

Some Greek aRa-Forms

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 Problem 1. πάρος 2. *al-/ol- 3. Hittite V Latin 1. nota, cognitus
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I.

At the V Fachtagung für allg. u. idg. Sprachw. I pointed out, that most Greek aRa-forms continue a sequence Rh₂-e (Flexion u. Wortbildung, ed. RIX, 1975, pp. 9-14). For the type κάματος WAANDERS arrived independently at the same conclusion (Mnemosyne 27, 1974, 1-6). In the following I present a few other forms that must be interpreted in this way.

1. ταναός.

That ταναός (Π 589, Alcman 3, 70 διέβα ταναοῖς πο[σί and ταναό-δαιρος; besides ταναήκης and ταναύποδα) is a reality is demonstrated by Myc. tanawa 'slender' (of wheels) and by Celtic forms, Corn. tanow < *tanauo-, OIr. tanae < *tanauio-. Hitherto this form has not been explained. There can be no doubt that it is an old IE word.

Mostly a form *tn(n)u- is posited for τανυ-, Skt. tanú-, but there seems to be no way to explain ταναF- from this basis. Would this form have been thematized, it would have given *tānos, cf. μάνυ, μᾶνός. Nor is it possible to imagine a direct PIE antecedent: *tn₂h- or the like seems hardly possible. Greek forms in -α(F)ος are rare, SCHWYZER 472f. ἄγανός and ἄγλαός seem non-IE (cf. FURNÉE index); κεράος may not have had -h-, given Myc. kerajapi; ταλαός, in Ar. Aves 687 (doubted by some scholars) and Quintus Smyrnaeus, may be an Augenblicksbildung. In any case we have here a root in h₂ of which the form ταλα- is widespread. It seems improbable that ταναός was created after one of these forms.

SZEMERÉNYI, Syncope 154-9, stresses that there is no suffix $-\alpha\text{Fo-}$, only $-\text{wo-}$ and $-\text{ewo-}$. He thinks that the fem. of $*\tau\alpha\nu\upsilon\varsigma$, $*\tau\alpha\nu\epsilon\text{F}\gamma\alpha$, was assimilated to $*\tau\alpha\nu\alpha\text{F}\gamma\alpha$ and that this form induced a masc. $*\tau\alpha\nu\alpha\text{Fo}\varsigma$. There are several objections to this view. 1) A fem. $*\tau\alpha\nu\epsilon\text{F}\gamma\alpha$ perhaps never existed. Classical $-\epsilon\iota\alpha$ seems to replace $-\upsilon\gamma\alpha < *_{\text{u}}\text{h}_2$, not via $-\text{ewya}$. This is suggested by Mycenaean, which has $-\text{eja}$ as the fem. of the nouns in $-\epsilon\upsilon\varsigma$. (On this form see Glotta 51, 1973, 230-3.) 2) It is improbable that the feminine characteristic $-\epsilon(\text{F})\gamma\alpha$ was affected by assimilation. Note $\beta\alpha\rho\epsilon\iota\alpha$. I confess that I have no solution for $\kappa\rho\alpha\tau\alpha\iota\acute{\eta}$ (there is no h_2 , because Skt. kratu- does not have $-\text{th-}$ as in prthí-). 3) From $*\tau\alpha\nu\alpha\text{F}\gamma\alpha$ we expect $*\tau\alpha\nu\alpha\iota\omicron\varsigma$, as $\kappa\rho\alpha\tau\alpha\iota\omicron\varsigma$ from $*\kappa\rho\alpha\tau\alpha\iota\alpha$ (supposed this development is correct) and $\acute{\epsilon}\tau\alpha\iota\text{-}\rho\omicron\varsigma$ for $\acute{\epsilon}\tau\alpha\rho\omicron\varsigma$ from $\acute{\epsilon}\tau\alpha\iota\text{r}\alpha$. 4) The most serious objection is that the same mechanism is required for Celtic: $*\text{tanewya} > *_{\text{o}}\text{wya-} > *_{\text{tanawya}}$, which induces $*_{\text{tanawyo-}}$ (Oir. tanae) and (!) $*_{\text{tanawo-}}$ (MCorn. tanow). The idea is therefore very improbable.

