

X. — *The Duenos Inscription.*

BY PROFESSOR GEORGE HEMPL,  
UNIVERSITY OF MICHIGAN.

I NEED not review the history of the elucidation of the Duenos inscription.<sup>1</sup> Jordan made an important advance in the grammatical interpretation. Conway accepted his text with three deviations, adopting the *io uei sat* of Deecke for the earlier division *iouei sat*, the *duenoi ne* of Bréal and Pauli for the *dze noine* of earlier interpreters, and the *malo* of Comparetti rather than *mano* : —

*io uei sat deiuos goi med mitat nei ted endo cosmis uirco sied,  
asted noisi ope toitiesiai pacari uois.  
duenos med feced en manom, einom duenoi ne med malo statod.*

This Conway translates : —

*May the gods Jove, Vejove, and Saturn (grant) that Proserpine, to whom they suffer this vase to be despatched, show thee no favour; unless indeed thou art willing to make thy peace with (or make atonement to, or be appeased towards) Ops Toitiesiai. Duenos made me (as a curse) against Manus, and let not evil fall to Duenos from me.*

Conway is the only commentator who has given a reasonable and consistent interpretation of the inscription. His general position, namely, that we have before us a curse, and most of his explanations are brilliant and convincing, and it is strange that minor infelicities in his interpretation sufficed to blind the eyes of other scholars to its unique satisfactoriness. My interpretation is based upon his and, while, in certain details, undertaking to extend and rectify it, confirms its essential correctness. The most important point in which

<sup>1</sup> For the literature of the subject see Maurenbrecher, *Philologus*, 54, p. 620, and Herbig, Bursian's *Jahresberichte*, 1900, 3. p. 40. The reader will find it of great assistance to have a facsimile at hand. I refer in all cases to Dressel's facsimiles in the *Annali dell' Istituto*, 1880, plate L. They are not perfect, for they do not exactly accord with one another. Still, they are the source of all others. Egbert's copy of the projection is perhaps most accessible.

my interpretation differs from Conway's lies in my rejection of his theory that the scribe was a Greek and betrayed his nationality in certain graphical errors and corrections. I shall not attempt to repeat Conway's arguments and rehearse the evidence he brings forward ; I shall simply take up those points in which I disagree with him. This will leave certain things unexplained and unjustified that I assume as established, and I must therefore warn the reader who is unfamiliar with Conway's paper that an acquaintance with it is essential to a just appreciation of my position.

Since Conway's article appeared, various scholars have attacked the problem and have poured out about the old inscription a flood of learning and conjecture,—but, unfortunately, to comparatively little purpose. It would be superfluous for me to consider or refute the many theories that have been put forward ; for that has been done sufficiently by others, each commentator having been keen to see and expose the weaknesses of the position of his predecessor.

After the fourth letter of the first line there is a straight perpendicular mark, rather longer than the adjacent letters are high. Some commentators (for example, Jordan) regard this as an *l* that was inserted after the neighboring letters had been written ; others (for example, Dressel) regard it as an attempt at punctuation ; still others (for example, Maurenbrecher) assume that it was more or less accidental ; and Thurneysen goes so far as to say that it is intended to show that the correction of *E* into *A* in *sat* was a mistake and that, instead, the *E* in *ue* should have been changed ! That the line was deliberately made is beyond all question. That it indicates punctuation is contrary, not only to the writer's practice in all the rest of the inscription, but also to what we know of ancient punctuation in general. Thurneysen's idea will certainly find acceptance nowhere. We must then go back to Jordan's conception that the mark is an inserted *l*, certainly the most obvious interpretation. It has been objected by Dressel and Maurenbrecher that the mark is too long and too slight to pass for an *l*. This argument

would hold, if it were claimed that the letter had been made from the start, along with the others ; but it has, of course, no weight whatever when we consider that the letter is a correction and obviously a later insertion. To demand that it should have been made thicker, is simply absurd, for the simple reason that there was not room for a thicker mark ; indeed, the line, thin as it is, could hardly be squeezed in between the neighboring characters. The form of the letter is, in fact, just such a one as was to be expected under the circumstances. And this applies to the extra length as well as to the thinness ; for, in insertions of this kind, the erect shaft of an inserted I, J, T, L, or the like, is almost invariably longer than those of the letters between which it is inserted. But, aside from that, altogether too much has been made of this matter of length. A glance at the facsimile will show that there was little attempt at uniformity. I do not refer to the psychological phenomenon of small round O and ʘ by the side of large letters made of straight lines. But the shafts of M in *med* are actually twice as long as the shaft of the adjoining E and those of the preceding N and D. They are, in fact, longer than the I in question. Moreover, not only is the I such as we should expect under the circumstances ; but the very reason for its omission and subsequent insertion is not difficult to trace. The engraver wrote IO for *iouis* and then wrote in like manner the first two letters of *ueiouis* and began to write the abbreviation of *saturnos* when it occurred to him that the VE would be more likely to suggest *uenos* than *ueiouis*. He consequently stopped and inserted an I after the VE. Returning to complete what he was writing, he wrote, instead of A, the E he still had in mind, and then, discovering his error, had to smooth out the arms of the E and make an A of it.

