part of this Codex has been edited before.

The author discusses the palaeography, the orthography, the phonology, the morphology, and the syntax, then gives the text (36–69),

[15] Cf. Bloomfield 396.
[16] Cf. Bloomfield 402–3.

notes (70–5), and a glossary of words that have some peculiarity of form or spelling or meaning (76–98). He sets the date of the Codex as in the early 15th century, which is later than the dating given to it by Burnam, since some changes in the forms of the third plural had begun to take place which were not present in the Rule of Benedict, dated 1385, as well as for other reasons summarized on page 2.

The work appears to be a careful study which, with others of its kind (mostly yet to be written), will serve to establish with certainty the details of the history of the Portuguese language.

ROLAND G. KENT

Chaucer's Romance Vocabulary. Pp. xii + 173. By JOSEPH MERSAND. New York University Dissertation. Brooklyn: Comet Press, 1937.

This elaborate study includes chapters on the Controversy over Chaucer's Romance Vocabulary (1–20), Early Attempts at Tabulation (21–36), Chaucer's Vocabulary: A New Estimate (37–43), The Vocabularies of Chaucer and his Contemporaries (44–52), Chaucer's Contributions to the English Vocabulary (53–7), Romance Words in Romaunt of the Rose (58–61), Romance Words in the Minor Poems (62–74), The Romance Element in Chaucer's Vocabulary (75–84), Chaucer's Romance Rimes (85–9), The Evolution of Chaucer's Romance Vocabulary (90–122), Other Objective Tests of Chaucer's Romance Vocabulary (123–36), General Conclusions (137–8); Bibliography (141–8), Statistical Records of Chaucer's Romance Vocabulary (151–5), Romance Words Introduced by Chaucer (159–73).

As will be seen, practically every aspect of the problem has been handled. The final Appendix, on Romance Words Introduced by Chaucer, includes (with place of occurrence, unless common) the 1180 words of Romance origin which are not cited in the NED as occurring before Chaucer. This is an astonishingly high percentage (total different Romance words in Chaucer, 4189), but it will probably be somewhat reduced by the listings of the Early Middle English Dictionary now in progress at the University of Michigan.

The development of Chaucer's Romance vocabulary is well brought out by means of statistical tables and the accompanying commentary. The general conclusions are formulated on pages 137–8; Chaucer's Romance vocabulary developed and increased as his knowledge of languages and literatures increased, but his use of Romance words was affected by the language of his sources, by the subject matter, by his

rime-schemes. After he lost his court position in 1386, he ceased to use many of his Romance words which probably he employed to gratify the tastes of the Court circles. But he took only a few of his Romance words direct from his literary sources; for the most part he drew them from the colloquial English of his time or formed them independently, and a very high percentage of them have become a permanent part of the English language.

On page 77, line 14 from bottom, the percentage 45.91 seems to be an error for 31.00. Such an error of calculation or of printing, amid so many figures, is easily forgiven in view of the author's contribution to the subject.

ROLAND G. KENT

Poems from the Book of the Dean of Lismore. Pp. xii + 111. By E. C. QUIGGIN. Edited by J. Fraser. Cambridge: University Press, 1937.

During the years 1512–26 Sir James Macgregor, Dean of Lismore (in Argyllshire), and his brother Duncan Macgregor, noted down from oral sources some two hundred and thirty-three Gaelic poems. They made a phonetic transcription. These lyrics are virtually our only specimens of Scots Gaelic poetry composed before fifteen hundred. For the Celtic linguist the interest in these transcriptions lies in the opportunity for studying the early pronunciation of Gaelic. The vocabulary does not offer a great deal of interest: it contains many words that are more reminiscent of Middle Irish than they are of the speech of the modern Gaelic-speaking crofter, but Gaelic such as this has been written well into the nineteenth century. One has only to compare the writings of the nineteenth-century Norman McLeod with the contemporary Gaelic column of Alasdair Mhor in the West Coast Advertiser and Stornaway Gazette to realize this.

Thomas M'Lauchlan and Alexander Cameron prepared editions of all but seventy-six of the poems of the Dean of Lismore.[1] The late Professor Quiggin of Cambridge University had made a transcription of these seventy-six and they are now published, seventeen years after Quiggin's death, by Professor Fraser of Oxford. This edition is a memorial to Quiggin, giving a sketch of his life and listing his bibliography. There is no linguistic discussion offered by the editors.

[1] Thomas M'Lauchlan and W. F. Skene, The Dean of Lismore's Book. Edinburgh, 1862; Alex. Macbain and John Kennedy, The Reliquiae Celticae of Alexander Cameron, Inverness, 1892, vol. 1.

Unfortunately it is almost impossible for the average linguistic scholar to read the phonetic spelling of Dean Macgregor. To do so requires a colloquial knowledge of Gaelic, and even then the text can not be read easily. Both M'Lauchlan and Cameron gave a parallel version in normalized orthography; but Quiggin and Fraser do not. Fraser says, 'This part of the plan was abandoned as impracticable'.[2] Why? There are very few readers who can interpret *Mark maillis mvrn in theil* as *Mairg mheallas muirn an t-saoghail*, without some laborious reading aloud, while the editors did not find it impossible to normalize the first half line of each poem in the Index given at the back!