That the formation of this word is not solved, appears from the remarks in RISCH, Wortb.d.hom.Spr., 1974², 168. It is supposed that the α comes "vom Gegenbegriff $\text{πλατυ-}/\text{πλαταF-}$ ", for which the starting-point might have been (KASTNER, Adj. zweier Endungen 27ff.) ταναFηκης after $*\text{πλαταFηκης}$. Of course, πλαταF- is found only in the feminine $\text{πλάτα}\alpha$, and compounds with $*\text{πλατα}(\text{F})\text{-}$ are not known in Greek. The last point specially makes this idea very uncertain, because we are far from sure that πλαταF- ever occurred in compounds. That the feminine $\text{πλαταF}\gamma\alpha$ was the starting is very improbable (you would expect $*\text{ταναF}\gamma\alpha \rightarrow *_{\text{tanana}}\iota\omicron\varsigma$), and whether πλαταF- occurred in the inflection of the masculine forms cannot be demonstrated. Also, the opposite of $\text{τανα}\omicron\varsigma$ is rather $\text{παχ}\acute{\upsilon}\varsigma$. The idea is typically a Verlegenheitsansatz. Of course, this explanation also overlooks the existence of $*_{\text{tanaw}}(\text{y})\text{o-}$ in Celtic.

The only other suggestion made is that it was assimilated from *τεναΦος, HIRT, Hb. gr. Laut- u. Formenl. 1912², 164. In The Development of the PIE Laryngeals in Greek 206, I objected to assuming assimilation in εRa (or oRa), because these forms appear to have been preserved unassimilated in Greek. Also the same assimilation would have to be assumed for Celtic. Further, if assimilation would be admitted, we would have to explain *τεναΦος. This would demand *tenh₂uos. In Development 190f. I denied the presence of a laryngeal, as is generally done, because there is no positive evidence for it and because Skt. fem. tanvī seems to exclude it. I think, however, that tanvī, for *tā(n)vī < *tñh₂uih₂ or *tanivī < *tenh₂uih₂, can be easily understood as a recent formation on the basis of masc. tanú-. Only J. MANESSY-GUITTON, Word 19 (1963), 31-7, assumes a laryngeal (*tōnə₂wo-). It might also be objected that, if Skt. tanú- had a zero grade, we would have got *tinu- < *tñh₂u-. But tanu- could as well continue *tenh₂u-, as full grade is also attested by Lith. tėvas, Lett. tiēvs and Lat. tenuis. The accentuation of the Baltic forms confirms the laryngeal. (Prof. KORTLANDT confirmed this to me.) In Lat. tenuis the laryngeal has been lost (as in culmus < *kolh₂mos). Germanic, e.g. Oic. þunnr < *þunwa-, has the regular development of *tñh₂uo-.

If the word would have a laryngeal, it is not necessary to assume assimilation from *τεναΦος. ταναός could then be easily explained as *tñh₂euo-. The Celtic forms could derive from the same form. This could be a thematization of *tñh₂eu-, which is understandable both as the acc. stem of a hysterodynamic/holokinetic paradigm (in which case *tñh₂u- of the Germanic forms could represent the stem of the oblique cases) and as the oblique stem of a proterodynamic/-kinetic noun (*ténh₂-u-s, found in Indian, Latin and Baltic, gen. *tñh₂-éu-s). I think that this easy explanation proves that we must accept the laryngeal. Then τανυ- in compounds will represent *tñh₂u-. In general forms like *tñ(n)u-, with vocalic resonant before vowel, are not very certain and most of them appear to contain a laryngeal.

