

Reviews and Discussions

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De Melo, from now on I will call him de Melo (without any reduction as d.M. or M), has undertaken a titanic work, namely the edition and commentary of Varro's sex books *De lingua Latina*, the only ones which arrived to us.

De Melo started from a correct, albeit evident aim, to prove that our modern grammar is better than Varro's grammar and linguistics. This aim was easy enough, because it is only natural that the linguistic and philological levels we have reached are much higher than the level we find in Varro's *De lingua Latina*. However, in this Edition and Commentary some questions are discussed which have little or nothing to do with Varro and some problems concerning Varro have been neglected or not thoroughly treated. This is clearly a consequence of the aim I mentioned above and does not mean that de Melo's work is bad. My suspicions were aroused as I learned (*Preface*, viii) that de Melo worked on Varro *De lingua Latina* and produced this work in little more than five years. In such a limited time he outlined the scheme of a large and important work and finished it. Considering how Varro's text is difficult from every point of view, I would have thought that a couple of lustra (ten years, not less) were necessary in order to give an edition and commentary of Varro's *De lingua Latina* of consistently high standard. But working time can vary depending on one's capacity. Nevertheless, with such a difficult text, I think that every reviewer should collaborate to improve, if possible, this work highlighting all the points he thinks need some improvement. I do not wish to shirk this duty.

P. 6: De Melo is right in reminding the reader that this text of Varro's was written in haste without revision and was itself not created in a tidy style. As for the transmission of this work, however, de Melo does not sufficiently highlight the history of text transmission in the Middle Ages, the difficult transmission, the destruction of a huge part of the work, and the difficult and partial salvage through the *librorum renouatio* in Montecassino; in few words, he did not give a contribution such as that by Guglielmo Cavallo (1975), which has not yet been taken into account by de Melo. Not even the much different state of preservation of the other two works transmitted in the same manuscript, namely the *Rhetorica ad*

C. Herennium and Cicero's *Pro Cluentio*, has been considered. As for *Ad Herennium* I can confirm that the Mediceus Laurentianus 51.10 is a good MS which I used constantly in my new edition of this work, although it was seriously contaminated. But as Maddalena Spallone (1980, 1997) showed, and I accepted (Calboli 2020: 120 n.135, but also 131 n.147), the *Ad Herennium* was brought to Montecassino by the German abbots Reicherius von Niederaltaich (1038–1055) and Fredericus Lotharius (1057–1058) at a time when the relationships between Montecassino and Germany had been made very strong thanks in particular to Abbot Desiderius (1058–1087), cf. also Ruth Taylor (1993: 131). It cannot be ruled out that the *Ad Herennium* arrived at Montecassino from Vivarium, i.e. from the books that Cassiodorus could also have brought from Constantinople (Calboli 2020: 131 n.147). Cassiodorus could bring from Constantinople a copy of Varro's *De lingua Latina*, which has been quoted by Priscian and therefore was present in Constantinople, but the bad conservation of *De lingua Latina* in F in comparison with the *Ad Herennium* suggests that those works arrived in Montecassino through different ways.¹ On the other hand, also Silvia Rizzo (1979: 87 and 101) confirms a bad transmission of the *Pro Cluentio* in MS Mediceo-Laurenziano 51.10, and this could be in accordance with Cavallo's opinion that the second *librorum renouatio* was grounded on the first one when the monks in Cassino tried to save what survived of the old books after the destructions of the previous times by Langobardi and Saracens (Cavallo 1975: 398f.). For his part, Franz Brunhölzl (1971) supposed that *De lingua Latina* was found in Varro's *uilla* near Montecassino. Anyhow, this text passed through very dangerous events and could have been completely destroyed. It is a situation which cannot be dismissed simply by accepting, as de Melo (p. 6) does, the view by Guy Serbat (1985: 271) in the review of Varro's *De lingua Latina* book VI by Flobert, namely that Varro wrote his work in haste and a little carelessly (which, on the other hand, had already been pointed out by P. Flobert [1985: XX], before Serbat's review).

P. 14: De Melo wrote that the diphthong [ae] was written <ɛ> in Beneventan script. I understand that Beneventan script concerns the MS Mediceo-Laurentianus 51.10 but we must stress that the use of a caudate ɛ for ae is typical not only of Beneventan script but also of Caroline and many local cursives as e.g. the Romanesque cursive script discovered by Paola Supino Martini (1974).

¹ All MSS of *De lingua Latina* came from F (Mediceus-Laurentianus 51.10), but the suspicion that some other sources could exist in the Renaissance is not completely ruled out (cf. on this question P. Flobert [1985: XXIV–XXIX], who said he had been working on this subject with Louis Holz, one of the best specialists: “M. L. Holz, mon reviseur, a fait bénéficier toute l'édition de sa science et de son exigeante méthode, mais son apport a été déterminant en ce qui concerne les manuscrits”, P. Flobert 1985: XXXV) cf. G. Piras 1997.

P. 17: De Melo tells us that he will follow mostly the **F**'s spelling, but I do not understand one of the reasons given by de Melo: "I am under no illusion that Varro's *De lingua Latina* might become a text read by beginners". That the criterion of facility for beginners could become a criterion of a critical edition is not more intelligible than the criterion of *Ad usum Delphini*. De Melo avoided producing an edition of Varro's *Ad usum Delphini* because it is a difficult text. I agree with the criterion "to reproduce the text found in F as closely as possible", not however with the second part of the assertion: "and not to overburden the critical apparatus with issues of spelling". I am already an old man, yet I do not stop learning something new every day. An easy text can be neglected (assassinated) by the editors? In the following pages (in particular p. 25) de Melo explains better his behaviour: "Conjectures by modern authors are quoted [i.e. in his new edition of Varro, *De lingua Latina*] sparingly, and only where I either adopt them or believe that they are adoptable, even though I do not take them on board. I want a simple apparatus that is helpful, not a learned display of scholarly monstrosities". But how many complete new editions have been produced after Goetz and Schoell's (1910) edition and the edition of Kent (1951)? Only this by de Melo (who adopted Goetz and Schoell's text). How and where can we find the "scholarly monstrosities" judged "monstrosities" by de Melo? What is the use of de Melo's own edition? The comparison of Varro's grammar with modern linguistics given by de Melo. The question is therefore whether de Melo's modern linguistics is really a modern linguistics or not; or is it rather a reduced kind of IE scholastic rules, though I believe that they are mostly correct and usable.

P. 21: The pages devoted to modern (Indo-European) etymology and Indo-European grammar can play an important role in comparing ancient (Varronian) and modern linguistics. Here, de Melo gives a good abstract of Indo-European linguistics. I am a little astonished to find neither old, basic works such as K. Brugmann (1911–1916), nor more modern ones such as J. H. Jasanoff (2003), but in particular at reading such assertions as on p. 66: "The procedure of creating a linguistic family tree is no different from creating a stemma of manuscripts, or a family tree for animal species". We are absolutely in "Stammbaumtheorie" by August Schleicher. I know Devoto's (1962: 15–39) defense of the "Stammbaumtheorie", but this theory is today a little more complicated than as described by de Melo. Hittite is clearly not so usual for de Melo, though he referred frequently to Hittite words in the etymologies he treated in Commentary. Therefore, the most interesting isogloss of both Latin and Hittite has been neglected and (p. 77) de Melo does not remind the reader that *quis* (Lat.), and *qwiš* (Hitt.) are the best proof of this connection and, considering that almost all conjunctions employed by Latin to introduce subordinate clauses were built upon this theme (*ut*<**kut*, *ubi*<**kubi*,

quom, *-quam*, *quod*, *cur*), this theme was very productive and the most important theme in Latin. Therefore, the connection Latin – Hittite is very interesting.

P. 21: Neither de Melo nor Kent mention the collaboration of Wilhelm Christ with Leonhard Spengel by presenting the editions by Leonhard Spengel (1826) and that by Leonhard's son Andreas (Spengel 1885). We find a reference to Leonhard's Spengel edition of *De lingua Latina* (1826) and to publication of the second edition realized by Leonhard's son, Andreas Spengel (1885), and in Commentary (p. 1239) a discussion of the text published on p. 628, where de Melo, following Andreas Spengel, accepts the conjecture suggested by Wilhelm Christ, namely to add the diacritics to the names of Bacchides and Chrysidēs:

Varro, *ling.* 10.71 *E quis hic not<h>ae fiunt declinations, de his aliae sunt priscae, ut Bacchidēs et Chrysidēs, aliae iuniores // iuniores Scioppius, minores F, I would prefer minores // , ut Chrysidēs et Bacchidēs, aliae recentes // recentes Aldus // , ut Crysidas et Bacchidas.*

Here, de Melo refers to his own previous paper (de Melo 2016), where he discussed the readings suggested by Wilhelm Christ, namely (acc.plur.) *Chrysidēs* and *Bacchidēs*, as a product of a *iunior* (or *minore*) pronunciation, while the Prisca Latin pronunciation was *Bacchidēs* and *Chrysidēs* (de Melo 2016: 40). De Melo did not indicate where Christ suggested this reading to Leonhard Spengel and wrote: “Despite long searches, I have not been able to find the original article in which Christ proposed this emendation” (de Melo 2016: 40). On the contrary, with the help of my student and collaborator Ramón Gutiérrez González, I could demonstrate as rather probable that this was an oral proposal by Christ (Calboli 2017: 121), since Leonhard Spengel and Wilhelm Christ after the publication of Leonhard's (1826) edition discussed Varro's text over several years and Christ suggested many new readings and improvements to Leonhard, as Leonhard himself attested in an article published in *Philologus* in 1861. On the other hand, Andreas Spengel seems to have quoted Leonhard when he wrote: “Vocalium notas posui auctore Christio qui recte contulit Charis. p. 148.38 K., Diomed.p.305.13”. In both passages of Charisius and Diomedes quoted by A. Spengel it is written *correpta ultima syllaba*, therefore every error of interpretation is avoided. It seems that this was one of the many readings proposed by Christ in his discussion with Leonhard Spengel. However, I cannot obviously rule out that the words “Vocalium notas posui auctore Christio eqs.” were pronounced by Andreas Spengel, but what does matter is that Christ's proposal could be an oral intervention in the frequent meetings of Christ with Leonhard Spengel, as attested by Leonhard (Spengel 1861) and Andreas. At any rate, it is unlikely that this passage escaped the attention of Leonhard Spengel and consequently of Christ. In order to determine Leonhard Spengel's attention to this passage, I took into account the edition of Varro's *De*

lingua Latina produced by Leonhard Spengel (1826) in a copy of this book I own myself and found a particular passage which shows the great attention dedicated by Leonhard to this point. For Leonhard's text (Spengel 1826) is as follows: *e quis quae hic nothae sint declinationes, de his aliae sunt priscae ut Bacchides et Chrysidēs: aliae iuniores ut Chrysidēs et Bacchides: aliae recentes ut Chrysidas et Bacchidas // corrig. ut Bacchideis et Chrysideis [...] ut Chrysidēs et Bacchides // ut Chrysideis, Bacchideis vulgat Vetr[anius]/*. Therefore, already Vetranius worked on the vocalism of *Chrysidēs* and *Bacchides*. It is natural that Leonhard Spengel would have taken into account and discussed it with Christ, and Christ compared on this occasion Charis. p.148.38 K. and Diomed. p.305.13, and finally found the right solution to the problem by the integration of *Bacchidēs* and *Chrysidēs* with *Chrysidēs* and *Bacchidēs*, a genial solution in every respect, which Leonhard Spengel ascribed honestly to its real inventor, Wilhelm Christ. Also in this respect we must be most clear, the core of this invention was the 'correptio' of the *ultima syllaba*, namely the *iuniores* (or the *minores*, as I would not like to change the reading of F, *minores*): *Cum his omnibus tribus utantur nostri, maxime qui sequuntur media in loquendo offendunt minimum, quod prima parum similia uidentur esse Gr<a>ecis, unde sint tralata, tertia parum similia nostris*.

P. 29: I read by de Melo: "Our first extant grammar of Greek, the Τέχνη γραμματική 'Ars of grammar', is ascribed to Dionysios Thrax (170–90 BC), an Alexandrian and former student of Aristarchus of Samotrache. [...] The best edition of this treatise is now Lallot (1998), who uses the old text by Uhlig (1883), but provides a good translation and commentary". Here, though I appreciate Lallot's edition, I must say that de Melo missed the most recent developments in this research field. This is not because Lallot inclines to accept Di Benedetto's opinion that the Torso of Τέχνη γραμματική which arrived to us is not authentic (I do not agree), but because we have now at our disposal an excellent edition of Dionysios' Τέχνη with Italian translation and a very good commentary by Manuela Callipo (2011; and my review Calboli 2014).

P. 31: Perhaps what de Melo says on p. 31 is right, namely that sometimes Caesar in writing his Commentaries "ignored his own prescriptions". Nevertheless, to be complete, de Melo had to take into account also Caes.frg.20 Garcea, pp. 25–27 about *Turbo*, *Turbōnis*. "Moreover – even if the proper noun *Turbō* is not explicitly attested in a literary work before Horace's *Sermones* (2.3.310)² where it denotes a gladiator – Caesar's choice [inspired – I think – by his Alexandrian grammatical

2 According to Zimmermann [ALL 13, 1904, 492], "wohl zusammengehörig mit dem appell. *turbo*, aber später von demselben durch die Flexion geschieden", whereas for Kajanto [The Latin Cognomina 1965: 339] Horace's *Turbo* "may be nickname".

position] was also supported by the established inflexion of proper nouns which were always declined as *Catō*, *Catōnis*".

Plin.*dub.serm.frg.*34 Mazzarino, 71 Della Casa '*Turbo, Turbonis*', *si proprium sit hominis nomen*. '*Turbinis*' *si procellam uoluerimus exprimere; aut in eo, inquit Plinius, qui est in lusu puerorum. Sed Caesar de analogia II* [7[15], p.149 Fun; 21, p.215 Garcea] '*turbōnem*' *etiam in tempestate dici debere ait ut 'Cato Catōnis', non ut 'homo hominis'*. It seems that Julius Caesar used only the genitive *-ōnis*, because "he did not draw distinction between proper and common nouns" (Garcea 2012: 217), or he drew such a distinction for the meaning, but not for the parts of speech, since Alexandrian grammarians considered the *προσηγορία* (*uocabulum*) as a part of the *ὄνομα* (*nomen*), and counted eight parts of speech, while the Stoic and Pergamenian grammarians counted nine parts of speech (cf. Calboli [2013], who pointed out how masterly and cleverly Caesar used in this case the Alexandrian distinction). Nothing of this kind by de Melo.

In de Melo's review of the ancient grammarians (pp. 27–36) I appreciated that de Melo gave the biography of many grammarians, but the mention of Siebenborn (1976: 24–25) (p. 34) cannot substitute Aristotle and Theophrastus. It is very strange that we never read in a survey, where a paragraph (3.7) is dedicated to Ἑλληνισμός, of Arist. *rhet.* III 1407a 19 ἔστι δ' ἄρχὴ τῆς λέξεως τὸ ἐλληνίζειν and that we never find the names of scholars who gave fundamental contributions in this field, such as W. W. Fontenbaugh (2005) and P. Chiron (2001). Then in treating the Ἑλληνισμός how could he neglect R. Vainio (1999), and, in particular, J. Stroux (1912)? The same can be said of the absence of K. Barwick (1957: 58–79), namely of the pages dedicated to Stoic etymology and Plato's *Kratylos*, and therefore the absence of any connexion of etymology with the doctrine of tropes and figures (cf. G. Calboli 2020: 35–37, 744, 750–751). As for the etymology, I found (p. 36) this claim: "I provide such a detailed outline of modern etymology, including summary of the most important sound changes in the various branches of Indo-European. I do so because I find it instructive to contrast ancient etymologies with modern ones". This is right but it is a pity that de Melo did not employ Walde and Pokorny (1948–1954) and Ernout and Meillet (1959–1960).

P. 38: De Melo here attributes to Sabine Grebe (2001: 142) something Sabine never said, probably because of a misunderstanding produced by the word order de Melo used: "neither in Greek grammar nor in Latin it is the goal of etymology to find the 'true meaning' of words (Grebe 2001: 142)". After considering attentively the question, I could understand that de Melo does not agree with Sabine. This appears clearly if we consider what Sabine wrote: "These were the *πρῶται φωναί*, that imitate the nature of things. Gradually words changed and the original connection could no longer be seen. Etymology aims to recover it and find the true (*ἔτυμον*) meaning" (Grebe 2001: 142).

However, de Melo changed the text by Goetz-Schoell and adopted Kent's text, partially suggested also by C. O. Müller (see below). Varro's passage seems to be very complicated, but never in the Commentary, pp. 654f., do we find a clear description, only: "The second set of changes concerns entire syllables, but only their lengthening is mentioned in F, as in *tāctus* 'touched' from a root **tāg-* 'to touch'. We need to add three more reasons, and Scaliger's solution is very attractive. Since we have lengthening, we must also have shortening". This is true, but Müller's integration was enough, namely to add. *aut correptione*. Better to say the alleged reason is enough and correct only to add the shortening (*correptione*), without adding clumsily what remains. On the other hand, I agree with Norden's opinion that Varro wrote in a very bad Latin, but we must be extremely prudent in improving it, because the danger of making it more complicated is always present. Now I will give the necessary portions of text from the different editors in order to make its whole difficulty more easily understandable. Friedrich Leo (1889: 289 n.5) highlighted that this Varro passage was in connection with the four Stoic categories of changing the words *adiectio*, *detractio*, *translatio*, *commutatio* we find in *Rhet.Her.IV* 21,29 (cf. G. Calboli 2020: 750–751), a connection which means a connection with the rhetorical theory of figures (as it arrived in the quoted *Rhet.Her.IV* 21,29).