Conway was perhaps the last to regard *deiuos* as a nominative plural with the original ending of *o*-stems, which was retained in Oscan and Umbrian (Planta, II, § 274, Brugmann, II, § 314). To this it has been repeatedly objected that the old nominal ending had been displaced in Latin by the pronominal ending *-oi*, later *-i*. To be sure, we have not yet

found the ending *-oi* in early Latin plurals, but we find quoted in later texts such plurals as *poploe*, *pilumnoe*, etc., which are generally, and doubtless correctly, regarded as transcriptions of *poploi* etc., made at a time when the old stressed *oi* that (instead of becoming normally  $\bar{o} > \bar{u}$ ) had been artificially preserved in the reading of archaic documents, had become *oe*. I shall, however, show (p. 155) that weak *oi* had in all probability passed on to *ei* at the time that the Duenos inscription was written, and we must, therefore, suppose that the time when the pronominal *-oi* entered the nominal declension was still earlier. In other words, when the Duenos inscription was written, the nominative plural of *o*-stems generally ended in *-oi*. I say generally, for we must not forget that the change of *-os* to *-oi* was not a phonological change but an analogical change, and analogical changes do not affect all the members of a group with equal force. That some remain unaffected, for example, in technical expressions, fixed phrases, or the formal language of the law and of religion, does not surprise us. Thus, we know that, when the pronominal *-ōrom -ōrum* displaced the nominal *-om -um*, and *-ai* displaced *-ās* in the genitive of the *ā*-stems, the old endings succeeded in maintaining themselves in various phrases and technical uses: *praefectus fabrum*, *triumvirum*, *nummum*, *paterfamiliās*, etc. It is of interest that among these is the very word we are now dealing with, *deus*, whose old plural genitive *deum* long held out against the new *deōrum*. That, in like manner, we could have a nominative plural *deiuos* even at a time when the nominative plural of *o*-stems generally ended in *-oi*, admits of no doubt. That religious conservatism may have had more or less to do with the maintenance of the old form is not improbable. Thus, in America, where the Old-French spelling *-our* has, for the most part, yielded to the Latin *-or*, we still write *The Saviour*, not *The Savior*.

Conway holds that *mitat* stands for *mittant*. To this he was led chiefly by "the discrepancy of *mitat* with *-t* from *sied feced* with *-d*." But there is no more difficulty with *mitat* by the side of *sied* and *feced* than there is with *dedit* by the side of *fecid* on the Cista Ficoroniana. This *d* was

becoming *t* (and so coinciding with the *t* that arose out of older *-ti*), and we have here the evidence of transition. The sound was, doubtless, at the stage of the unvoiced but still weak stop, and thus intermediate between the old voiced weak *d* and the voiceless strong *t* that it ultimately became (Sievers, *Phonetik*<sup>4</sup>, § 480, 774). At this stage, T and D were equally good, and equally inexact, spellings for it. We have, therefore, no reason for taking *mitat* to be anything but *mitat* (whether *mītat* or already *mittat*) and are at liberty to give to *io uei sat deiuos qoi med mitat* the most natural rendering: "May the Gods, Jove, Vejove, and Saturn (grant) to him who is going to send me." *Mitat* is the future, or prospective, subjunctive.

Conway (p. 452) suggests that *qoi* may be nominative plural, in which case "the clause *qoi med mitat* will be attached simply to the preceding words." This would give the very improbable rendering: "May the Gods Jove, Vejove, and Saturn, who suffer this vase to be despatched." But Conway (p. 454) prefers to follow Pauli in regarding *qoi* as dative singular and referring anticipatorily to *uirco*, as shown in the translation above (p. 150). This strikes me as violent and as a suggestion that could be entertained only in despair of anything better. But we have just seen that there is nothing desperate in the situation and that the clause admits of a perfectly simple and satisfactory explanation.

Maurenbrecher's claim (p. 624, 625) that we cannot take *nei* as a final conjunction, because the earliest certain case of its use in this function is 186 B.C., does not hold, in view of the fact that we have so very few records that are of an earlier date.

Conway accepts Jordan's interpretation (p. 235) of the beginning of line two as *asted*, that is, an archaic form of *ast*, like *postid* for *post*. This Thurneysen (p. 197) and Maurenbrecher (p. 625) have, as it seems to me, satisfactorily disproved. Bréal and Thurneysen hold that *asted* = *ast(t)ed*, with *t* written but once, cf. *manom (m)einom* below. This strikes me as an excellent idea. For the position of the reflexive, see p. 162 below.