In studying the pronunciation as indicated by the Dean of Lismore it must be borne in mind that there are many minor variations in modern Gaelic. It would be worthwhile to compare the Dean's spellings with pronunciation recorded today in the same part of Argyllshire. (My comparisons have been with general Gaelic pronounciation.) The Dean himself shows considerable variation. The word *ceann* 'head' he gives variously as *kenn*, or *kann*; *rinneas* 'who made' he writes as either *rynis* or *roynis*. The first of these is usually [kyaun:] today and the second is still pronounced in the two ways indicated. For *leam* 'with me' the Dean gives *lamm*, *lomm*, or *lwm*. Today one hears [lɛm], as well as [lyum] or [lum]. Unaccented *a* is given regularly as *i*; cp. *mir* for *mar* 'as', *gi* for *gach* 'each'. Although such an *a* is usually represented by [ʌ] in standard modern Gaelic the *i* pronunciation is present in the Hebrides, and doubtless elsewhere. A very strange transcription is the Dean's use of *z* to represent the spirant *g*. Could it have been true that modern *Goll* was pronounced then as *zoolle*, *gabh* as *zawe*, *dha*[3] as *za*? We will pass briefly over certain other spellings. The Dean wrote *quhyme* for *chaidheamaid* 'we would go', showing that the final *id* was silent and not a [tʃ] as usual today. He does not insert an *s* between *r* and *t*, as indicated in his *huwrt* for *thubhairt*, *etc.* He does not have the *ö* sound for *ao*; viz., *leich* for *laoch*, *teive* for *thaobh*. Usually *cn-* and *mn-* do not show the *n* as having passed to *r*: *mnaan* for *mnathan* 'women', *gnok* for *chnoc* 'hill'; but *mraan* does occur. There is no insertion of a vowel sound between *r* and certain consonants as in the Dean's form *zerk* for *derg* 'red' which today is usually [dʒarək]. It would be possible to multiply observations such as these; but they cannot have scientific value unless comparison is made with a specific Argyllshire dialect. The fact that Quiggin and Fraser offer no study

[2] P. vii, note 1.

[3] A *dh* followed by *a*, *o*, or *u* is pronounced like a spirant *g*.

of this sort, and the fact that their text is so difficult to read, render their publication of limited value to the linguistic student. Their contribution is for the amateur of Gaelic poetry, since it may now be said that the contents of the book of the Dean of Lismore have been printed in entirety. Fraser has added a catalogue of the contents and an index of first lines, both of which he found partially completed among Quiggin's papers.

URBAN T. HOLMES, JR.

Brut y Brenhinedd (Cotton Cleopatra Version). Pp. xviii + 243, with six plates. By JOHN JAY PARRY. Cambridge (Mass.): The Mediaeval Academy of America, 1937.

This is an edition of one of the Welsh derivatives of Geoffrey of Monmouth's Historia Regum Britanniae. Probably this Welsh version was compiled in the latter part of the thirteenth century. Professor Parry publishes it entire, for the first time, reproducing his manuscript 'as closely as type will permit' (xvii). Beneath the text there is an admirable English translation with variants from two other manuscripts. There is no linguistic discussion. Needless to say this edition will be a splendid tool for any Celticist who wishes to study linguistically an early Welsh monument. The scholarship of Professor Parry is impeccable; but at the same time it gives an impression of being too impersonal. I know that it is considered the height of skill nowadays, in editing a work, to stand aside and let nothing but the text speak for itself. But some of us yearn just the same for more expression of opinion, more interpretation, more analysis of language and style—particularly when the editor is a master of his subject as Professor Parry undoubtedly is.

URBAN T. HOLMES, JR.

A Concise Comparative Celtic Grammar. Pp. xix + 442. By HENRY LEWIS and HOLGER PEDERSEN. Göttingen: Vandenhoek & Ruprecht, 1937.

The Preface tells us that this book is not intended to replace Pedersen's Vergleichende Grammatik der keltischen Sprachen (2 vols., 1909–13), but to serve as a preparation for it, giving in condensed form as much of the material in the larger work as seems to suffice for university courses in the Celtic countries. Accordingly, the arrangement of topics is the same, and at the top of every page stand references to the corresponding sections of the VGKS.

There are however some omissions. Thus we no longer have the

introduction on the relationships of the Celtic languages, their borrowings, the peculiarities of Celtic (VGKS 1.1–29); the chapter on the Celtic reflexes of the IE alternations (1.171–89), and the sections giving the ancestry of the sounds, traced back from the individual languages (1.506–33); those on nominal composition (2.1–14), derivation of nouns (2.15–62), and on nicknames (62–3). The index of the Celtic words had 126 pages in the older edition, against 39 in the new work; the index of words in non-Celtic languages (30 pp.) and the index of topics (6 pp.) are omitted in the new work.

Even apart from these omissions, the 403 pages of text are a great reduction from the 544 + 678 pages of the VGKS, which has been achieved by the omission of subsidiary matter largely repeated at other places where it was more in point, and by a reduction in the number of linguistic examples. At the same time the etymological comparisons with other IE branches are not slighted, and the whole has been brought up to date. In the bibliography (xiii–xix) nearly one-third of the items are later than 1913, when the second volume of the VGKS was issued. The present work will be most serviceable for its professed purpose.