Varro, *ling.V* 5–6 Goetz-Schöll: *quare illa quae iam maioribus nostris ademit obliuio, fugitiva secuta sedulitas Muti et Bruti retrahere nequit. [...]. quorum verborum novorum ac veterum discordia omnis. [at this point the omission of est is more expressive]. in consuetudine com<m>uni quot modis litterarum commutatio sit facta qui animadverterit, facilius scrutari origines patietur verborum: reperiet enim esse commutata, ut in superioribus libris ostendi, maxime propter bis quaternas causas. litterarum enim fit demptione et additione et propter earum tra<ie>c[t]ationem aut commutationem, item syllabarum tproductione // vix delendum cum Sp., sed velut sic scribendum duce Scal et Mue: productione correptione, adictione detractioe (cf.adn. [ad pp.245sq. Adnotationum: 'Nullo modo probari potest Wilmannsii ratio de Varr.l.gr.p.25 Woefflino probata (Arch.VIII pp.425sq.): aut production aut correptione, denique vocabulorum compositione aut conlisione: nam non bis quaternae essent causae, sed quaternae et bis binae. productione del etiam Vsener 'Ein ant. Lehrgeb. der Philologie' 'Sitzungsbr. der bayr.Ac.' a.1893 p.625. V. inter alia Quint. I 6,32 et cf. Leo Herm.XXIV p.289 adn.5.')] // ; quae quoniam in superioribus libris [qui]cuiusmodi essent exemplis satis demonstravi, hic ammonendum esse modo putavi.*

C. O. Mueller ad p.3 §6 item syllabarum productione * aut correptione, denique one*: // *aut...one*] Lacunam indicavit et ex parte explevit, ita ut causa appareret eadem, propter quam Varronis sexcenti loci mutili sunt, ὁμοιοτέλευτον. //

Kent-de Melo: *Quorum uerborum nouorum ac ueterum discordia omnis in consuetudine com<m>uni, quot modis [litterarum] commutatio sit facta qui animaduerverit, facilius scrutari origines patietur uerborum: reperiet enim esse commutata, ut in superioribus libris ostendi, maxime propter bis quaternas causas. Litterarum enim fit demptione aut additione et propter earum tra<ie>ctionem aut commutationem, item syllabarum productione <aut correptione et*

*adiectioe aut detractioe>; quae quoniam in superioribus libris [qui] cuiusmodi essent exemplis satis demonstrari, hic ammonendum esse modo putavi. Hanc uariam lectionem apud Kent inuenias: ‘detractioe>’³; // Added by Kent, after Scaliger, Mue., GS.; Quintilian, *Inst.Orat.* I 6.32’. De Melo autem in apparatu: aut correptione et adiectione aut detractioe *add. Scaliger Müller*, quam exacte, quod ad Mueller attinet, tute, lector, facile uideris. Hic est autem Quintiliani laudatus locus: *Iam illa minora, in quibus maxime studiosi eius rei fatigantur, qui uerba paulum declinata uarie et multipliciter ad ueritatem reducant aut correptis aut porrectis, aut adiectis aut detractis, aut permutatis litteris syllabisue*. Quintilianus autem magis cum Goet.-Schl. facere uidetur, qui nihil post *syllabarum productione* posuerit (quod possis forsitan ad Quintiliani textum prorsus conformare, *productione* in *permutatione* immutata [*syllabarum productione ~ permutatis litteris syllabisue*, attamen seruato dubitationis signo †]). Nec aliter sensit Fridericus Leo (1889: 289 n.5), quamquam a de Melo non satis probatus est: qui omnium optimam uel unam inuenerat confirmationem, qua totus locus integraretur, de ea autem loquor integratione, quam C.O.Mueller proposuit, non de nimia ea et improuida, quam Kent et de Melo protulerunt.*

Pp. 55–60: We find an excursus on Latin metre, which is excellent, though reduced as it is natural: at p. 57 the rule κατὰ τέταρτον τροχαῖον is mentioned, so why not also the rule κατὰ τὸν τρίτον τροχαῖον? I agree with de Melo on the conclusion about Saturnian metre (p. 60): “What we can all agree on is that Saturnians regularly fell into two halves, the first of them typically longer than the second”. I not only agree with this recognition but would accept Pighi’s (1968: 257–282) opinion that this could be explained as a kind of word rhythm, a rhythm upon which Pighi insisted a lot. All this is well done. I would have expected de Melo to use as an example of some rules some verses of them quoted by Varro, *ling.* book 7.

P. 77: Here de Melo has to remind the reader that this labiovelar (*qu-*) is conserved also in Hittite (the most important isogloss Latin – Hittite, see also later p. 111), While the pronoun *quis/qui* is never downgraded, the change which happened *quom > cum* (from an early *kom* ? De Melo writes) concerned the most frequent conjunction *cum* (and I would like to add for the same or similar reason **kuti > uti*, **kubi > ubi*).

P. 109: It is possible that Hittites spoke Luwian (the two languages were not very different and in Hittite script many Akkadic and Sumerian words and expressions occur) but it is certain that the treaty between Muwattalis and Alexander of Wilusa (Troy) was written in Hittite and was understood by Alexander who obviously spoke Hittite. We must be careful not to follow de Melo everywhere.

Pp. 110–111: De Melo reminds us that Friedrich (1974 [1960]) and Hoffner and Melchert (2008) are excellent introductions to Hittite and I agree without reserve. As a matter of fact, the Hittite verb is much simpler than Vedic, and as concerns moods, subjunctive and optative are not present in Hittite or must be expressed through the particle *man/maan*. But in Russian too subj. and opt. are expressed through the particle *by* and the past tense, nevertheless can we say that Russian is not an IE language? On the other hand, Hittite disappeared 400–500 years before

Vedic was written. We know Hittite only through inscriptions and tablets. Hittite was written mostly in cuneiform, Vedic in Devanagari and, for written texts BC a millennium passed between Old Hittite (the text of laws) 1800 BC and Vedic written 800 BC.

P. 129: Here de Melo treats the ‘Parts of speech’ but misses a good opportunity to explain that Alexandrian grammarians acknowledged eight parts of speech, Stoics nine, and that Lalot (1994) is good but unconvincing on this subject. I was rather convinced by Mattaios (1999 and 2002) and the absence of these works in the related literature of de Melo arouses some suspicion in me.

P. 137: The discussion concerns valency, the valency of two or three degrees of a verb. It seems that de Melo does not know or consider the Montague grammar (cf. Richard Montague 1974) which could be employed to clear up this subject, and my question is what kind of modern linguistics was used by de Melo.

P. 140: I read: “The Indo-Aryan languages typically have a tense-based split system: the past tenses follow the ergative-absolutive pattern, the non-past tenses follow nominative-accusative marking. Sanskrit is still a nominative-accusative language. The ergative-absolutive pattern came about when passive clauses were re-interpreted as active ones. The passive is more common in past tenses because, statistically, past tenses tend to focus on the patient. Patient-focus can be achieved by passivisation, but past passives became so frequent that they became the unmarked way of presenting past events, and, consequently, such clauses were re-analysed as active. The instrumental, originally used to mark the oblique passive agent, became the new ergative subject”. I must say that I like this scheme of development where also the ergative is combined with past tense-medio-passive and which we find explained and confirmed by the Meid-Neu hypothesis (cf. E. Neu 1985). I started myself from ergative (Calboli 1983: 15–31), but we must avoid an oversimplification from which de Melo seems not to be completely free. E.g., it is true that Sanskrit is a prevailing nominative-accusative system like Hittite and Latin, but the enormous use of past participle *-ta* (originally an adjective as showed by Brugmann 1895) is a proof that Sanskrit remained a language connected partially with the original system of passivisation. The element which played an important role in such a conservation is the *-r* element, employed in 3rd pers. active of past tense and in medio-passive (cf. G. Calboli 1962: 56–115; P. Flobert 1975: 466). However, in this scheme de Melo neglected a little the aspects which are at the basis of the Meid-Neu hypothesis. Another very important point is the absence of the accusative with the ergative of Caucasian languages, and another the presence in Hittite of few imperatives of first person such as *ašallu* (*ešlut*, *ašlit*) ‘ich will sein’, of the *mi*-Conjugation and, for the *hi*-Conjugation, *šeggallu* ‘ich will wissen’, *akkallu* ‘ich will sterben’, cf. J. Friedrich 1974 [1960]: 80, 99, 139; K. Strunk 1984; G. Calboli 2011: 21 n.2, 25, 80–81). Hittite language seems to me in general very old and

the presence of only a few forms, in particular, in the verbal system, is, in my opinion, the effect of a conservation, not the product of a reduction. Therefore, I do not believe the quoted imperatives are residual forms of a previous system, but rather isolated forms which did not converge into an organised system. This means that the development from a system with ergative and passive and imperative to give expression of oblique moods to a new system with active and optative and subjunctive and accusative, as in Sanskrit, was not easy and simple. It seems however that it was extremely difficult and reduced to a few forms as in Hittite or as at least conservative of some old conditions as the frequent passivisation in Vedic, Sanskrit, Latin, and, as for Latin and Celtic languages, with the connection of past tense and perfect aspect with medio-passive forms in *-r*. Anyhow this really fascinating development presented by de Melo – a development which I like a lot, I repeat – cannot avoid the criticism of being a kind of oversimplification, if substantial elements of the development have not received enough care and attention.

P. 159: De Melo is accustomed to stray from the theme, e.g. pp. 157–161. The stories he tells are interesting but not always adapted to Varro. Varro is judged (pp. 160–161) as not very competent in this field (“Varro did not have our modern knowledge of sound change [...] Varro ignores second diminutives with another diminutive suffix added on, for instance *puellula* ‘little girl’ and *bellulus* ‘nice and pretty’”). This is a natural effect of comparing Varro with modern science. I do not criticize this kind of evaluation of Varro (it may be instructive to know the precise action of modern science upon minimal things), but it seems to me that this is rather a secondary aspect of Varro’s knowledge of grammar.

P. 168: Here de Melo touches on a big question: the use of article. However, I spoke of article in English and German and Romance languages, while in Latin an article was not developed and de Melo neglects asking the real question: why Latin did not develop an article, while Greek did it. I dedicated a whole book to investigate this question from every point of view (cf. G. Calboli 1997, 2013). I started from the doctrine of modern logicians like Quine (1960, 1974) and Carnap (1960), and I made a proposal which has not until now been demonstrated as unacceptable: namely, that article was developed using demonstrative pronouns such as *ille*, *ipse* in Romance languages, the theme **so-*, *tod* in Greek when the nominal constructions such as the AcI were strongly challenged in Greek and Late Latin by subordinate clauses introduced by *ὅτι*, *ὥς*, *quod*, *quia*, *quoniam* in dependence of a verb of speaking, thinking, believing and similar (cf. Cuzzolin 1994). That was easier and almost natural in an AcI construction where the subordinate clause was introduced into the principal such that they were a kind of united clause, while, in the case of a subordinate clause introduced by a conjunction derived from a relative pronominal theme, the group remained composed of two distinctly separate clauses and the reference of nouns connected with the principal clause

had to be marked through a demonstrative pronoun which, frequently used, became an article. The article was therefore produced by the necessity of highlighting clearly the references of the involved nouns. Through the frequent use of these reference-pronouns appeared a kind of obligatoriness which depended, actually, only upon high frequency, and could change by changing language, for instance, English and German, as showed rightly by de Melo. But I had to use Chomsky's structure (Chomsky's [1981] binding theory about subordinate clauses; and Chomsky's [1986a and 1986b] *Barriers*), a kind of modern linguistic model never employed by de Melo. It feels a little strange that de Melo criticizes Varro on p. 159 for using a grammar and linguistics of more than two millennia old when de Melo himself did not use a grammar and linguistics developed 40 years ago.

P. 171: De Melo writes that Varro is wrong because he confused the ending *-um* of *denarium* meaning thousand *denarii*, originally a genitive *mille denarium* > *mille denariorum*, "and this ending confuses Varro": However, Varro was not confused: Varro, *ling. IX* 85 Goetz-Schöll in *ipsis uocabulis ubi additur certus numerus in miliaris aliter atque in reliquis dicitur: nam sic loquuntur, 'hoc mille denarium', non 'hoc mille denari<orum>' // denariorum* L. Spengel *denarii* F, also de Melo reads *denariorum* //, et 'haec duo milia denarium, non 'duo milia denariorum'. De Melo is right in giving the same text and translating it correctly (p. 577): "when in the words themselves a definite number is added, one speaks differently in the thousands from how one speaks in the rest. For they speak like this, neuter singular *mille dēnārium* 'a thousand (of) *dēnari*' not neuter singular *mille dēnariōrum*, and neuter plural *duo milia dēnārium* 'two thousand (of) *denarii*', not *duo milia dēnariōrum*." I do not understand why de Melo accuses Varro: Varro is perfectly conscious that *denarium* means thousand *denarii* and the two gen. forms have been employed to explain the difference by flecting *dēnariōrum* and saving without inflection *dēnārium*, which is the correct use of his time.

P. 177: How can de Melo say that *Diēspiter* did not exist? We can only say that we do not find *Diēs-piter* either in inscription or in literary works, but we must respect the witness of a native speaker.

P. 181: I must acknowledge that de Melo has the merit of giving many important data, for instance, of mentioning (p. 181) the distinction between telic and atelic, illustrated through a clear example: Philip is a runner without a precise end-point, a τέλος, this is atelic (continuing), if however Philip has an inherent end-point, the example is telic. If we interrupt an atelic process, the sentence remains true; if we interrupt a telic, the sentence is not true. The IE verbal roots are telic or atelic. A description of different forms of verb follows with the distinction between perfective and imperfective aspect.

P. 184: As for subjunctive and future, I would like to add some morphological data, namely to add to the correct observation of de Melo that in Latin "there are

not future subjunctives”, the note that Latin future forms seem all derived from old subjunctives, and the Latin subjunctives we find in historical Latin are all or almost all originally optatives. In other words, subjunctive, optative and future in Latin and Greek are connected forms interchangeable in the historical development of these languages. The history of optative and subjunctive shows that in Latin only the optative remains, in Greek only the subjunctive (G. Calboli 1966: 250, 336; Schwyzer and Debrunner 1959: 316, 337f.). That does not lessen de Melo’s observation but can substantiate it with stronger elements.

P. 189: Every discussion on subordinate clause reveals a kind of weakness of de Melo, in particular, his ignorance of Chomsky’s *Barriers* (1986a) which, in my opinion, explains in depth how subordinate clauses were connected in Latin with the principal clause. One of the recent books on Latin subjunctive, that by Müller-Wetzel (2001), is dismissed without any appreciation: “In consecutive *ut*-clauses (‘so that’), there is no real purpose. The clauses are factual too. So, it is rather odd that the subjunctive is used. There are historical reasons for this, but synchronically it makes little sense. We should take this as a warning not to try to look for a unified explanation for the use of subjunctive, like Müller-Wetzel (2001), for example”. I do not agree. I drew, however, great attention in my ‘Bericht’ about Greek and Latin moods (Calboli 2012: 48–56) to the change of moods indicative – subjunctive in consecutive clauses, and discussed the specific literature about this use: I compared Hittite (where only coordination occurs), Greek (ὥστε + indicative or ὥστε + infinitive) and Latin (subjunctive) and Late Latin (subjunctive and indicative), and I compared as well the negation *ut non/ne* in consecutive clauses and clauses depending upon *accidit*, *euenit*, and wrote in conclusion:

Thus, according to my scheme, the really consecutive clauses must tend towards the indicative. The perfect subjunctive, not following the *consecutio temporum* and not using the negation *nē*, goes in this direction and is in accord with the indicative and with the use of the Greek οὐ. On the contrary the clauses that keep the subjunctive and the *consecutio temporum* are attracted by the deontic sphere (volitive or desiderative or simply preceptive) and therefore tend towards the use of *nē*. But the origin of this lies in the fact that with the use of the *consecutio temporum* the subordinate clause remains in the sphere of the main clause subject and reduces the strength of the barrier, while without the *consecutio temporum* the condition is more similar to indicative with a barrier and the position is full opaque (*de dicto*). This schema has its consistency confirmed by the fact that Latin and Greek behave analogously. (Calboli 1995: 149)³

³ Then I wrote in German in my *Aduersaria*: “Ch. Touratier 2008: 115, hebt zu Recht den Unterschied zwischen *nē* und *nōn* hervor, aber zieht eine falsche Schlussfolgerung: ‘Une telle variation de la négation *ne* ne peut se comprendre que si l’on admet que le subjonctif est le signifiant de deux morphèmes différents, deux morphèmes homonymes donc, que l’on appellera respectivement morphème de «volonté» avec la négation *nē*, et morphème de «possibilité» avec la

P. 191: Again, on subjunctive, a field which de Melo does not seem to master completely. For he writes: “Commands can be in the imperative, the subjunctive, occasionally the future indicative, but not in past tenses. And exclamations are often in the accusative and infinitive”. This is true, but in past tenses we cannot distinguish between subj. perfect and fut.II, e.g. *fecesis>feceris*. I tried to reduce the strong connection which exists between subj. and futur (Calboli 1966: 267f.), but I could not go against the evidence, namely that both indicative fut.II and perfect subjunctive have originated from the same forms of optative, the distinction consisted in old Latin in the difference of *-ī-* subj., and *-ī-* indicative fut.II, a difference which disappeared in later classical Latin. In Late Latin remained “nur noch ein ganz unbestimmtes Gefühl für den Unterschied zwischen Konj. Perf. und Fut. II” (Hofmann and Szantyr 1972: 324). As for the meaning the subjunctive ‘potentialis’ is very close to future, for instance, ‘*Catilina dixerit*’ ‘it is possible that Catiline says’ = ‘*Catilina will say*’. Then de Melo (p. 191) adds with another oversimplification: “Latin has four sentence types: statements, questions, commands, and exclamations. These are syntactically distinct. For instance, questions are typically introduced by a question word like *quis* ‘who’ or a particle like *-ne*, which is neutral in tone. Commands can be in the imperative, the subjunctive, occasionally the future indicative, but not in past tenses. And exclamations are often in the accusative and infinitive”. He has to distinguish between positive and negative commands and explain why in the negation we find *nē dixeris*. On the other hand, the use of the AcI to serve a purpose in the form of an exclamation is more complicated than appears at first glance and the connection with interrogative clauses in indirect speech meets some problems, as I tried to explain in Calboli (1981), e.g. Ter. Andr. 609 *servon fortunas meas me commisisse futtili!* 870 *tantum laborem capere ob talem filium!* (cf. G. Calboli 1981: 133–143).

négation nōn’. Denn man bringt ohne Schwierigkeit zwei Einwände vor, dass einerseits die Negationen dazu tendieren, sich auf eine einzige zu reduzieren, d.h., auf *non* im Lateinischen und in den romanischen Sprachen, auf *μή* im Griechischen und auf *mā* im Sanskrit (obgleich die Negation *na* überwiegend geblieben ist, vgl. Speyer 1896: 73), und dass sich andererseits selbst die ursprünglichen zwei Modi auf einen Modus beschränkten. Dies entspricht zwar einer diachronen Ansicht der Sprache, aber das ist gerade, was ich kaum akzeptabel finde, dass man sich auf einen synchronen Gesichtspunkt beschränken und auf jede Diachronie verzichten darf [de Melo wrote: ‘synchronically it makes little sense’; that is true, but an absolute synchrony does not exist, cf. Soussure 1949: 117, who nevertheless points out that some aspects of a language can be studied only synchronically. Anyhow we must take into account the note where T. De Mauro (1967: 421–425) informs us of the big discussion produced by this page of Saussure’s], während m.E. beide Gesichtspunkte immer miteinander berücksichtigt werden müssen”. The same conclusion by De Mauro.

