On Laryngeals and Pronouns

For Frits Kortlandt

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I. *The Type Skt. goṣáṇiḥ*

In his famous Notes (1942, 70–91) Kuiper discussed the origin of *go-ṣáḥ/go-ṣáṇiḥ* 'winning cows'. Comparing Lat. *indigena, agricola*, he started from a form in **-senH-s* > *-saniḥ* and assumed that this nominative was replaced by *-sāḥ* on the basis of the oblique cases, e.g. gen. sg. *-sanah* < **-senH-os* on the model of a supposed **vṛtra-háḥ, -hánaḥ*.

His explanation is rejected by Kellens, Noms racines 1974, 106–11, who holds that there is no evidence for forms like **vṛtra-háḥ*, who stresses that Ved. gen. *paśu-ṣás*, LAv. *fšu-šō* only require a root *sā-* (with zero grade **-sH-as*), and who points out that gen. *-sanah* can be **-sn̥H-es* as well as **-senH-es*. He therefore assumes zero grade everywhere: **-sn̥H-s* > **-saHs* > *-sās*, **sn̥H-m̥* > **-sanam*, **sn̥H-es* > *-sanas* etc., and explains the *i*-forms as derivatives with *i* (thus also Debrunner, AiGr II 2, p. 294).

The first part of Kellens' interpretation seems to me quite convincing. It may also be noted that, apart from the absence of a nominative *-s*, there are strong doubts on the evidence (for full grade) of the Latin words, on which see Leumann, Lat. Gramm. 1977, 280f. But about the *i*-forms Kuiper must have been right in assuming that the *i* represents the laryngeal. A formation with *i* (*-san(H)-i-*) is of course quite possible, but 1) it would not explain why it is found only in the nominative and accusative, and 2) it is

not necessary to assume another formation. (On p. 31 Debrunner takes this view, AiGr. II 2.)

For the forms I refer to Kuiper 79f., 82 and Debrunner l.c. We can reconstruct the following paradigms from the Rigveda:

nom. sg.	*-sắḥ*	*-sániḥ*
acc.	*-sắm*	*-sánim*
gen.	*-sás*, LAv. *-šō*	*-sanaḥ*
dat.	*-sé*	(*-váne* VS)
loc.		*-svani*
nom. pl.	*-sắḥ*	
voc.		*-svanaḥ*
acc.		LAv. *-šānō*, *-hānō*

(The length of the *ā* of the last, Avestan form, is undoubtedly secondary; Kellens 111.)

It is obvious that these two paradigms are different developments of the one older paradigm which had something abnormal, as was Kuiper's view. I agree with Kellens that we must start from a paradigm with zero grade, because if we start from a full grade nominative there is no good way to explain *-sāḥ*. I think we can explain both paradigms, if we take into account the successive stages of development.

PIE *-snH-s*	>	PIIr. *-saH-s*	>	*-sās*
-snH-m	>	*-sanH-a(m)*	>	**-sanam*
-snH-es	>	*-sanH-as*	>	*-sanas*

In the accusative *-sn̥Hm̥* resulted in *-san(H)a(m)*; thus also Kellens.

Especially the discrepancy between the nominative and the accusative was intolerable. So the paradigm was broken up. It is simple to understand that **-saH-s*, *-sās* go the inflection of other nominatives of this shape, acc. **-saH-m*, gen. **-sH-as* etc. If we assume that the other forms got a new nominative at the third stage (above), it would certainly have got ***-sā* after the *n*-stems. This is not what happened. But if the change took place at stage 2, the accusative and the other cases with **-sanH-* would have got a nominative **-sanH-s* (> *-saniḥ*). (Note that an acc. *-sanim* cannot represent **-sanHm*, though this is often stated, because in such a form Sanskrit vocalized the nasal, which would have given *-sanHa(m)*.) After all the changes are very simple: in the accusative the *n* was dropped, or in the nominative an *n* was introduced.

I don't think that the distribution of the forms refutes this explanation. I counted in the Rigveda:

nom. acc. sg. m. f.	*-sā-*	50	*-sani-*	6
other cases	*-s(ā)-*	8	*-san-*	2
superl. (*-sātama-*)		18		0

-sani- was the younger form, which made its appearance in the religious language only late. (*-sāḥ* was very resistent, because it was very frequent.) Kuiper (81) stresses that we have always *-vani-*, *-svani-*, never **-vā-*, **-svā-*. (Hitt. *assusani* is no good evidence for skt. *-sani-*.)

A well known parallel for two full paradigms developed from one is Skt. *jani-* beside *gnā-* 'woman', GAv. *jani-*, *gənā*.

Add. During a lecture in Aarhus, J. E. Rasmussen and one of his students suggested to me a simpler solution: full grade in the nominative but zero grade in the oblique cases, with generalization of the zero grade. Thus:

1.	2.	3.
sanH-s	*sani-s*	*snH-s*
snH-as	*snH-as*	*snH-as*

In phase 1. a split into two paradigms is improbable, as the type was quite normal. Essential is that the development *-Hs* > *-is* was prior to that of *snH-as* > *san(H)-as* (otherwise the two paradigms could not have developed from nom. *sanH-s*, gen. *san(H)-as*), and prior to *snH-s* > *saH-s*. I see no objection to this assumption. In this way we get a relative chronology of these developments.—In my explanation the existence of *sanH-as*, with *-H-*, from *snH-as* could be doubted.

II. *The Laryngeal Umlaut in Sanskrit*

1. When Kuiper discussed *goṣáṇiḥ* he had not yet developed his view that *n̥HV* became *inV* in Sanskrit. The evidence presented for it (ActOr. 20, 1948, 29–35) was small, but seemed to provide a simple solution for some difficult forms. It may be well to look at these forms again in the light of our present views.

The forms given were: *sína-*, *simá-*, *śimī-*, *stimita-* and *timirá-*.

2. *sína-* 'provision'(?) RV would belong to the root of *sanóti*. The connection is not more than a possibility. It is not generally accepted. (Wackernagel's alternative (AiGr. 1.17), connection with *ásinvant-*, is accepted by Renou, EVP 14, 84.)

3. *simá-* 'self' was explained as **sm̥Ho-*.

For Sanskrit two words *sama-* have been established: *samá-* 'equal, like, the same' and *sama-* 'irgendein, jeder', The first corresponds with Gr. *homós*, Goth. *sama*, OIc. *samr*, the latter with Gr. *hamo-* (in compounds), Goth. *sums*, OIc. *sumr*. When we want to reconstruct the PIE form of these words, we must start from

the comparative evidence before we connect them with other words, however evident it may seem to us to connect the one or other, or both, with other words, e.g. with the IE word for 'one'. It is therefore not correct to posit that one of these forms (i.c. the second) cannot have had a laryngeal because **sem-/sm-* 'one' does not have a laryngeal, as Strunk did (Proc. Int. Congr. Lingu. 11, 1972, 376ff.). The first group of forms requires **somHo-*, with a laryngeal to account for the absence of long vowel. The second form shows the development of a vocalic nasal before vowel, so it must have been **sm̥Ho-*.[1]) This means that Skt. *sama-* 'irgendeiner' in all probability continues **sm̥Ho-*, with the development *m̥HV* > *amV*. This means that *simá-* cannot go back to a form **sm̥Ho-*. It should be noted also that there is no comparative evidence for a form **sm̥Ho-* with the meaning 'self'. Therefore *simá-* is not cognate with *samá-* or *sama-*, and there is no ground to posit a pre-form **sm̥Ho-* for it. It can be no argument for a development *m̥HV* > *imV*.

4. The origin of *simá-* has to my mind been given by Burrow, Skt. Lg. 1955, 274: *simá-* has the same relation to *sīm* as *imá-* to *īm*. That is, just as there are 'thematic' forms beside **im* (later *īm*), there were made thematic forms on **sim* (later *sīm*).

The meaning of *simá-* is now generally given as 'self', probably on the basis of Wackernagel, AiGr. 3, 578, who writes: "Die richtige Erklärung gab Geldner Ved. St. 2 (1892) 188f.; bestätigt und verbessert wurde sie von Oldenberg zu RV 1,95,7." He does not say, however, in what way Oldenberg corrected it. Geldner assumed 'selbst, sich selbst', but Oldenberg showed, convincingly I think, that it was "betontes 'er', 'er selbst'" and once 'sich' (it is not well possible to express these differences in English). This means that it has exactly the same meaning as *sīm*. *simá-* simply provides the other case forms to *sīm*. It may be recalled that Sanskrit has no

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[1]) Sihler, JIES 1 (1973) 111–3, suggests that the laryngeal is the dual marker. He accepts Anttila's interpretation of *sem-* as '(several things) taken together as one', cf. *zusammen*, as opposed to **oi-* 'one (alone), a single'. **smH-* would then be 'a pair'. I doubt whether **sm-H-* could be taken as '*two* together'. The notion 'together', posited by Anttila, Introd. 1972, 366f., does not convince me. It can well be derived from 'one'. There is a clear enough opposition between (a neutral) 'one' and **oi-* 'alone, single' (like Lat. *unus* as against *singulus*, *solus*). **somHo-* and **smHo-* are evidently derived from **sem-* 'one'. I have no difficulty with this: Dutch *eender* 'equal' is a derivation from *een* 'one', as is *enig*, *enige* (Germ. *einige*) 'a certain, several'.

gen.-dat. **se*. (It has been asked whether Sanskrit lost **se* or whether Iranian and MIA created it. Emeneau (Lg. 51, 1975, 467), for example, thinks Sanskrit never had it. But OHitt. *se* < **soi* shows that it was of PIE date. In any case Indo-Aryan lost it and *simásmai* was created in Sanskrit. Once *simá-* existed, it provided even a nominative.) That it gets the value 'er selbst' is rather due to the use of a pronoun in the nominative. (This explanation of *simá-* confirms that *sīm* goes back to **sim*.) See part III.