Now one might also consider whether $\tau\acute{\alpha}\nu\nu\tau\alpha\iota$ does not represent $*\underline{t_{\text{h}}}_{-2}\text{-u-}$ either. In Development 279 I objected to the analysis $*\underline{t_{\text{h}}}\text{-n-eu-}$ that $\underline{-nn-}$ would have given $-\alpha\nu\nu-$. This might as well hold for Sanskrit; see AiGr. I Nachtr. zu 11, 14. The alternative $*\underline{t_{\text{e}}}\text{-u-}$ (or how one wants to indicate it) becomes less probable now that several instances where \underline{e} was assumed, appear to be better explained without it. Skt. tanóti cannot represent $*\underline{t_{\text{h}}}_{-2}\text{euti}$, as this would have given $*\underline{tinóti}$. Sanskrit seems to have generalized the full grade, as appears also in tarute (this is a set- root, so that $*\underline{t_{\text{h}}}\text{Hu-}$ would have given $*\underline{tiru-}$, but even an anit- root ($*\underline{t_{\text{r}}}\text{-u-}$) is supposed to have given $*\underline{tiru-}$). After all, Skt. might have an anit- form $*\underline{ten-eu-}$, but Gr. $\tau\alpha\nu\nu-$ is hardly understandable without laryngeal.

In conclusion I would say that Greek $\tau\alpha\nu-$ could be explained by $*\underline{t_{\text{e}}}\text{-n-}$, what would require the existence of such forms or such a phoneme, but that $\tau\alpha\nu\alpha-$ F(o)-, Celt. $*\underline{tana}\text{-u-}$ would require the much more improbable assumption of $*\underline{t_{\text{e}}}\text{-n-e}\text{-}$. Also, in compounds one expects $*\underline{tnu-}$, not $*\underline{t_{\text{e}}}\text{-nu-}$. All these difficulties are solved at once by assuming a laryngeal, which is confirmed by the Baltic accentuation.

2. Corn. carow etc.

The parallelism of Corn. carow, Bret. karo (Welsh carw) to Corn. tanow, Bret. tanao, tano (W. teneu is not a regular development) indicates for these forms a basis $*\underline{\hat{k}r_{\text{h}}}_{-2}\text{-eu-o-}$, not $*\underline{k_{\text{r}}}\text{-uo-}$ as is mostly posited. It seems that here a form $*\underline{\text{karau-}}$ was not posited, because this stem nowhere else shows $\underline{-a-}$ in the root. Though the situation concerning these words is very complicated, it seems safe to connect these Celtic words for 'deer' with Lat. cervus (that could have had a laryngeal) and SCr. kràva etc., Lith. kàrvė 'cow', which certainly point to a laryngeal.

II. γάλως.

Though much has been written on γάλως 'husband's sister' and its cognates (for litt. see FRISK, and WH s.v. glōs), there is still no satisfactory view of the whole problem. I think we can now arrive at one.

1. First something may be said of Hesychius' γελαρος· ἀδελφοῦ γυνή, Φρυγιστί¹⁾ (on the meaning see below). Ed. HERMANN proposed to read *γε-λαρος for it, and this is accepted by many, though others think that a corrected gloss has no value. I think that the form is probable because it fits in with all facts and theory, and I shall use it, but the argumentation does not depend on it, except on one point where I shall state that.

Those who assumed that Phrygian was a satem-language, tried to explain the γ- (Russ. zólva etc. point to a palatal ǝ). As on this question, however, there is as yet no certainty, it need not detain us. However, nobody seems to have doubted that the gloss really is Phrygian. It is well known, however, that this label is certainly not always reliable (cf. HAAS, Phryg. Sprachdenkmäler, 1966, 129ff.). Though I can give no positive argument, I think it quite possible that the word is in fact Greek.

2. The Homeric forms dat.sg., nom.pl. γαλόω, gen.pl. γαλόων were derived from *γαλω-, a supposed thematization of an ancient γάλως of the type πάτρως (e.g. SCHWYZER, Gr.Gr.I, 480). This not convincing. It is not clear how a form like dat.sg.*γαλω(F)ω became γαλόω in Homer: when the F was still present, there could be no shortening or metathesis, and when the F had disappeared, -ωω- (or -ωο-) would have been contracted soon, so that in Homer no form with shortened -ω- could be expected. It is not probable, then, that γαλόω ever was a reality, and not rather an instance of diektasis. This diektasis, however, supposes a short vowel before the F (otherwise the diektasis would have given γαλω- again). This vowel, which was contracted with the thematic -ο- into -ω- must have been α. This α is also shown by Phryg. (?) *γελα-