P. 195: Also here de Melo ignores the big problems which are connected with relationship passive ~ active (cf. G. Calboli 1997: 135–179). The total ignorance of Transformational Grammar plays a negative role in de Melo's research field (see Jaeggli 1986 and G. Calboli 1997: 135–179).

P. 199: Here de Melo compares the following two sentences:

"How are we today?
and (Ter.Ad.634)
Aperite aliquis actutum ostium.
Someone open the door at once!

[...] In the second sentence, someone ought to open the door, but no one in particular is addressed, as it is not clear who is inside the house; hence the use of the third singular indefinite *aliquis*, which does not fit the second person plural imperative and could be seen as a vocative". De Melo is right in describing the state of mind of the *adulescens* Aeschinus who kidnapped a young girl, a *psaltria*, for his brother Ctesipho and after realizes that his lover Pamphila and her family believes that Aechinus kidnapped the young girl for himself. Aeschinus is desperate, runs to his lover's house and is knocking on the door, asking that someone opens the door immediately (I will also give the context ignored by de Melo and also the line 635): Ter.Ad.634–635 *heus heus Aeschinus ego sum: aperite aliquis actutum ostium. / prodit nescioquis: concedam huc*. Can we be sure that *aliquis* is a vocative instead of a nominative by considering the following *nescioquis* which is a kind of synonym of the previous *aliquis*? The only difference suggested by the context seems to be that *aliquis* is someone with the presupposition that it does not matter 'who', *nescioquis* is without such a negative presupposition. Another case of a too hasty analysis by de Melo.

P. 203: De Melo discusses a question already taken into account by Adams (2003: 497–503) and writes: "In nouns, an educated writer may follow Greek inflectional patterns. Problems arise when Latin requires an ablative, the case Greek does not have. In this case, Cicero's strategy differs from declension class to declension class. In the Greek second and first declensions, he combines *ab* and similar prepositions with the Greek datives in *-ω* and *-α*. By the classical period, they were only written as diphthongs, but in pronunciation they had become long vowels and thus resembled Latin ablatives in *-ō* and *-ā*". With reference to Adams (2003: 497–503). However, attention! I discussed already Adams' passage and solution (Calboli 2008: 476) and I challenged all Cicero's examples, and I demonstrated that not one of Cicero's examples is sure or that they actually do not exist. Only one of Antonius' examples exists, given by Cic. Att. 10,8A: *ab offensione nostra, quae magis a ξηλοτυπία mea quam ab iniuria tua nata est*. Cicero was very attentive to these uses, more attentive than Antony.

P. 213: To present and discuss Varro's terminology is a very good idea for which we must be grateful to de Melo. For problems about the term *accusatiuus*, see Calboli (1975: 99–102).

P. 219: The Latin word *figura* corresponds to Greek σχῆμα in the theory of Figures in the rhetoric. In the first Latin treatise on figures, namely the *Rhetorica ad C. Herennium*, the term *figura* is employed to name the genders of the speech, the χαρακτήρες τῆς λέξεως, grand, middle and plain, while *figura* to name securely the σχῆμα occurs firstly by Quintilian, *inst.*1,8,16. W. Kroll, Commentary to Cic. *orat.*83, suggested that already Cic. *opt.gen.*14 and 23 used timidly *figura* for σχῆμα (cf. G. Calboli 2020: 649).

P. 224: Varro, *ling.*8.45 <*uocabula*> *ut scutum, gladium; nomina ut Romulus, Remus* // *uocabula* add. Laetus. If we accept Laetus' integration *uocabula* as Goetz-Schöll and de Melo, we are very clearly dealing with the Alexandrian and Stoic distinction with the only difference being that for the Alexandrian grammarians *uocabula* and *nomina* belonged to the same part of speech, i.e. the noun, while for the Stoics to two distinct parts of speech, with the consequence that in the Alexandrian system the parts of speech were eight, in Stoic nine (cf. Calboli 1962: 171–176, 2013: 33–44). We find the distinction between *uocabulum* and *nomen* in de Melo's Commentary 1045, without the distinction between Alexandrians and Stoics, though such a distinction is not in Varro and de Melo could omit this, albeit important, notice.

P. 229: De Melo's knowledge of logic and theoretical linguistics is too thin and limited. At p. 229 he rejected as “not tenable” the opinion by Sophie Roesch that the meaning of *res* is distinct from *materia* and he refers to his own discussion on p. 1145 (9.56). However, we read in the referred page: “I believe that the two terms [*res* and *materia*] are synonymous and that both indicate grammatical substance”. What does that mean for de Melo? That every difference between two synonymous words is excluded? It would be strange that two words *res* and *materia* are employed everywhere without any difference, though I can concede that *res* sometimes invades the semantic field of *materia* in force of its hyperonym meaning. When I started studying synonymy (Calboli 1965: 52–64), I used previously Carnap's (1955) clear definition of synonymy (coincidence of intensionality and extensionality) but also the theoretical linguistics of these times which provided a reduction of synonymy also for expressions which remained inside of the synonymy: a gradation of synonymy was confirmed, a complete similarity was excluded or, better, challenged by various situations.

What de Melo writes on the same page about the Latin *sextus casus* and the final observation that *ablatiuus* “gives our case too narrow a function” is correct, as for the name *ablatiuus*, but against the history of cases, as for the use, because the *ablatiuus* was extended to so many similar functions.

P. 232: *Tempus* for Varro is tense or aspect but what is for Varro the relationship tense-aspect? Or does Varro know the aspect? This is the question. De Melo plays with time and tense and does not take this question clearly into account.

P. 233: Varro seeks to avoid every ambiguity between *uerbum* ‘word’ and *uerbum* ‘verb’ and by using *uerbum temporale* Varro shows that he noticed and perceived the problem of this difference, namely between *uerbum* ‘word’ and *uerbum* ‘verb’.

P. 234: De Melo writes: “**Vocābulum** [...] This is a very frequent technical term, with 132 attestations, but not a very well defined one. In VIII 40 and X 20, it contrasts with *nōmen*, and the examples provided show that the *uocābulum* indicates the common noun, while *nōmen* marks the personal name (in the Stoa, this is the distinction between *προσηγορία* and *ὄνομα*)”. Here de Melo shows a lack of information about the difference between *ὄνομα* and *προσηγορία* the Stoics (and Pergamenian grammarians) and Alexandrians grammarians acknowledged, because both schools distinguished the different meaning of *ὄνομα* and *προσηγορία* but the Alexandrians considered the *προσηγορία* as a part of the *ὄνομα*: a unitarian category existed, the *ὄνομα*, which was divided into specific *ὄνομα* to call people and *προσηγορία* to call things and the Alexandrians distinguished eight parts of speech, the Stoics one category more, since they made two categories of the noun: Dionisius Trax §11 (p. 23 Uhlig) Τοῦ δὲ λόγου μέρη ἐστὶν ὀκτώ. ὄνομα, ῥῆμα, μετοχή, ἄρθρον, ἀντωνομία, πρόθεσις, ἐπίρρημα, σύνδεσμος. ἡ γὰρ προσηγορία ὡς εἶδος τῷ ὀνόματι ὑποβέβληται. On the other hand: Schol.58,20.214.17 Οἱ Στωικοὶ ὀνόματα τὰ μὲν κύρια ἔλεγον, τὰ δὲ προσηγορικά οὐκ ὀνόματα, Quint. *inst.*1.4.19–20 *Paulatim a philosophis ac maxime Stoicis auctus est numerus, ac primum conuinctionibus [= coniunctionibus] (1) articuli (2) adiecti, post praepositiones (3): nominibus (4) appellatio (5), deinde pronomen (6), deinde mixtum uerbo participium (7), ipsis uerbis (8) aduerbia (9). Noster sermo articulos non desiderat ideoque in alias partes orationis sparguntur, sed accedit superioribus interiectio. Alii tamen ex idoneis dumtaxat auctoribus octo partes secuti sunt, ut Aristarchus et aetate nostra Palaemon, qui uocabulum siue appellationem nomini subiecerunt tamquam speciem eius, at qui aliud nomen, aliud uocabulum faciunt, nouem.* In Calboli (1962: 171–176) I demonstrated that already the Auctor of the *Rhetorica ad C. Herennium*, as it appears in the language employed in the *Rhet.Her.*, adopted an Alexandrian system with eight parts of speech and should adopt the same system in the grammar which he promises to write (*Rhet.Her.*IV 12,17). It seems that Varro knew well this system and such distinctions, though he omitted to mention this question. In the same years, Caesar followed the Alexandrian grammarians and used for example the name *Turbo*, *Turbonis*, a gladiator whose name was the same as the noun of a storm and therefore corresponded perfectly to the Alexandrian

system where ὄνομα and προσηγορία were distinct but belonged to the same part of speech (cf. A. Garcea 2012: 39 and 217; and G. Calboli 2013: 43–44).

P. 235: Here also de Melo ignores how complicated the noun of *accusatiuus* and the literature on this subject is (cf. G. Calboli 1975: 99–102). The term *uōx* is taken into account and de Melo rightly observes that “Occasionally, this shades into ‘word’ and becomes almost synonymous with *uerbum* in its broadest sense, but there is always an element of speech involved that sets *uox* apart from the near-synonyms”. This is true and right (albeit not clear), but de Melo did not make the necessary distinction between technical and semi-technical use of the words as *uox*, as ‘a part of speech’. Anyhow, what matters is that, if I want to consider this word, I must consult the Index by Goetz-Schoell, p. 340 or by Kent, p. 674. In de Melo’s Index you can find Cantonese, Hungarian, Mohawk language, Ojibwa language, Swahili, Yagua language (pp. 1321–1322), not the Greek, Latin and Romance languages which “are quoted so often that it would be impractical to list every instance”, as they are however quoted by Goetz-Schoell and Kent who left me uninformed about ‘Cantonese, Swahili, etc.’ – what a pity. The same happens with the following ἀναλογία (and the Latin correspondence *ratio* as viii 57) and ἀνωμαλία.

P. 237: On p. 237 I found a strange explanation of the use of *quod*-clauses instead of *AcI* and a too hasty treatment of a question of great importance in the development of Latin to Romance languages: “To me, – de Melo writes – style always has to do with choice. For instance, in the classical period, *scīre* ‘to know’ takes the accusative-and-infinitive construction exclusively. It is only much later that *quod*-clauses can be used instead, and it is only then that the choice involves stylistic element.” Another case of over-simplification: what de Melo calls ‘stylistic element’ corresponds to influence of Greek and Hebrew over the Church Fathers as S. Jerome, a scholar who through the text of Bible, the *Vulgata*, translated by himself (after the *Vetus Latina*), had an enormous influence on Medieval Latin, which is much more than a kind of style (cf. Calboli 2012). Anyhow the reference book on this question is Pierluigi Cuzzolin (1994), another absence in de Melo’s bibliography.

P. 240: As for the spelling *-es* and *-is*, it is possible that Varro was more consistent “in line with certain carefully crafted ancient inscription” or he was as careless as the medieval copists. We do not know what Varro’s practice was, de Melo has not attempted the readings of **F**. I agree completely with this solution and point out that this could be elected as general criterion, excluding precise different conditions.

P. 241: The fact that *quīs* (= *quibus*) is always combined with a preposition, in order to avoid any misunderstanding, as it seems, is rightly highlighted by de Melo. He stressed also opportunely that the adverb in *-tim* and *-ātim* acquired a

good productivity. That “This productivity is mostly pre-classical as can be seen by Plautine coinages like *assulātīm* ‘bit by bit’ (*Men.*859)” (p. 242) is probable as well as that *stillātīm* ‘drop by drop’ found only in Varro V 27, “may have been coined by him”. However, I do not understand why de Melo did not use the book by Judith Schaffner-Rimann (1958) about the adverbs in *-tīm* in order to have a complete collection of these adverbs.

P. 246: Also in the discussion on the “Impersonal Constructions”, de Melo quotes the form *itur*, which appears in Verg.*Aen.* VI 179 *itur in antiquam siluam*, a kind of formula whose antiquity has been highlighted already by E. Norden (1958: 188) and G. Calboli (1962: 64f.). Also in this case, I must point out the almost incredible lack of information about this ancient and much discussed question of IE grammar, namely the impersonal passive in *-r*, substantiated by Meid-Neu hypothesis and grounded on Hittite past.3.pers.pl. in *-r* and medio-passive in *-ri*. After the long discussion held on this subject, I took it again into account (G. Calboli 1962: 56–115, 1997: 138) followed by P. Flobert (1975: 466) in the opinion that we have originally to do with intransitive forms and that the personal, as for Latin, precedes the impersonal. At p. 247 we read: “Perhaps *sponsum erat* is used because it is gender-neutral, as in the discussion both a man and a woman are getting engaged to each other”. Instead of this psychological explanation of the impersonal, of every impersonal which does not explain why you choose impersonal, de Melo had rather to study the question of impersonal passive, better to say medio-passive and to avoid such an unacceptable explanation as that he gives (p. 247) of *nascitur* (V 70) and *potest ordiri* (IX 56): “I believe that these are instances of the impersonal third person singular, with *aliquis* or *quis* not used because of Varro’s stylistic economy”. De Melo masters in this way Latin impersonal. We have to do with two people who ignore in this question the development of IE linguistics, Varro and de Melo, but Varro because in his time IE linguistics did not exist. But this is not enough. A couple of lines later de Melo discussed one of the examples treated in this question, *sequendum est orationem* (9.38), which corresponds to Plaut.*Trin.*869 *mi aduenienti hac nocte agitandumst uigilia*. De Melo’s explanation (p. 247) is grounded on W. Blümel’s (1979) opinion that gerund is older than gerundive: “The impersonal gerund with the accusative object is simply the older construction here, while the gerundive arose as some kind of assimilation”. However, this explanation is, in my opinion, unacceptable, since the gerundive has been demonstrated to be older than the gerund by E. Risch (1984: 159–165). The *-ndus* adjective became substantive, which frequently happened with many adjectives. I took into account both excellent contributions by P. Aalto (1949) and W. Blümel (1979) but was persuaded by Risch’s arguments, in particular by the larger frequency of gerundive in comparison with gerund in oldest Latin (Calboli 1990: 280). Nevertheless, already A. Ronconi (1959: 203) suggested that *agitandumst uigilias*

could be compared with constructions which occur with the *nomina actionis* in *-tio* as Plaut.*Amph.*519 *quid tibi hanc curatio est rem?* Aul.423 *quid tibi nos tactio?* Therefore it seems that the gerund, though a substantive form of the gerundive, could be connected with an accusative like a noun as *curatio*, *tactio* in force of the fact that it developed a rich declination by the use of all cases as a substantive (cf. P. Aalto 1949: 92–94, 147). A consistent role was played in this connection of gerund and noun also by the reduction of passive diathesis pointed out for the Latin gerund by Lucia Calboli Montefusco (1971). In this case, the right explanation is very different from de Melo's explanation but not so far from it. However, also in this case, I criticize de Melo's oversimplification of the problem and the complete absence of the necessary literature.

P. 248: The argument is: "Mood in Indirect Questions", and de Melo: "Few studies exist on the subject", namely on use of indicative/subjunctive in indirect questions. Then he quotes Bräunlich (1920) and Adams (2013), but I wrote (Calboli 2012: 98): "In den letzten Jahren sind drei Bücher zu den indirekten Fragesätzen im Lateinischen [...] erschienen, zwei aus der Hand von Colette Bodelot, kurz nacheinander (1987 und 1990) und eines von Günter Eckert (1992)", then I have dedicated twelve pages to this question, I started from system and statistic by Bodelot and discussed mostly with Stephens (1985). In case we have at our disposal an excellent treatment of this question by J. Adams (2013: 747–770), where Adams took into account all important literature on the indirect question, in particular A. F. Bräunlich (1920) and L. Stephens (1985). Adams came to this conclusion (a conclusion with which, in my opinion, everyone must agree): in early Latin many apparently indirect questions (IQ) were free-standing exclamations or questions in a paratactic construction. In most cases an IQ however also in early Latin was expressed as dependent with the subjunctive (cf. Ch. Bennett 1910: 120–122). Already in early Latin some formulae appeared as *uide ut*, *non uides ut*, *uide si*, *uiden* with ind., and these remained in the following Latin. In classical Latin subj. was imposed also with the help of modern editors who "have rarely accepted such indicatives", though transmitted by the MSS (Adams 2013: 754, that is a right meritorious observation). Then Adams took into account some authors such as Vergil, Catullus, Horace (who avoided ind. in the *Odes*, but admitted in *Epistles* and *Satires*), Vitruvius, Propertius and Petronius. To Petronius some pages (Adams 2013: 762–765) have been dedicated by Adams, and, as for Latin grammarians, Adams, who had started (Adams 2013: 748) from Diomedes' precise and strong rule (GL 1.395.15–24) of ascribing to *imperitia* the use of indicative, shows with many examples (Adams 2013: 766–767) that Pompeius "roughly contemporary with Diomedes [...] used the indicative and subjunctive indifferently". Adams' conclusion (Adams 2013: 769) is worth mentioning: "The indicative in indirect questions occurs frequently in early Latin, though often in ambiguous context

where the clause may be interpreted as free-standing. By the classical period it had receded (though modern editions are often out of line with the manuscripts [and choose rather the subj.]), and there is some evidence that the old idea that it was colloquial [as supported by Bräunlich 1920: 162] is correct. Those uses that do turn up at this period are mainly stereotyped, and traceable back to early Latin. In the early imperial period, the distribution of the indicative in Petronius suggests that it was now socially stigmatized, but there are not ground for thinking that it was the norm in lower sociolects: it was an occasional variant” (Adams 2013: 769).