5. *śimī-*. With *śim(ī)-* we have: *śimīvant-* RV (10×), *śim(i)yā* RV 1.151.1, *śima-* and *śímyati* TS. It must be pointed out that *śímyati* cannot represent **k'm̥H-i̯e-*, as this gives *śā́myati* (RV); and that *śimīvant-* cannot be **k'(e)mH-uent-*, which would be **śamīvant-*, **śāmvant-*. Kuiper did not have this problem, because he posited a form **k'$_e$mH-C* > *śimi-C*. However, this reduced vowel is now no longer acknowledged by most scholars. Kuiper suggested (ActOr. 20, 1948, 30f.) that the instr. sg. *śím(i)yā* would be an old ablauting form from *śámī*. The Rigvedic forms of *śamī* are:

acc. sg.	*śam-īm* 2×	ins. sg.	*śámī* 8×
gen.	*-yās* 2×		*śámiyā* 2× (1. 83,4; 2. 1,9)
ins. pl.	*-ībhis* 8×		*śímiyā* 1× (1. 151,1)

Judging from the numbers and the distribution in the Rigveda the normal instr. sg. was *śámī*, *śámiyā* (and *śímiyā*) being younger forms. This is confirmed by the fact that the instr. sg. in *-ī* in the *devī́*-type occurs only with this word. There can be no doubt, then, that the aberrant *śámī* is an archaic form. See App. 3. It is improbable that *śímiyā* is another (very) archaic instr. sg. Further, an ablaut nom. sg. *śámī*, instr. sg. *śímiyā* would be of a type so archaic that it is hard to find anything comparable (even) in the Rigveda. Compared with *śámī*, *śímiyā* has the normal, i.e. (in this case) younger, ending *-(i)yā*. And lastly, here too we have the problem that we would expect **śāmyā*. We must conclude that it is improbable that *śímiyā* is an archaic form and that it probably did not belong originally to the paradigm of *śámī*. For, as *śámī* is the old instr. sg., and *śámiyā* the younger one, it is almost impossible that *śímiyā* would also belong to this paradigm.

It is remarkable that there is no **śamīvant-* beside *śímīvant-* (10×). This suggests that we have to do with two different words. On semantic grounds the two roots have also been separated. Renou, EVP 10, 89 says: "mieux vaut ... conserver à l'hapax *śímī* (...) le sens de 'force violente, force dans le combat' plutôt que 'zèle',

et séparer décidément d'avec *śam-*; plus tard (TS. etc.) *śim(yati)* est 'préparer la victime animale', euphémisme connu en partant de" faire un acte de violence". On the other hand *śimyati* and *śamyati* occur in exactly the same context. The relation between these forms is not clear. Perhaps the association between the two words is of later date. Essential is, however, that none of the forms seems to go back directly to a PIE pre-form.

6. For *stimita-* (class.) 'stiff, motionless, quiet' Kuiper accepted the connection with OIc. *stemma*. I think Mayrhofer (Etym. Wb. s. v.) is right in questioning this connection: "Unglaubhaft bleibt aber die Verbindung des späten Wortes mit an. *stemma* . . ." (On *timita-*, *tímyati* see Kuiper p. 32). Here again the attested form cannot have *im* < *ṃH* regularly, as **stṃHto-* would have given **stāta-*.

7. For *timirá-* (epic) 'dark(ness)' connection with *tam(i)-* seems probable, but it should be noted that **tṃH-ro-* would have given **tāmra-*. The suffix *-ira-* which could explain **tṃH-ira-* > *timira-*, is probably not old: the *i* originated itself from a laryngeal. No cognate forms are known with *imV* that could have had *ṃHV*.

8. Five forms were adduced as evidence, *sína-*, *simá-*, *śimī-*, *stimita-* and *timirá-*. Of these the interpretation of *sína-* is not generally accepted. For *simá-* there is no reason why it would represent **sṃHo-* (which is found in *sama-*); and there is another explanation. *śimī-* is probably not of the same root as *śámī*, and *śímiyā* not an old instrumental of *śámī*, for both formal and semantic reasons. The etymology of *stimita-* is uncertain. Both this word and *timirá-* cannot have *im* < *ṃH* regularly: the following *i* itself represents a laryngeal and the words can only have been formed after *H* > *i*, which means probably also after *ṃH* lost its laryngeal. Also both these words are late. Thus, *simá-* and *śimī-* are almost certainly no evidence for the supposed development, *stimita-* and *timirá-* are late and cannot go back directly on a form with *ṃH*. Only *sína-* would be possible, but it cannot alone bear the burden of proof.

It may also be noted that since Kuiper first published this interpretation, no new evidence has been found.

9. Positive evidence for *NH* > *aN* is not very abundant (otherwise there would have been no problem). This may be due partly to the fact that *aN* can also continue a full grade. It might be suggested that *aN* replaced phonetic *iN*, as forms with *iN* would

give strange paradigms. This is not probable, because 1) it is not more strange than *ir* beside *ar*; and because 2) there should be *some* isolated forms to prove original *iN*.

The positive evidence I can adduce is as follows:

sama- 'irgendeiner' < **smHo-*, see II 3 above;

the forms with *-san-* according to the explanation given in part I above;

"Die scheinbar nicht abgelauteten schwachen Formen (from *ami-*) *amánti* [. . .] sind regulär, da auch idg. *m̥* vor Vokal *am* ergibt." Hoffmann, KZ 83 (1969) 193 = Aufsätze 288;

of the root *sani-* we have thematic aorists *sana-*, Av. *hana-*, which will represent **snh₂-e/o-*. In the same way, from *vani-* 'win', RV *vanáti*, *vanéma*, GAv. *vanaēmā* will represent **u̯n̥H-o-*. Thus perhaps *tamat* from *tami-* 'faint'.

tanú- 'thin' probably continues **tn̥h₂u-*. All cognates clearly have zero grade except Lat. *tenuis* and Lith. *tę́vas*, Lett. *tiẽvs*. (On Gr. *tanaós* etc. see Beekes, MSS 34, 1976, 9–12.) Lat. *tenu-*, however, can be **tn̥Hu-* as well; for *n̥H* > *en* see App. 4. The Baltic forms can only go back to **tenHuo-*, but this does not prove full grade for the *u*-stem, as it can be a *deiuos* form. As all other forms have zero grade or can have it (Lat., Skt.), it is probably that all forms had zero grade.[2])

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[2]) Skt. *uttāná-*, Av. *ustāna-* seems to have puzzled scholars. Mayrhofer (Wb.) does not explain it and only cites Insler's suggestion (IF 73, 1968, 61 n. 5, without any adstruction; Gāthā's 1975, 116) that it is an aorist participle **-tn-āna-*. This is clearly no more than an attempt. As far as I know such participles never occur in compounds (Macdonell, Vedic Gramm. 171: "A present participle occasionally occurs . . .; also a perfect-participle)." The right interpretation was given long ago, e.g. P. Persson, Beitr. z. idg. Wortforsch. 1912, 648, taken over by Walde-Pokorny 1, 722, Pok. 1065 (*tn̥no-* misprint for **tn̄no-*): it was **tn̥H-nó-*. This form has probably not been accepted because *tatá-* shows an anit root. But the laryngeal is clear in the adjective, and we have to accept more often the coexistence of roots with and without laryngeal. Debrunner (AiGr. II 2,727) rejected a *na*-participle, because of the *ā* and the accentuation. It is not quite clear what his problem is with the *ā*. Probably that *-na-* after *ā* occurs "erst nach dem RV" (§ 560b). But this refers to roots in long *ā*, which would have had *ǐ* or *ir*, *ūr* in the zero grade. *-tāna-* however is an old zero grade. The accentuation cannot be a decisive counter argument. And there are parallels, see II 1,225 § 93a with Nachtr. 225,35. In II 2, § 30e Debrunner analyses it as *-tān-a-*, but this type has "gewöhnlich aktivische Bedeutung, doch kommt auch passivische oder intransitiv-reflexive vor". But of the passive meaning *uttāná-* is the only clear example. The counter arguments, then,

sanutár probably has *sanu-* < **sn̥Hu-*. This form requires a longer discussion, which is given in section 10.

The evidence is reliable enough. So we must accept that *NHV* became *aNV* in Sanskrit.

10. A number of comparable forms meaning 'without' is found. We can distinguish:

1) *s-n*: *sanutár*, Av. *hanarə*, Toch. *sne/snai*, Lat. *sine*, OIr. *sain*;
2) ?*s-n*: Gr. *áneu*, *ánis*;
3) *s-n/m*: OE *sundir*;
4) ?*s-n/m*: Gr. *áter*;
5) *sv-n*: OCS *svěnъ*;
6) *V-n*: Goth. *inu*, OHG *ānu*.

Number 5) is very aberrant, in initial as well as in vocalism, and must be left out of consideration.

We should first consider group. 1) *sine* has been derived from **sani*, **s°ni* or **seni*. The first two are not possible, see App. 4. OIr. *sain* < **sani-* can be **sn̥Hi-* or **sH(e)ni-*. As the latter form would give Lat. **sani*, it is less probable. As **sn̥Hi* would give Lat. **seni* (see App. 4), **sn̥Hi* explains Latin and Old Irish directly. Toch. *sne/snai* is given as derived from **sanai*. However, Proto-Tocharian **sanai* would have given **sāne/sānai*. Also PToch. **sanai* would presuppose **sH(e)nH(e)i*, of which the first laryngeal cannot be reconciled with Latin. *sne/snai* can be derived directly from **snHi*, for in Tocharian a laryngeal is vocalized before the (other) resonants, and **snHi* > PToch. **snai* gives *sne/snai*. (This interpretation not only gives nice confirmation of the laryngal theory, but it also shows clearly that the vocalization is a fact of the individual languages: **sn̥Hi* cannot account for the Tocharian forms.)

As to 4), Gr. *áter* cannot derive from **snH-*, so does not belong to group 1). I think Gonda (Reflections Num. 1953, 33f.) has shown that *áter* (for **atér* as Aeolism or as proclitic) and 3) *sundir* etc. belong together as **sm-ter*, from **sem-* 'one'. Both words mean 'apart, separately' from 'one of a number of individuals'.