The *oi* in *noisi* and *uois* has caused much trouble. Of the explanations made by those who interpret the words as = *nīsī* and *vīs*, Conway's is the least objectionable. He regards the *oi* as related to *ei* by vowel gradation. But even this has not stood the test, and must be abandoned. There is, however, a very simple explanation. In the inscription we find *oi* five times:—

<i>qoi</i>	<i>noisi</i>
<i>opetoi</i>	<i>uois</i>
<i>duenoi</i>	

In the first three we expect *oi* (*qoi* being the strong form, afterwards displaced by the weak, Brugmann<sup>2</sup>, I, p. 228 A 1); in the last two we expect *ei* (>  $\bar{e}$  >  $\bar{i}$ ). In these two cases the *oi* is followed by *s*. Now, we know that in the dative, ablative, locative, instrumental plurals of the *o*-stems, the ending *-ois* became *-eis* (>  $\bar{e}s$  >  $\bar{i}s$ ). That the old spelling was kept up for a time by analogy to the other cases with *-o* was natural. This ending was exceedingly common (see p. 156 below) and thus the spelling *ois* for *eis* was very familiar and may be expected in cases where the *eis* did not arise out of *ois*;—exactly as, when *ans* had become  $\bar{a}s$ , the spelling *ans* was used for any  $\bar{a}s$ , for example, *occansio* (Lindsay, *Latin Language*, p. 69); and as, when *ei* had become  $\bar{i}$ , the spelling *ei* was used also for  $\bar{i}$  that had not arisen out of *ei* (see p. 156 below). This explanation<sup>1</sup> of the spellings *noisi* and *uois* is a perfectly well grounded and consistent one. Thus, in the Duenos inscription *ei* is ordinarily spelled *ei* (*deinos*, *nei*, [*m*]einom), but *eis* is spelled *ois* (*noisi*, *uois*). *noisi*, then, is *nei-sī* (< *nei* + *sei*) and is a cousin to the classical *nīsī* (< *ne* + *sei*).

That the weak *sī* (< *sei*) in *nei-sī* ("noisi") is written *-si* and not *-sei*, has caused Thurneysen (p. 197) and others unnecessary difficulty. It is, in fact, very interesting and instructive. It proves that weak *ei* had become  $\bar{i}$  at the time when weak *oi* had not yet become  $\bar{i}$ , but was identified with

<sup>1</sup> After I had worked this out, I discovered that Solmsen (*Studien*, pp. 87, 88) had suggested a similar explanation. I would not, however, have the two confounded: Solmsen's position and mine are really quite different.

*ei*. This is just what was, from a phonological point of view, to be expected; for *e* stands nearest to *i*, and would yield to it sooner than other vowels would. Thus, in Germanic languages, *i* mutated *e* long before it mutated *a*, *o*, *u*.<sup>1</sup>

If Solmsen (*IF*, 4, 24, Brugmann<sup>2</sup>, I, p. 184, ft. 2) is right in deriving *sī* from weak *\*sai*=Oscan *svai* (compare the like loss of *w* in Old-English *s(w)ā* < Germanic *swai*), as I think he is, we must take it for granted that not only weak original *ei* had become *ī* but also the weak *ei* that had arisen out of *ai*. This is in harmony with what we are led by phonological considerations to believe was the chronological development of *ī* out of the various diphthongs:—

	(1)	(2)	(3)	(4)
Weak	<i>ei</i>	>	<i>ī</i>	
“	<i>ai</i>	>	<i>ei</i>	>
“	<i>oi</i>	>	<i>ei</i>	>
Stressed	<i>ei</i>			>

At the time of our inscription, Latin had arrived at the stage (3). It will be observed that this accords with the theory proposed above, namely, that at this time the vast majority of existent *eis*'s were found in the dative and ablative plural of *o*-stems, the *-ais* of feminine stems having already passed on through *-eis* to *-īs* and being, in all probability, written with *i*, as the *ī* (< *ei* < weak *ai*) in *neisī* ("noisi") was.

When, at a later period, stressed *ei* and the weak *ei* that had arisen out of *oi* had likewise passed on to *ī*, the spelling *ei* was used to represent also the *ī* that had arisen earlier out of weak *ei* and out of *ei* < weak *ai* (hence the spelling *ni-sei*) and even that *ī* that had always been *ī* (hence *ueiuos*), cf. Brugmann, I, pp. 102, 184. We thus see the significance of the later spelling *nisei* and its relation to the *noisi* of our inscription. But the spelling *nise*, often cited from the *Lex Rubrica*, has no significance. This form is found only once and that in *niseiei*. Now, it is clear that we have here one of those countless cases of the loss of a final letter because

<sup>1</sup> From this it must not be inferred that I am confounding these changes with *i*-mutation. On another occasion I shall have something to say on the phonology of the changes that Latin diphthongs suffered.

identical with that beginning the next word. It was not the practice of the engraver of the *Lex Rubrica* to write but one letter in such cases, for he separates his words by points; but as *nise* ends the line, there was no occasion for a point, and he went on with the *i* in *iei*. In the only other case that the word is used in the *Lex Rubrica*, three lines below *nise iei*, it is spelled *nisei*.

To return, — there is thus no reason why *noisi* and *uois* should cause any difficulty so far as their phonology is concerned, while their meanings are certainly just what is wanted.