De Melo accepts Adams’ conclusions and points out (p. 249) that in Varro “the subjunctives far outnumber the indicatives”. Then he mentions three occurrences with indicative (5.105; 6.12; 6.39) which could be interpreted as relative clauses. In VIII 1 *prima pars, quemadmodum uocabula rebus essent imposita, secunda, quo pacto de his declinata in discrimina ierunt // ierunt* F de Melo *ierint* Schiop G.-Schl. Kent // *tertia ut ea inter se ratione coniuncta sententiam efferant* it seems to me that de Melo was right in following Adams (2013: 754) and saving *ierunt*. In VII 2 *uerba non omnia quae habe[re]nt* ἔτυμα *possunt dici // habe³ nt* (= haberent) G.Sch. Kent de Melo //. De Melo considers this an IQ but saves *habent* as a correction of the ‘inconcinnum’ tense *haberent*. However, I am not completely sure that Varro did not jump to a kind of irrealis or pass which produced, in complete freedom from every grammatical rule, *haberent*. Please listen out the whole group of clauses: *sed ut in soluta oratione sic in poematis uerba <non> omnia quae haberent* ἔτυμα *possunt dici, neque multa ab eo, quem non erunt in lucubratione litterae prosecutae, multum licet legeret*. Is the final subj. *legeret* in a different position as *haberent*? Was it not worthwhile to have such a doubt? In VIII 71 *item qu<a>erunt: si sit analogia, cur appellant omnes aedem Deum Consentium et non Deorum Consentium?* De Melo’s interpretation that this is a direct question seems to me better than G.-Schl.’s and Kent’s interpretation that it is an IQ as it appears from the punctuation, a comma in G.-Schl. and Kent, colon in de Melo. He then reminds us (p. 250) that of the twelve instances interpreted as IQ, five are of *sunt* which in minuscule script “looks very similar to *sint*”. To sum up the treatment of this question is, in my opinion, accurate and well discussed. I can easily attribute it to the excellent discussion by Adams which played as a guide.

I appreciate also the matter collected by Adams and de Melo and the comparison indicative/subjunctive. At any rate the factuality of the indicative appears very clear. I would like therefore to repeat the conclusion I achieved at the end of my discussion of exchange indicative/subjunctive in these clauses (Calboli 2011–2012, 2012, 110): “Tatsächlich gibt es den Konj. in den i[ndirekten] F[ragesätzen] für die Dauer des klassischen und scholastischen Lateins. Das Altlatein und das Italienische haben normalerweise den Ind. (aber nicht immer, und es hängt vom übergeordneten Verb ab). Das bestätigt jedoch meine Erklärung: Der ‘modal shift’

[i.e. the subjunctive] hängt von einer stark auf die Subordination und den Gebrauch des AcI [und des Partizips] des klassischen Lateins ausgerichteten Struktur ab". Through the 'modal shift', namely the use of subjunctive produced by subordination in Latin, i.e. AcI, participle, extension of subjunctive, the factuality of indicative has been, thanks to the opposition, in some way more highlighted.

The last question treated in the Introduction (pp. 250–253) concerns the word order: "subordinators do not occupy the first position in the subordinate clauses they introduce". For instance, if the subordinator is the conjunction *si*, which is rather frequent in Books 5–10 (with 63 less than one-third of all occurrences; more in Books 8–10 where the argumentation is more frequent) VIII 55 *Quoniam taberna ubi uenit uinum a uino uinaria ... dicitur, ἀνὰ λόγῳν si essent uocabula, ubi caro uenit carnaria ... dicerentur* or VIII 79 *Magnitudinis uocabula cum possint esse terna, ut cista, cistula, castella, in quibusdam media non sunt*. De Melo rightly explains that the focalization and topicalization have been the main reasons why these (topicalized) elements were placed before subordinators. At this point de Melo introduces a third rule, the extraposition, without clearing topicalization, focalization, extraposition, while these concepts have been made clearer in modern linguistics. In particular, Ch. Touratier (1980: 197–211) employed largely the extraposition of the relative clause, called 'attraction inverse', concluding with an interesting example by Cato: *agr.34.2 In creta et uligine et rubrica et ager qui aquosus erit, semen adorem potissimum serito. Quae loca sicca et non herbosa erunt, aperta ab umbra, ibi triticum serito*. Here we find two different positions of a relative clause with both postposition and anteposition of relative clause. Both cases, in my opinion, are the effect of topicalization. De Melo for his part concludes the discussion of this section with a noteworthy example 8.6: *Qua enim ratione in uno uocabulo declinare didiceris, in infinito numero nominum uti possis*. Here *qua ratione* is the object in ablative for the verb *uti* and the relative pronoun *qua* needs to stand itself in ablative. De Melo's explanation is correct and well discussed (p. 252): "I have described the phenomenon as if it were some innovation, with the expected order *rationē quā* and some subsequent shift in word order pattern. However, historically, the order *quā rationē* is older; it is an inherited type of relative clause, which in early Latin is often found in a fuller form, 'which law (relative clause) ... this law (main clause)'. The element 'which law' is topical in this type of corellative clause". I agree and, as for the existence of this structure, I would like to add a confirmation from the Hittite, quoting only one example:

KBo VI 34 II 44 f. *ki-i-wa¹ ku-i² Ū-UL-wa³ ŠA⁴SAL⁵ TÚGNÍ.LÁM.MEŠ⁶*
 'That¹ which² (is) (here), (are) not³ the garments⁶ of⁴ a woman⁵?'

The type we found in Cato, *agr.34.2 Quae loca sicca [...] erunt [...] ibi triticum serito*, namely where relative adjective occurs before his modificand, in Hittite seems to be

a little less (195) than the relatives which follow it (261) (cf. W. H. Held 1957: 15, 40; and H. A. Hoffner and H. C. Melchert 2008: 175, 13.10). However, I cannot confirm that the type ‘which law ... this law’, which de Melo gave without references, actually exists in Latin. I did not find it in *Th.L.L.* VII,2, 1238.31–1256.56. After I wrote this suspicion, I checked Brepolis which previously was not yet activated on my computer and remarked that my suspicion was correct: the expression *qua lege ... ea lege* does not exist in Latin of every time. The most similar expression occurs by Alcuinus, *Comm. In Ioh.Euangelium, epist. Ad Islam col.852 Qui non nouerant legem ipsi credebant in eum; et qui docebant legem, eum qui miserat legem, contemnebant*. However, the developed form *ea lege qua lege* occurs twice in Cicero’s speech *Pro Cluentio* 148 and 156: the attention and frequent treatment of the law contributed to the development of the new expression. De Melo’s hypothesis seems to be right, but more attention is required in the use of the examples.

Now I direct my attention to some points treated in the Commentary, but I must say that I admire de Melo’s large work and the thorough treatment of so many etymologies. He did not employ Ernout-Meillet 1960⁴: but he used almost always the specific literature for every word taken into account. This is a merit I would like to highlight. What is the price of this admirable collection of etymologies? Unfortunately, sometimes a lot of questionable actions, as the disregard of Ernout-Meillet 1960⁴: e.g. p. 681. Here de Melo did not remind us that an IE etymology of *fundus/fundamentum* from a root *bh-udh-* (as supposed by de Melo from DV 250) has been discouraged by both Ernout-Meillet and Walde-Hofmann (Ernout and Meillet 1959: 261f. “On ne peut même affirmer que *f*-du Latin repose sur *bh-*: car on a des forms à *dh*-initial: v.sl. *dũno* « fond », lit. *dũgnas* « fond » et arm. *an-dundk’* « abîme »; ce *dh*-ne peut être séparé de celui qui apparaît dans les mots signifiant « profond » (cf. *profundus*): got. *diups*, lit. *dubùs*, irl. *domain*, *fundomain*, gall. *dwfn*”); Walde and Hofmann (1965: 565 “Auf weitere Analyse von idg. **bhudhnos* ist daher besser zu verzichten”). De Melo can answer that it is not sure that Ernout-Meillet and Walde-Hofmann are right, but, in my opinion, de Melo had to discuss it, not follow without commentary de Vaan, who hypothesized *bh-udh-ó-*, albeit after quoting EM, WH.

P. 657: De Melo treats the fourth level of etymology and after quoting the most recent explanations, namely that this is the level of a king (Romulus, Latinus), adds with a felicitous intuition: “After the establishment of the Republic, the religious functions were taken over by a religious *rēx* or ‘high priest’.” I remember that I myself (Calboli 1966, “*Et ambulando discitur*”: 218–224) discussed this fourth level of etymology and demonstrated (by using the bibliography available at that time) that in the expression (V 8 *Quartus, ubi est adytum et initia regis*) means with *initia regis* the God *Janus*, God of the *prima*, Janus and his *sacerdos*, the *rex sacrorum* who opened the series of *sacerdotes* in the procession, before the three

flamines and the *Pontifex maximus*. My reference to *Janus* and his *sacerdos*, the *rex sacrorum*, was not a substitution but an integration of *reges Romulus* and *Latinus* as onomatopoeies, since the *rex* (*rex sacrorum*) was like *Janus* to begin.

P. 687 Comm.: Varro, V 43 *Itaque eo ex Vrbe aduehebantur ratibus, cuius uestigia, quod ea qua <vec>tum dicitur Velabrum // qua <vec>tum* L. Spengel *quatum* F.

The reading we find in F (f.3v) is clearly: *ea quatu³ dē*. This point has been brought to my attention by Emil Vetter (1958: 259) who wrote “GSch setzen vor *ea* die *crux*, K[ent] setzt nach L.Spengel hinter *tum* ein überflüssiges *advectum* in den Text [...], denn *aduehebantur* im Impf. ist aus dem Früheren zu ergänzen”. Kent in app. added: *advectum* an integration by L.Sp. However, as I took into account L. Spengel’s edition (1826: 49), I neither found *advectum* by Kent nor *<vec>tum* by L.Sp. and A. Spengel (1885) and de Melo, but *ea qua tum dicitur velabrum*. I found, however, Kent’s reading in L. Spengel (1830: 11 n.12): “Etiam quae praecedunt verba *ea qua tum dicitur velabrum* corrupta; cod. Gothanus *qua dum*, in quo haud dubie latet: *ea qua advectum, dicitur velabrum*”. The right reference (to Spengel, *Emend. Varron.* p. 11) is given also in C. O. Müller’s ed. (1833: 17 in app.). Later L. Spengel improved his conjecture and changed *advectum* into the simpler *<vec>tum*. Nothing is either in de Melo’s text nor in Comm. (p. 687). Nothing where L. Spengel’s integration *<vec>tum* was suggested. Therefore, I mean that de Melo made two mistakes: (1) he missed a discussion to explain why he chose *ea qua <vec>tum dicitur Velabrum* as conjectured by L. Spengel instead of *ea qua tum dicitur Velabrum* of Mediceus; (2) he accepted the conjecture *<vec>tum* by L. Spengel instead of the original reading of Mediceus *tum*. In this point I agree with Vetter. For, in my opinion, we must ask: at Varro’s time, were Romans prevented from understanding the word *Velabrum*, a synecdoche for flooded *loca*, which in Varro’s times were not more flooded as in early times (and we can imagine also the jokes by the other citizens of Rome)? What is the difference between the opposition *tum ~ dicitur* and the series *<vec>tum dicitur*? The elimination of the reference to early times (*tum*) excludes every reference to an important element in this resumptive expression *ea (loca) qua tum*, where three data are expressed: (1) the flooded parts of the city, *ea* (2) the movement through these spaces *qua*, (3) the early presence of this situation, *tum*⁴. In my opinion (a questionable opinion, but strong enough) for Varro it was not necessary to enforce the already strong expression *qua* by adding *<vec>tum*. This concerns not only this passage, but also the whole language of Varro.

⁴ I would like to exclude the too precise *tunc* in comparison with the more generic (the early times) *tum*.

P. 688: Varro, *ling. V 46 In Suburanae regionis parte princeps est Caelius mons a Caele Vibenna Tusco duce nobili, qui cum sua manu dicitur Romulo uenisse auxilio contra Tatium regem*. This event has been described also by the Emperor Claudius in the *Tabula Lugudunensis*, and here de Melo gives an imprecise notice, perhaps influenced by the argumentation of Tim Cornell: “Interestingly, the Emperor Claudius, based on an Etruscan tradition, considered Caele Vibenna a contemporary of the Tarquini rather than of Romulus (Cornell 1976: 414–15)”. Actually Emperor Claudius in the *Tabula Lugudunensis* wrote: “*Priscus Tarquinius [...], postquam Romam migravit, regnum adeptus est. Huic quoque et filio nepotivae eius, nam et hoc inter actores discrepant, insertus Servius Tullius, si nostros sequimur, captiva natus Oeresia; si Tuscos, Caeli quondam Vivennae sodalis fidelissimus omnisque eius casus comes, postquam varia fortuna exactus cum omnibus reliquis Caeliani exercitus Etruria excessit, montem Caelium occupavit et a duce suo Caelio ita appellavit, mutatoque nomine, nam Tusce Mastarna ei nomen erat, ita appellatus est, ut dixi, et regnum summa cum rei p[ublica]e utilitate obtinuit*”. T. Cornell (1976 and 1995) distinguishes Servius Tullius from Mastarna and thinks they were two different individuals, implicated in two different events, Ser. Tullius became king of Rome, Mastarna with the brothers Caele and Aulus Vibenna occupied the *C. mons*. The earliest “reference to Mastarna in a Roman work could be a statement of Verrius Flaccus (ap. Fest., p.486 L.), if the group of Letters *Max* is restored with the addition of a suffix *-tarna* and produces *Max[tarna]* (another possible integration would be *max[ime]*) (Cornell 1976: 415). But Festus showed in another passage that he followed Varro’s opinion, namely that the *mons Caelius* was named after a contemporary of Romulus: Fest., p.38 L. *C. mons dictus est a Cale quodam ex Etruria, qui Romulo auxilium adversus Sabinos praebuit eo quod in eo domicilium habuit*. Perhaps the Etruscan historians contaminated the two events and ascribed the occupation of Caelius and Capitol (*Capitolium* <*Caput Oli*<*Caput Auli*, Aulus, whose caput was found in Capitol, was the brother of Caeles Vibenna) to brothers Vibennas in order to ‘etruscanise’ an important past of Roman history and make Etruscan “the best loved of the Roman kings”, Servius Tullius (Cornell 1976: 417). Emperor Claudius, whose first wife was Plautia Urgulanilla (of Etruscan origin), was much interested in Etruscan culture and would find the identification of Mastarna with Serv. Tullius in Etruscan sources we do not know. Tim Cornell for his part insists that Serv. Tullius and Mastarna were two different people and the unification was a work of the *Tuscae Historiae* which seems in a passage by Censorin.17.6 to be well known by Varro: *Quare in Tuscis Historiis, quae octavo eorum saeculo scriptae sunt [about II saec.], ut Varro testatur* eqs. Therefore, de Melo’s reference to “an Etruscan tradition” is correct as for Cornell’s construction. However, after Cornell (1976 and 1995) two other scholars took into account this question, C. Letta (2013) and S. Malloch (2020). The contribution by Malloch was

excluded for being too late, so Letta had to be employed by Melo, because Letta told, in my opinion, a more credible story. He starts from an attentive discussion of the painting in the tomb of François of Vulci. Serv.Tullius and Mastarna are the same person and Mastarna is an adjective which means something like a *magister equitum* (ἡγεμὼν τῶν ἰππέων, Dion.Hal. IV 3.2). Letta (2013: 107–108) concludes that in Etruscan tradition brothers Vibenna and Mastarna were soldiers of Tarquinius Priscus and Mastarna was a kind of *magister equitum*. Mastarna, owing to a fortunate series of events (*varia fortuna exactus* Emperor Claudius wrote, Tab.Lug.1.20), some of which were bloody (as shown in the painting of the tomb of François), became king of Rome and was called by the Romans Serv.Tullius. The Roman antiquarians avoided mentioning Mastarna (who was called securely first Mastarna by Claudius, Tab.Lug. 1.22) and ascribed this event to Romulus' time (“una parte della tradizione a partire da Varrone, per evitare qualsiasi rischio, preferì spostare tutta la vicenda dei Vibenna al tempo di Romolo”, Letta 2013: 109). Then Malloch (2020: 93–110) discussed thoroughly the whole question from all points of view and confirmed that Mastarna was not a proper name but something like a *magster populi*, a nickname (Malloch 2020: 117–118) and introduced by Etruscan tradition: “Again there is a danger of anachronism: the explanation of a linguistic feature of the sixth century on the basis of a later narrative that merged the Etruscan adventurer Mastarna with the Roman king Servius Tullius”. However, as I think, the witness of Emperor Claudius is the only sure item we have in this intricate question.

Anyhow, I think that de Melo was right in accepting Tim Cornell's opinion that the ‘Mastarna’ and Servius Tullius were historically distinct individuals, however I am not sure that this is the faithful solution. I am only sure that what I added (or only discussed) would be a necessary integration.

P. 733, Varro, *ling.V* 96 *Ex quo fructus maior, hi[n]c est qui Gr<a>ecis usus: <sus> quod ὕς, bos quod βοῦς, taurus quod <ταῦρος>, item ouis quod ὄις; ita enim antiqui dicebant, non ut nunc πρόβατον*. Here as for *taurus*, ταῦρος, added by Rhodlandellus, de Melo's discussion is excellent, since he reminds us that De Vaan (2008: 607) “suspects a loan from a Semitic language, while Walde and Hofmann (W-H 1965: II. 650–652) assume an Indo-European loan in Semitic”. Then that “Among Indo-European languages, we find Old Irish *tarb* ‘bull’ and Lithuanian *taūras* ‘aurochs’. In Aramaic there is *tōr* ‘bull’, corresponding to Hebrew *šor*, Isid. *orig.*12.1.29 believes that *taurus* is a loan from Greek”. All this is right, but I would add what I found in Ernout and Meillet (1960: 677), who pointed out the popular aspect of this word (*taurus*), and I think that this has more to do with Varro and is not less interesting than a Semitic origin of the Greek correspondence. Ernout and Meillet (1960: 677) wrote: “Le mot a l’instabilité d’un terme populaire” and previously: “la forme diverge dans gaul. *tarvos*, irl. *tarb* « taureau » (avec la même

altération qu'offre lat. *paruus* en face de grec παῦρος) et dans got. *stiur* « taureau » (sans doute sous l'influence d'une étymologie populaire; cf. av. *staora-* « gros bétail »).