The initial of 2) Gr. *áneu*, *ánis* must be **n̥H-* (unless one would posit **h_2n-*). It should be pointed out that the final group *-eu* is without parallel in Greek, and must be very archaic. The *u* may

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are not valid, the alternative explanations not convincing. In favour of **tnH-nó-* pleads that *-nó* is found frequently with disyllabic roots, see II 2,728 sub e.

be cognate with that of *sanutár*. If *ánis* is old, and not after *khōrís*, it corresponds exactly (enlarged with *-s*) to *sine* etc. It seems therefore probable that it goes back to the same form as *sine*, **snH-i*, **snH-eu*. It has been supposed that there were *s*-less forms, as with *super*: Skt. *upári*, but the assumption seems unnecessary.[3])

On the assumption of such *s*-less forms group 6) has been connected. They go back to **enu*, **ēnu* in first instance. As the vocalism is also different, it is better to keep them apart (though **enu* could stand as **enH-u* beside **snH-eu*).

Returning to *sanutár* and Av. *hanarə*, it is clear that they belong to the first group, and therefore it is probable that they go back to **snH-u-tér*, **snH-ér*, rather than to **senH-*.

11. Another consideration which I have not seen yet in this connection is that one would expect the same treatment of *ṃ̥H* before resonant as before vowel. We find *ṃ̥H* > *ām* before resonant, which shows the development of *ṃ̥* into vowel + nasal (and a long vowel). If *ṃ̥H* developed into *im* before vowel, we would expect *īm* before resonant. There is no trace at all of *īm*. We do find *im* (*śímyati*), but this cannot be old because of its short vowel (and it was shown above that *śim-* does not belong to the root *śami-*). (See App. 1 on the explanation of the long vowel.) The forms witn *ām* < *ṃ̥H* before resonant, then, confirm that *ṃ̥H* became *am* before vowel.

III. *Skt. sīm and Related Forms*

1. The following sections are to a large part based on an observation made by my colleague F. H. H. Kortlandt. As he has no time at present to publish it, he authorized me to do so. (Of course, I can only present it as best as I can; the author of the idea may have done better.)

The presentation and discussion of the Latin, Oscan and Umbrian paradigms of the pronoun Lat. *is ea id* by Blümel, Unters. Lautsyst. Morph. vorklass. Lat., 1972, 78–81, brought Kortlandt to the following reconstruction of the Oscan-Umbrian and Proto-Italic para-

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[3]) I wonder whether **smHo-* > Gr. *hamo-* contradicts Kortlandt's assumption (Lingua Posnan. 23, 1980, 127f.) that h_2 (and h_1) were neutralized into h_3 before (and after) *o*. I would then expect **smh₂o* > **smh₃o-* > **homo-*. Alternatively it would show that there was no laryngeal umlaut in Greek either. This problem is also raised by *áneu* if it represents **(s)nH-eu*. A h_2 would have given *-au* (for restoration of *-eu* there is no basis), h_1 would show that *ṇ̥h₁* did not give *en-*.

digm, which has consequences for PIE. The most important form for my subject is the nom. sg.

(Laryngeals have been neglected, for once.)

	Oscan			Umbrian			Proto-Osco-Umbrian		
sg. N.	izic	ioc	idic	ere(c)		*eřec*	es	eiā	id
A.	ionc	*iak*	idic	eu	eam	*eřek*	eiom	eiām	id
G.	eizeis			erer	erar		eiseis	eisās	
D.				esmei			esmōi	eisāi	
A.	eizuc	eizac		eru	erak		eisōd	eisād	
L.	eizeic	[*e*]*ísaí*		*esme*			esmei	eisāi	
pl. N.	iusc		ioc	eur-ont		eo	eiōs	eiās	eiā
A.			ioc		eaf	eo	eions	eiāns	eiā
pl. G.	eisunc	eizazunc		ero(m)			eisom	eisāsom	
D.	eizois	eizasc		erir-ont	*ererer-unt*		eisōis	eisās	
L.	eizois	eizasc					eisōis	eisās	

	Latin			Proto-Italic			Vedic		
sg. N.	is	ea	id	es	eiā	id	ayám	iyám	idám
A.	em, eum	im, eam	id	im	eiām	id	imám	imā́m	idám
G.	eius			esio	esiās		asyá	asyā́s	
D.	eī			esmōi	esiāi		asmái	asyái	
A.	eō	eā		eisōd	eisād		asmā́d	asyā́s	
L.	eō	eā		esmei	esiāi		asmín	asyā́m	
pl. N.	(i)ī, eī	eae	ea	ei(oi)	eiās	eiā	imé	imā́s	imā́
A.	eōs	eās	ea	e(io)ns	eiāns	eiā	imā́n	imā́s	imā́
G.	eōrum	eārum		eisom	eisāsom		eṣā́m	āsā́m	
D.	īs	īs		eifos	eiāfos		ebhyás	ābhyás	
L.	īs	īs		eisōis	eisās		eṣú	āsú	

	Proto-Indo-European					
sg. N.	e	ī	id	pl. ei	ās	ā
A.	im	īm	id	ens	āns	ā
G.	esio	esiās		eisom	āsom	
D.	esmōi	esiāi		eimus	āmus	
A.	esmōd	esiās		eios	āios	
L.	esmin	esiām		eisu	āsu	

Oscan-Umbrian

To several forms -(*i*)*ke* has been added.

In Oscan *e* developed into *i* before all vowels except *e*; *eio- eia-* became *io- ia-*. In Umbrian *eiso-* became *ero-*.

Nom. sg. Blümel assumes that the *e-* of Umbr. *erec* is due to the other forms. He points to the interchange *i-*/*e-* in this pronoun. But the question is how precisely this interchange originated. The forms

Osc. *esídum* (: *izic*)

Osc. *essuf*, Umbr. *esuf* < **es-pso* (: Lat. *ipse*)

rather show that Oscan too had originally *e-* in the nom. sg. Otherwise the forms with *e-* in Oscan can hardly be explained. The Umbrian *e-* is confirmed by

Umbr. *esto-* (: Lat. *iste*; Kuiper, Med. Kon. Ned. Akad. Wet., Nr. 1.9, 1938, 6ff. already stressed that *e-* must be old in this pronoun).

In Umbrian the *e-* was retained because it was supported by the oblique cases, in Oscan it was changed because here many forms got. *i-*.

Proto-Italic

By comparing Latin with Proto-Osco-Umbrian we must reconstruct Proto-Italic.

Nom. sg. Latin replaced **es* by *is* after the acc. *im*.

Proto-Indo-European

Nom. sg. The Indo-Iranian evidence shows, like that of Italic, that this form was **e*, not **i*. Av. *aēm* < **ayam* confirms that Skt. *ayám* is the Indo-Iranian form (on OP below). IIr. **ayam* is based on earlier **ai* < **ei*. This must be **a* (< **e*) with added *-i* from the acc. sg. (and after the presence of **ai-* < **ei-* in the plural). If the nominative would have been **i*, it would not have been changed into **ei* > **ai*, because it would have been identical to the stem of the acc. sg.; when these two go together, it is impossible that one of them is changed by influence of the other cases. OP *iyam* (m.) has its *i-* instead of *a-* from *im-*. It shows that there was rather a tendency to adapt the nominative to the accusative (which is also what happened in Indo-Aryan, though in a different way). It should be noted that the masculine and feminine forms were probably not identical at the time the *i-* was introduced:

IIr.	*ayam	*iHam	*idam
OP	*iyam	*iHam	*idam

IIr. **a*, the basis of **ai*, shows that Italic must have had **e*.

Germanic confirms this reconstruction. Compare:

Goth.			OE			OS			OHG		
is	si	ita	hĕ	hēo	hit	hē, hĭ	siu	it	er	si, siu	iz
ina	ija	ita	hine	hīe	hit	ina	sia	it	in	sia	iz
is	izos		his	hiere		is	ira		(sīn)	ira	es

etc.

The OHG *e-* is unexplained. It simply continues PIE **e*. Goth. *is* can be the phonetic development of **es*. OS, OE have **e* with *h-* added (this *h-* spread to all forms in OE) OHG gen. sg. n. *es* shows that *e-* was the original stem in the oblique cases (except the acc.). (It is not probable that the *e-* of the nom. sg. m. came from this form (or the oblique cases), because in that case one would expect it also in the nom. sg. neuter.) The *i-* of West Germanic must derive from the accusative **im*, the *i-* of Gothic can have the same origin or represent the regular development $e > i$. (Fem **sī* is **ī* with added *s-* after (**so*) **sā*. In West Germanic **sī* was reshaped into **siā*. Its *s-* went in OS, OHG to the acc. sg. f. and to all nom. acc. plural forms.)

The reconstructed **e* is further confirmed by the Old Irish personal pronoun *é sí ed*. Here the *e*-vocalism of *é* has been explained as due to the oblique cases, but it could as well be old. Here again it is improbable that the influence of the acc. **im* would not have been prevalent. (Thurneysen, GOI § 450, considers **ei(-s)* to account for the length. In that case we would have the same development as in Indo-Iranian. But **e* would also become *é*, GOI p. 32b.)

It is especially this form that proved essential for my considerations in the following sections. We shall find indirect evidence that the reconstruction **e* is correct.

Dat. pl. On the ending *-mus* see Kortlandt, Slavic Accentuation, 1975, 50. The reconstruction of the PIE ending is based on OLith. *-mus*, OCS *-mъ*, the raising in North and West Germanic *-um* < **-omus*, and the absence of **bh* from the Armenian dat. pl. ending, where **m* was lost before **u* (cf. AArmL 2, 30). Indo-Iranian and Italo-Celtic introduced **bh* from the instr. pl. ending into the dat. and abl. pl. forms.

Acc. pl. I wonder whether the ending **-ens* is found in the Hittite gen. pl. ending *-enz-an*.

2. This part started as another appendix to part II, (3–4 on *simá-*) but it has grown in size and, perhaps, in importance.