Φος, but this form is admittedly unreliable. (If the word is Phrygian, the evidence is even less secure, since h₁ and h₃ also became a in Phrygian; only if the word would be Greek, would it demonstrate h₂.) Another indication is found in γαλαῶνη, given only by Suidas. This form, which is nowhere cited in the literature, was adduced by VEY, BSL 51 (1955) 87-9, who pointed out that the form *γαλαῶνη, generally read there, was a conjecture. I could only find the original reading in the Thesaurus. It seems very probable, then, that the Greek forms are based upon *γαλαFo-. This is also the opinion of COWGILL, apud ANTTILA, PIE Schwebeablaut 83. (FRISK and CHANTRAINE are not clear on this point.)

3. This (second) α will continue a laryngeal. This was, as far as I see, until now only assumed by VEY (l.c.)²⁾ and ANTTILA (l.c.)³⁾. The best evidence for a laryngeal is that it also solves another problem, that of the root vocalism of Greek. For now we can explain *γαλαF- by *ǵ_hh₂-eu-. (COWGILL l.c. assumed assimilation from *γελα-, on which see above (I.1). I pointed out that, though the possibility of this assimilation can hardly be denied, it is not probable as an overall explanation. In a case like γέλας/γαλήνη for example, assimilation is no explanation.)

4. This reconstruction means that Greek no longer provides evidence for a noun in -ōu-s. Nevertheless this type is probable (because of the parallel πάτρως - though it is not clear why only this form was thematized), and Lat. glōs is evidence for it.

Now the word appears to have had a laryngeal, we would have to assume that glōs represents *ǵ_hh₂-ōu-s, with the -l- not vocalized. Such a form is perhaps not impossible (see below), but ANTTILA's suggestion that a vowel may have been syncopated, is attractive: *ǵ_hh₂-ōus might have given *golōs > glōs in Latin.

The Slavic words, CS zъlъva, Russ. zolóvka etc., for which *zъlъv- < *ǵ_hluu- is postulated, may have its ending modified after *svekry⁴⁾. I am not competent to judge the root vocalism.

The vocalism of Arm. tal must have the same origin as the Greek word. As l here developed into al, it is probable that -lh₂e- resulted in -ala-.

5. Now we have the following evidence for vocalism:

root	<u>*ǵel₂-</u>	?γελαFος	suffix	<u>-ou-</u>	<u>glōs</u>
	<u>ǵlh₂-</u>	γάλως, ?glōs		<u>-eu-</u>	*γαλαFo-

The suffix form -ou- points to a hysterodynamic inflection, so that we might expect a paradigm:

nom.	<u>*ǵél₂-ou-s</u>
acc.	<u>*ǵlh₂-éu-m</u>
gen.	<u>*ǵlh₂-u-ós</u>

(On the paradigm cf. KZ 86, 1972, 30-63 and Flexion u. Wortbildung, 9.) Of course we must be careful, because we cannot say that we really have evidence for this paradigm, that is for this combination of root- and suffix-forms. E. g. *γελαF- seems to point to *ǵel₂(e)u-. Most important is, I think, that we do have proof for -eu- beside -ou-, for *γαλαF- cannot represent *ǵlh₂ou- > (*γαλοF-).

6. As to the meaning of the word, Ed. HERMANN, GGN 1918, 222f., pointed out that probably the unmarried husband's sister was meant, as only then it could be understood why she had a place in the extended family. For, when she married, she would leave her family and take her place as wife, daughter-in-law etc. This special meaning is attested for Latin in Corp. gloss. II 24, 29 cited in the Thesaurus as follows: ἡ τοῦ ἀνδρὸς ἀδελφῆ, γάλως, ἀδελφὴ γαμετὴ παρὰ Πλαῦτω. In the edition of the Corpus we do not find the words ἀδελφὴ γαμετὴ (in this form it is cited by DELBRÜCK, Idg. Verw. 148). It appears that gloss 32 of the same page has: glumea: Χλωρας. ἀδελφὴ γαμετὴ ὥς Πλαῦτος. It seems evident that the part after Χλωρας belonged to the lemma glos. The reconstruction, as given by GUNDERMANN in Archiv. f. lat. Lexicogr. 12 (1902) 414, is entirely convincing.