ROLAND G. KENT

The Dialect of the Nuzu Tablets. Pp. 50. By CYRUS H. GORDON. Reprinted from Orientalia 7.32–63, 215–32. Rome: Pontifical Biblical Institute, 1938.

This study is a comprehensive and independent treatment of the same subject which Albrecht Goetze has discussed in a recent issue of LANGUAGE.[1] The two accounts coincide naturally in a number of details, but their respective differences outweigh the similarities. These differences concern primarily questions of scope and mode of approach. Gordon's presentation is essentially descriptive. Goetze examines the material from the standpoint of comparative Akkadian and shows a keen interest in problems of syntax. The two treatments supplement each other admirably.

It should be stated at the outset that Gordon's work is mature, judicious, and well-documented. The following remarks aim to further discussion on a few controversial issues and to offer suggestions on a number of minor points.

Both Gordon and Goetze now employ the term *Nuzu*, instead of the common *Nuzi*, to designate the city from which the dialect under discussion has taken its name. The form *Nuzi* is found hundreds of

[1] LANG. 14.134–43.

times, whereas *Nuzu* occurs rarely, by itself[2] or as the second element in personal names.[3] The reasoning which underlies the proposed change is this: *Nuzi* is found only after prepositions; in Akkadian, prepositions govern the genitive, which happens to be indicated by *-i*. The nominative element is *-u*; an occasional *Nuzu* is therefore presumptive evidence that the subject case of the city-name was *Nuzu*, not *Nuzi*.

This argument is far from conclusive. First, the name has no Akkadian etymology, having originated in all likelihood in one of the non-Semitic groups settled on the fringes of ancient Akkad;[4] consequently, it is not subject to the rules of Akkadian grammar. Second, the few occurrences of the city-name as *Nuzu* are not decisive because they, too, come after prepositions, where a genitive is required;[5] in other words, the supposed nominative would be inorganic anyway. Now it is a well-known fact, which Goetze himself has emphasized in another connection, that in these texts *u* and *i* are often confused for phonetic reasons, regardless of any flexional considerations.[6] Such confusion would account for the appearance of the form *Nuzu*. Third, an Elamite text lists our city as *Nu-u-za*.[7] This occurrence, while casting added doubts on the existence of Akkadian influence in this instance, attests the variation *i*:*a* which is paralleled by the partly analogous forms *Kakzi* and *Kakza*;[8] these forms represent another well-known city-name of plainly non-Semitic origin.

[2] Cf. Harvard Semitic Series (HSS) 5.55.5; 78.9.

[3] Cf. Joint Expedition at Nuzi (N) 505.5 (*It-ḫi-ip-Nu-zu*).

[4] For the common ending *-i* in place-names of the Lullu area (east of the district from which these tablets come) see Speiser, Mesopotamian Origins 91 (1930). C. J. Gadd, Revue d'Assyriologie 23.65 (1926) aptly compares *Nuzi* with the well-known Assyrian military station *Kakzi* (between Mosul and Arbil). The only available etymology for *Nuzu* would be Sumerian ('not know'), more suggestive than plausible in this particular connection. From the standpoint of historical geography one might adduce with some show of reason Kassite *nazi* 'shadow, protection', adding that Nuzian *u* corresponds to Kassite *a* in the respective designations of the people themselves (*Kuššuḫay-*:*Kaššû*); but any etymology of the place-name under discussion will remain obscure until the languages of the region north and northeast of Akkad are better known than they are today.

[5] Personal names compounded with the element *Nuzu* belong to a different category. Even at that, the above *Itḫip-Nuzu* (see note 3) is in a genitive relation.

[6] Lang. 14.139. To Goetze's and Gordon's discussions of the Nuzi sibilants there may now be added the fuller treatment by Speiser, JAOS 58.175–93 (1938).

[7] See G. Hüsing, Assyriologische Bibliothek 24 (1916) 54.17.

[8] Cf. R. P. Boudou, Liste de Noms Géographiques 92 (1929).

To sum up, there is absolutely no proof that *Nuzi* should be *Nuzu* in the subject case, if the name was declinable at all. On comparative onomastic grounds there is the inherent probability that the form ended in *-i* and that the rare variant forms should not be judged by the rules of Akkadian morphology. Since *Nuzi* has enjoyed wide currency from the first publication of the texts in question twelve years ago, it would be unfortunate indeed if the countless references to the site by that name were to be upset by what is at best an extremely doubtful correction.

1.12. Gordon cites two instances to illustrate the change of short *u* to *i* in unaccented open syllables. These occurrences represent Akkadian words and reflect an attempt at treating reduced vowels.

1.19. It is stated here that *l* may be assimilated to *n* under the influence of a labial. Not one of the examples given is convincing. In the alleged instances of *na/la*, **na* is palaeographically closer to *la* than to *na*; moreover, in *na-aḫ-*na-ap-du* (for *naḫlaptu* 'cloak') one should reckon with the possibility of dittographic confusion. The word for 'cloak' is interesting for a reason not mentioned by Gordon: it is found twice under the form *naḫaptu*, where the loss of *l* is confirmed by the writing *na-ḫa-áp-ta* in an independent text from Ashur.[9] Parenthetically, one should be exceptionally cautious in operating with the interchange of *n* and *l* in the Akkadian material from Nuzi because such an interchange is attested indeed in the Hurrian material from the same site.