The origin of Skt. *sīm* is difficult. It has been proposed that it is a recent formation, based on dat. **se* after *īm* (Mayrhofer Wb. 3, 475). This seems less probable to me. One would rather expect the dative to be based on the accusative than the other way round. Nor was there much reason to create *sīm*.

Because of **se*, which recalls Gr. *hoī*, which probably belongs to the reflexive pronoun, it has been supposed that *sīm* originates in

the reflexive. But the reflexive seems rather clear: it had **seue*, **soi*, **se*, and it is difficult to see how **sim* would fit in.

The problem would require a broad discussion, which I cannot undertake here. I would like only to make some suggestions.

One way to put the problem is as follows. To *sīm* belongs nom. sg. f. **sī* (to which *sīm* owes its long vowel). Now a pronoun consisting of a nominative feminine and an accusative (in Sanskrit m., f. and n.) only is too defective to be acceptable; pronouns may be defective, but not in this way. We must see whether there is a nominative masculine.

I base myself on two starting-points. 1) **sim* is an anaphoric pronoun, and it would be as well to look for an anaphoric origin of **sim*. Therefore **so *sā *tod* comes to mind, which Meillet, Introd. 325, describes as: "Un démonstratif (anaphorique) renvoyant à une personne, à une chose précédemment nommées ou déjà connues." 2) Kortlandt's reconstruction of Lat. *is ea id* as PIE **e *ī *id*, given in the preceding section. If now we assume a formation parallel to **e *ī* (**id*) with the *s-* of **so *sā* (**tod*), I come to a nom. **se*, acc. **sim*, thus:

nom.	*se	f.	*sī	beside	*e	f.	ī
acc.	*sim				*im		

I supposed that these forms existed beside **so *sā*, just like **kʷi-* beside **kʷo-*, but Kortlandt, suggested to me that these forms perhaps originated because the **e *ī *id* forms got *s-* (and *t-*) from **so *sā *tod*. I will return to this question in some of the sections, but I just want to draw attention to the existence of these forms here.

3. I suggest that this system provided the reflexive pronoun, for which I quote Szemerényi, Einführung 204: "diese Formen (of the reflexive) sind von dem anaphorischen **se* nach dem Muster von **t(w)e* usw. gebildet worden" (with litt.; the same view by Brugmann, Grundr. II 2, 395f.).

It is easy to see how **se* 'he' could become a reflexive pronoun. A reflexive pronoun is an object pronoun indicating that the object is identical with the subject. There is no need, then, to have a separate pronoun for every person: it is sufficient to indicate that the object is identical with the subject in general, and it is not necessary to indicate which person the object is. Thus Hittite has one form (particle) only. — If there is no such pronoun, the identity can be indicated, at least suggested, by repeating te subject (after the objectpronoun), stressing that it is really the subject (mentioned)

itself which is the actor. Thus Lat. *semet ipse*, or older *sumpse* 'him-he'. This second pronoun can also be put in the accusative, as in Gr. *heautón*. The second element can therefore be regarded as a more precise statement of the object (as in English *himself*). On the other hand the personal pronouns, to which category the anaphoric > reflexive was very close, had an accusative in *-e*. The fact that in German (and in Dutch) there is only a reflexive for the 3. pers. confirms that the reflexive can have originated from a 3. pers. pronoun.

In this way form and meaning of the reflexive can be accounted for.

One would have to show the different developments of the system.

4. One may ask what the neuter of this paradigm was. Judging by Skt. *ayám, iyám, idám*, Lat. *is. ea, id*, one would expect a form in *-id*. For a form **sid* I find no evidence, but if the interchange of *s-* and *t-* operated here as well as in **so- *to-*, we could expect **ti(d)*. This form is found in the Anatolian reflexive particle **-ti*, Hitt. *-z*, Luw., HLuw. *-ti* (perhaps Lyc. *ti*). My attention to the importance of this particle was drawn by G. Schmidt, Stammbildung u. Flexion idg. Pers. pron. 1978, 145–166 (espec. 149f.). He connected this particle with **siH, *sim*. Schmidt rightly pointed out that **ti* must be very archaic. He supposed that the original form of the acc. sg. belonging to **si-* was **ti*, which was in some languages replaced by **si-m*.

Schmidt pointed out that this pronoun was halfway between the gender pronouns and the personal pronouns and therefore has the characteristics of both. I think he is right in this, but he has gone one step too far. He saw that it was necessary to reconstruct a paradigm hithertoo unknown, but my objection is that his paradigm fits into none of the known types, neither that of the gender pronouns nor that of the personal pronouns. (Especially when he goes on to posit that **si-H* was the original gender-indifferent nominative, I cannot follow him, because this is speculation for which there is no evidence.) On the other hand my paradigm is one of the gender pronouns (parallel with Skt. *ayám* etc.), as one might expect for a demonstrative/anaphoric pronoun. Therefore my paradigm, has a neuter beside a masculine accusative singular, thus:

nom.	**se*	**si-H*	**ti(d)*
acc.	**si-m*	**si-H-m*?	**ti(d)*

I think there is no objection to the Anatolian particle deriving from a neuter form. Note that, in the case of the neuter, there is no re-

interpretation of a nominative as an accusative, as both forms were identical. Of course, this identification would remain not more than a possibility, but we shall see that there is at least other evidence for this neuter form.

I postulate a neuter **ti* without *-d*, for several reasons to be mentioned below but especially for Anat. **ti*, but I am not sure about this. Here I shall point only to other evidence for *d*-less forms. In Hittite we find:

sg.	*ki, kē*	*apāt*	*at*	*eni*
pl.	*ki, kē*	*apē*	*e, at*	*e*

The form in *-i, -e* occurs more often in the plural. It seems improbable to me, however, that they originated in the plural: there is no reason why the plural form would have replaced that of the singular. Though the plural forms could have *-ih_2, I would consider it possible that the plural forms are identical to the singular ones, there being originally no special neuter form for the plural. (Cf. my note on Hittite neuter plurals in IIJ 23, 1981, 278f.) As to *eni* (Laroche in Meid-Neu, Hitt. u. Idg. 147ff.), it has *-ni* probably from the acc. sg. *uni* (< *un-i*). This form shows that *at* (in the singular) is secondary for **e*. This seems further indicated by HLuw. *api* beside Hitt. *apāt* (if this form is reliable).

Perhaps Skt. ntr. *kím* is relevant. This form has not been definitely explained (AiGr. 3, 558ff.; Burrow, Skt. Lg. 272). The co-occurrence of *kím* and *kát* in the Rigveda suggests that the *i*-stem did not have a *-d*. The form is confirmed by the particle *kīm*. This form is no doubt identical in origin with *kím*; it got its long vowel from the particles *īm, sīm*. It confirms the antiquity of the *m*-form: such particles are mostly neuters. And the particle is found also in Avestan, LAv. *mā(δa)cim, naēδacim* (but GAv. *naēcīm* is acc. sg. m.). If it is not of PIE date (where it is without parallel), *kím* must either be the acc.sg.m.f. replacing *cit* (found as particle in Sanskrit and Avestan; LAv. also has *cit* as a neuter pronoun), or it must replace another original form (*cit*, **ci*?) reshaped into **cim*. The first of these possibilities (an original acc. sg. m.f.) seems to me the easier one. Note that LAv. seems to have also *ciš* as neuter.

Another problem presents Goth. ntr. *hwa* as against OIc. *hvat*, and Goth. *þata*. Krahe-Seebold, HLFL 1967², 104 suppose *k^wom, comparing Skt. *kím*, Krahe, Germ. Sprachwiss. 2, 70 also considers an endingless form. Gutenbrunner, HLFL Altisl. 1951, 113, states that Old Norwegian and Old Swedish point to the same form as Gothic. I have no opinion on this form. Note that this is an *o*-stem.

5. A few other points may be noted first. According to the **so *seh₂ *tod* paradigm we should expect *t-* outside the nom. m.f. Indeed, one could suppose that the other forms originally were **toi, *tim*. As to the dative, even Hittite has *-si*, OHitt. *-se* < **-soi*, so the *s-* here was perhaps already of PIE date (see § 12). As to **tim*, Schmidt, Stammb. 150f., suggests that LAv. *dim* etc., OP *dim* etc., OPr. *din* etc. have *d-* for older *t-* due to a preceding voiced consonant. He further connects Gr. *min, nin* from **nim* from **dim* after nasals. Here the older explanation (from *-d im*) is to be preferred; see App. 5. On **sim* itself see § 12.

As to the acc. f., one may expect **siH-m*. (A younger form is found in Old Irish, for which I quote Vendryes, Lex. étym. irl. anc. 1974, p. S 2: "A l'acc. fém. après préposition, le pronom apparaît en irl. suffixé sous une forme *-e* qui assourdit la finale sonore de la préposition ou gémine l'*r*, etc. . . . On peut en induire une forme **-he* < **-se*, venant elle-même d'un celt. **sian* ou **siin*, acc. fém. d'une forme élargie du démonstratif (cf. l'acc. sg. vha. *sia*) . . ." This **siin* < **siām* is **sī* with the ending of the *ā*-stems.)

6. Risch has shown (Asiat. Stud. 8, 1954, 151f.) that OP *$h^a y^a$*, *$t^a y^a$* must be read *haya, taya* instead of *hya, tya*, and Rüdiger Schmidt defended this interpretation in an 'Epitaphios' (Studia Iran. 5, 1976, 149–154). This means that Skt. *sya- tya-* is isolated in Indo-Iranian, and the evidence for PIE **sio- *tio-* elsewhere was already called doubtful. It may be useful to have a look at the Sanskrit forms.

The only serious explanation which does not assume PIE **sio-* is that it has the suffix *-i(i̯)o-* (Brugmann, Grundr. II 2, 320). This would explain the forms with *-iy-* and the frequent adjectival use. However, one does not expect such a derivation from a pronoun. Also one would expect in that case a nominative *syáḥ*, but this form is late, the old form being *syá*. Most important is that it does not explain the distribution to be mentioned below.