P. 942: in Varro, *ling.* VII 36 we find the first attestation of the versus Saturnius after Ennius (*Ann.* 214 V²).⁵ '*Versibus quo< s> olim Fauni [et] uatesque canebant*' (*Enn. Ann.* 214 V2). *Fauni dei Latinorum, ita ut Faunus et Fauna sit. Hos uersibus quos uocant Saturnis in siluestribus locis traditum est fari <futura>, <a> quo fando Faunos dictos. Antiqui poetas uates appellabant a uersibus uiendis, ut <de> poematis cum scribam ostendam.*

Here de Melo (pp. 942–943) gives a strange discussion where he explains *Faunus* and *uates*, not *uersibus* [...] *Saturnios*. However, in this passage the most important question concerns the *uersus Saturnius*, though Varro himself gave up treating it in any way. A brief notice, in my opinion, was necessary, and even more because we have at our disposal rich collections of ancient and modern opinions and studies on this subject, namely the *Saturnius*, collections given by M. Barchiesi (1962: 310–327, modern theories), B. Luiselli (1967: 117–223 ancient and modern th.), G. B. Pighi (1968: 257–294). Barchiesi tried to combine the rhythm of words (namely Pighi's explanation, see below) with quantitative explanations refined in the excellent study by Friedrich Leo (1905), "Der saturnische Vers". Anyhow, Barchiesi accepted Pighi's hypothesis of word rhythm but only for the earliest times and added that the word rhythm slowly changed into a quantitative system. For his part, Pighi insisted in defending his hypothesis of a word rhythm as peculiar nature of all the Saturnians, without difference of time. Luiselli, after partly accepting, partly criticising Pighi's hypothesis (Luiselli 1967: 181–191), specified the development from the early times and tried to show through the alternation arsis and thesis and the development of more arses how Saturnian verse arrived to the condition as it was employed in Roman poetry. Nevertheless Luiselli continued to speak of rhythm and excluded the Hellenism: "Per noi quell'origine [greca del saturnio] dovrebbe intendersi non più che come un passaggio dal mondo greco al mondo latino dell'essenza ritmica stessa della versificazione greca (di tipo ionico), comunque quest'ultima si configurasse quanto a forma poetica. [...]. Ma la versificazione saturnia in sé e per sé, il suo impiego e il suo sviluppo dovettero essere un fenomeno di salda tradizione latina, cui non può non riferirsi [...] l'enniano *olim Fauni uatesque canebant*" (Luiselli 1967: 293–294). At this point it would be interesting to know, what was the structure of the ancient Indo-European language written with some refinement. Following Bruno Luiselli (1969: 123), I distinguished two kinds of such a language, in particular as for Latin: "nämlich daß es in der ältesten lateinischen Prosa zwei verschiedene Stufen des

5 In B. Luiselli (1967: 105–114), all ancient attestations of 'Saturnius'.

Ausdrucks gibt, eine niedrige und ärmliche der Gesetze und eine andere hohe und erhabene, die in den sakralen Texten vorkommt” (G. Calboli 1987b: 137). The ancient carmina belong to the second grade of this language as well as every work or part of work addressed to celebrate the king (history, mention of achievements of the king). In my paper just quoted you find a discussion of some of these instruments as the formula which occurs in the *carmina*, i.e., *quouis rei ergo ... harunce rerum ergo* (Cato, *agr.*141.2–3), as the alliteration, *pastores pequaqua salua seruassis duique duonam*⁶ *salute* (Cato.*agr.*141.3), as the parallelismus of *cola* (which occur already in Hittite, cf. Calboli 2021: 643–645). Now I want to give a brief example of a Hittite language in order to have among so many I.-E. roots reconstructed with perfect technique by de Melo, a bit of the oldest I.-E. language, the Hittite: Tab. Legum Hitt., II 35 (Friedrich 1959: 76–77; Held, Schmalstieg and Gertz 1988: 123) (46) *ták-ku*¹ A.ŠĀ² *an*² ZAG-*an*³ *ku-iš-ki*⁴ *pár-ši-ia*⁵ *I ag-ga-la-an*⁶ (47) *pí-en-na-a-i*⁷ EN⁸ A.ŠĀ⁹ A.ŠĀ¹⁰ *I gi-pí-eš-šar*¹¹ *kar-aš-zi*¹² (48) *ta*¹³-*az*¹⁴ *da-a-i*¹⁵ ZAG-*an*¹⁶-*na*¹⁷ *ku-iš*¹⁸ *pár-ši-ia*¹⁹ I UDU²⁰ 10 NINDA²¹ (49) I DUG²² KA²³-KAK²⁴ *pa-a-i*²⁵ *ta*²⁶ A.ŠĀ²⁷ EGIR-*pa*²⁸ *šu-up-pí-ia-aḫ-ḫi*²⁹. (46) Wenn¹ jemand⁴ die Grenze² eines Feldes³ zerbricht⁵, (indem) er 1 Furche⁶ (bis auf des Nachbars Feld ?) treibt⁷, (47) schneidet¹² der Besitzer⁸ des Feldes⁹ 1 Elle¹¹ Feld¹⁰ ab¹² (48) und¹³ nimmt¹⁵ (sie) für sich¹⁴. Und¹⁷ derjenige¹⁸ der¹⁸ die Grenze¹⁶ zerbricht¹⁹, gibt²⁵ 1 Schaf²⁰, 10 Brote²¹ (49) ein Gefäß²² von²³ Dünnbier²⁴ und²⁶ reinigt²⁹ das Feld²⁷ wieder²⁸. For an English word for word translation and the cuneiform characters, see Held, Schmalstieg and Gertz (1988). We can see how the parallelism of *cola* and the repetition of some words are strong, and the clef function of the ([relative]-indefinite) words *ku-iš-ki*⁴ ~ *ku-iš*¹⁹. Marcus Terentius Varro, der arme Geselle, der die Indo-Europäischen Wurzeln und die hethitische Sprache nicht kannte.

P. 946 Varro, *ling.* VII 39 *Apud N<a>euium: ‘atque prius pariet lucusta[m] lucam bouem’*. Here de Melo gives the following scansion: - uu - uu - | - u - u -, and adds that “the extant verses fall into two halves of roughly equal length”. This verse, a Saturnian, in my opinion, must be divided as suggested by Pigghi (1968): ‘*atque prius pariet lucusta | lucam bouem*’, with the subject *lucusta* in the first part of the Saturnian. Barchiesi (1962: 543f.) gives the verse as by C. O. Müller (p. 135):

atque || p̄rius pariet locusta
Lucam bovem

Barchiesi mentions also the division by Fraenkel in which *locusta* is connected with *Lucam bovem* to save the alliteration. However, I think that the alliteration could function as a connector between the two parts of the Saturnian.

⁶ *duonam* restituerunt P. Cugusi and M. T. Sblendorio Cugusi (2001: 228).

P. 979 Comm.: Varro, *ling.*VII 66 (I give firstly the text by de Melo, and then I correct the wrong punctuation by de Melo with the correct punctuation by G.-Sch.) de Melo: *Claudius scribit axitiosas demonstrari 'consuplicationes'. Ab agendo axitiosas: ut ab una faciendo factiosae, sic ab una agendo < axitiosae, ut > actiosae, dictae. // axitiosae ut add. Götz et Schöll (et axitiosae add. Müller post actiosae).* Goetz-Schoell: *Claudius scribit axitiosas demonstrari consuplicationes, ab agendo axitiosas. ut ab una faciendo factiosae, sic ab una agendo ac<si>tiosae dictae. // ab una agendo actiose dē F ac<si>tiose G.-Sch. actiose * et axitiosae * dictae Müller.* Here, de Melo did not quote Ernout's work (1949) on the adjectives in -ōsus. As concerns *āctiōsus*, Ernout (1949: 54) pointed out that Varro, *ling.*VII 66, quoted this adj., "En dehors de Cassiod, Var.11 pr.4 qui l'emploie comme synonyme de *negōtiōsus*, ne figure que dans Varron, L.L.7,66, qui le cite, et peut-être l'invente pour expliquer la formation de l'adj. *axitiōsus* (*asci-*) ...*ab agendo 'axitiōsus'*. *Vt ab una faciendo 'factiosus', sic ab una agendo 'actiosae' dictae*". Then, as for *factiōsus* cf. Ernout (1949: 8, 59, 78f., 104). However, by omitting Ernout's work, de Melo fails in two ways, firstly missing Ernout's hypothesis (correct or false) that Varro invented *āctiōsus* in order to explain *axitiōsus* (cf. also *Th.L.L.*I 444, 52–56 *āctiōsus* occurs two times, Varro, *ling.*7,66, and *Casiod.uar.*11 pr.4 *ne quis forsitan possit offendi, quod in praetoriano culmine constitutus sit omnimodis actioso* [i.e. *negotioso*] *pauca dictaverim* [Cassiodorus knew Varro]; II 1640, 43–54), secondly, because de Melo did not let the reader know that a general book such as that by Ernout exists on these formations in -ōsus.

Pp. 1047–1048 Comm.: Varro, *ling.*VIII 16 *Propter eorum qui dicunt usum declinati casus, uti is qui de altero diceret, distinguere posset, cum uocaret, cum daret, cum accusaret, sic alia eiusdem discrimina, quae nos et Graecos ad declinandum duxerunt. Sin[a]e controuersia sunt qui<n>qu[a]e. <Sunt qui sex putent> quis uocetur, ut <H>ercules, quemadmodum uocetur, ut <H>ercule, quo uocetur, ut ad <H>erculem, a quo uocetur, ut ab <H>ercule, cui uocetur, ut <H>erculi, cuius uocetur, ut <H>erculis.*

// *qui<n>qu[a]e* Laetus *qui que*, **F** [I give the real form of this word: *qque*, later we will see why we must give the actual form of this reading] *sunt qui sex putent* Dahlmann.

We must start with the palaeography, a science substantially neglected by de Melo, but necessary considering the status of *Mediceus* (**F**). The group of letters *q quę* could be read *quinque* as by Laetus and de Melo, but also *quidem*, provided that the letter *d* could be written in the MS father of **F** as in the Roman minuscule or semi-uncial (cf. G. Cencetti 1997: 63, 84) with horizontal, not vertical stroke (like that *ð* or *ƿ* or *ð*) which in a damaged MS, as certainly the model of *Mediceus* (**F**) was, could be read as a superposed abbreviation line. This kind of *d* (*ð*) is typical of the *Oncialis Script* but occurs also in the minuscule 'Romanesca' discovered and

highlighted by Paola Supino Martini (1974). In this script such a letter is called ‘*d oncialis*’ (P. Supino Martini 1974: 5–25).⁷ Of course, the use of such a script concerns also the transmission of the whole *De lingua Latina* and must be considered in the paleographic transmission of this text (Oncialis, minuscule of Lateran scriptorium and script of Farfa and some other abbeys of South Italy). As for an example of *d* with completely horizontal stroke, cf. Vat.Lat.1189 c.134r (1° col., 10–11 lin. *Judei igitur durum & In domitum cor habentes*).

Therefore, in the father of MS **F** the reading *quidem* could appear almost *qoue*, badly interpreted as *q que*, with the horizontal stroke firstly considered as an abbreviation of *m* (*quid ē*), in a second time and by another librarius as a general abbreviation (*qōē*). In this way in the passage from father of **F** to **F**, *quidem* (>*qoue*) became *q que*. This group of letters was read *quinque* by Pomponius Laetus who added an *n* after *qui*. The new reading had to be transcribed *quinque*. All this only from a paleographical point of view. Of course *quidem* could become *q que* in some other ways through a damaged MS as the father, antigraphus, of Mediceus (that the antigraphus of Mediceus had to be seriously damaged and almost destroyed an MS is the opinion of specialists of Cassinates MSS as G. Cavallo [1975: 397]). I wanted only to give an example of such a (reduced) corruption which concerned only a couple of letters. However I excluded *quinque* by Pomponius and substituted it with *quidem* for some other reasons, and *quidem* was accepted also by Burkahrt Cardauns (2001: 37) for some other reasons: the minimum change from *q que* and the coherence with the following text which in this way was made coherent with the number of sex cases without an addition ‘ad hoc’ as *sunt qui sex putent*. De Melo preferred Dahlmann’s reading, namely to add to the text transmitted by **F** the conjecture <*sunt qui sex putent (uelint)*>, because he was forced to adapt the series of sex cases, since the reading of Pomponius Laetus was *quinque* from *q que*. For *Sine controuersia sunt quinque* meets a complete contradiction with the following sex cases enounced. On the other hand, de Melo (*Commentary*, p. 1047) gave a completely incorrect report of my explanation and wrote: “Calboli writes not *quinque*, but *quidem*, and does not have Dahlmann’s addition. For Calboli, then, six cases are uncontroversial, but that means that there must be at least one that is controversial. For Calboli that is the seventh case; the sixth and seventh cases are ablatives, with and without prepositions. This is preposterous, not only because Varro never mentions a seventh case anywhere else, but especially because there is no reason for him to assume two formally identical only in presence or absence of a preposition. I therefore prefer Dahlmann’s view that five cases are uncontroversial and that a sixth one is disputed”. The confusion produced by de Melo is complete: Varro is giving the cases which are **all** uncontroversial (*Sine controuersia sunt*); that one must be controversial

7 On this subject, cf. A. Petrucci (1971).

is a product of de Melo's mind who thinks that I had to read *quinque*, a reading I never accepted (in my opinion *quinque* does not exist), and we must take into account all the cases quoted and exemplified by Varro; in this passage not one is controversial, because the only possible controversial case was the nominative (uncontroversial for the Stoics, controversial for the Peripatetics) and I excluded the nominative from my series. Then de Melo does not consider that here we have to do with the second ordering of the cases pointed out by W. Belardi (1974) with dative and genitive at the end. I did not however take into account the number of cases (five or six) produced by the wrong reading by Pomponius Laetus (*quinque* from *quiquae*), accepted without criticism by de Melo. I interpreted therefore the cases enumerated by Varro as vocative (*Hercules*), ablative without preposition (*Hercule*), accusative (*Herculem*), ablative with preposition (*ab Hercule*), dative (*Herculi*), and genitive (*Herculis*), and I wrote (Calboli 1987a: 144–145):

Intendere *quemadmodum uocetur*, ut *Hercule* come un vocativo non urta soltanto contro la forma *Hercule*, che, seppure meno frequente di *Hercules* fuori delle esclamazioni, è tuttavia attestata per il vocativo [Neue-Wagener, *Formenlehre* I³, pp. 447–449, *Hercule* come vocativo solo in *CIL* 3, 1563, contro numerosi esempi anche epigrafici di *Hercules* vocativo], ma soprattutto contro il *quemadmodum* che corrisponde piuttosto a uno strumentale del modo. La somma delle due difficoltà rende per me estremamente difficile riconoscere in *quemadmodum uocetur*, ut *Hercule* un vocativo anziché un ablativo, come è normalmente *Hercule* e come corrisponde a *quemadmodum*. Di fatto questo ablativo senza preposizione corrisponde al *septimus casus* di cui troviamo un esplicito riferimento in Quintiliano, *inst.*1,4,26 *quaeret etiam sitne apud Graecos uis quaedam sexti casus et apud nos quoque septimi. Nam cum dico 'hasta percussi' non utor ablatiui natura; nec si idem Graece dicam. datiui*. K. Barwick (*Remmius Palaemon*, 1922: 268) ha attribuito a Remmio Palemone l'invenzione di questo caso che poi nei grammatici successivi a Quintiliano ha assunto anche altri valori e impieghi oltre quello indicato da Quintiliano (cf. G. Calboli, *La linguistica moderna*, pp. 107–109). Questo *septimus casus* prende ovviamente il nome dall'ordinamento I, quello che si può considerare normale e canonico [while here in *ling.* VIII 16 the ordering II with dative and genitive at the end is employed, an ordering which Varro took into account here]. Abbiamo dunque a disposizione due casi per i nomi in ablativo, il *sextus casus* corrispondente all'ablativo con preposizione e il *septimus casus*, corrispondente all'ablativo senza preposizione. Questo certamente in Quintiliano. È difficile che a Varrone sia sfuggita la particolarità dei due impieghi dell'ablativo latino. Con ciò non intendo affermare che in Varrone abbiamo già il *septimus casus*. Fra l'altro non si può neppure escludere che per Varrone fosse *sextus casus* la forma *Hercule* e *septimus casus* la forma *ab Hercule*. È importante comunque che egli lo potesse riconoscere come caso [what actually happened], in ciò prevenendo Remmio Palemone (tenendo conto dell'ipotesi del Barwick) e certamente Quintiliano. Nel nostro passo di Varrone, *ling.* 8,16, avremmo così due casi, *Hercule* e *ab Hercule*, come due casi corrispondenti all'ablativo abbiamo poi in Quintiliano. Devo poi ricordare che la denominazione *ablatiuis* non ricorre in Varrone e si trova a partire da Plinio il Vecchio (Plin. *dub. serm.* apud Char. *gramm.* I,120,17sg., cf. G. Calboli 1975: 105). (Calboli 1987a: 144–145)

Anyhow, it is a pity that de Melo could not take into account for obvious reasons of time, Javier Uría's (2017) paper, whose conclusion stresses (Uría 2017: 249–250) the substantial effect or natural development of my explanation and solved the question of the seventh case which worries de Melo: "All in all, Calboli's proposal solves many problems with a minor textual intervention, whereas the traditional view had left many questions open despite having admitted major textual changes." And at the conclusion:

In the history of the seventh case, a distinction can be observed between the concept and the term. As for the former, an acute reading of Varro *ling.* 8, 16 by Calboli (see Section 4) makes it possible to trace the notion of a seventh case to the first century BC. However, the first attestation of the term is not found until Quintilian, who seems to deal with the issue as a current scholarly topic, so the suggestion to trace it back to Palemon is well founded. In fact, the coinage of the term *septimus casus* presumes the existence of the label *ablativus*, and, even it is not directly attested until Quintilian, it is usually acknowledged that it may have been used already by Pliny (*Char. gramm.*, p. 160, 15–17 and p. 170, 13–18) and even by Verrius Flaccus (Schreiner 1954: 63; drawing from Festus, p. 356, 1–4). (Uría 2017: 263)

More recently this passage (i.e., Varro, *ling.* VIII 16) has been discussed also by M. Callipo (2017: 385–387), who made very good observations and, as for this question and the order of the cases, accepted my solution, added some elements of confirmation and showed that Varro in *ling.* VIII 16 used the second order of the cases I proposed, an order namely which she rightly called "eterodosso" in comparison with the order of the cases employed by Dionysius Thrax and later imposed, while the second order was in use by Alexandrian grammarians and also by Apollonius until II saec. A.D. Now, what is my answer to de Melo? 'Sunt qui prudentius putent', in better Latin: 'sunt qui prudentius sentiant'.⁸

P. 965: Varro, *ling.* VIII 53. As for the adjective *cassabundus* from *cadere*, cf. *Th.L.L.* III 3, 516.31–37 [Goetz].