FRIEDRICH, *Ethnology* 5 (1966) 8, states that "both the Sanskrit and Slavic forms have been independently folk-etymologized [sic] as 'the spiteful, malevolent girl'"; I cannot check this. However, this refers more to her relation to her brother's wife (which was the same as that of the mother-in-law to her daughter-in-law; see e.g. GHURYE, *Family and Kin*, 1962², 314) as to her status of not-married woman. It seems probable, then, that the word had this narrow meaning in PIE, but we cannot be sure.

As to the meaning 'brother's wife', it has been held that this was recent: for Latin we only have it in a gloss (Non.p. 557), as is the case for Greek; for the Phrygian (?) form exactly this meaning is given. BENVENISTE, *Voc.d.inst.* 251, supposes it was an old reciprocal term. This is explicitly stated by Aelius Dion. (p. 113 ERBSE; cited in CHANTRAINE, *Dict.étym.*) καὶ ἡ τοῦ ἀδελφοῦ γυνή..., πρὸς ἀλλήλας γὰρ λέγονται. Note that in Dravidian, Kannada *nāḍini* has both meanings (while Skt. *nānāṇḍar-*, which may be a Dravidian loan, has only the meaning 'husband's sister'). However, as the position of the two women in the family was entirely different (see above), it seems more probable that the reciprocity was recent - and this is what is suggested by the testimonies - dating from a time when the extended family did no longer function in this respect and when the distinction was no longer of any use (as in Dutch *zwager* is used of husband's brother as well as of wife's brother).

III. γυνή.

It is generally agreed that the oldest inflection of the word for 'wife' had a genitive **g^uneh₂-s*, but full grade of the root in the nominative. HAMP, *Glotta* 38 (1960) 200-3, assumes a third ablaut form on the basis of OIr. gen.pl. *ban*, from **g^unh₂-*. HAMP supposes that Aeol. βαν-α continues this form **g^unh₂-*, and assumes a Sievers' form **g^{uo}n-* for γυν-. The first assumption seems highly probable. As to the second, I think that a Sievers' form is a priori improbable - do we have one sure instance of this type in anlaut in Greek ? - and

that two dialect forms ($\gamma\upsilon\nu-$, $\beta\alpha\nu-$) a priori rather will represent different dialectical developments of one form. The difficulty, admittedly, is that, if $*\underline{g}^u \underline{m}_-$ gives $\beta\alpha\mu-$ ($\beta\alpha\iota\nu\omega$), for $*\underline{g}^u \underline{nh}_2-$ one would even more expect \underline{a} -vocalism, as it is reinforced by the \underline{a} -colouring laryngeal.

(Note that, if the original nom. was $*\underline{g}^u \underline{enh}_2-s$, as is suggested by Skt. janiḥ, the proterodynamic type seems to have had a gen. pl. with zero grade.)

IV. The $\underline{H}_2\text{o}$ -Problem

1. $\pi\acute{\alpha}\rho\omicron\varsigma$.