To my mind the origin becomes manifest as soon as one considers the facts presented by Wackernagel, AiGr. 3, 550. The Rigvedic forms are:

sg.	syá	40		syā́	16	tyád ⎫	37
	tyám	42				tyád ⎭	
	tyásya	1		tyásyās	1		
			ins.	tyā́	1		
du.	tyā́	17		tyé	2		

pl.	tyé	27	tyás	4	tyáni	1
	tyán	3	tyás	1	tyá	3
	tyám	2				
	tyébhis	1				

(For the meaning see Delbrück, Altind. Synt. 221: 'jener bekannte'.) The pronoun occurs predominantly in the nom. acc. sg. and the nom. pl. (m.).

The distribution mentioned above is that *syá* is *siyá* in one fourth of its occurrences, that *tya-* is *tiya-* in 1/5, and that *syá* is never ****siyá***. This cannot be accidental (we would expect 1/5 or 1/4 from 40 = 8 or 10 forms with *-iy-*). It seems permitted to reconstruct for a period slightly anterior to the Rigveda:

**syá* **siyá* **tiyád*

This makes its origin evident: the starting point was the fem. **sī*, which was changed into **siyá* (or **siH* into **siHaH*). This fits in exactly with the fact that Sanskrit has no *sī*, whereas Avestan, which has *sī*, does not have **s(i)yá*. It is clear that it is an innovation of Indo-Aryan.

If the neuter was **ti*, addition of the final of *tád* would have given *tyád*, not *tiyád*. *-iy-* was not taken over from *siyá*, because then we would expect it also, and even earlier, in *syá*. If the neuter was **tad*, *-iy-* cannot have come from *siyá* for the same reason. The only remaining possibility is that *-y-* was taken from *siyá* and added to **ti*, and to *s-*. Only the existence of forms with *(t)i-* elsewhere in the relevant forms can explain *tiya-*, whereas the absence of *-iya-* in *syá* requires a form without *i* there. This is found in our paradigm:

PIE	*se	*siH	*ti	Skt.	*sa	*sī	*ti
	*tim	*tiHm?	*ti		*tim		*ti

It is clear that this **sa* cannot have been **so*, which belongs with **sā* **tod*; it must have been **se*. (Remarkable is the absence of an acc. sg. fem. Perhaps *sīm*, which functions for all genders, was in origin feminine.)

Here is another reason for positing **ti* rather than **tid*, though **tid* could have been reshaped to *tiyád*.

Some scholars came very close to this solution. Thus Brugmann opens the section after **si̯o-* (§ 324) with: "Bei der nahen Verwandtschaft des Formans *-(i)i̯o-* mit dem fem. *-ī-* (. . .) mag hier noch das ebenfalls zu **so-* gehörige **sī* genannt werden." And Schwyzer (Fs. Schrijnen, 1929, 365 n. 1): "Doch ist mir mit Rücksicht auf **sī* (. . .) wahrscheinlicher, daß es neben **se* **so* **te* **to*

auch einmal die Varianten **si *ti* gab (wie **kʷi *k'i* neben Formen auf *-e -o* stehen), und daß diese die Ausgänge von **so *sā *tod* annahmen." We have stressed already that it is exactly the co-existence of forms in *-e* and in *-i(H)* which explains the origin of this pronoun. It is another attempt, beside *simá-* (see II 4), to reorganize the original paradigm.

7. The Germanic forms cited as evidence for PIE **sio- *tio-* are OIc. *siá* and the OHG, OS forms *diu, thiu* etc.

The OHG and OS forms are explained by Krahe (Germ. Sprachwiss. 1965[4] 2, 59ff. and Guxmann a.o. (Sravn. Gramm. Germ. Jaz. 1963, 3, 339f.) from PIE **si̯- *ti̯-*. This is very unlikely, because it is ununderstandable why this stem would have been used only in a few forms. I give the relevant forms with their correspondences in the other languages:

	Goth.	OIc.	OE	OS	OHG
m. ISg.	þe	þvé	þȳ	thiu	diu
f. NSg.	so	sú	sēo	thiu	diu
ASg.	þo	þá	þā	thia	dia
NAPl.	þos	þaer	=m.	=m.	dio
ntr. NAPl.	þo	þau	=m.	thiu	diu

The instr. sg. seems clear if one looks at the interrogative pronoun, where we find OE *hwȳ, hwī*, OS *hwī, hwīu*, OHG *(h)wiu*. We have a form in *-ī* from the *i*-stem pronouns, enlarged with the ending *-ō* of the *o*-stems.

The forms that remain are nom. acc. fem. and neuter. OE has only one form with *-i-*, nom. sg. f. *sēo*, which corresponds to *thiu, diu* (for **siā*). The conclusion is evident: it was the fem. **sī* that was rebuilt into **siā*, exactly as in Sanskrit (independently, of course). This is confirmed by the 3 sg. fem. personal pronoun:

Goth.	OE	OS	OHG
si	hēo	siu	si, siu

The feminine plural forms will be analogic after the singular.

Interesting are the neuter plurals. Evidently all forms ending in *-ī* were rebuilt into *-iā* (or its reflex). This means that these languages had a neuter plural **tī*. It cannot have been taken from elsewhere, because the rare pronominal *i*-stem forms of Germanic present no neuter plurals. Therefore these forms confirm the reconstruction of a pronoun with neuter plural **ti-H*, which implies a neuter singular **ti(d)*.

We should of course be careful not to repeat the mistake of projecting forms back into PIE too soon. Perhaps, when **sā* existed

beside **siā*, ntr. pl. **tā* was replaced by **tiā*. But I see no motivation for such a change, and a formal model alone is not enough.

That this interpretation is correct is shown by another fact. Some languages have a deviant form in the nom. sg. masc. The forms are:

	Goth.	OIc.	OE	OS	OHG
NSm.	sa	sá	se, sē	sē, thē	der

sa and *sá* continue **so*. But the other languages, the West Germanic languages, exactly those that have **siā* for older **sī* in the feminine, have *se*. The long vowel is due to secondary lengthening under the accent. We can be certain that Old High German also had **se* originally. This form has been explained as **so-i* and compared with Goth. *sai* 'see'. However, the Gothic form is rather a shortened imperative than **so-i* (thus Mossé, Manuel langue got. 1956², 310). Important is that no language outside Germanic has a form **so-i*. The Slavic parallel forms cited by Brugmann, Abh. Sächs. Ges. Wiss. (Phil.-hist. Kl.) 22, 1904, 28 (OCS *se*, Serb. *eto*, *eno*, *evo*) as well as Lith. *taĩ* are neuters; Lat. *ecce* is not clear (see Ernout-Meillet, Dict.), but the explanations given assume here too a neuter. The connection with Goth. *sai* must be given up. In that case WGm. **se* could still be **so-i*. As said before, there is no evidence for such a form in PIE. And in the rest of the paradigm there is no such *-i*, nor is there anywhere else in Germanic. It is clearly the only solution that one could find until now. The real solution is evident: WGm. **se* is **se*, with an old, PIE, *-e*.

OIc. *siá*, nom. sg. masc. and fem., has been connected with OHG *diu* etc., and a preform **sio-* postulated. Later this interpretation has been abandoned. It belongs to a paradigm where the *s*-pronoun was enlarged with a particle. Therefore it is now explained (Gutenbrunner, HLFL Altisl. 1951, 113) as m. **sē+a*, f. **sī+a*. The latter form contains **sī*, which is thus also attested in Old Icelandic in the *so/se*-paradigm. The form **se* is identical with OE etc. *se*. The form is important as it shows that **se* was not only West Germanic.

The pronoun 'this', to which *siá* belongs, has more forms that are relevant. It has either a particle *-a* or *-se*. The West Germanic neuters, which are called difficult, are:

OE *þis* OS *thit* OHG *diz* (*z* = *ts*)

They could go back to **tid* > **þit*, preserved in Old Saxon, with *-t* replaced by *-s* in Old English (where *-s* is found in all other forms) and with *-s* added in Old High German. The existence of 'urnor-

disch' *þit* is doubtful, but a stem *ti-* is postulated by W. Krause, Sprache urnord. Runeninschr. 1971, 53 (and 120) for acc. sg. *þin* 'this'.

From Viking times we have the following forms, here grouped according to the particle (after Gutenbrunner 112; the classical forms in the last column):

sg. m. N.	sa-si			
A.	þan-si		þan-a, þin-a	þenna
G.				þessa
D.	þaim-si			þessom
n. N.	þat-si		þit-a	þetta
f. N.	su-si			siá
A.	þa-si	þa-sa, þi-sa		þessa
pl. m. N.	þai(R)-si, þisiR			þesser
A.	þa-si	þa-sa, þi-sa		þesse
G.				þessa
D.	þaim-si			þessom
n. N.	þau-si, þi-si			þesse
f. N.	þa-si	þisaR		þessar

As to the explanation of the distribution of the *se*-forms Kortlandt thinks that the pronoun 'this' is not **so *sā *tod* (with a particle), but **e *ī *id* with *s-/t-* added. Then it becomes understandable that from here *se/ti*-forms crept into the *so/to*-paradigm. (On **e *ī *id* as personal pronoun see § 1.)

8. I think the forms reconstructed explain the form of the Old Irish (Insular Celtic) article. The neuter singular has *sa*^{n}, the other forms point to **sindo-*. What must be explained is the origin of these two forms, and why they are so different.

Pokorny (IF 39, 1921, 217–20), who rejects earlier explanations, explains **sind-* from **sēm-dhe*, the neuter *sa*^{n} and Gaul. *-sin* from **sēm*, from the numeral 'one' and a particle **dhe*. This cannot be correct for several reasons. First the numeral 'one' gives the *in*definite article, not the definite one, which always goes back to demonstrative pronouns (beside French, Germanic, Greek, Bulgarian and Armenian note Breton *an* which comes from **sindo-* beside indefinite *un* from the numeral). This is decisive. Then from the numeral 'one' you only have derivatives, no forms with (demonstrative) particles. Further an *ē*-grade for **sem-* is certain only in **sēmi* 'half'. If my reconstruction (App. 2) is correct, it occurred only in the locative, from where it cannot have been generalized. Lastly a neuter **sēm*, with lengthened grade, is unknown, ablauting forms always having the short grade in the neuter.