2.12. Gordon entertains the idea that some Nuzi scribes may have tried to specify the gender of the suffixed possessive pronoun *šu* ('his' in Akkadian; 'his' and 'her' in Nuzi) by specializing the two common signs for *šu* to represent masculine and feminine respectively. The assumption is hazardous, to say the least: because (a) a simpler way would have been to use the normal feminine *ša*, as was done indeed on occasion by the Nuzians;[10] (b) the native language of Nuzi (Hurrian) seems to have had no specific forms for the feminine.[11] What we are asked to believe is that a scribe went out of his way to devise an artificial differentiation of gender in writing, although his native language

[9] Keilschrifttexte aus Assur juristischen Inhalts 77.9. For the occurrences of this form in the Nuzi texts see M. Berkooz, The Nuzi Dialect of Akkadian (Language Dissertations 23) 57.

[10] This fact is pointed out by Gordon himself in Amer. Jour. Sem. Lang. 51.5 (1934); for the feminine suffix *-ši* after verbs see ibid. 7–8.

[11] LANG. 14.141 note 59.

did not recognize such distinctions and the language in which he wrote could attend to the problem quite satisfactorily.

3.9, 16. Gordon follows now the reviewer's treatment of the Hurrian numerals, retracting his previous objections; cf. 30 (175).

3.20. The compound *iš-še-ir-ta* (add reference to N 344.36) does not contain the numeral for 'ten'. The context shows clearly that the meaning is 'on the morrow' (which happens to be on the sixth day) and not 'on the tenth day'. The component elements are *ina šêrti* 'in the morning', followed by the common adverbial ending *-a*.

3.26. The Hurrian numeral *kik* does not call for the broad statement that it 'stands for some numeral up to 9, excepting 2 and 4 whose Hurrian values are known'. Elsewhere[12] I have suggested 'three' on the basis of corresponding passages employing Akkadian numerals.[13] 'Five' is less likely, and anything higher may be left out of consideration.[14]

4.12 (5). I see no good reason for assuming 'a native phoneme intermediate between *z* and *r*, perhaps close to Czech *ř*'. The word in question (*ḫuzzuzu* 'memorandum note') is Akkadian, not native; moreover, the presence of a native phoneme approximating *ř* is open to serious doubts.[15]

Ibid. (10). Gordon's remarks on *trḫ* in the texts from Ras Shamra help to rule out of court the alleged Syrian counterpart of the biblical Terah.

4.17 (156). Add a reference to [*z*]*i-li-ku-uḫ-lu-* N 389.22. Gordon's earlier identification of this Hurrian word with Akkadian *šîbu* 'witness' is now doubted by E. R. Lacheman (cf. 32), who would translate 'cowherd'. I have been entertaining similar doubts on the ground that we have the Hurrian term for 'witness' in the phrase *širumma epēšu* 'testify' (cf. 4.17, no. 178). In such passages as N 347.11 'shepherd' (not 'cowherd') would do just as well as 'witness', and in the personal name *Teḫip-ziluk*[16] either value would be appropriate. Unfortunately,

[12] JAOS 56.405 (1936).

[13] Cf. D. Cross, Movable Property in the Nuzi Documents (Amer. Or. Series 10) 21-2 (1937).

[14] Ages higher than five years occur only twice in connection with cattle (ibid. 16). The numeral 'nine' is mentioned twice with lambs (ibid. 27) and must refer therefore to months, not years. For the passages with *kik* see Speiser, Annual of the Amer. Sch. Or. Res. (AASOR) 16.131. In two of those passages the numeral refers to asses, and the only (Akkadian) numerals used to specify the ages of these animals are 'three' and 'five' (Cross, Movable Property 21).

[15] JAOS 58.180-1.

[16] AASOR 16.73.36.

however, herdsmen seem out of place in N 389, where the *zil.* are mentioned in connection with sesame, and Gordon's original suggestion was probably sound.

5.6. Instead of *Nabû-ilu* read *Nabû-ilâ,* on account of the interesting form of this name in AASOR 16.59.39 (cf. ibid. 109), supported by the Assyrian eponym-name *Nergal-ilâ-ya.*[17]

8.14. Additional material concerning the Hurrian substratum in Nuzi is given in AASOR 16.136–42.

E. A. Speiser

[17] Reallexikon der Assyriologie 2.452, 457 (*Urigal-*).

EDUARD PROKOSCH

Eduard Prokosch was born in Eger, Bohemia, on the 15th of May, 1876. His father was the principal of a grammar school. Prokosch graduated from the classical gymnasium of his native city in 1894, and then studied law in Prague and Vienna, passing the state bar examination in 1897. He came to the United States in 1898 and served for a time as secretary of the Austrian vice-consulate in Milwaukee. In 1900 he completed a course of training at the German-American Teachers' Seminary.