P. 1053, Varro, *ling.* VIII 26 *Omnis oratio cum debeat dirigi ad utilitatem, ad quam tum denique peruenit, si est aperta et brevis, quae petimus, quod obscurus et longus orator est odio; et ut cum efficiat aperta, ut intellegatur, brevis, ut cito intellegatur, et aperta <m> consuetudo, breuem temperantia loquentis, et utrumque fieri possit sine analogia, ni[c]hil ea opus est. Neque enim, utrum Herculi an Herculis clauam dici oporteat, si docet analogia, cum utrumque sit in consuetudine, non neglegendum [sunt], quod aequae sunt et brevis <a> et aperta.* De Melo wrote: "He [sc.

⁸ Hellfried Dahlmann (pers. comm. 1987) sent me a letter where he kindly appreciated my paper and defined as follows my solution: "Ich habe Ihren Aufsatz sogleich mit dem größten Interesse gelesen und mit hoher Bewunderung Ihrer Gelehrsamkeit und der scharfsinnigen Behandlung der schwierigen Frage der Überlieferung des Textes: Ihre Lösung des Problems scheint mir in jeder Hinsicht wohlüberlegt und durchhaus möglich und erwägenswert".

Varro] recognizes *uirtūtēs orationis* or ‘virtues of speech’. While later authors believe that there are two overarching qualities, usefulness and elegance, Varro only mentions the former here”. De Melo is right but does not distinguish that here Varro mentions the first (the σαφήνεια) and the last one (the βραχύτης) of the five *uirtutes orationis* (ἀρεταὶ τῆς λέξεως) constituted by Theophrastus, σαφήνεια, Ἑλληνισμός, πρέπον, κατασκευή, which were completed by the Stoics with the addition of the βραχύτης or συντομία, cf. G. Calboli (2020: 502f.), with the specific bibliography. I consider as very usable the collection of the doctrine used by 28 rhetoricians on this subject presented by D. Vottero (2004: 250f.).

De Melo quoted the paper by Thomas Baier (2001) and assumed from Baier the idea that Varro combined *utilitas* and *elegantia* and a kind of complementarity of *analogia* and *anomalía*. However, Baier himself tried to abandon the rhetoric and, by treating the *orator obscurus* (VIII 26), therefore *odiosus*, he wrote (Baier 2001: 11): “In questo egli [sc. Varro] poteva risalire ad una terminologia già predefinita, cioè ad una terminologia retorica, che egli però non impiegava in senso proprio. Detto in altro modo, l’appello a brevità e chiarezza non è da intendere nel senso delle categorie retoriche, ma sorge da una teoria sull’origine della cultura di conio epicureo”. Also Baier pointed out that Varro started from rhetorical terms, in this case from an *utilitas* which consisted of two members, of being *aperta* (certainly σαφής) and *brevis* (βραχύς). In the following §31 Varro adds the *elegantia*, and the *elegantia* too was a kind of *utilitas*, following Baier, and, as Baier presents it, she too was one of the five virtues of the Stoic grammarians, the πρέπον, a virtue not only Theophrastean but already Aristotelian (*rhet.* III 1408a 10f): “La *elegantia* non è in fondo veramente una qualità nuova, ma piuttosto una forma sublimata di *utilitas*. 37 [37 Lo stesso argomento è *cum grano salis* utilizzato in ling. 9,46] [...] Provoca scandalo tuttavia anche il modo di presentarsi troppo elegante. L’opportuno per l’Uomo, l’*honestum*, è la via di mezzo” (Baier 2001: 14–15), and this seems to me to be nothing but the πρέπον. On the other hand, I do not accept completely Baier’s hypothesis and I believe that the combination of rhetoric and grammar has been realized already by Aristotle in the third book of his ‘Rhetoric’, whereas Baier (2001: 7) ascribes it to the Stoics, an ancient opinion. Then Varro was not the first one who considered *elegantia* as a combination of two of Theophrastean virtues: we find the *elegantia* already in *Rhet. Her.* IV 12,17, as a combination of *Latinitas* and *explanatio* (σαφήνεια), cf. G. Calboli (2020: 670–674 and 678–681). As for the Epicurean linguistics, taken into account by Baier in the quoted passage, I must add that Baier (2001: 8) concretized the Epicurean school with Lucretius (V 1028–1029) and we know that Lucretius referred only to Epicurus and Metrodorus and Epicurus used only the σαφήνεια (cf. G. Calboli 2003: 192–194, 2007: 142–146). De Melo avoided all these problems. We meet another case of the major faults of de Melo, the superficiality and lack of interest in deeper questions. It

seems that he rather neglected the ancient rhetoric (or, perhaps, did not master it completely).

P. 1067: Varro, *ling.*VIII 46 *Haec singulatim triplicia esse debent quod <ad> sexum, multitudinem, casum*. De Melo wrote: “The form *singulātīm* ‘one by one, each’ is considerably rarer in Latin than its doublet *singillātīm*; Varro uses it only here in the *De lingua Latina*, and does not use the latter at all in this work”. J. Schaffner-Rimann, ignored by de Melo (1958: 65): “*Paulatim* mit der Bedeutung ‘allmählich’ und *singillatim* ‘einzeln’ kommen sehr häufig vor. *Singulatim* dagegen steht nur noch Varro, *ling.*8,46 und eventuell Nigid. ap. Non.176,6 (vgl. FUNAIOLI, *gramm. Rom. frg.* p.179) und dann wieder Plin. *nat.*18,72.”

P. 1069: Varro, *ling.*VIII 50 *Primum si esset analogia [ut] in infeineiteis articulis ut est quis, quem, quouis, sic diceretur qua[e], quam, quaius*.

De Melo changed the reading *quae* of **F** to *qua*, without any reason, only following the dangerous Pomponius Laetus. Then he accepted L. Spengel’s and Kent’s reading and excluded the first *ut* (in *ut in infeineiteis F*) and in Comm., p. 1069, wrote: “In our first set I have followed Laetus in changing transmitted *quae* to *qua*; this makes the series indefinite rather than relative”. However, this change was not necessary, because also *quae* is a pron. indefinite as attested by Neue and Wagener (1892: 441–445): “Oft aber wird das Fem. des Pronom. indefin. in *qua* verkürzt. [...] Jedoch nicht selten auch *quae*.” A whole page and a half of examples of *quae* follow, from *CIL*, Plautus, Terence, *Lucr.*5,1209; Cicero, *Hor.serm.*2,6,10; cf. also Sommer (1948: 436). On the other hand, it is possible that de Melo adopted the nominative *qua* as in the following example: *ut est quis quoi* (dat.), *sic diceretur qua quai* // *quai* Lommatzsch *quae F* // (dat. non existing as Varro says against analogy). *nam est proportione simile: ut deae bonae quae, sic // sic L. Spengel sit F // dea bona qua[e] // qua* Scioppius *quae F* // *est; et ut est quem, quis, sic quos, ques. Quare quod nunc dicitur qui homines, dici oportuit ques*. De Melo (p. 1070), after quoting Kent’s explanation, namely that the three forms (*bonae deae quae*) are datives, concludes: “I find this unlikely [...]. A different interpretation is required”. This is a kind of crux and perhaps is the only possible conclusion. Nevertheless, de Melo gives a solution which I appreciate and could be confirmed by the following relation singular~plural (*quem, quis ~ quos, ques*): “Let us begin with the question of whether we are dealing with an indefinite pronoun or a relative form. As we know from 8.45, Varro does not argue for analogy between nouns and pronouns. This means that our third set, *deae bonae quae* and *dea bona qua*, is best interpreted as two coherent phrases without change of case. Consequently, *quae* must be relative. But what is its case? If the odd form is the last word, *qua*, then *quae* must be normal. This entails that *deae bonae quae* is nominative plural, ‘good goddesses who’: This gives us the correct result, a second phrase in the nominative singular, *dea bona qua*, with an analogically formed, but non-existent, nominative

of the relative pronoun". The only difficulty in this explanation is that *bona dea* appears usually in singular (*Th.L.L.* II 2070,66–2071,44; *Bona Dea* seems to have been the name of Fausta, cf. G. Dumézil (1966: 344): "elle reçoit en décembre un culte d'État"). Therefore, the plural *bonae deae* seems to be strange.

Anyhow, I ask myself whether the text transmitted by **F** is absolutely impossible: tentatively I would consider *quae* as a relative pronoun and change the punctuation: *ut deae bonae quae sit dea bona quae est*. Here Varro is explaining the previous example of the dative, and he says: 'as for the *deae bonae* (dat.) which would be that what is the *dea bona* (nom.), in the same proportion'. With other words *quae* would be only a relative pronoun, not in question, but only introducing a relative clause in both cases (first with *sit*, then with *est*). I would like to enter into Varro's text also in following chapter 51, where the librari of the Middle Ages worked to improve the text as they knew and were followed by the modern scholars, from the famous Laetus (famous in Ciceronian sense: *pater me uetuit ad famosas accedere*, de orat.2,277), so that we can say 'Goodbye!' to Varro.

P. 1074: Varro, *ling.* VIII 54 *Et cum debuerit esse ut a cubatione cubiculum, sic a sessione sediculum, non est*. De Melo explains with his usual superficiality: "Unlike the English, the Romans simply had not need for 'a sitting room' to relax in, because meals were taken lying down". Also in this case, de Melo is too hurried and not completely wrong but only partially right. However, this is true for a minority of Romans (about 0.2%), while they were sitting down on a seat or a bench, as a large majority, and we must distinguish also the time: Hermann Dessau (P.-W III,2, 1895,60–1896,5, 1899 (=1958)) tells us that at the beginning the Romans took the meals sitting at table, later the men were lying down, while the women, the children, and people of second rank continued to be sitting on *subsellia*: "Man speiste in alter Zeit sitzend. Varro bei Serv.*Aen.* VII 176 und bei Isid.*or.* XX 11,9. Serv.*Aen.* I 79. 214. 708. Die Vorstellung hat auch Verg.*Aen.* VII 176. VIII 176. Später die Männer auf dem Lectus liegend, die Frauen sitzend (Val.Max.II 1,2, Isid.a.O.), bis endlich, noch vor Ende der Republik, das Liegen (*accubare*) allgemein üblich wurde. Doch pflegten auch damals noch, und auch in der kaiserlichen Familie die Kinder sitzend an einem besonderen Tische zu speisen. Act.Ar.v. 27.Mai 218. Suet.*Aug.* 64; *Claud.* 32. Tac.*ann.* XII 16. Auch Parasiten und sonstige untergeordnete Personen setzte man wohl auf *subsellia*, Suet.*vita Ter.* p.28 Reiff. Plaut.*Capt.* 471; *Stich.* 489". On the other hand, if Varro noted the oddity of **sediculum*, it means that he knew the *subsellia*.

Pp. 1075–1085: Some time, the enthusiasm in the explanation and discussion brings de Melo beyond Varro. But this is a *felix culpa*, because in this way de Melo enlarges our knowledge of the matter treated by Varro and we must be very grateful to him.

P. 1080: About the 'impersonal passive' de Melo treats the impersonal passive of the composed forms of the verb as secondary forms in comparison with the

impersonal passive of the present: “*curritur* ‘there is running’. Such an impersonal passive can occur in the perfect as well, in which case a perfect passive participle is involved, as in *cursum est* ‘there was running’. Thus, the participle does exist, but not in the personal usage demanded by Varro. The absence of personal passives is of course, semantically motivated. If there is not patient that can be promoted to subject status, there can be no personal passive”. Here, de Melo ignores that the impersonal passive of the composed forms is the only one that occurs in ancient Latin where personal constructions are prevailing over an impersonal, a situation probably influenced by the adjectival original use of the suffix *-to* (cf. K. Brugmann 1895; and in particular on personal and impersonal, G. Calboli 1962: 95 and 107f.).

P. 1083: Here de Melo’s enthusiasm excluded any discussion of the original meaning of *parricida*. The explanation by de Melo ‘killer of a kinsman’ is a good explanation (already suggested by Devoto and Baer [see below], but he could also be a ‘killer of a man of the same social condition, of a *par*, not a slave’, and this explanation continues to persuade me as the best one, cf. G. Devoto [1940: 78] “*parricidas esto* [...]: cioè sia equiparato all’uccisore di un « pari », di un membro dello stesso aggregato sociale, insomma della stessa gente”; examples *Th.L.L.* X,1 439 and 440 [Baer]: “significatur homo, qui scelere impio, nefario contaminatus est, eo quod hominem occidit (cum constet usu auctorum indicari fere interfectorem hominum propinquorum, cognatorum”, examples 441,4–443,33). A very good discussion has been given by Walde and Hofmann (1965: II 253f.) and Ernout and Meillet (1960: 483). In my opinion, the social position of the victim could not be completely ignored as for the early times, though Cicero excludes it, but in his time, *Cic.Mil.17 Intersit inter uitae dignitatem summorum atque infimorum; mors quidem inlata per scelus isdem et poenis tenetur et legibus. Nisi forte magis erit parricida, si qui consularem patrem quam si qui humilem necarit*. Cicero in this passage is ironical, however, in Laws of Hittites (First Tabl.) a clear and precise difference is given if a freeman/free woman or a slave has been killed (cf. J. Friedrich 1959: 17 and 89f.): the penalty is much heavier in the case of a freeman than of a slave. On *parricidas* and the archaic *paricidas*, see now also M. Mancini (2017).

P. 1084: On the nature of Latin ablative, de Melo is correct writing that “Latin ablative continues three Indo-European cases, the ablative proper, the locative, and the instrumental”.

P. 1087: De Melo touched very briefly on the translation of Latin expression into Greek by Latin speakers which was realized translating the Latin abl., preceded by *ab* or *ex* into Greek datives in *-α* or *-φ*. However, I demonstrated, as I said previously, ad p. 203, that no one Ciceronian example exists, but only one by M. Antonius, though Adams (2003), quoted less accurately four to five examples.

P. 1090: Varro, *ling.VIII* 68–69 *Sic item quoniam simile est recto casu ‘surus, lupus, lepus,’ rogant quor non dicatur pro portione[m] item ‘suro, lupo, lepo.’ Sin*

respondeatur similia non esse, quod ea uocemus dissimiliter ‘sure, lupe, lepus’ (sic enim respondere uoluit Aristarc<h>us Crateti; nam cum scripsisset similia esse Philomedes, Heraclides, Melicertes, dixit non esse similia, in uocando enim cum <E> breui dici Philomede<s>, cum E longo Heraclide, cum <A> breui Melicerta), in hoc dicunt Aristarc<h>um non intellexisse quod qu<a>ere<re>tur se non soluere [t]. This is not the only passage where this debate has been mentioned (see also Varro, *ling.* VIII 41–43; IX 43; 91–93).

Here, Varro mentions the debate held between Crates Mallotes and Aristarchus, presented and discussed by H. Dahlmann (1966: 116f.), a debate which was mentioned in other passages by Varro (VIII 63; IX 43; 91–93). I dedicated a paper to this question (Calboli 2001: 36–48) and realized that this discussion was very sharp and refined. De Melo, on the contrary, missed the central point, the core, of this polemic which is grounded on the difference between nominative, vocative and remaining cases (as partially showed by Belardi and Cipriano 1990: 123–134). Crates asked why the three quoted names, Philomedes, Heraclides, Melicertes, have an identical form in nominative, but a different in oblique cases. Aristarchus answered that the three names are not the same, because they are different in vocative. Why did Aristarchus in answering to Crates used the vocative? Obviously vocative was not an oblique case like genitive, dative, accusative, but a special case; in opposition to the three cases, the vocative was the first in the series (Scholia Londinensia, *Gr. Gr.* I 1, p. 548, 14–16 Uhlig Περὶ δὲ τῆς τάξεως ἔστι τοῦτο εἰπεῖν, ὅτι τινὲς ἐνόμισαν τὴν κλητικὴν πρώτην, ἐπεὶ δευτέρου ἐστὶ προσώπου, αἱ δὲ ἄλλαι τρίτου, τὰ δὲ δευτέρα προτερεύουσι τῶν τρίτων, οὐκοῦν προτερεύει ἡ κλητικὴ. “Riguardo all’ordine si può dire questo, che alcuni hanno giudicato primo il vocativo, poichè è della seconda persona, mentre gli altri casi della terza. I secondi vengono prima dei terzi. Quindi il caso vocativo viene prima”, G. Calboli 2001: 46). And also by Varro, *ling.* VIII 16 *cum uocaret, cum daret, cum accusaret*, the vocative is mentioned as first.

My conclusion (Calboli 2001: 45f.) is that the nominative can be a πῶσις as well as a κῆσις, while gen., dat., acc. (and abl. in Latin) are πῶσεις, but never will be κῆσεις. The same can be said for the vocative: it is not enough that *surus, lupus, lepus* or the Greek terms, *Philomedes, Heraclides, Melicertes*, are the same in nominative, they must be the same also in vocative. Only in this case we could say that they are the same and the difference in the flexion (in oblique cases, gen, dat., acc., abl.) should be proof that the analogy is not apt. However, they are not the same in vocative – as Aristarchus observed – and this means that Crates’ argument is not correct. De Melo quoted, albeit with a mistake, my paper (Calboli 2001) in ‘References’, p. 1276, and after reading it he wrote, p. 1090: “Since the Latin examples are all about a difference in declension class, the argument is less interesting to modern scholar”. On the contrary it should be interesting to a modern

scholar the series of Indo-European roots with which Varro had nothing to do, added by de Melo. I do not understand his conclusion, p. 1091: “This is, of course, a misrepresentation of what Aristarchus and his followers were doing. No one denies the outward similarity of *lupus* ‘wolf’ and *lepus* ‘hare’. The oblique cases are not adduced in order to judge outward shape; rather, they help us to determine declension classes, which are internal differences that cannot be spotted by looking at any isolated case form”. Anyhow, the debate between Crates~Aristarchus seems to have actually been held, and we must explain it, not avoid every discussion as de Melo does.