I suppose that **sindo-* derives from the accusative **sim* (> **sin*). It is easiest to assume that the form was adapted, just as in Gaulish *sosin* the stem *so-* was added in front of **sim*, by adding at the end the (inflected) form **tom*. After the acc. **sintom* > **sindon* the other forms were made.

There are some difficulties. That the second part was **to-* was supposed by Windisch and accepted by Thurneysen (Handbuch § 42), but Pokorny (l.c.) objected that *to-* had not yet become *do-*, because this development occured only about 700 as would appear from Adamnán's Vita Columbae. But this applies only to initial *t-* (Thurneysen GrOIr. § 178). And the preverb *to-* was supported by the prototonic forms where it was accented. Here we have *t* in inlaut, and after *n*. Also, the article could well have been subject to change earlier, because it is among the weakest of proclitics. I think, therefore, that we cannot exclude the possibility of original *-nt-*.

Pedersen objected (KVG 2 § 504) that *t-* was replaced by *s-* in Celtic. But **sinton* can have been formed before that. In **sin* the *s-* is probably much older, compare Skt. *sim* beside *ta-*.

Earlier explanations operated with **sendo-* (differently motivated), which would have got *-i-* through raising. Raising must have occured in the gen. dat. sg. masc. and fem. and the nom. pl. masc. One may object that the influence of these forms was not sufficient to dominate the whole paradigm. This would mean that the other forms, which cause lowering, were dominant, but *i* before *nd* was not lowered (Thurneysen, GOI p. 46).

Gaul. *sosin* 'this' may confirm the existence of **sim* in Celtic, but we cannot be sure.

Kortlandt thinks that this explanation shows that **sim* was an isolated form, not part of a paradigm; then it is clear why the form of the article was determined by the accusative.

9. For the neuter *sa^n* (the nasalisation can be secondary) there is an archaic form *se* in *fri-se-llar* 'to the ground' (Lewis-Pedersen, CCG 219, § 360 n. 3). This makes it probable that it is identical with the pronoun acc. sg. n. *se* (see § 10). The same form is also found in *sech* used with the copula in the meaning 'id est' (Thurneysen, GOI 550 § 883), to which Kortlandt draws my attention. *sech* will represent **se-k^{w}e*.

This means that this form cannot represent **tod* changed into **sod*.

sa^n has been derived from **sin* because of Gaul *sosin* (which is supposed to be neuter). But in that case it would have been reshaped

into **sindon*. For the same reason it cannot continue a form **si* for **ti*, the type of forms discussed in this article.

Pedersen, KVG 2, 193, explained the demonstrative *se* from **sjod* for **tjod*. "Natürlich könnte . . . auch ein idg. *e* vorliegen . . .; eine Grundform **sed* < **ted* läßt sich aber kaum durch die anderen idg. Sprachen stützen." As PIE did not have a pronoun **sio-* (cf. III 5), this explanation is impossible. And even if PIE had **sio-*, it is not very probable that it would occur in this one form in Celtic.

Another objection to Pokorny's explanation of the article is that I cannot see why the neuter would not have had the particle *-dhe*.

Kortlandt points out to me that it will be cognate with *ed* 'it'. This form (belonging to the group *é*, *sí*, *ed*) must go back to **id*. Because the *-d* is preserved, it must have had an added particle, as Goth. *it-a*. This particle can have lowered the *i* to *e*, but the *e* can also have been introduced from the oblique cases (**esio* etc.). (There is no evidence for **ed* elsewhere (or for PIE **ed*). It has been assumed for Lat. *ecce*, but this is quite uncertain. If Skt. *adás* stands for **ad-au*, *ad-* could be compared, but the origin of this form (and *amúm*) is quite unclear to me.)

se had no added particle and lost its *-d*. It is a form of the type OS *thit*, i.e. it goes back to **id* with the consonant of the *so/to*-pronoun added; in Celtic *s*-was generalized. The *e* must have been introduced before the *s-* was added from the oblique cases. So we have: **id*, **ed*, (**ted*?), **sed* > *se*.

10. Old Irish has a number of demonstratives with *s-*. It may be useful to give a short survey of them. (It is difficult to get that from the larger handbooks; the following is based on Pokorny, Altir. Gramm., Göschen 1969², 57f.) Forms with *í*, details of their use and adverbial uses are left out.

I. *Particle*

1S	sa, se	1P nai, ni
2S	su, so, siu	2P si
3SPm	som, sum, sam;	seom, sium, sem (arch. *sa*)
f	si	
n	som, sum, sem	

Use: poss. + noun + X
pers. pron. + X *duit-siu* for you
vb + X

II. *Pronouns*

1. so, sa (se, seo, sea) dieser
sin jener dort

(tall, ucut jener)
Use: art. + noun + X

2. inso, inse, so, se dieser
insin, sin dieser dort
Use: obj., subj. *do-gní (in)sin* er tut dies
prep. + suff. pers. + X airi sin deshalb
(equative + X)

3. An. *se* this *sin* that
Dn. *síu, sund* *sin*
Use: prep. + X *co-sse* bisher
de-síu davon *iar sin* darauf
di-sund „

4. m. f. suide (*i̯o-*, *i̯ā*-stem) dieser
n. sodain
Use: prep. + X *do suidib* zu diesen, *la sodain* hiermit
(compar. + X)

Ad I. Pedersen, KVG 2, 137f.: if *sa* is the old form, it represents **so*, if *se* is the old form, it must be **sjo-*. Thurneysen, Gr. 252 § 403 says that in archaic texts *se* also occurs after non-palatal souns, so it seems certain that *se* is old (of course, *sa* < **so* could be old as well). As **sio-* never existed, *se* could be **se*, i.e. **e* + *s-*.

3 sg. *som* has been derived from **somHos* 'same' > 'self'. Vendryes Lex. s.v. *som* states that archaic texts also have *sa*. This is not surprising as **se*, **so* means 'he'.

Ad II 1. As to *sin*, I don't think it is a form of **sindo-*. In that case probably the *-nd* would have been preserved, as in *sund* (II 3). I think it is better explained from **sim* (with *-i*? GOI § 482). This form became isolated after the creation of **sindo-* in the article, as in Sanskrit, and it came to be used as a multi-purpose (gender and case indifferent) pronoun.

I assume that nom. **so* : acc. **sim* got the secondary notions 'here near me' : 'there near him', from which they developed to simple 'here' : 'there'. The essential point is that the subject is closer to the speaker than the object. This may seem far fetched, but we have to accept that this distinction arose at some place because the whole system of Old Irish demonstratives is based on the *s*-pronoun.

The same *sin* is found in 2 and 3.
Ad II 2. These forms are identical to II 1.
Ad II 3. *sin* has been explained above.
On *se* see § 9 on the neuter of the article.

A glance at the paradigm teaches that *sund* is the old dative and *siu* a recent formation: it can easily be analogical. *sund* has been compared with W. *hwnn*, which is nom. sg. masc., and derived from a form **sondo-* parallel to **sindo-*. This is improbable, because **sondo-* cannot be explain in an integrated view, but especially as it is methodically wrong to compare a dative with a (nominative) masculine. I think that *sund* is what it seems to be, the dative of *sindo-*. This must have been **sindōi* > **sindū* > **siund*. I assume that this form became *sund* analogically (compare *side* < *suide*). (The Welsh forms may have been created analogically to the neuter.)

11. I think that Phrygian *semoun*, f. *sa* belongs to these forms. It clearly means 'the, this'. It is often reinforced by an adverb/particle *tou*: *semoun tou knoumanei*; *sa skeledriai* etc. **semou* could simply be **se-m-ōi* or **se-sm-ōi*. This could be **esmōi* with added *s-*, or **tesmōi* with *t-* replaced by *s-*, as in Celtic.

12. At first I thought that the forms *se sī ti(d)* were a variant of **so *sā *tod*, just like **kʷi-* beside **kʷo-* etc. Kortlandt convinced me that most if not all forms of the *se*-type are in fact reshapings of **e* through addition of *s-/t-*. We do not have, then, again another PIE pronoun with the problem of its relation to the *so*-forms.

Why is **e *ī *id* so different from **kʷis *kʷid*: why is there a separate feminine; why do you have *e* against *i*; why is there no form **es*?[4]) Kortlandt writes me that he thinks that these questions can be answered. That there is a separate feminine in the first pronoun but not in the second is just as understandable as Eng. *he* : *she* against *who* m/f. That 'who' has *i* and 'he' has *e* is for the same reason why 'who' has *-s* and 'he' has not, and for the same reason

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[4]) I rejected the idea that *quisque* is parallel with *quisquis* and contains a nom. sg. m. **-kʷe*. It seems rather clear that *-que* is 'and', see Leumann, Lat. Gr. 1977, p. 747, who also stresses that *quisque* cannot be directly compared with Skt. *kaś ca*, Ac. *cišca* which are found only after the relative *ya-*. (That Gr. *tís te* has nothing to do with these forms was definitely shown by Ruijgh, Autour de *te* épique. For Hitt. *kuiski* see J. Tischler, Heth. etym. Gloss. 3 (1980) 564ff. There are two problems with an identification with *-que*: the labio-velar (in enclisis and/or through dissimilation?) and the form *-ka* (in the gen. *kuelka* and after adverbs). Do they continue **kʷi* and **kʷo*? For the interpretation of *-que* in *quisque* as 'and' plead *-cumque* 'wenn auch (immer)' (Leumann ib.), and the Hittite parallel *kuissa*, Luw. *kuisha*, which contains *-(h)a* 'and'.—As to Lat. *ipse*, even if it contains a form of the *s*-pronoun, it can be **-so* as well as **-se*.

why the OCS accusative of 'who' is *kogo* but of 'he' is *jь*: 'who' is mostly construed with a dynamic verb but 'he' with a static one. (See Kortlandt's article on PIE syntax to appear in JIES.)