Prokosch was married in 1901 to Mathilde Dapprich of Baltimore; his wife survives him, as do their three children, Gertrude, who is the wife of Professor Hans Kurath of Brown University, Frederic, well known as a poet and novelist, and Walther, an architect.

From 1901 to 1904 Prokosch served as an instructor in German at the University of Chicago; at the same time he pursued his studies, receiving the degree of M.A. in 1901. During the summer semester of 1904 he studied at Heidelberg, and during the following two semesters at Leipsic, where in 1905 he took his doctor's degree under Sievers. His thesis was entitled: Beiträge zur Lehre vom Demonstrativpronomen in den altgermanischen Dialekten.[1]

From 1905 to 1913 Prokosch taught at the University of Wisconsin. In the latter year he was called to the University of Texas as professor of Germanic philology; he lost this position during the war, chiefly because of a correct factual statement in his text book Introduction to German (published in 1911) concerning the representation, under the Empire, of the German people in the Reichstag.

An enlightened administration called Prokosch to Bryn Mawr College; he taught there from 1919 to 1928. During the academic year 1927–28, in addition to his professorship at Bryn Mawr, Prokosch held a research fellowship at Yale and served as head of the department of German at New York University; this latter position he held also during the next year. In 1929 he was called to Yale University as professor (after 1930, Stirling professor) of Germanic languages.

[1] Published Halle a. S., 1906.

Prokosch was a SIGNER OF THE CALL that lead to the foundation of the LINGUISTIC SOCIETY OF AMERICA, president of the Society in 1930, and its delegate to the American Council of Learned Societies at the time of his death. For several summers he taught in the Linguistic Institute of the Society.

He took an equally active part in the Modern Language Association of America, serving as treasurer 1924–8, on the Executive Council, on the Committee on Research Activities, and as president in 1937.

In recent years he has helped many displaced German scholars start life anew in this country.

His life ended in an automobile accident on August 11, 1938.

Eduard Prokosch found it possible to work sixteen hours a day; he forgot nothing that he had attentively heard or read; he did not know the meaning of worry, pain, or fear and he dealt kindly and understandingly with people and with dogs. His knowledge was vast, not only in his professional domain and in German literature (which he treated as a hobby), but also in apparently remote matters of science and of practical life. He walked, skated, and swam without fatigue, made himself equally at home in forest or city, and dealt plainly and agreeably with all manner of persons and situations. His sense of humor was excellent and even boisterous, but one was likely to find humor in his serious speech, for, seeing more clearly than most men, he ignored so many pretences, postures, and superstitions that his serious statements had often the devastating effect of sheer humor. Affairs of society and politics, including even the Great War, left him intellectually unmoved, however senseless he might find them. He once called himself an anarchist, a puzzling description, since he belonged to no sect; doubtless he meant to indicate his failure to participate in obnoxious or merely pompous conventions. One evening, watering the lawn, he laid down the hose to step toward me, who had come for a visit; his three-year-old daughter picked up the hose and drenched him. 'Now Papa will have to go upstairs', he said, 'and change his clothes.' This method of educating children as well as pupils succeeded admirably, though lesser men might not be well advised to try it.

Of Prokosch as a teacher one can speak only in personal terms. In the summer of 1906 I came, fresh out of college, to Madison, to be looked over for an assistantship. Desiring to earn an academic living, I had developed no understanding or inclination for any branch of science. The kindly Professor Hohlfeld delegated Prokosch, one of his young instructors, to entertain me for the day. On a small table in

Prokosch's dining room there stood a dozen technical books (I seem to remember that Leskien's Old Bulgarian grammar was among them) and in the interval before lunch Prokosch explained to me their use and content. By the time we sat down to the meal, a matter perhaps of fifteen minutes, I had decided that I should always work in linguistics. At the end of the two years of pupilhood that followed, I knew no greater intellectual pleasure than to listen to Prokosch. Apart from the specific direction to linguistics, this incident must be multiplied into hundreds, if one would estimate the effect of Prokosch as a guide to younger people.

Beside his graduate classes, Prokosch taught his quota of sections of elementary German. The vague optimism of treatises on Methods of Teaching Foreign Languages turned into dust when Prokosch explained the nature of this work. Every form, lexical or grammatical, which he uttered before the class, was registered in a card index, with provision for its recurrence. He used, of course, the 'direct method', though these words may mean little or nothing. Copious deliberations and surveys have brought this matter no farther today than Prokosch had carried it in 1906. His Introduction to German is probably still the best beginner's German book, but it is scholarly, requiring study on the part of the pupil and a command of German on the part of the teacher.[2] Later, less sinewy versions and supplements adapted the content to more popular use.[3] The beginner's book in Russian covers less ground, but dealing with a more difficult language, serves better to illustrate the methodical approach.[4]

Of Prokosch's scientific publications, Sounds and History of the German Language and An Outline History of the German Language pursue a pedagogic aim, but contain many original views.[5] The Faust essay was the outgrowth of a hobby.[6] Prokosch's more strictly tech-

[2] New York (Holt) 1911.