Conway (p. 455) regrets that he is not able to perfect Jordan's interpretation of the second line. He sees the difficulty in regarding *ope* as a dative, and suggests that it might be an instrumental, *ope Toitiesiai* being 'by the aid of Toitiesia.' The hopelessness of any interpretation that recognizes the form *Toitiesiai* is frankly confessed: "Who or what *Toitiesia* is, human or divine, or whether she really exists at all, no one yet knows. . . . I am far from certain that the words are rightly separated." They surely are not. For a long time scholars were bewitched by the suggestion of *Ops*, but of late two or three new divisions have been made. Of the attempts of Bréal and Pauli, Maurenbrecher (p. 627) says: "Ihre abstrusen Wortgebilde bedürfen keiner Widerlegung." But he himself proposes (p. 628): *asted noisi, ope Toitiesiai pakari uois* = *adstet nobis, ut opera Tuteriae cum uobis pacemur*! And Thurneysen (p. 198) asks us to accept *as(t) ted noisi op et oites iai pa(c)ari uois* = *ast te, nobis ad id utens, ei pacari uis*!

It is strange that the *oi* has not suggested to some one that this is the dative masculine ending of an adjective agreeing with the proper name ending in the dative in *-ai*: *opetoi tesiai*. This *opetoi* is evidently the dative of *opetos*, the displaced positive<sup>1</sup> of *opitumos*, later *optimus*, which is thus a true superlative in *-umos -imus* and not, as generally said (cf. Stolz und Schmalz<sup>3</sup>, p. 148), a positive in *-tumos -timus* that came to be used as a superlative. The positive *opetos*

<sup>1</sup> On another occasion I shall show that irregular comparison is largely due to the displacement of the positive by the encroachment of some other word.



earlier \**opitos* is an adjective from *ops*, meaning 'helpful, useful, good'; whence the verb *optō* 'regard as useful or good,' 'like,' 'choose,' as well as the masculine noun *optiō* 'assistant, helper,' cf. *mīriō* from *mirus* (Brugmann, II, p. 337-338, and the paper mentioned on page 163).

It is not necessary to suppose with Jordan (p. 246) that the *s* of *tesiai*, by the side of the *r* in *pacari*, shows that the scribe was a foreigner, or with Conway (p. 455) that the word is foreign or that the writer was employing an archaic form; for *s* would be a correct early spelling for either *ss* or *ns*. *Tesiai*, in all probability, stands for *Tensiai*, with the frequent, even very early, omission of the weakened nasal (Brugmann<sup>2</sup>, I, p. 371). Compare also *malo(m) statod* below. *Te[n]sia* I would explain as an old abstract (from *tensus* 'stretched,' 'strained,' 'tense,' 'stiff') in *-iā*<sup>1</sup> (Brugmann, II, p. 443, 117, etc.), used as the name of a person having the quality denoted by the participle *tensus* (Brugmann, II, § 157, p. 443 top). The name might be imitated in English by *Stiffness* or *Stretchy*, cf. the English family names *Strain*, *Stretch*, *Stiff*. For the use of abstracts as names of persons, compare *Potestas*, *Nice*, *Victoria*, *Fortuna*, *Auctoritas*; the Christian use of *Spes*, *Concordia*, *Constantia*; the English family names *Bliss*, *Joy*, *Pride*, etc.; our Puritan given names *Grace*, *Faith*, *Mercy*, etc.; the playful designations *Miss Impudence*, *Miss Simplicity*, and the like; the dignified *His Holiness*, *Her Majesty*, *Your Honor*; and the hosts of Germanic names like *Gaman* 'joy,' *Agis* 'fear,' *Craft* 'strength' (cf. Modern German *Herr Kraft*, *Herr Muth*, etc.), *Frið* 'peace,' and compounds like *Ælfred*, *Cyneðryð* (*ðryð* 'strength'), *Ceolmod* (*mōd* 'courage'), *Æðelmund* (*mund* 'protection'). Observe that *Cyneðryð*, *Eadmund*, etc., like *Tensia*, are feminine abstracts used as names of men. I need hardly refer to masculine names like *Hadria*, *Scaevola*, *Mūrēna*, etc., and masculine nouns like *scriba*, *nauta*, *aurīga*, etc. Of course, the fact that we do not find elsewhere the name of the man who made or sent the vase is not at all serious, as certainly would be our

<sup>1</sup> This old abstract was displaced by *tensio*, formed with the younger compound ending *-i-ōn* (Brugmann, II, p. 336 top, 338).

inability to find any other trace of such a goddess as *Toitesia*, if we were to suppose her mentioned here. Furthermore, while it was customary for one to curse another for an offence suffered at his hands, and we may well imagine that our *Tensia* desired to make it hard for his enemy if he carried his resentment toward him into the other world; it would, on the other hand, be strange if, as Conway seems to assume, a mortal took up the cause of a god and ran the risks incumbent upon the practice of cursing, all in order that a dead man might be frightened into making his peace with one of the divinities.