A new question perhaps turns up: why is it that **so* has no *e/i*-forms and that **e* has no *e/o*-forms? There seems to be a close association between **e* and **so*: their meanings seem identical and we saw that **so* influenced the forms of the other. Are they in fact the *e/i*- and *o*-form of one pronoun? In general it looks as if the *o*-forms are adiectival, whereas the *i*-forms are independent. This would explain, for example, why **e* provides the personal pronoun (for the third person), whereas **so* does not.

A new problem is presented by the Hittite forms. The enclitic and the independent pronoun (Laroche, HuIdg. 147–52) can be reconstructed as follows:

-as	-at	as-i	-eni	*as	*e	cf.	kas ke
-an		un-i(n)		*un			kun
-se		edani		*e-			ke-

It appears to have had the same forms as the other demonstratives (e.g. *kas*). There is, then, a good possibility that **e* was reshaped after the other pronouns in Hittite. The ending *-um* has not yet been explained. Benveniste (HI 69–73) connected it with Skt. *amúm*. But this form belongs to a paradigm where the nominative (*asáu*) contains a particle *u*, so the *u* of *amúm* probably has the same origin. (See Klein, JIES 5, 1977, 161–77, who stresses that Indo-Iranian did not have a system *ámaḥ, amúm*. I am not convinced that his explanation of *asáu, amúm* is correct. Especially the prefixing of **au* to **am* I find hard to accept.)

At the end three forms still require some comment: **sī*, **soi* and **sim*. For these forms seem to be older than the other forms to which this paper drew attention.

As to **sī*, it is quite possible that it is **ī* with the *s-* of **sā*. The only question is that you would expect **se* as well, and as early. Perhaps this form is found in the reflexive pronoun (§ 2). If this is correct we would have influence of **so* on **e* at different times, the first already in PIE.

**soi* cannot be a case form of a demonstrative pronoun. I assume that it originated when **se* had come in the sphere of the personal pronouns (to become the reflexive), and that **soi* was created after the personal pronouns like **moi*.

**sim*, the starting point of our research, cannot easily be **im* + *s-*, for one would expect **tim* (as we did find in Germanic). Therefore

we may consider the same origin as for **dim*: it took its *s-* from preceding forms; see App. 5.

Appendix 1.: *ṛHC* > Skt. *īrC*

I want to comment upon another laryngeal development in Sanskrit.

There is no problem with the development *tm̥Ht* > *taHt* > *tāt*. But how must we understand *tṛHt* > *tīrt*? There are two possibilities. One is that the disappearance of the laryngeal caused the development of *ṛ* to *īr*. This is quite as ununderstandable as assuming *tr̥̄t* > *tīrt*. We must clearly assume a first development to *tirHt*, the (presence of the) laryngeal causing a special development of the *ṛ*. The next step cannot have been, I think, loss of the laryngeal with compensatory lengthening of *ir*, because such compensatory lengthening always restores the original length of a syllable that would otherwise become short. But in *tirHT* > *tirt* the syllable would remain long. And from *tirHa* (< *tṛHa*) we would expect compensatory lengthening, because in *tira* the syllable did become short. Therefore I think that *tirHt* first became *tiHrt*, and then *tīrt*. Only in this way can I understand the final result. —Avestan must have had *tṛHt* > *tarHt* > *tart*, with loss of the laryngeal (and no metathesis and no lengthening).

In the same way *tm̥HR* must have developed into *tamHR* (with the development of *R̥* which is usual before sonant), the group *HR* apparently behaving as a sonant) > *taHmR* > *tāmR*.

Appendix 2.: The Inflexion of **sem-* 'one'

Darms (MSS 35, 1976, 7–32) reconstructed a paradigm **sēms, *sem-* and assumed that the lengthened grade spread to the other forms, so that the locative became **sēmi*, which he finds in the word for 'half'. I accept the last interpretation, but I have two objections to his paradigm. One is that the spread of the lengthened grade, which must be of PIE date to account for **sēmi*, leaves Gr. *hen-* unexplained. (That the shortened *e* spread from the nominative is not probable, cf. *-eus, -ēuos*.) The other point is that he does not account for the forms with *ō*, notably OCS *samъ*. It is often said that these forms are secondary, but there is no convincing explanation for this long *ō*. I therefore propose:

nom.	sōms	or	sō[m]	cf.	dō[m]	Gr. dō (dōma)
acc.	sem-m̥	or	som-m̥			
gen.	sem-s	or	sm-os		dem-s	GAv. dāng
loc.	sēm (and *sēmi*?)				dēm	GAv. dąm

Thus *samə* can be derived from the old nominative.

As to the locative I note: 1) Darms interpretation of **sēmi* as a locative shows that we had a long vowel locative here; 2) the Avestan for *dąm* is comparable in structure; 3) the long vowel is explained by the fact that the form is a monosyllable. (That at some time an *-i* was added is no problem.)

I wonder whether the form had a laryngeal, so that **somHo-* can be derived from it. In many forms it would have left no traces. One point is that then probably the nominative and locative did not get a long vowel. Another point is that it seems impossible to accomodate **sōm(H)-*, **somH*, **sēm(H)*, **sem(H)-* and **sm̥H-* all in one paradigm. And *sm̥-* in compounds shows that there was no laryngeal in **sem-* 'one'.

Tocharian gives no evidence for **ō*. The essential forms are:

ṣem- in B points to **sēms*, with palatalized *s* and *ē* reduced to *e* in closed syllable according to Kortlandt. (This form spread to the other cases.) In A the *s-* can be explained as assimilated to the second *s*. The *-o-* may be due to the influence of the labial *m*. So there is no evidence for *ō*.

G. Schmidt (Stammb. Pers. 164) suggests that Lat. *sōlus* 'alone' is cognate with Hit. *sel*, the genitive of the anaphoric pronoun. He thinks it was derived from a case-form, a locative or an ablative to account for the meaning ('bei sich selbst seiend > allein'). This is not convincing. The semantic development is a forced one; forms derived from case-forms are rare; the existence of an old reflexive is dubious (see part III); and even if it existed it is uncertain that it had special forms for the locative and ablative. 'Alone' rather suggests connection with 'one'. I may suggest that it was derived from the nominative **sō* (if it had that shape, or got it in Latin); the form **sō* may have become isolated and been reshaped into **sō-lo-*.

As to the Hittite pronominal ending *-el*, I cannot imagine that, beside the genitive endings we already have (*-os*, *-osio*, *-so*; *-iH*) we would have here still another one. A priori it seems to me more probable that it is an adjectival formation, comparable to that in **smh₂-el(-)* in *similis* etc. (see App. 4), thus meaning 'belonging to, from'. But I do not know what form the Hittite ending continues

(the bare stem, a nominative in *-ēl*, or in *-el*?). G. Schmidt further connected Gm. and Ven. **selbho-*, assuming a syntagm **sel sbhei* 'von sich (und) für sich'. I can hardly imagine that a word originates in this way. Rather I would suggest that it derives from the same adjective stem **s-el-*.

Appendix 3.: Skt. *śámī*

"*śámi* ist noch ein Rätsel" wrote Geldner in 1901 (Ved. Stud. 3, 149) and that is still true. Vedic and Avestan always have *-yā* in the instrumental singular of the *devī*-type. It is quite impossible that the suffix had another form than *-ieh*$_2$- in the instrumental. The type *vṛkíḥ* has always *-iā* < **-iH-aH*. It has been doubted whether *śámī* really is an instrumental. Geldner supposed it was a locative of a root noun *śam-*. His argument is that in *dhiyā́ śámi*, which is found thrice, the two words cannot be on the same syntactic level because of *dhiyā́ śaśámate* (6.2.4): "*dhiyā́* hängt also von *śámi* ab." Wüst, PHMA 8–11 (1962) 221 n. 2 accepts this interpretation, but gives no new argument. However, this syntagm of verb + noun does by no means exclude that the two nouns can have been used parallel. Geldner's translation 'bei der Arbeit mit dem Denken' is less convincing than Grassmann's 'mit Gebet und Opferwerk' or 'mit Plan und Werk', d. h. mit Absicht und Ausführung', or Renou's (EVP 5, 12) '(notre) acte-poétique, (notre) effort-rituel' or Gonda's 'with 'vision' and ritual effort' (Vision 1963, 158). Interpretation as locative is possible because we find both *-ī* and *-i*. On this matter see Kuiper, Shortening 11f. For an instrumental plead the great number of instrumentals plural of *śámī*, and the instrumental *suśámī* (thirce).

As *śámī* is an instrumental, there is only one conclusion: the form was not an *ī*-stem. If it was an *i*-stem, however, it is not clear why it switched to the *ī*-stems so early (cf. AiGr. 3, 134ff.). There are two things that may be relevant: 1) the instrumental is by far the most frequent form of this word (if our texts don't give a specific use; but it will be due to the meaning of the word, 'effort'; this may also account for the preservation of (the form) *śámī*); 2) the root ended in a laryngeal. I would suggest that the word was a root noun originally. The development $H > i$ may have caused an *i*-inflection. Then in the instrumental plural **śamHbhis* > **śamibhis* (for **śābhis* < **k'mHbhis*?) the *i* may have become lengthened, as so often with $i < H$, and caused the transition to the *ī*-stems. (If the word was proterodynamic (akrostatic), perhaps there was an instrumental singular **k'emH-H* (with zero grade ending) > **śamiH*

> *śamī*. There is no evidence for such a form, but it is no more strange than the ending *-ī* < *-i-H* with the proterodynamic *i*-stems, which has been interpreted as an akrostatic *i*-stem form). The exact history is beyond reconstruction, but I think we must assume a complicated inflection (in Indo-Aryan) to account for the early transition to the *ī*-stems and the persistence of the *i*-stem form. (*śam* in *śam yoḥ* has been explained as a root noun. I could only imagine haplology from an accusative **śam-am* < **k'emH-m*.)

Appendix 4.: $\underset{\circ}{N}H$ in Latin

We have now sufficient evidence that $\underset{\circ}{N}H$ became *eN* in Latin, not *aN*.