P. 1090: Varro, *ling.*VIII 68–69 again about the debate between Crates and Aristarchus but not even here does de Melo realize that the key point is the fact that we have to do with three kinds of cases, nominative, vocative and oblique cases, gen., dat., acc., as pointed out by Belardi and Cipriano (1990: 154), and partially accepted by myself with some integrations (Calboli 2001: 45). Correct is however de Melo’s mention that *Melicertēs* is an adaptation “of the Phoenician name *Melqart* which is not pertinent to the discussed question, but is an interesting enrichment of our knowledges”.

P. 1094: Varro, *ling.*VII 72 *Est enim ut hi qui his, quis a<u>t sicut quibus hibus*. Also here de Melo missed giving reference to Neue-Wagener, I³ 39; II³ 419, to be integrated with reference to *ibus*, which was different only in the graphic form from *hibus*, II³ 386f: *hibus* occurs in Plautus’ *Curc.*506, as de Melo also reminds us. The quotation by Charisius (*gramm.* 54.19; 68,7 Barwick) and by Priscianus, *gramm.*III 10–15, is more significant, in particular, Prisciani attestation: *His, quanuis et hibus pro his antiqui protulerunt*. Cf. *Th.L.L.*VI,3 2701,37–39 [Ehlers]. The form *ibus* of *is*, does not occur in Inscriptions (or rather, it has not been detected thus far). However, it is attested by Nonius, p.486.11 as employed by Plaut.*Mil.*74; Don. *Ter.-Eun.*432; Titin.59 and Pomponius, *Atell.* 104, and in a Senate’s decision in year 655 of Rome, given by Gell.4.6.2 who quoted the *Senatusconsultum*: *uti M.Antonius Consul hostiis maioribus, Ioui et Marti procuraret et ceteris dis, quibus uideretur, lactentibus. <Ibus> uti procurasset, satis habendum censuerunt //* *<Ibus>* add. Scioppius. Cf. *Th.L.L.*VII,2 459,28–36 [Buchwald].

P. 1094f.: Varro, *ling.*VIII 73 *Cum dicatur da patri familiai*. The form *familiai*, transmitted by F, has been defended with good arguments by de Melo. While G.Sch. and Kent read *familias* with the strong argument, quoted by G.-Sch.: “*i pro s ut saepius*”, Varro himself added as a conclusion of the paragraph: *Item plures patres familias, sed ut Sisenna scribit, patres familiarum*. As for Sisenna, we know that he was a fanatic user of the analogy, that in Senate said *adsentio* instead of *assentior*, obviously in analogy with the simple *sentio* as it has been attested by Gell.2.25.9 ‘*Sentior*’ inquit, nemo dicit et id per se nihil est, ‘*adsentior*’ tamen omnes fere dicunt. *Sisenna unus ‘adsentio’ in senatu dicebat et eum postea multi secuti,*

neque tamen uincere consuetudinem potuerunt. Then Sisenna did not hesitate to coin a new word, ‘sputatilica’, i.e. unworthy crimes: Cic. *Brut.* 260 *Sisenna autem quasi emendator sermonis usitati cum esse uellet, ne a C. Rusio quidem accusatore deterri potuit, quominus inusitatis uerbis uteretur. Quidnam hic est? inquit Brutus; aut quis est iste C. Rusius? Et ille: fuit accusator, inquit, uetus, quo accusante C. Hirtilium Sisenna defendens dixit quaedam eius «sputatilica» esse crimina. Tum C. Rusius: circumuenior, inquit, iudices, nisi subuenitis; Sisenna quid dicat nescio, metuo insidias. «sputatilica», quid est hoc? «sputa» quid sit scio, «tilica» nescio: maximi risus.* I dedicated a paper to this episode (Calboli 1990) and pointed out the analogy with some other adjectives in *-icus* as *barbaricus*, *ciuicus*, *famelicus*, *modicus* (cf. M. Leumann 1977: 336f.; W. D. Lebek 1970: 58). On the other hand, the combination with *familia* produced other expressions where the assimilation was highlighted by Sisenna and other authors as the Auctor ad C. Herennium. For they not only adapted *pater* to *familia* in number, producing *patres familiarum* (mentioned also by de Melo, p. 1095), but employed another kind of assimilation reminded by Seruius (Comm. to Verg. *Aen.* 11. 801 *multi uolunt in numero plurali nomen utrumque declinari, ut dicamus ‘hi patres familiae’, ‘horum patrum familiarum*, to which *matribus familiis* must be added). In *Rhet. Her.* IV 8. 12, I accepted the reading of the most ancient manuscripts *matribus familiis* (the Recentiores and some Integri had *familias* instead of *familiis*; Cic. *S. Rosc.* 48 *a patribus familiis // codd //* and *Verr.* 4. 183 *patribus familiis // codd uno O excepto //*). I discussed these examples and pointed out the probable reasons why the Auctor ad Herennium and Cicero used these forms (analogical assimilation, novelty, outstripping the traditional nobility’s language by both the Auctor and Cicero, cf. G. Calboli 1962: 182–192, 2020: 294): “Quamquam me non effugit, hanc esse difficilem lectionem, quae nisi bonis codicibus tradita esset, retinenda non esset”. For his part, Varro avoided going so far, and for some reason did not go beyond Sisenna’s *patres familiārum*.

P. 1095f.: Varro, *ling.* VIII 74. De Melo wrote: “*Iūpiter* as the name of a deity very rarely has a plural, and only in reference to statues or the like, where *duo Iouēs* can mean ‘two statues of Jupiter’ [...] the plural is already inherent in *signa*”. This is not always right, because it is incomplete. Even in Roman Capitol they were *duo Iouēs* ‘two Jupiters’, *Iupiter Optimus Maximus* and *Iupiter Tonans*, with a temple for each one, namely with two distinct temples, the ancient temple traditionally built by Romulus and the temple built by Augustus to *Iūpiter Tonans*, at the entrance of Capitol (see G. Calboli 1992: 226–236).

Pp. 1096–1106: De Melo treated well the paragraphs 75–84, namely the conclusion of book VIII. At p. 1097, he is right in underlining at the beginning of §75 *Deinceps dicam de altero genere uocabulorum* the bold or odd use of *alter* outside of an opposition within a pair. It seems that a kind of pair has been presupposed by

Varro based on *deinceps* in the sense of Paul. Ex Fest. p.71,2 Müll. *Deinceps qui deinde cepit, ut princeps qui primum cepit*. Cf. *Th.L.L.V*,1 404,19–21: a passage of Festus is quoted where a kind of opposition was introduced between *deinceps* and *princeps*: Fest.p.71 *deinceps qui deinde coepit, ut princeps qui primum coepit*. Nevertheless, de Melo is absolutely right: Varro operated with his usual haste in §75 and reduced attention. In the following §76 *In aliis uerbis* it seems to have perhaps reintroduced the distinction *alter ~ alius*. Anyhow, *alius* here is correctly employed.

P. 1101: Varro, *ling.* IX 1: Here we must quote Varro's passage and recall that this debate between Crates and Aristarchus has been mentioned by Varro in some other points of *De lingua Latina*, VIII 68; IX 43; 91–93, as stressed above. For here also Chrysippus appears and he appears as Cleantis mentor and first Aristarchus' adversary: *<Insignis eorum est error qui malunt quae> nesciunt docere quam discere quae ignorant. In quo fuit Crates, nobilis grammaticus, qui fretus Chrysippo, homine acutissimo qui reliquit Περί ἀνωμαλίας IV libros, contra analogian atque Aristarchum est nixus, sed ita, ut scripta indicant eius, ut neutrius uideatur peruidisse uoluntatem, quod et Chrysippus, de inaequalitate cum scribit sermonis, propositum habet ostendere similes res dissimilibus uerbis et dissimili[bu]s similibus esse uocabulis notatas, id quod est uer[bu]m et quod Aristarchus, de aequabilitate cum scribit et de uerborum <similitudine>, similitudinem qua[ru]ndam inclinationes sequi iubet, quoad patitur similitudo*. It seems that Aristarchus and the Alexandrian grammarians assumed the principle of the analogy as usual principle of the language, Chrysippus however invalidated this principle with some exceptions and was followed by his pupil Crates who insisted. However, Aristarchus answered to Crates' arguments (Chrysippus probably already died) and demonstrated that the similar names, *Philomēdes Heraclides Melicertes*, of whom the oblique cases were different, were apparently similar because they were different in vocative, the paradigmatic case (nominative was not a case in Peripatetic and Alexandrian opinion), *Philomēdes, Heraclidē, Melicerta*. Of course, this is connected with the doctrine Varro employed in VIII 16. It has been disdainfully dismissed by de Melo, who refused to enter into this question at least to reduce his incompetence in this sector. Aristarchus wrote a work *Περί ἀναλογίας* (*de aequabilitate*, *Th.L.L.* VII 1, 70–71), Chrysippus *Περί ἀνωμαλίας* (cf. also von Arnim SVF 1978: II 151, p. 45, which is incomplete; and M. Pohlenz 1955: 452). It seems that Aristarchus originally observed that in language the analogy acts, and this was a kind of ascertainment, Chrysippus and in particular Crates insisted on the use. Nevertheless both held a debate and de Melo, following J. Fink (1952: 378), reduced erroneously all to a misunderstanding: "Chrysippus merely notes the difference between the outward shape of a word and its meaning, as is the Latin male name *Perpenna*, which is masculine despite belonging to the overwhelmingly feminine first

declension [it is sure that Chrysippus did not use a Latin example]. Aristarchus is concerned with inflectional analogy regardless of meaning. And it is Crates who fails to realize that the two positions are not contradictory". Crates and Aristarchus actually spoke of different aspects of the language, because they gave different values to different peculiarities of language. However, substantial differences between Alexandrian (Peripatetic) and Pergamenian (Stoic) grammarians are undeniable. It is possible that Varro enlarged the importance of the differences, as already J. Collart (1954: 136–144) stressed. Detlev Fehling (1956, 1957, 1958) supposed that even the whole polemic between analogists (Aristarchus and the Alexandrian grammarians) and anomalists (Crates and the Stoic grammarians) was invented by Varro as a consequence of his discussing *in utramque partem*. I refused Fehling's exaggeration (Calboli 1962: 176–182) and, following Collart and now A. Garcea (2012: 83–109), accepted only the idea that Varro increased a polemic which nevertheless actually took place.

Pp. 1112f.: Varro, *ling.* 9. 12. De Melo discusses the comparison of grammar and painting. Such a connection was very expanded in the Greek and Roman world, so expanded that, considering the time from Aristotle to Cicero, Jean Cousin (1936: I, 658) wrote: "tous ces qui ont écrit sur l'art oratoire ont uni l'étude des beaux-arts à celle de l'éloquence"; Horace for his part found in *Ars Poetica*, *Epist.* 2.3.361 a perfect synthesis of this connection with the expression *ut pictura poesis* (preceded by *Rhet. Her.* IV 29.39, which refers to Simonides' maxim: *poema loquens pictura*, < *pictura* > *taciturnum poema debet esse*; cf. Calboli 2020: 645f., 781; on Horace's maxim, cf. Ch. O. Brink 1982: 368–372). Here too de Melo's discussion is rather lacking.

P. 1124, Varro, *ling.* IX 31 *At non uides, ut Graeci habeant eam* [sc. *orationem* 'speech'] *quadripertitam, unam in qua si<n>t casus*. De Melo's quotations of Transformational (Generative) Grammar are very rare, but here we meet such a quotation: "*Quadripertitam* allows us to interpret *ūnam* as *ūnam partem*: this type of construction should not exist according to Generative Grammar", with reference to Ross, a PhD thesis: as I know, in Latin *unam partem* exists in the deep structure and arrived to the surface structure through a rule of deletion, namely of *partem*, in the TG. That is what actually happened, and the existence of *partem* in deep structure allows us to understand immediately and without problem in the surface structure *ūnam* as *ūnam partem*.

P. 1124: Usually de Melo's grammar is elementary but correct. However, I was astonished as I found this text: "In Latin, each of these tenses [sc. past, present, future] can be formed for the *infectum* stem (imperfect, present, simple future) and for the *perfectum* stem (pluperfect, perfect, future perfect) [...] A full symmetrical system like this is rare; it is not shared by Greek either". It is rare but exists in Hittite (very simplified). In Greek and Sanskrit we meet also the aorist, but this is a past tense without any duration, opposed to the perfect lacking immediacy.

P. 1128: Varro, *ling.* IX 36. De Melo is absolutely right in mentioning that “Originally the Latin adpositions were postpositions rather than prepositions, as can be seen from fossilized phrases like *mēcum* or *quāpropter*”. This is true but here the Hittite has to be recalled where *nu*, *sa*, *a* corresponding to Latin *et* are all postposed and mostly enclitic, and Hittite is the oldest IE language.

P. 1129: Varro, *ling.* IX 37. The future imperative (e.g. *animaduertito* in comparison with *animaduerte*) is an explanation the Roman grammarians gave (Diomedes, see *infra*). However, I remember that R. T. Lakoff (1968: 174f.) asserted that the imperative future is not a future imperative but a perfect. I would rather think that we are dealing with a kind of aorist and I could quote an example where Propertius translated with *uocato* the Greek βόασον of Leonidas Tarentinus, a Greek imp. aorist with a future imp.: Leon.Tar. *Anth.Pal.* 9,337f. κάμὲ τὸν ὕληωρὸν ἀπὸ κρηνοῖο βόασον/Πᾶνα συναγρεύω καὶ κυσὶ καὶ καλάμοις – Prop.3.13.43f. *et me Pana tibi comitem de rupe uocato, siue petes calamo praemia siue cane*. On the other hand, already the grammarian Diomedes defined this fut.imp. an almost futur: Diom. Gramm. I 339.13–16 *futurum uero tempus* [sc. *imperatiui modi*] *differt a ceteris futuris, quia non ut confestim fiat imperamus, sed in futurum fieri [...] iure ergo diceretur quasi futuri* (cf. L. Löfstedt 1966: 22ff.). The desinence -to is lacking in every reference to future (cf. also G. Calboli 1966–1968: I 233).

P. 1145: Varro, *ling.* IX 56. I give now de Melo’s reading, which is the best in comparison with G.-Sch. and Kent but itself not completely correct, and after this I will give the correct reading following **F**: *Ad h<a>ec dicimus, omnis orationis quamuis res natura<e> // natura<e> uulgo // subsit, tamen si ea in usu<m> non peruenerit, eo non peruenire uerba. Ideo equus dicitur et equa. In usu enim horum discrimina // discrimina Augustinus discrimine **F** // Coruus et corua non, quod sine usu id quod dissimilis natura<e> .* I would change in a couple of points and after that I will explain why. *Ad h<a>ec dicimus omnis orationis quamuis res natura subsit, tamen si ea in usu<m> non peruenerit, eo non peruenire uerba. Ideo ‘equus’ dicitur et ‘equa’. In usu enim horum discrimina. ‘Coruus’ et ‘corua’ non, quod sine usu id quod dissimilis natura.* I would like to translate in my language, i.e., in Italian: ‘A questo riguardo diciamo che, benché per natura un oggetto reale di ogni espressione linguistica si trovi alla sua base, tuttavia, se essa [sc. l’espressione linguistica] non giunge all’uso, le parole non la raggiungono. Perciò [si dice] *equus* ed *equa*. Infatti, le loro differenze sono tuttavia in uso. *Coruus* e *corua* no, perché è senza uso quello che è una dissimile natura’. In this passage, we find two oppositions, *natura* and *usus*, *res* and *uerba*. Varro adapted all to the concept that the similarity by nature is acting only if the *usus* also permits an expression to survive, which arrives with *equus*, *equa*, similar by nature and frequently employed, not yet with *coruus*, *corua*, of which only *coruus* is confirmed by employment. I tried to adapt the text of **F** to this criterion, by reducing ad minimum the change of the manuscript.

Nevertheless, I acknowledge that Varro is difficult and not clear at all. However, it seems to me that the passage in the first part is much clearer in my reading (*dicimus omnis orationis quamuis res natura subsit, tamen si ea in usu<m> non peruenerit, eo non peruenire uerba*) than in the imposed text (*uulgo*) in which only de Melo is exact in reading of *natura*. Anyhow the two correspondent *equus*, *equa* and *coruus*, *corua* must be different between each other by nature: *equus*, *equa* (*discrimina*, but despite this both employed) *coruus*, *corua* (*dissimilis natura*, and only *coruus* employed). On the other hand, I recognize that, as for Latin, *id quod dissimilis naturae* is better than *id quod dissimilis natura*, an odd predicate noun, but how elegant is Varro's Latin?

On the other hand, it is strange that two generic words such as *res* and *materia* are employed without difference. De Melo refers correctly, albeit with some simplification, to Sophie Roesch's opinion: "Roesch (1999: 68–9 believes that *rēs* is distinct from *māteria*; the former stands for the *signifié*, the latter for the grammatical substance (case, gender, and so on). She adduces this chapter as an argument in favour of her idea. I believe that the two terms are synonymous and that both indicate grammatical substance". That is not a solution, because de Melo does not explain what synonymous means. I have read again Roesch's article and I noticed that the meaning of *materia* is clear and stable, whereas the meaning and use of *res* is rather uncertain, and we must read Roesch's (1999: 80) conclusion: "Mais pourquoi Varron, qui ne mélange pas la *uox* et la *res*, ne cherche-t-il pas à différencier les multiples sens de *res* ? Nous pensons que cela s'explique par le fait que ce qui compte pour lui n'est pas comment le mot se rattache à la chose (cela est en effet un acquis: un onomathète a attribué les mots aux choses; après cela, peu importe comment un mot peut avoir un référent, ou un sens), mais pourquoi tel signifiant désigne tel objet, et ce que cela nous apprend sur l'objet. Il s'intéresse au rapport qui unit le mot et le référent, et ne s'interroge pas sur la question de savoir comment fonctionne cette relation". I rather agree with Roesch and think that de Melo was too quick in simplifying this question: he did not enter here into the core of the problem.