It is generally said (compare, for example, Conway, p. 447-448) that in *pakari* and *feked* we have a K changed to a C. As Dressel's facsimile does not betray which lines are the later, one at a distance must judge by other considerations. In *feked* we find 𐌚. This is certainly not a K changed to a C. In the first place, there appears to be no trace of a change of any kind. The side strokes are rather heavy, that is all, and we find similarly heavy lines elsewhere in the inscription; observe the top arm of the preceding E, the shaft of the l in *meinom*, and one or another stroke of some of the O's, for example, that before *statod*. In the second place, the C's of this inscription, including the one in *pakari*, are all small superior ɔ. Our character is simply one of the forms that the old *zeta* 𐌚 assumed in Italy, namely 𐌚 𐌛 𐌜, retrograde 𐌚 𐌛, etc. This, having no phonological use in Latin, came to be regarded as a variant of the similar *kappa* 𐌑 𐌒 and was employed as a *k*, ultimately getting separate recognition with the value of *g*,—as I showed in detail some years ago in a paper read before this society (*The Origin of the Latin Letters G and Z*, *Trans.* vol. 30, p. 24-41). The character in *pakari* is clearly 𐌛. This certainly does contain such a C as is characteristic of this inscription. But it is hard to understand how this can be regarded as a K changed into a C. If we take away the ɔ, we have 𐌚 left. This could be nothing but a part of a K of the form 𐌚, like that in *feked*. It might be said that the engraver discovered his mistake before he had completed the K. To this there are two objections. In

the first place, it is not likely that any one would make the lower arm before he made the upper one. In the second place, had he done so and then decided to change the K to a C, he certainly would not have left the obtrusive  $\lrcorner$  standing; for we find that he was in the habit of smoothing out incorrect lines, for example, the E first written after the S of *sat* and the D first written after *manom*. On the other hand, there is nothing in the way of supposing that the scribe wrote  $\spadesuit$  and then corrected it into a *k* (as held by Dressel, Jordan, and Maurenbrecher) by adding the erect shaft and the lower oblique bar. In this way the upper part of the  $\spadesuit$  was allowed to serve as the upper bar of a  $\beth$ , for which it was a trifle too short, and only the lower part of the  $\spadesuit$  was superfluous and escaped erasure. But in this, the corrected letter was no more different from the  $\beth$  in *feked* than the second E in *feked*, with its four arms, was from all the other E's. We see, therefore, that we have in the letters representing the voiceless velars in *pakari* and *feked* no evidence whatever that a Greek had written his native K and then tried to change it into a C, "consciously following a recognized Latin usage" (Conway, p. 448). Instead, we have simply a vacillation in usage between C and a form of K. The C was evidently already getting the upper hand; but after writing it in *pacari*, the scribe, in accordance with a well known usage, decided to change it to a K before the following A.

The *r* in *pakari* has been much discussed. It is generally taken for granted that it represents an original *s* > *z* > *r*. Thurneysen objects (p. 210) that the retention of the old vowels, diphthongs, and final consonants indicates a Latin that must be older than that of the time of rhotacism; he fails, however, to give of the *r* any other explanation that is at all acceptable. Thurneysen errs in two directions. We must not suppose that the writing of R began in the entry of proper names in public documents and that we are, therefore, to date the change of *z* to *r* at the time of the Papirii, Valerii, and Furi, that is, at the middle of the fourth century B.C. Proper names retain archaic spellings long after these are abandoned in ordinary words (observe the retention of the

3-like Old-English *g* or *y* in the name *Mackenzie* — until it was actually taken for a *z* and so pronounced), and legal documents are similarly conservative. If we date the change of *z* to *r* fifty or seventy-five years earlier, we shall probably not be far out of the way. In the second place, Thurneysen overestimates the archaic character of our inscription. It is not true that we have “tadellos erhaltene vocale, diphthonge und auslautende consonanten.” For I have shown that weak *oi* had become *ei*, while weak *ei* and weak *ai* had become *ī*. Moreover, *opetoi*, with its *e* for the older *i*, shows a degeneration of the weak vowel that had not yet taken place in the elsewhere-recorded *opitumos*. In other words, our inscription represents that intermediate stage in the development of Latin in which the vocalic elements of the *stressed* syllables still remained intact (unless, perhaps, *ei* had become a close *ē*), while those in weak syllables had yielded to a considerable extent. So far as the consonants are concerned, we have seen (p. 158) that a nasal consonant before *s* had degenerated into an unwritten nasalization of the preceding vowel; and (p. 154) that final verbal *d* had become unvoiced but not yet identical with strong voiceless *t*. What gives the inscription its most archaic look is the use of  $\varphi$  without  $\vee$ . But this is largely a matter of writing and we must not fall into the error of confounding it with phonological development. The inscription on the Forum stele, in whose usage I can see nothing younger, has  $\varphi\vee$  and  $\varphi\vee$ .