**tnHu-* > *tenu-* in *tenuis*, see II 9.

**snHi* > **seni* > **sini* > *sine*, see II 10.

**smh₂eli-* > **semali-* > **semili-* > *simili-* may need some adstruction. Cognate are OIr. *samail* and Gr. *homalós*. The Latin form can be **semh₂l-*, *semh₂el-* or *smh₂el-*, the Greek one **somh₂l-* or **somh₂el-*, only the Celtic form is unambiguous: it must represent **smh₂el-* > **samali-*. Given the close relation between Italic and Celtic it is most probable that Latin derives from the same form, **smh₂eli-*, as given above. Probably Greek goes back to the same form, **hamalós* < **smh₂el-o-* being replaced by *homalós* through influence of *homós*. Their meanings, 'equal' and 'of equal kind', are very close. (These forms confirm the laryngeal of **somHos*.)

The essential point is that the last two forms cannot have had *aN*, as this would not have given *iN* in initial syllable. On the other hand *e* in initial syllable was assimilated to a following *i*: Leumann, Lat. Gramm. 1977⁶, 101 § 111; Sommer-Pfister, HLFL Lat. 1977, 94. (An *a* in the first syllable rather assimilated the following sound, or prevented change to *e* or *i*: Leumann § 109, Pfister § 78.)

According to Meillet, Diall. I.-e. 35, *-°ni-* (that is *nHi*) would have given *ini*. The evidence given is: *cilium*, *cinis*, *milium*, *similis*, *sine*. The last two words now have a different explanation. The other three are quite uncertain. For *cilium* a form **k°liyo-* is postulated. Comparison is with Gr. *kalīá* which is itself unclear. The discussion in Walde-Hoffmann is much better than that in Ernout-Meillet. In any case **(super-)keliom* is quite as possible. S.v. *cinis* Ernout-Meillet give: "le seul rapprochement connu est avec Gr. *kónis* . . ., et encore le vocalisme ne concorde-t-il pas mieux que le sens." They posit **k°nis*, but Frisk, Gr. et. Wb. 1.912 and Leumann, Lat. Gr. 1977, 101 assume *-e-*. *milium* 'millet' is compared with Gr. *melínē*

(*mélas*) (beside Lith. *málnos*), so there is no reason to posit **m°liy-* rather than **meli*, assumed by Leumann 1977, 101.

The conclusion is that this material does not prove *°ni* > *ini*. Everything points to *ŋHi* > *eni*, just as *ŋHV* > *enV* in general.

Appendix 5.: *dim*, Gr. *min*, *nin*

Caland suggested that Av. *dim* originated from **im* with the final *-d* of preceding words added to it, e.g. *ā dim* from **ād im* (KZ 42, 1909, 171–3). OP *dim* and OPr. *din* would have originated independently in the same way. This idea was accepted by Meillet (MSL 19, 1916, 53–5: "son explication semble évidente") and Van Wijk, Altpr. Stud. 120. Benveniste rejected it (Studi Baltici 3, 1933, 121–30), and most modern scholars seem to follow him, e.g. Toporov, Prusskij Jazyk, Slovar' A–D, 1975, 341–4; Fr. Bader, BSL 68 (1973) n. 4, 6, 33 (but not so Hoffmann, Aufsätze 74). Benveniste objected that this kind of reanalysis is rare, which is true but does not mean that it is impossible. There are several certain instances, and in the case of an enclitic one might expect it earlier than elsewhere. (A good parallel is nom. du. pl. OIc. *þit*, *þér* 'you' with *þ* from the verbal ending. A comparable fact is that Old Persian generalized the variant *šim* at the expense of **him*.) He further stated that there is no certain instance of the same reanalysis in more than one language. This may be true, but it is not a decisive objection. We come back to this point below. Benveniste states that *āt im* does not have the same meaning as *ā dim*. This again may be true, but the development must not have started exactly in this collocation. Then he points out that Avestan has *im* after words ending in a dental, and not *dim*. If this is relevant (I would rather suppose that it confirms Caland's explanation), it may be a later distribution of the forms then available. But *dim* is found after dental too, see below.

Benveniste himself assumes that all PIE demonstratives could form an anaphoric form in *-im*. I think that this statement is not born out by the facts, that it does not explain *dim*, and that it is wrong for several reasons.

First we note that, of the PIE demonstratives given by Meillet, Introduction 325ff., there are no forms in *-im* from *t-* (**so *sā *to-*), *k'-*, Av. *ava-* and OCS *ovъ*, *ol-*.

Secondly, for *dim* precisely there is no PIE pronoun. Here we are referred to Gr. *dé*, *dḗ*, OCS *da*, Lat. *denique*, *donec*. Toporov gives a great number of forms with a *d-*, but all this—even if it is ac-

cepted that they all contain one and the same element—does not change the fact that there is no evidence for a PIE *pronoun* with *d-*. One might assume that there had been such a pronoun in earlier phase of PIE (from which all these elements derive), but that would mean that **dim* must have been preserved over a very long period, and it is very improbable that it was preserved beside the (more recent) forms **im* and **sim*. This means that precisely in the case of **dim* Benveniste's explanation is impossible or at least improbable.

But the most essential objection, the third, is that PIE would have had a large number of anaphorics: Benveniste assumes **im*, **sim*, **dim*, **mim*, **nim*, **k^{w}im*. And we may assume that there were one or two more which have disappeared without trace (above we mentioned four stems of demonstratives that should have had a form in *-im*). This is quite improbable: a language does not have six or more anaphorics. I can do no better than cite Meillet (l. c.): "Un premier fait rend suspectes les formes *dim*, *diš* du vieux perse: elles ne se distinguent en rien de *šim*, *šiš* pour le sens. Une synonymie aussi exacte est toujours surprenante: car les langues ne conservent guère de synonymes parfaits, surtout pour les forms pronominales." Again one might try to escape by assuming that all these forms originally did have different values (just as Armenian has three anaphorics in agreement with the three demonstrative pronouns). This is very improbable in the case of *dim*, for (granted that there was an older PIE pronoun with *d-*) it is improbable that the isolated form *dim* would have preserved a separate value long after the disappearance of the pronoun from the PIE system of demonstratives. The essential point is that we must realize what kind of language we are reconstructing, a fact of which Meillet was well aware.

The fourth point is that Gathic does not have **dim*. This was one of Caland's starting points, but Benveniste does not mention it. Given the large number of occurrences of *īm* (11), *īš* (8), *hīm* (4) (to give only the more frequent forms) this cannot be accidental. Though it is not absolutely impossible that Gathic lost **dim* whereas Late Avestan retained it, the more probable conclusion is that *dim* is post-Gathic (thus Hoffmann, Aufs. 74).

We must now see whether it is probable that **im* got a variant **dim*, and that independently in three languages. It should be pointed out that PIE forms ended mostly in the following consonants: *t*, *d*, *s*, *n*, *m*, *r*; there are hardly any forms ending in a labial, a velar, or a d^{h} and very few in *l*. As the *s* is not relevant here (there

was already a form **sim*), we retain *t*, *d*, *n*, *m*, *r*. The possibility, then, that in an (old) Indo-European language *im* came after a dental stop was very large. Benveniste objects that in Avestan *dim* is found very often after a dental that was *preserved*: *āaṯ dim*, *nōiṯ dim*, etc. This is not an objection. First it is possible that */nait-im/* as well as */nait-dim/* were pronounced [*naidim*], so that this form could be interpreted as containing *dim* as well as *im*. But more essential is that the *-d* must not have disappeared. Latin, for example, extracted a suffix *-ānus* (beside *-nus*) from forms like *Rōmānus*, though *Rōma* had not become **Rōm*. Given the frequency of forms ending in a dental stop in PIE, it is quite probable that *im* got a variant *dim* in more than one language. We know that in Indo-Iranian a final *t* became *d* before a following word beginning with a vowel. Stang, vgl. Gramm. p. 234, objects that there is no evidence for this pronunciatuon in Baltic. But how could one find evidence: *-t*, *-d* are lost in Baltic. OP *din* is the only evidence. Of course, *dim* was a convenient form to be used after vowel.

I wonder whether the *d* of the Old Irish infixed pronouns B and/or C ultimately derive from *dim*; in that case Irish too must have created *dim*. Pedersen, KVG 2, § 486, 2 n. 2, § 503, 3 and § 537, points out that these forms originated in the third person. Thurneysen, GOI § 451 states that the other infixed pronouns for the third person were **im*, **id*. (Watkins' particle *de*, Celtica 6 (1963) 24–8, cannot explain why it started in the third person.)

For Gr. *min*, *nin* Benveniste reconstructs **mim* and **nim*. As *nin* is Doric and *min* East Greek, it is probable, however, that they go back to one development. This is also assumed by Rix, Hist. Gramm. Griech. 186, who implicitly also rejects Benveniste's interpretation. Again Benveniste's interpretation is not based on sound method. Rix assumes a reduplicated **imim*. This is improbable for two reasons. First, reduplication, which is rare in pronouns anyhow, is found in demonstratives (with full, or even strengthened force) and indefinites, but in an anaphoric one would not expect it. Secondly, Rix must assume that *i-* was lost (in enclisis). Meillet is sceptic here: "il est aussi aisé de faire des hypothèses en l'air, qu'impossible de rien démontrer." Nevertheless I would like to point to the fact that Greek has several forms with a facultative *-n* (< **-m*?), which could have been the starting point: the nasal was not a fixed part of the preceding word, so it could easily be taken with the pronoun. Kortlandt gives me the following explanation for *min* : *nin*. In East Greek a final *-m* was taken with the pronoun. This develop-

ment reached Doric only when *-m* had become *-n*. He compares developments in the Slavic languages, which spread from West to East of from North to South, and slowly spread over the whole or a large part of the Slavic territory, so that there could be interference between the two developments (in this case *-N iN* giving *NiN*, and *-m* > *-n*); cf. his article KZ 94 (1980) 243ff.

We can safely return, then, to Caland and Meillet: **dim* and the Greek forms originated in the separate languages and are based on **im*.

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