It seems now generally accepted that $\pi\acute{\alpha}\rho\omicron\varsigma$, with Skt. purás, continues $*\underline{prh}_2\text{ós}$ (cf. also KZ 87, 1973, 215-221). As far as I see the importance of this form has hitherto been overlooked. For this one form definitely proves that \underline{h}_2 did not change \underline{o} into \underline{a} . The α of $\pi\acute{\alpha}\rho\omicron\varsigma$ proves that the laryngeal was \underline{h}_2 (\underline{h}_1 would have given $\epsilon\rho$, \underline{h}_3 $\omicron\rho$), and the Greek \omicron requires PIE \underline{o} . It seems improbable that, if the regular outcome were $*\pi\alpha\rho\alpha\varsigma$, this $-\alpha\varsigma$ would have been changed to $-\omicron\varsigma$ by analogy. Adverbs in $-\alpha\varsigma$ are not less well attested as those in $-\omicron\varsigma$: $\acute{\epsilon}\kappa\acute{\alpha}\varsigma$, $\pi\acute{\epsilon}\lambda\alpha\varsigma$, $\acute{\epsilon}\nu\tau\upsilon\pi\acute{\alpha}\varsigma$, $\acute{\alpha}\tau\rho\acute{\epsilon}\mu\alpha(\varsigma)$, $\acute{\epsilon}\gamma\kappa\acute{\alpha}\varsigma$, $\eta\acute{\rho}\acute{\epsilon}\mu\alpha(\varsigma)$; $\acute{\alpha}\gamma\kappa\acute{\alpha}\varsigma$: $\acute{\epsilon}\nu\tau\acute{\omicron}\varsigma$, $\acute{\epsilon}\kappa\tau\acute{\omicron}\varsigma$, $\pi\rho\acute{\omicron}\varsigma$, $\acute{\epsilon}\nu\alpha\gamma\chi\omicron\varsigma$, $\pi\rho\omicron\iota\kappa\acute{\omicron}\varsigma$ (both lists being complete, as far as I see). The last two words in $-\omicron\varsigma$ are rare, and the others, two in $-\tau\acute{\omicron}\varsigma$ and $\pi\rho\acute{\omicron}\varsigma$, seem no basis to cause a change of $*\pi\alpha\rho\alpha\varsigma$ into $\pi\acute{\alpha}\rho\omicron\varsigma$. The word must then be added to the sure cases in Die Sprache 18 (1972) 131.

The forms to be expected from $\underline{RH-V}$ may be listed here:

\underline{rh}_1-e	> $\epsilon\rho\epsilon$	\underline{rh}_1-o	> $\epsilon\rho\omicron$
\underline{rh}_2-e	> $\alpha\rho\alpha$	\underline{rh}_2-o	> $\alpha\rho\omicron$
\underline{rh}_3-e	> $\omicron\rho\omicron$	\underline{rh}_3-o	> $\omicron\rho\omicron$

(The same with λ, μ, ν .)

2. I may add here another clear instance of $\underline{a/o}$ that should be added to the evidence, though it does not belong to the central theme of this article. It is the root

*al-/ol- 'darüber hinaus', POK. p. 24. The most clear forms are OIr. al 'jenseits, über... hinaus', tall (< *to-al-nā) 'jenseits, dort', anall 'von jenseits, von dort, herüber' with OIr. ind-oll 'ultra', ol-chen(a)e 'außerdem, sonst' (< 'jenseits (und) diesseits davon'), ol-foirbthe 'plusquamperfectum', Lat. uls, ultra. Less evident though probable is the connection of this root with Lat. olim (*h₂oli-, a rare instance where we are sure of long vowel in the neighbourhood of laryngeal) and the forms continuing *alios (perhaps from an adverb *ali). If *al- was *h₂el-, *ol- must be *h₂ol- with the o unaffected⁵).

3. I may also recall Hitt. hau₁- 'sheep', hastai 'bone', for Hitt. h points to h₂ (e.g. EICHNER, MSS 31, 1972, 87 below) and many languages show o-vocalism (ὄλις, ὀστέον; Lat. ovis, os).

V. Latin.

1. nota, cognitus.

The type which was the starting-point for WAANDERS and me (see above) for the discovery of the explanation of the aRa sequence, the nouns with -eto/ā-, may solve a Latin problem. "Aucune forme normale de la racine de (g)nōscō n'expliquerait l'ō de nota" is the conclusion of ERNOUT-MEILLET. SCHULZE's note on nota, KZ 45 (1913) 23, suggesting that *gno-t- developed from *gnh₃-t- has won a certain fame. Undeservedly, I think, because it has always been evident that Latin did not have the 'triple reflex' of the shwa/laryngeal found in Greek⁶). Also it is doubtful, whether a vocalization *gnh₃t-, instead of *gnh₃t- occurred. (ἀγνοέω cannot be considered proof of *gnh₃- > γνο-.)⁷

I think we might consider *gnh₃-et-ā to have given nōtā. As far as I know, Latin has no other instances of -eto/ā-, but this cannot be a decisive counter-argument. The only remarkable thing in this reconstruction is that it would not have had vocalic n. But there seem to be parallels for it, e.g. Skt. vratā- < *urh₁-eto-, glōs < *gh₂ōus cited above, perhaps gravis below.