[3] German for beginners, 1913; Deutsches Lese- und Übungsbuch, 1913; Deutscher Lehrgang. Erstes Jahr, 1916; First German lessons in phonetic spelling, 1916; Konversations- und Lesebuch (with C. M. Purin), 1916; all published by Holt in New York. Compare also The teaching of German in secondary schools, Austin, Texas, 1915 (Bulletin of the University of Texas, Number 4); I have not seen The college teaching of German, 1920.

[4] Elementary Russian grammar, Chicago (University of Chicago Press) 1920.

[5] The former was published in 1916 by Holt, the latter in 1934 by the Oxford Press.

[6] In Studies in German literature in honor of A. R. Hohlfeld 184–216. Madison, Wisconsin, 1925. (University of Wisconsin studies in language and literature, Number 22.)

nical studies—apart from the comparative grammar of the Germanic languages, which is still in press—sufficed to give him an international reputation and to put him easily at the head of Germanic students in our country.[7] One finds here clarity, originality, and an immediate grasp of the concrete. Nevertheless, those who knew Prokosch found that these writings expressed only a minor phase of the man. He strode gallantly and brightly through a world that was not ready to make full use of his powers.

L. BLOOMFIELD

[7] The following is surely an incomplete list: AJP 32.434 (1911); 33.195 (1912); 38.432 (1917); GR 1.47 (1926); JEGP 11.1 (1912); 16.1 (1917); 20.468 (1921); 21.119 (1922); MP 11.71 (1913); 26.459 (1929); IF 33.377 (1914); PMLA 42.331 (1927); Studies in English philology in honor of F. Klaeber 196 (1929); Studies in honor of H. Collitz 70 (1930); a series of articles in the 1930 volume of Monatshefte für deutsche Sprache und Pädagogik, Milwaukee. Reviews: JEGP 21.353 (1922); 22.298 (1923); 24.135 (1925); 32.225 (1933); MP 15.59 (1917); 21.331 (1923); LANGUAGE 9.89 (1933).

NOTES AND PERSONALIA

THE MEETING OF THE LINGUISTIC SOCIETY, held at Ann Arbor July 29–30, was highly successful. At each session from 75 to 100 persons were present, and the attendance of 58 members was recorded. The Proceedings will be printed in Bulletin No. 12, to be issued with the first issue of LANGUAGE for 1939.

PRINCE NIKOLAI TRUBETZKOY, Professor of Slavic Philology in the University of Vienna, died on June 25, 1938, in his forty-ninth year. He was elected to Honorary Membership in the LINGUISTIC SOCIETY at the end of 1936.

TRUMAN MICHELSON, Ethnologist of the Ethnological Bureau of the Smithsonian Institution, and a Signer of the Call that led to the formation of the LINGUISTIC SOCIETY OF AMERICA, died on July 26, 1938, in his fifty-ninth year.

He was born at New Rochelle, N. Y., on Aug. 11, 1879, and studied at Harvard University, where he received the A.B. in 1902, A.M. in 1903, and Ph.D. in 1904. He afterward studied also at the Universities of Leipzig and of Bonn, and with Prof. Boas at Columbia University. Most of his scholarly work was in the American languages. Since 1910, when he became attached to the staff of the Smithsonian Institution, he spent all his vacations in the field among the Algonquin tribes. The fruits of his work are found in a number of larger volumes and almost countless shorter papers and reports. He was a member of many learned societies.

ARNOLD WALTHER, Assistant Professor of Hittitology in the University of Chicago, and a member of the LINGUISTIC SOCIETY OF AMERICA from 1931, died suddenly on May 19, 1938.

He was born in 1880 at Hamburg, Germany, and was educated at the Universities of Rostock, Halle, and Marburg; he studied theology and oriental languages, and passed the two state examinations in theology, but he later decided to devote himself to oriental languages, and received the Ph.D. degree at Leipzig in 1914, with a dissertation in the Babylonian field. After the World War he specialized in Hittitology,

then a very young science, editing for the Berlin Museum five volumes of Keilinschriften aus Boghazköi (vols. 5, 6, 16, 18, 22), containing Hittite divination texts. In 1930 he joined the staff of the Oriental Institute of the University of Chicago, working upon the Assyrian Dictionary project, and in 1931 he was appointed Assistant Professor of Hittitology.

Dr. Walther was a most methodical and conscientious worker, whose patient study is evidenced by the accuracy of his work on Hittite texts even in the years when their interpretation was still very uncertain. His publications were largely in Hittite and the Assyro-Babylonian field, but do not convey an adequate idea of the range of his learning, which extended also to ancient and modern Indo-European languages, and to general linguistics. His wide learning made him a most valuable collaborator on the Assyrian Dictionary project, and it will be difficult, if not impossible, to repair his loss.

Those who came into close contact with him retain especially two impressions of his personality: his unpretentiousness in behavior and appearance, despite his upright carriage and the spiritual features of his bearded face, which immediately betrayed his scholarly vocation; and his unbounded readiness to help others with either his knowledge or his restricted means. He will be much missed.

ARNO POEBEL

AMERICO ULYSSES N. CAMERA, Professor of Romance Languages and Head of the Department in Brooklyn College, and a Foundation Member of the LINGUISTIC SOCIETY OF AMERICA, died in New Orleans on August 28, 1938, after an illness of only two days. He was fifty-nine years old.