I agree with Thurneysen (p. 197, 200) in his conception of the words *ted . . . pakari uois*: “Die stellung des *ted* unmittelbar hinter dem satzeinleitenden worte ist die zu erwartende, auch die ganze construction *ted . . . pa(c)ari uois* die regelmässige, wenn *pacari* passivisch als ‘ausgesöhnt werden’ oder ähnlich zu fassen ist. Sie fällt aber auch nicht auf, wenn *pacari* neutral ‘sich aussöhnen’ bedeutet, vgl. *nolt placere sese amicae* Plaut. Asin. 183 und die vielen ähnlichen fälle. Endlich Könnte *pa(c)ari* mit Kurzem *i* zu lesen sein und als ältere form des activen infinitivs *pacare* zu betrachten sein, *ted* also das object bezeichnen. . . . es scheint eine construction wie gr. *διαλλάττειν τινί* vorzuliegen.” Thurney-

sen is quite right in saying that the sentence is normal : it is so in every respect, and I need quote but three or four of the vast number of similar sentences : *Sed se in hac urbe florere voluerunt*, Rabir., 3, 25 ; *Equidem me Caesaris militem dici volui*, B. C., 2, 32, 14 ; *Hi se praetores appellari volebant*, Leg. Agr., 2, 93 ; *qui se popularis haberi volunt*, Cat., 4, 10 ; *quantam vos in me esse voluistis*, Balb., 1. The only point that might arouse criticism is the placing of the reflexive between two closely connected words. But this, too, is thoroughly justified. We find it occasionally in such neutral sentences as : *Qui se ex his minus timidos existimari volebant*, B. G. 1, 39 ; but more usually where one or both of the two separated words are particularly emphatic, as is the case in the clause in our inscription. Compare the following from Caesar and Cicero : *qui se ipse scurram improbissimum existimari volt*, Verr., 3, 146 ; *omnia se cetera*, Verr., 4, 111 ; *rem se totam*, Verr., 3, 139 ; *multis sese nobilibus*, B. G., 1, 44 ; *optimis se viris*, Cael., 12 ; *isdem se copiis*, Quir., 66 ; *his se rebus*, Verr. a. pr., 15.

Conway regards *duenos* and *manom* as proper names ; the former is, rather, the common adjective *duenos* > *duonos* > > *bonus*, while the latter is the old singular of *mānēs* and has here the meaning 'the spirit of a dead man.' In the paper referred to below I have shown that the old *o*-stem *mānos* (*man-uo- men-uo-*, cf. *maneo* = μένω) is a gradation variant of *μόνος* (*mon-uo-*) 'alone' and *μανός* (*mū-uo-*) 'rarus, loose in texture, porous, scanty.' *mānos* shows the loss of *u* in *-nūo-* that we find in *-sūo-* and *-quō-* ; whence, by analogy, *Māna*, but \**Manuāna* with the *u* preserved before *ā*, cf. *Jānus* and the analogical *Jāna*, but the phonetic *jānuā*, *Jānuālis*, etc. The change of *o*-stems, especially adjective *o*-stems, to *i*-stems, is characteristic of Latin. Compare *similis* with *ὁμαλός* and observe *inermis* by the side of *inermus* (Brugmann, II, p. 265). The *o*-stem lingered for a time as a sort of proper name. With *Genita Māna* compare the *Cerus Mānus* of the Salian Hymns (Paul. ex Fest., Müller, p. 122, 4). Whether the word ever really had the meaning 'good,' as stated by Varro (*De Lingua Latina*, 6, 4) and later grammarians, may be

doubted; for it is when they are employing it as an etymon of other words that they ascribe to it that meaning. The idea may have arisen from the use of both *Duonos Ceros* and *Ceros Manos* in the Salian Hymns, which were learned and sung long after they had ceased to be understood. The *mānēs* were the 'rare ones' or the 'thin ones,' the 'spirits' or 'shades' of the dead, otherwise known as *animae tenuēs* and *umbrae tenuēs*. On the new *ī*-stem was based *Mānia*, with much the same meaning as the older *Māna*.

None of the attempts to explain *einom* as a Latin word are such as would be seriously entertained except in despair of anything better. Thurneysen recognized that we might have in *manomeinom* a *manom meinom*. This *meinom* he attempts to identify with a Celtic *u*-stem, meaning 'desire, lust,' by calling in the meaning 'intention' found in modern Gaelic and translating *en manom meinom* 'zu guter absicht.' This explanation has, as was to be expected, found no favor. But there is a *meinom* quite at hand whose meaning fits perfectly into our sentence. Old-Latin *meinom* is identical in form and meaning with the Greek *μει(ν)ον* 'less, inferior,' seen in *ἀ-μείνων* and *μείων* (*μείνων* and *πλέων* became *μείων* and *πλείων* by mutual concession), as I have shown at length in a paper that will probably appear soon in *Indogermanische Forschungen*. For its use as a conjunction, or quasi conjunction, compare the later use of its cousin *minus* with *quo* in Latin, the use in Old-English of *læs*, modern *lest* and *unless*, the phrases *um so weniger* and *desto weniger* in German, and similar idioms in other languages.