In the same way cognitus might represent $*\text{-}\hat{\text{g}}\text{nh}_3\text{-etos} > *\text{-gnōtos} > \text{-gnitus}$. I think this explanation is more probable than that of J. P. SMIT, which I presented in Development 204 ($*\text{-}\hat{\text{g}}\text{ñHtos} > *\text{-gnaHtos} > *\text{-gnatos}$ with specially Latin loss of H in compounds).

2. gravis.

Gravis is mostly explained as $*\text{g}^{\text{u}}\text{rə-}\underline{\text{u-}}$. I would not deny the possibility of such a form ($*\text{g}^{\text{u}}\text{rh}_{\text{o}2}\text{-}\underline{\text{u-}}$). We might now, however, consider $*\text{g}^{\text{u}}\text{rh}_{\text{o}2}\text{-}\underline{\text{eu-}} > *\text{grav-}$. It supposes a feminine ending $*\text{-eu-ih}_2$, which would have to be demonstrated elsewhere.

As to the laryngeal, both the vocalism of βαρύς, gurú-, and the vocalism of gravis, require forms like $*\text{g}^{\text{u}}\text{e}\underline{\text{r-}}$ and $*\text{g}^{\text{u}}\text{r-}\underline{\text{e u-}}$. In this situation, highly parallel to that with τανυ- and τανᾶF-, it is probable that a laryngeal was in the game. Again, the Skt. fem. gurvī does not seem conclusive counter-evidence.

For a vocalization $*\text{g}^{\text{u}}\text{rh}_{\text{o}2}\underline{\text{u-}}$ we might compare ratus, where I see no other possibility than $*\text{rh}_{\text{o}1}\underline{\text{tos}}$ ($*\text{rh}_1\text{-etos}$ would have given *retus), which does not mean that the one possibility we see is necessarily the correct one.

Notes:

- 1) Often γαλλαρὸς is cited as a varia lectio (thus still LATTE), but this is glossed as Φρυγιακὸν ὄνομα [παρὰ Λάκωνιν]. It is evident that there is no reason for this interpretation. Cf. HAAS, Phryg. Sprachdenkmäler 1966, 161: "nicht zu begründen".
- 2) VEY's theory as a whole is unacceptable to me; see my Development 209. Also he assumes an g-stem for this word, which is most improbable (the -s- > -h- is needed for his theory).
- 3) ANTTILA suggests that Lat. glōs contains $*\hat{\text{g}}\text{loH-}$, but I don't understand whether he rejects this idea or not. It is very improbable, as the word will continue the u-stem of Greek and Slavic.

- 4) I do not accept an ablaut $\bar{o}u/\bar{u} < u\bar{o} < \bar{e}u/\bar{a}u < \bar{e}u$ as does e.g. SCHMEJA, IF 68 (1963) 24f.
- 5) For Lat. $\bar{o}l-$ $*h_{2s}l-$ (instead of $*h_{2s}ol-$) might be considered, but there is no positive evidence for it. I think $\bar{o}l-$ was the regular outcome of $*h_{2s}l-$, the laryngeal not influencing the development. (As $*h_{2s}l-$ in Lat. gives $\bar{l}a$, it seems not impossible that $*h_{2s}l-$ gave $*al-$.)
- 6) RIX, MSS 27 (1970) 94f., also assumed developments that suppose that the three laryngeals were still distinct in Latin. See my discussion in IJ 14 (1972) 73f.
- 7) SZEMERÉNYI's connection with the root $*gnet$ 'to press' (OE cnedan) has not been substantiated (Glotta 38 (1960) 239f.).