Born in Italy, he came to this country as a boy and was graduated in 1900 from the New York University, receiving also the M.A. in 1901 from Columbia University and the Ph.D. in 1912 from New York University. He was head of the Department of Romance Languages in the Townsend Harris Academy, then became a member of the faculty of the City College, and when the Brooklyn branch was established in 1926 he was transferred to it, serving there until his death. He was secretary of the Faculty of Brooklyn College from the time when it became an independent college in 1930, and an important worker in faculty committees. His scholarly interest was in the nineteenth century romantic poets.

JOSEPH WILLIAM HEWITT, Professor of the Greek Language and Literature at Wesleyan University (Middletown, Conn.), and a member of the LINGUISTIC SOCIETY OF AMERICA from 1926 to 1937, died suddenly at Middletown, Connecticut, on July 8, 1938.

He was born in Leeds, England, on August 23, 1875, and came to the United States when ten years old. He received the A.B. degree from Bowdoin College in 1897, and the A.M. and Ph.D. from Harvard University in 1900 and 1902. After teaching at the High School of Hanover, N. H., at Worcester Academy, and at Bowdoin College, he went to Wesleyan University in 1905, where he served until his death, doing important administrative work in addition to his teaching and research. From 1925 to 1931 he was Secretary and Treasurer of the American Philological Association, and Editor of its Publications. He was a member of the Archaeological Institute of America, and a member of the Managing Committee of the American School of Classical Studies at Athens.

DR. CURT R. GOEDSCHE, Instructor in German in the Northwestern University, has been promoted to an Assistant Professorship.

DR. MARY R. HAAS has received from the American Philosophical Society a research grant for investigating the history and development of the extant towns which formerly comprised the Creek Confederacy. The problem is to be attacked from a linguistic as well as an historical angle. In particular, it is expected that much new light will be thrown on the history of the towns by a study of the dialectal differentiations among them.

DR. R.-M. S. HEFFNER, of the German Department of Harvard University, has gone to the University of Wisconsin.

DR. RALPH MARCUS, Professor in the Jewish Institute of Religion, officially represented the Linguistic Society on the Committee of Sponsors for Testimonial Celebration in honor of Dr. Cyrus Adler on his Seventy-fifth Birthday, September 13, 1938.

DR. GEORGE J. METCALF, of the University of Kansas, has gone to Washington University, St. Louis, as Assistant Professor of German.

DR. HARRY A. ROSITZKE, formerly of the Municipal University of Omaha, is now at Rochester University, as Assistant Professor of English.

THE FOLLOWING NEW MEMBERS FOR 1938 have been received into the Linguistic Society of America subsequent to the last published list, and up to September 13, 1938:

Harold B. Allen, A.M., Asst. Editor of the Early Modern English Dictionary, Univ. of Michigan; 420 W. Jefferson St., Ann Arbor, Mich.

Arthur F. Carlson, A.B., Teacher of English and Latin in the High School of Trout Creek, Mich.; 221 East Oak St., Ironwood, Mich.

William F. Edgerton, Ph.D., Professor of Egyptology, Oriental Institute, Univ. of Chicago, Chicago, Ill.

Albert F. Gerberich, Ph.D., Associate Prof. of Modern Languages, Dickinson College; 36 W. Pomfret St., Carlisle, Pa.; *Germanic linguistics.*

S. I. Hayakawa, Ph.D., Instructor in English, Extension Bldg., Univ. of Wisconsin, Madison, Wis.; *semantics.*

Laura E. Heminger, M.A., Instructor in English, Michigan State College, East Lansing, Mich.

Carleton Taylor Hodge, 915 S. First St., Springfield, Ill.; *epigraphy.*

Vernam Hull, Ph.D., Associate Editor of the Early Modern English Dictionary, Univ. of Michigan; 1705 Hill St., Ann Arbor, Mich.; *Celtic.*

William Rockwell Leete, M.A., 383 Winthrop Ave., New Haven, Conn.; *Chinese.*

Stuart E. Mann, B.A., Teacher of English in the English Institute of Brno; Sirotci 55, Brno, Czechoslovakia; *Albanian.*

Karl H. Menges, Ph.D., Professor of Slavonic Languages in the Faculty of History, Languages, and Geography, University of Ankara; Ankara-Yenişehir, Okāla sakağı 6, Ankara, Turkey; *Turkish and Mongolian languages and ethnology.*

Herbert H. Petit, M.A., Instructor in English and Spanish, John Carroll University; 2532 Green Road, South Euclid, Ohio; *Anglo-Norman.*

Mrs. James M. Plumer (Carol Plumer), 1505 Golden Ave., Ann Arbor, Mich.; *Indic languages.*

Mabel Quay, M.A., Elementary Teacher in Waco Public Schools; 2816 Morrow St., Waco, Texas; *Latin.*

Albert van Erden, Ph.D., Instructor in Modern Languages, Princeton Univ.; 22 Edwards Place, Princeton, N. J.; *Germanic linguistics.*

BIBLIOGRAPHY OF AMERICAN INDIAN LINGUISTICS 1936–1937

MORRIS SWADESH

UNIVERSITY OF WISCONSIN

[This bibliography is planned to furnish complete annual lists of books and articles on American Indian languages, with brief indication of contents. It will include texts in the original languages, with or without translations, as well as grammatical and etymological studies and dictionaries and vocabularies. Works which are not primarily linguistic (e.g., ethnological works, travelogues) will be included when they contain linguistic material of considerable quantity or of special importance because of lack of other published data. Critical reviews of specifically linguistic works will be included.