Grienberger (*IF.*, II, p. 342) undertakes to do away with the I of [*m*]*einom* by claiming that it is only "ein Substanzverlust im weichen Thon ohne irgendwelche litterale Bedeutung." At first sight there is a kind of plausibility in this suggestion. The fact that the *¶* of *duenos med* runs down into the *ʔ* of *deiuos qoi*, proves (if any proof were needed) that the line beginning with *duenos* was written later than the line *io uei sat* etc. But the plate (figure 2) shows an extension of the I of [*m*]*einom* downward, and it is clear that this line was made before the arms of the E of *endo*, for they cross

it. If, then, this mark was made before the first line was written, it was there before the second line was written and, therefore, has no significance in the line. But this argument takes it for granted that the line that is crossed by the arms of the E of *endo* is really an extension of the l found in *[m]einom* above. This is, however, only an assumption. A glance at figure 4 shows that the l of *[m]einom* is as clear and distinct an l as there is in the inscription and that its foot simply chanches to coincide with a very different slight line that starts at the foot of the E of *endo* and grows fainter as it approaches the l of *[m]einom*. This faint line is, then, obviously nothing but a slight impression made by the stylus because the engraver did not lift it quite high enough when he had completed the erect shaft of the E of *endo* and was passing up to make the arms — just such a “Bindestrich aus dem Duktus der schreibenden Hand” as Grienberger points out in *duenos*.<sup>1</sup> The foundation of Grienberger's argument is thus clean cut away. But even if this were not so, Grienberger's supposition is in itself so very improbable that it could not be seriously entertained. We must not forget that the clay was soft when the letters were written. Now, if we find the engraver smoothing out his errors (cf. p. 160, above), we surely cannot assume that he would calmly permit an accidental mark that was exactly like an l to stand in the middle of one of his words.

The old idea that the letter inserted between the D and the E of *duenoi* is some form of *zeta*, has been so often and so thoroughly exploded that I shall add only a reference to my paper on *The Origin of Latin G and Z* (p. 159 above). When considering the form of this letter, as in considering that of


<sup>1</sup> But when Grienberger says that the mark between the D and the V of *duenoi* (see p. 165) is such a Bindestrich and that there is another between the medial consonant and the O of the word before the last, he permits his theory to run away with his judgment — as a little consideration will make clear to any observer. Furthermore, it is puzzling to understand how Grienberger can regard his “Buchstaben-Berührungen” as “Ansätze zu einer zusammenhängenden Kursive.” Cursive writing arises out of neglect to raise the stylus and there is nothing whatever in such contacts as Grienberger points out that could lead to cursive writing.

the *l* of *uei*, we must not forget, as so many have done, that we are dealing with a letter that was inserted after the neighboring letters had been written, and that it had to be crowded into such space as there was. Thurneysen is undoubtedly correct in saying (p. 207) that the little hook at the top of the *V* of *duenoi* is accidental; it is evidently due to inaccurate placing of the style in starting the letter, a thing that could easily happen to one when trying to squeeze a *V* into so small a space. But Thurneysen is in error in saying we have to deal with a *V* of the form *Y*; it is a *V* with a long shaft at the right, and a short one at the left; compare the *√* of *uois*. In *duenoi* the long shaft had to stand nearly erect, because there was no room for it to slant. For the same reason, the short shaft, in order to have any length at all, had to pass the foot of the long one.

As *ne* stands before *med*, it is clear that its force falls upon *med*. Its use after *meinom*, which implies a negative, is to be compared to the use of *ne* — *quidem* after a negative. It is not, however, equivalent to *ne* — *quidem*; for *ne* — *quidem* brings prominently under the negation a factor that might otherwise be supposed to lie outside the range of the preceding negative, but our *ne* directs the negation particularly to *med* as that which should by no means, or last of all, be the cause of such mischief, — a task that *meinom* hardly could perform. Its force might be brought out in English by the translation: 'the less may any evil attach itself to the good man, especially not through me.' The common *ne* — *quidem*, with *ne* standing before the word it specifically negates, is, then, probably a last relic of a time when *ne* could be placed before the word particularly to be negated, although there was already a general negative in the sentence (cf. Fowler, *The Negatives of the Indo-European Languages*, 1896, p. 6, 2) — just as in Old English (*þæt hira ne mehte nān tō oðrum* 'so that there couldn't neither of them [get at] the other') and other Germanic languages, and, for that matter, in vulgar modern English ("I hain't seen nobody"). Probably the most frequent occasion for the introduction of such an extra negative was when the word before which it was placed



might otherwise be regarded as exempt from the general negative, that is, under exactly those circumstances under which a Roman would say *ne* — *quidem* and we *not even*; hence the persistence of the extra negative in this connection after it had fallen away in less common ones.