P. 1163: Varro, *ling. IX* 79. Firstly, I give de Melo's text, then the text we read in **F**, in order to show how much the text transmitted by **F** has been improved or, better, changed for better or worse, I do not want to decide now. I begin by giving de Melo's text with some words stressed through *Italic* (which is a mistake, because one cannot distinguish between de Melo's reading and the text of the MSS): *Item reprehendunt, quod dicatur haec strues, hic Hercules, hic homo; debuisse enim dici, si esset analogia, hic Hercul, haec strus, hic hom<en>. Iniuria, nam n>on haec ostendunt [noua] non analogian esse, sed obliquos hic Hercul, haec strus, hic hom<en>. Iniuria, nam n>on haec ostendunt [noua] non analogian esse, sed obliquos casus non habere caput ex sua analogia. Non, ut si in Alexandri statua imposueris*

caput Philippi, membra conueniant ad rationem, sic et [Alexandri] <Philippi> membrorum simulacrum <seruet analogias>, <licet> caput quod respondeat item <non> sit? // Hercules p Laetus, Herculis F hom<en iniuria nam n>on Mette, hom<en n>on Götz et Schöll noua del. uolgo nomina Kent sic G H Augustinus, sit F. Now the text given by F: Item reprehendunt, quod dicatur 'haec strues, hic Hercules, hic homo'; debuisset enim dici, si esset analogia, 'hic Hercul, haec strus, hic hom<i>n'. Haec ostendunt noua non analogian esse, sed obliquos casus non habere caput ex sua analogia. Non, ut si in Alexandri statua imposueris caput Philippi, membra conueniant ad rationem, sic et ad Alexandri membrorum simulacrum caput quod respondeat item sit?

In this case, too, Varro is particularly complicated or the text is irremediably corrupted. However, it seems to me that a sense in F's text is possible (I use also in this case my language, Italian: "Parimenti criticano che si dica 'haec strues, hic Hercules, hic homo'; si sarebbe dovuto dire, infatti, se ci fosse l'analogia 'hic Hercul, haec strus, hic hom<i>n. Questi nuovi esempi mostrano non che esiste l'analogia, ma (solo) che i casi obliqui non hanno un capo che derivi dalla loro propria analogia. Non può darsi che, come se tu ponessi su una statua di Alessandro, una testa di Filippo, le membra si adatterebbero alla stessa condizione, così allo stesso modo ci dovrebbe essere un qualche capo che corrispondesse alla raffigurazione plastica delle membra di Alessandro?" In other words Varro excludes or tries to exclude that through the relationship between nominative and oblique cases (*caput* and *membra*) one can demonstrate that analogy does not exist, because in every combination of two members, we have always to do with an invariable element *hic*, *haec*, *hic*, and a variable one and the variable can or cannot combine with the invariable, as Philipp's head (namely the head of Alexander's father, Philippus) can combine with Alexander's body. If a head cannot combine, this does not exclude that some other head combines. This proof is a little stiff, but the transmitted text has been saved from a too strong reconstruction as de Melo did and also de Melo's proof, by changing Philipp's head with Philipp's body, is arbitrary and not acceptable at all.

P. 1163: End, Varro, *ling.* IX 78. De Melo in Comm. wrote: "Instead of *aliquam aliam partem* found here, Varro mostly uses *aliam quam partem*, with a clitic indefinite pronoun. *Aliquis* after a negation is rare, it indicates that Varro insists that some part is missing". Both of de Melo's assertions seem to be true, but only the second one is really true, namely that *aliquis* in a negative position is rare and shows a particular attention by Varro in pointing out the negation. The first assertion, that Varro uses *aliquam aliam partem* and *aliam quam partem*, does not highlight that they are not synonymous. The pronoun *quam* is an indefinite clitic pronoun (the enclitic position of indefinite-relative pronoun is sometimes employed in Hittite, cf. W. H. Held Jr. 1957: 15–22; 33–35). *Aliquis* and *quis* are both

indefinite pronouns. However, they are consistently different: *aliquis* means that somebody exists but we do not know who he is, while indefinite *quis* is employed, if we do not know not only who somebody is, but even whether he exists or not (cf. P. Ferrarino 1941–1942: 110–114 and B. Bortolussi 2015: 53–58; 77–81). This is the reason why *aliquis* occurs in a positive rather than in a negative clause.

P. 1169: Varro, *ling.* IX 83 *Quare cum ad analogias quod pertineat non <opus> est ut omnia similia dicantur, sed ut in suo quaque genere similiter declinentur*. De Melo explains the subjunctive *quod pertineant* as depending upon the causal *cum*-clause with a complicated relationship to the classical use of subj.: “Thematic *quod*-clauses normally stand in the indicative. The kind of modal attraction would be straightforward because the *quod*-clause is embedded into a causal *cum*-clause. However, this causal *cum*-clause is unusual in being in the indicative, as in early Latin, whereas by classical period *cum*-clauses that are not to be interpreted as purely temporal regularly take the subjunctive”. In this case the attraction of the subjunctive would act without the subjunctive (*non <opus> est*), a very strange use. However, here, the subjunctive is a potential of eventuality. I defended the modal attraction (Calboli 1968: 441–449), but I believe the modal attraction has nothing to do with this passage. De Melo does not master well his Latin.

P. 1175: De Melo presents a little history of *quoniam* from *quom* (*cum*) and *iam*, from temporal meaning to introductory function of subordinate clauses of several types. He had to consider and quote the fundamental paper by Rosemarie Lühr (1989) on this subject.

P. 1181: Varro, *ling.* IX 96: That subjunctive is without future, it is observed by de Melo, but this is an old doctrine. At any rate, it is interesting that future and subj. are connected together from a morphological point of view: the future seems to have been an old subj. in particular in ancient Italian dialects (see G. Calboli 2011: 40–42, 64, to be integrated by Calboli 1966: 260–268; J. Jasanoff 1991: 102). However, de Melo seems not to recognize the complexity of the relationship fut.~subj. As I wrote in my ‘Bericht’ about the moods of Greek and Latin verbs (Calboli 2011) the most advanced solution is that adsumed by Lightfoot (1975), namely (Calboli 2011: 124) “der Konjunktiv sei” – I wrote my ‘Bericht’ in German – “ein Futurum ohne Existenzvoraussetzung, während die Existenzvoraussetzung dem Ind.Futurum zugeschrieben wird”, though I converted in the same page the presupposition of existence into a kind of ‘telicity’ (‘factuality in prevision’), which means that the absence of such a ‘factuality in prevision’ can be considered a kind of ‘atelicity’.

P. 1201: Varro, *ling.* 10.11 *Quarum ego principia prima duum generum sola arbitror esse, ad quae similitudines exigi oporteat: e quis unum positum in uerborum materia, alterum ut in materiae figura quae ex declinatione fit*. This chapter is defined by de Melo as the “most fundamental of the entire book”. Indeed de Melo’s commentary and the reference to Introduction (6.2) about *quis* employed by Varro

instead of *quibus* are absolutely right: *quis* is used by Varro with a rather different frequency than *quibus* (*quibus* 72 times, *quis* 17). I would only like to add that, considering also *duum* instead of *duorum*, the whole passage seems to have been marked by a stamp of majestic and definitorial brevity.

P. 1207: Varro, *ling.X* 20 (19) *Vt in articulis duae partes, finitae et infinitae, sic in nominatibus duae, uocabulum et nomen. Non enim idem oppidum et Roma, cum oppidum sit uocabulum, Roma nomen, quorum discrimen in his reddendis retionibus alii discernunt, alii non. // nominatibus* L.Spengel Emend.Varr. p.6 Müller A.Spengel Kent *uocalibus* F L.Spengel (1826) †*vocalibus* G-Sch.

I am not sure that *nominatibus* by L. Spengel A. Spengel Müller and now de Melo is right, because it is right that *uocabulum* is one of the two parts of the *nominatus* (and the Stoic grammarians not only distinguished but ascribed to a different genre the *uocabulum* and the *nomen*, but Alexandrians did not believe that *uocabulum* and *nomen* were of different genre). In this case, however, the general appellation was ὄνομα, *nomen*, not προσήγορία, *uocabulum*. But how can we exclude that, since Varro was concentrated on the difference, finitum~infinitum, and the *infinitum* (*uocabulum*, *oppidum*) contained the *finitum* (*nomen*, *Roma*), he therefore called *uocabulum* the whole category of *nomen*? In this case, L. Spengel's correction would be a correction of Varro, not of his tradition, a danger not always easy to avoid. On the other hand, the term *nominatus* occurs in the following paragraph (§21): *Nominatui ut similis sit nominatus, habere debet ut sit eodem genere, specie eadem, sic casu exituque; [genere]<specie>, ut si nomen est quod conferas, cum quo conferas sit nomen // genere et specie* transp. Luebbert. In this passage, Varro presents a kind of attention to the word's suffix (as observed by Pfaffel [1981: 177–179], whose reading *exituque* was accepted by de Melo), while Varro used habitually the whole word “[sc.Varro] arbeitete ansonsten mit den komplexen, ganzen Wörtern” (Pfaffel 1981: 178). Perhaps in this difficult passage we had an insurable tradition, where Varro himself went longer than he was accustomed to move. The manuscript **F** at cartha 29r seems at first glance not to have been damaged, though Müller conjectured to add *unum* after *solum* (“Fortasse etiam *unum* post *solum* inferendum est”) but did not introduce this *unum* in his text, though Pfaffel (1981: 178) wrote *solum* <*unum sed*> and ascribed it to Müller. Then it is possible that *genere* and *specie* had been changed of position. However, this difficult passage should suggest to rely on prudence also in changing word order.

P. 1212: Varro, *ling.X* 27. Sometimes we find interesting explanations as to why *mancipium* and *scortum* are of neutral gender, namely because they were considered rather “commodities than fully fledged human beings”, and the feminine gender of some trees as *ficus* (‘fig tree’) which “belongs to the second

declension and therefore ought to be masculine, but is feminine because it is a tree”.

P. 1223: Varro, *ling.X* 43 *In hac formula numerorum duo inerunt quos dixi λόγοι, qui diuersas faciant analogias: unus duplex qui est in obliquis uersibus, quod est ut unus ad duo, sic duo ad quattuor; alter decemplex* [cf. *Th.L.L.V*, 1127, 68–72 also *Nep. Milt.5,5 ut decemplicem numerum hostium profligarint*, then *Tert.adu.Marc.2,14 p.354,25*] *in directis ordinibus, quod est ut unum ad decem, sic decem ad centum*. The nominative λόγοι instead of the accusative is rightly explained: “Instead of λόγοι, one might expect λόγους, but the nominative can be explained by the fact that the noun is not just the object of *dixi*, but also the subject of *inerunt* in the main clause”. That is right, but must be a little changed in the formulation: ‘but the subject of *inerunt*’ without ‘also’ and the punctuation: ‘*inerunt, quos dixi, λόγοι.*’

P. 1225f.: Varro, *ling.X* 48. In distinguishing perfectum and infectum, de Melo is right: “The perfect is anterior as well as present, which means that it can refer to events that happened before the moment of speech”, on its side infectum “indicates simultaneity”. I do not understand why de Melo, to explain the two Varro’s terms perfectum and infectum, does not use the term ‘aspect’ which de Melo knows well – I am sure – and is the most appropriate in this case. It is well known that tense system of every IE language consists of tenses and aspects.

P. 1239f.: Varro, *ling.X* 71 *E quis quae hic not<h>ae fiunt declinationes, de his aliae sunt priscae, ut Bacchidēs et Chrysidēs, aliae iuniores, ut Chrysidēs et Bacchidēs, aliae recentes, ut Chrysidas et Bacchidas. Cum his omnibus tribus utantur nostri, maxime qui sequuntur media in loquendo offendunt minimum, quod prima parum similia uidentur esse Gr<a>ecis, unde sint tralata, tertia parum similia nostris. // notae F nothae G-Sch. Kent de Melo *Bachides Chrysides* F *Bacc- Chry-edd. minores* F *iuniores* Scioppius Chri- Bac- F *Chrys- Bacc-edd. notas quantitatis add. Christ regentes* F *recentes* Aldus *Chrisidas et Bachidas* Quibus ex omnibus correctionibus tres tantum seruatas uelim: *Bacc- Chrys-iuniores recentes* quae nihil aliud quam emendatae lectiones sunt, atque sic illae quidem emendatae, ut ab omnibus accipiendae sint uel accipi possint (Calboli haec Latine addidit).*

Here, de Melo (together with modern edd.) changed so strongly Varro’s text, that the modernisation is complete and Latin text disappeared, while Varro a couple of words later wrote *Cum his omnibus tribus utantur nostri*⁹ and in this way showed that we must remain inside of the Latin language and confirms with the following words that he keeps the Latin use even of originally Greek terms (*parum similia uidentur esse Graecis, unde sint tralata*). De Melo accepted Christ’s suggestion to add diacritics and distinguish in this way the oldest *Bacchidēs et Chrysidēs* from more recent *Chrysidēs et Bacchidēs*, albeit he continues to ignore

9 Evidenced by myself.

where Christ produced (by writing or talking) such a suggestion (see above). All this, since *Chrysidēs et Bacchidēs* are the middle forms in this series, a form which strikes less the Latin language. De Melo's solution (p. 1240) is clever and deserves consideration: "When educated Romans learned Greek, they learned the Greek of Homer, Sophocles, and Plato, and this is the kind of Greek that makes its way into high Roman literature. However, Varro knew the Hellenistic inflections, and an accusative in -ēs [at the previous p. 1239 de Melo wrote: "The middle forms I consider to be Hellenistic accusatives in -εϛ] must have looked like a good compromise between the purely Latin -es and the classical Greek -ᾶς". Then de Melo, p. 1239, presents his own solution: "The interpretation I suggest adopts Christ's diacritics, but interprets the forms differently. Since the recent forms are unambiguously Greek accusatives, the other forms must be accusatives too. For the old forms this is unproblematic: they are Latin accusatives in -ēs. The middle form I consider to be Hellenistic accusatives in -εϛ".

As a matter of fact, de Melo's solution is a kind of interpretation of Varro's attention to relationship of Latin and Greek, and he is in accord with what Varro said: *Cum his omnibus tribus utantur nostri, maxime qui sequuntur media in loquendo offendant minimum*, i.e., that the middle forms disturb less. But he confirms at the same time that all three forms were employed by Romans as Latin forms (*Cum his omnibus utantur nostri*). *Chrysidas et Bacchidas* are defined *parum similia nostris*, and *Crysidas* is attested only by Varro in the quoted passage, *Bacchidas* by Varro, Charisius (*gramm.*I 149,2; 189,15 Barwick) and Diomedes (*gramm.*I 305,15–16), see Neue-Wagener, IV³ 47 and 67; *Th.L.L.* II 1662,22–27; *Onom.* II 422,76–78). But *Bacchis* and *Chrysis* are two characters of Roman theatre and the Roman authors of comedies were surely more interested in the metrical aspect of these names (Christ's solution) than in a possible correspondence with Greek forms, as they were written in Hellenistic times. On the other hand, Varro knew the Roman theatre incomparably better than we know today.

Pp. 1249–1271: *Fragmenta*, Vol.I, pp. 835–650. This section of the Commentary is dedicated to the 38 Fragments of *De lingua Latina*. Here, de Melo (p. 1250) points out rightly that we are dealing only with Varro's text. In many cases, Varro's text is confused with Gellius' words or the words of the author of the reference, and they are paraphrases not quotations *verbatimim*. I have very few remarks to make. They are the following ones: p. 1253, in frg.3 it is a pity that we do not find in de Melo's text Vergil's and Lucretius' quoted passages; p. 1256, de Melo wrote about the frg.5 quoted by Gellius, 2.25.5–10: "All this leaves me with the impression that Gellius is not quoting Varro directly, despite pretending to do so, and that he is paraphrasing and to some extent misrepresenting, even if not deliberately". De Melo's hypothesis is possible, but cannot surely be demonstrated and is improbable that Gellius, repeating three times *inquit* (sc. Varro), was not directly quoting.

P. 1262: frg.19 *rure ordinatum arbustum* seems strange to de Melo, because the abl. *rure* “is indicating motion from some place, while here we have *rūre* as an ablative proper, marking place”. I think, however, that the *arbusta* were naturally random, but in this case, they received an order from a kind of agricultural rule, therefore abl. *rure*, marking a figurative origin. Pp. 1264f.: on adverb *indiscriminātim* (frg.25) cf. *Th.L.L.* VII 1204,3–6 and J. Schaffner-Rimann (1958: 54), it is an ἄπας εἰρημένον, quoted only “als Gegenteil” of *discriminātim* (this has already been pointed out by de Melo); as for *promisce/promiscue* cf. *Th.L.L.* 10,2 1852,70–1853,6, and the similar *distingens/distinguens*, the first without *u* as proved to be closer to Greek origin and more ancient, defined by Vellius Longus, *gramm.*V 67.19–68.2 as *dictum* παρὰ τὸ τέγγειν, G. Calboli (2020: 308).

P. 1265: Frg.27, on *ighuies/gula* the root taken into account by de Melo **gleu-* seems more probable than the root **gel-* /*g^wer-*, supposed by G. Devoto (1962: 454 and 494). In the last frg.38, from Gell.16.8,1–14, we find an excellent discussion on ἄξιωμα/ἄξιώματα. It is a pity that de Melo missed (pp. 1270f.) this occasion to elucidate such an interesting and modern argument.

To sum up: I think that we must be grateful to de Melo for producing in only five years (*Preface*, p.viii) this large commentary of Varro’s text, one of the most difficult works of Latin literature, which is very difficult because of the bad transmission through a damaged manuscript and because it includes a rather abstruse matter, in a style which we cannot consider elegant and clear. De Melo had at his disposal a good or excellent knowledge of Indo-European linguistics, perhaps a little reduced only in Hittite, but the Indo-European linguistics and the collection of roots concerns only indirectly Varro and shows a little of the prehistory of Latin and how much richer our linguistic science is in comparison with that of Varro. However, I found some lack in paleography, history of Greek and Roman grammar and rhetoric, Roman antiquity, Romance philology and modern linguistics, and a dangerous haste which sometimes thwarted the necessary thoroughness. The final product can be considered a middle product which, therefore, needs many and considerable improvements. Five years were the time employed by de Melo to make the whole work; I would suggest five more to improve and correct this work. As it is now, notwithstanding some good contributions, it appears unsatisfactory and rather disappointing. But I am sure that after the necessary improvements and corrections it will become a good or excellent contribution.

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