Readers are asked to send to the compiler the data of any items omitted, for listing in the next bibliography. Authors are urged to send offprints, particularly if their work has appeared in a journal where it might be overlooked. All cooperation is appreciated, though it is impractical to attempt specific acknowledgement of each case. For this issue acknowledgement is due to others and especially to Kenneth L. Pike for searching a number of Latin-American journals.

Entries are alphabetical by authors, numbers being assigned to facilitate cross-reference. A summary by stocks and regional areas is given at the beginning.]

Abbreviations

AmA American Anthropologist, The American Anthropological Association.

Ant. Anthropos, St. Gabriel-Mödling.

BBAE Bulletin of the Bureau of American Ethnology.

CUCA Columbia University Contributions to Anthropology, New York.

HAIL Handbook of American Indian Languages, vols. 3 ff. (vols. 1 and 2, BBAE); New York: Columbia University Press.

IJAL International Journal of American Linguistics; New York: Columbia University Press.

ILM Investigaciones Lingüísticas, Mexico.

JSAP Journal de la Société des Américanistes de Paris.

Kroeber Vol. Essays in Anthropology Presented to A. L. Kroeber in Celebration of his Sixtieth Birthday; Berkeley: University of California Press.
Lg. Language, journal of the Linguistic Society of America.
PAES Publications of the American Ethnological Society, New York.
UCP University of California Publications in American Archaeology and Ethnology, Berkeley.

Summary

	1936	1937
Algonkian	15,23,24	9,10,18,19
Athapaskan	16,20	8,15,21
Siouan	10,21	3
Uto-Aztekan	2,13,17,27,28,29	25,31,32,34,38–9,44,49
Other N. America	4,5,8,9,18,22,26	2,4,5,6,12,14,20,22,26, 30,32
Central America	12	7,13,23,24,27,28,33–7, 40–3,45–50
South America	1,3,6,7,11,19,25	1,11,16,17,29

1936

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3. Gusinde, Martin: Plantas medicinales que los indios Araucanos recomiendan; Ant. 31.555–71, 850–73. Includes botanical vocabulary.
4. Haas, Mary R.: review of Hoijer, Tonkawa (HAIL 3.1–148); AmA 38.115–6.
5. Haeckel, Josef: review of Boas, Geographical Place Names of the Kwakiutl Indians (CUCA 20), Ant. 31.1006–8.
6. Henry, Jules: The Linguistic Expression of Emotion; AmA 38. 250–6. Remarks on intonation and stylistic devices in Kaingang.
7. Imbelloni, J.: Lenguas Indígenas del Territorio Argentino; Historia de la Nación Argentina 1.177–208, Buenos Aires.
8. Jacobs, Melville: review of Andrade, Quileute (HAIL 3.151–292); Andrade, Quileute Texts (CUCA 12, 1931); AmA 38.314–5.

9. Jacobs, Melville: Texts in Chinook Jargon; University of Washington Publications in Anthropology 7.1–27. Text and translation.
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11. Lehmann-Nitsche, R.: Die sprachliche Stellung der Choropi (Gran Chaco); Zeitschrift für Ethnologie 68.118–24, 303–4.
12. Long, Richard C. E.: Maya Writing and its Decipherment; Maya Research 3.309–15. Criticises Whorf's essays at decipherment.
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18. Ray, Verne F.: Native Villages and Groupings in the Columbia Basin; Pacific Northwest Quarterly 27.99–153.
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20. Sapir, Edward: Internal Linguistic Evidence Suggestive of the Northern Origin of the Navaho; AmA 38.224–35.
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25. von Kinder, Leopoldo: Gramática y Vocabulario de la Lengua Huitota; pp. xiii + 243, Pasto, Colombia, Imprenta del Departamento. Practical grammar (with exercises) with Spanish-Huitoto vocabulary.
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2. Bass, Althea: With Benefit of Grammar; The Colophon, New Series 2.517–36. The experiences of Ann Eliza Worcester Robertson in learning Creek and translating liturgical literature.
3. Boas, Franz: Some Traits of the Dakota Language; Lg. 13.137–41. Contraction in fixed combinations, popular re-etymologizing of compounds, sound symbolism, particles expressing attitude.
4. Bunzel, Ruth: Zuni; HAIL 4.389–415. Review, 26 below.
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6. Clark, C. U.: Documents sur les Indiens de la Californie; JSAP 29.227–34. Extract from ms. of Prince Paul of Wurttemberg containing short vocabularies of the 'Hok' and 'Kosumé' languages of California.
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8. Drucker, Philip: The Tolowa and their Southwest Oregon Kin, UCP 36.221–300. Ethnological treatment, containing vocabulary.
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17. Loukotka, Čestmír: La familia lingüística coroado; JSAP 29. 157–214.
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