Besides the velars in *pakari* and *feked*, there is another letter which Conway, like Comparetti, believed shows traces of a change from a Greek to a Latin form: "In the last word but one, *malo*, there is a curious sign (↖) which was at first read as *n* (N), but seems clearly to be  $\Lambda = \lambda$ , corrected into a Latin  $\Delta = l$ " (p. 447). The form shown by Conway is (doubtless through lack of care on the part of the printer) quite inaccurate. It will be best to reproduce the whole word: . The third letter (from the right) is certainly not the normal form of any letter. I find but two possible explanations. (1) The letter was intended for *U*, but, as the line was crowded and bent by the line below, the letters were tipped and the first (right-hand) stroke of the *n* leaned to the right and the medial stroke, instead of being oblique, became practically horizontal. Then the engraver attached the third stroke to the first, which had the position that the second should have had; whether he did this consciously or inadvertently, would be a matter of little moment. (2) The engraver intended to write *malom*, but, under the influence of the preceding *manom*, started to write *man*, when he discovered his mistake and changed the *n* to an *l*. In support of this idea, it may be said that the facsimile represents the top stroke (the one that would be superfluous in an *l*) as partially erased. Of the two explanations, I regard the second as the more likely. Both are more natural and simple than Conway's, which assumes the otherwise unsupported theory of the Greek nationality of the engraver. The suggestion that the letter is an *A* erroneously written (through the influence of the preceding *A*) for some other letter and left uncorrected (Dressel, Bücheler, Jordan, etc.) is quite improbable. In the first place, it contradicts what we know of the engraver's practice, namely, his habit of smoothing out erroneous letters. In the second place, the character differs from

all the A's in the inscription, without there being any obvious reason for such difference.

Conway takes *statod* intransitively and renders *let not evil fall to Duenos from me*. Thurneysen objects that this is a rendering that can hardly be called a translation. To be sure, it is no literal translation, to render *stare* by 'fall'; but the same idea can be expressed in different languages by very different figures. *Stāre* here has the common value of 'stand, continue, persist.' The maker of the vase is trying to prevent any contamination of the curse from affecting (or, as one would say in German, *anhaften*) the curser. That this danger was feared and was guarded against by such a clause is thoroughly established (Conway, p. 456).

I read : —

IO. VEI. SAT. DEIVOS QOI MED MITAT NEI TED ENDO  
COSMIS VIRCO SIED;  
AST [T]ED NOISI OPETOI TE[N]SIAI PAKARI VOIS.  
DVENOS MED FEKED EN MANOM, [M]EINOM DVENOI  
NE MED MALO[M] STATOD.<sup>1</sup>

This I translate : —

(May) the gods Jove, Vejove, (and) Saturn (grant to him) who is going to send me, that the maid [=Proserpine] be not gracious to you; unless indeed you are willing to become reconciled to the excellent Tensia. A good man made me against the spirit of a dead man; the less may any evil persist through me to the disadvantage of the good man.

---

I may add one or two inferences that are to be drawn from our inscription. We have seen above (p. 158) that the engraver, like the makers of other early inscriptions, ignores, in writing, the nasal which, before a voiceless fricative, had degenerated into a nasalization of the preceding vowel. This, too, we find that he does in the case of a final nasal followed by an initial fricative : — *malo*[*m*] *statod*. It might be asked: Is

<sup>1</sup> In this the [ ] indicate letters that were omitted, not by error, but in accordance with the practice of the writer.

not the latter simply a case of the disappearance of weak final *-m*? That it is not, is shown by the spelling [m]einom *duenoi*, in which the final *-m* is written before the initial *d-*. Appeal cannot be made to the writing of but one *m* in *manomeinom* for *manom meinom*; for this is but a case of the writing of a single consonant for a double one, the usual practice in early inscriptions and repeated in this inscription in *asted* for *ast* [t]ed. Moreover, by a peculiar accident, we have evidence that the *m* written in *manomeinom* really belongs to the *manom*. When the engraver had written *manom*, he for a moment supposed that in writing *-nom* he had written the last three letters of the *meinom* that was to follow and that he already had in mind, and so he started to write the next word, *duenoi*. When he had made the D, he discovered that he had left out *meinom* and, therefore, smoothed out the bow of the D and wrote *einom*, without repeating the *m* already written at the end of *manom*. The trace of the bow of the D is still discernible under the oblique bars of the E. Baehrens' attempt (*Neue Jahrbücher*, 129, p. 834-835) to explain this trace of an erased D as a sign that the bottom stroke was inadvertently made and should not be counted (in other words, that the letter is F, not E) is one of those marvellous suggestions that philologists, in some unaccountable way, are at times guilty of making. Compare Thurneysen's explanation of the added l of *uei* (p. 151, above), and Schroeder's contention (*Jahreshefte des öster. archäolog. Instituts*, 3) that the seventh letter is an A changed to an E.

But the ignoring of the nasal, whether *n* or *m*, before a voiceless fricative, at the same time that final *-m* before other consonants (in [m]einom *duenoi* and in *manom d-* which was changed to *manom* [m]einom) was consistently written, has for us grammatical significance. It implies that the change of the consonant nasal before a voiceless fricative into a nasalization of the preceding vowel took place earlier than the disintegration of final *-m*.

Furthermore, from what has been said above, it is clear that the proper expansion of such forms as *manomeinom* and

*asted* is *manom* [m]einom and *ast* [t]ed and not *mano*[m]meinom and *as*[t] ted, as found in Thurneysen. The latter method of expansion was doubtless suggested by the expansion of texts with loss of final consonants, for example, *malo*[m] statod. But in cases like *asted* we are not dealing with a phonological phenomenon, but with a graphic one — namely, the neglect of a letter just written.