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THE INSTITUTE FOR THE STUDY OF MAN

Suite 520, 1629 K Street, N.W.
Washington, D.C. 20006

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REPLY TO SCHMALSTIEG AND KORTLANDT

JAY H. JASANOFF

Cornell University

I am grateful to Profs. Schmalstieg and Kortlandt for their comments on my article, and grateful to the editors of *JIES* for permitting me the opportunity to reply. The remarks below are addressed chiefly to the criticisms of Kortlandt. I shall not take systematic issue with Schmalstieg for two reasons: first, because I have nothing to add to Kortlandt's own able rebuttal of Schmalstieg's views; and second, because Schmalstieg's notion of what constitutes a historical explanation is so different from mine that I do not think it would be possible to answer his objections within a reasonable amount of space. I will say merely that I find the charge of writing like Meillet or Streitberg a curious form of censure; time will tell if I am mistaken in not preferring to write like Schmalstieg.

Kortlandt's comments are so rich in facts and opinions that it is easy to overlook the basic difference between us: K. believes that the IE gen. pl. ended in **-ōm*, while I assume a preform in **-om*. The evidence, I think, strongly favors this latter view, which is also the standard one. Only a trimoric long vowel can account for the disyllabic scansion of Ved. *-ām(-am)*, the circumflex accentuation of Grk. *-ōν*, and the preservation of *o*-quality in OHG *-o* (=Go. *-o* in feminines). The quantity and intonation of the **-o* are ambiguous in most of the other languages; substantial positive evidence for the short-vowel ending is confined to Balto-Slavic. I thus disagree with K's interpretation of the following forms:

1) Ved. *asmākam*, *yuṣmākam*. The *-m* is clearly secondary, as shown by the survival in the Rigveda of the older variants *asmāka* (1x), *yuṣmāka* (2x). Closely related are the adjectival stems *asmāka-*, *yuṣmāka-*, the petrified nom. -acc. not. forms of which have traditionally, and probably correctly, been taken as the source of the attested genitives.

2) OIr. *fer n-*, etc. K. claims that since OIr. dat. sg. *fiur* continues **uirōi*, a genl pl. **uirōn* (< *-om*) would have given OIr. **fiur n-*. This is a *non sequitur*: there is no reason why the shortening of **-ōn* to **-on* could not have predated the change of **-ōi* to *-ni* (> **-n̄*). It should be noted, moreover,

that *fjur* can as easily continue instr. sg. **wirō* as dat. sg. **wirōi*, so that the relevance of the dat. sg. for the form of the gen. pl. is altogether doubtful.

3) Go. *gaste*, *dage*, etc. K. takes *-e* [ē] from the *i*-stem ending **-eiom*, and composes the hypothetical lowering of *ei* [ī] to *e* with the lowering of **ē* to *ai* [ē] in *saian* 'sow'. But IE **-eiom* and **-iōm* would both have yielded pre-Gmc. **-ian*, which could only have given Go. *-ī* or *-i* (cf. acc. sg. *hardi* < **-iōm*); obvious counterexamples to K.'s lowering rule are provided by the class I weak verbs in *-jan* < **-jan(an)* and by the class III weak verb *fijan* (ptcp. *fijands*) 'hate'. The Gothic gen. pl. remains a mystery, and as such can shed no light on the corresponding ending in the parent language.

It is neither surprising nor interesting that V.'s account of the Slavic gen. pl. in *-b* is "simpler" than mine: the reason for his assumption of a preform in **-om* after all, is to explain this ending. The cost, however, is high. Not only must K. claim that Indo-Iranian, Greek and Germanic independently substituted **-o-om* (**-ōm*) for **-om* in thematic nouns; he must also assume that this ending was subsequently extended to all the other stem-classes, so that **-om* was completely lost even from the pronominal and consonant-stem declensions. This, for me, is so improbable a scenario that I can see no alternative to assuming **-ōm* at the outset, whatever the special problems posed by the appearance of *-b* in Slavic.

Once **-ōm* is recognized to be the IE ending, it is clear that the attested *-b* must either have arisen by irregular shortening and subsequent raising (**-ōm* > **-om* > **-un* > *-b*), or by raising and subsequent shortening (**-ōm* > **-un* > **-un* > *-b*), the main reason for favoring the latter sequence is the form of a second ending in Slavic — the *-y* of the nom. sg. of masculine *n*-stems. In theory **-ō*, **-ō̄*, **-om* and **-ōm* could all have served as the Balto-Slavic ancestor of this termination, but three of these possibilities are excluded on other grounds. The regular reflex of **-ō* is *-a*, as can be seen from the form of the nom.-acc. dual of *o*-stems; **-ōm* would presumably have fallen together with **-ōm*, which yielded *-b* < **-im* ex hypothesis; **-om*, however one interprets the *-q* of 1 sg. *vedq*, etc., would hardly have remained as **-u(n)* > *-y* while its circumflexed counterpart was shortened. This leaves **-ō̄*, which *pace* K., is also the likeliest source of the nom. sg. in *-uo* of *n*-stems in Lithuanian. The obvious inference

is that the raising of **-ō̄* to **-ū* was a regular change of final syllables, and that in the gen. pl. the shortening of **-ūm* to **-un* was a later development conditioned by the final nasal.

There are no convincing counterexamples to this rule. Despite K.'s assertion to the contrary, the Lith. *o*-stem gen. sg. in *o*-points most naturally to a preform in **-ā(d)* < **-o-h2ed*, so that the Slavic gen. sg. in *-a*, which presumably continues the same sequence, is of no relevance to the present discussion. Whether the Old Latin abl. ag. in *-ō(d)* is an analogical replacement of **-ā(d)* or the phonologically regular contraction product of an earlier disyllabic **oad* is impossible to determine.

Special problems, of course, arise in connection with the *o*-stem dat. sg. in *-u* < **-ōi* and inst. pl. in *-y* < **-ōis*. Both Schmalstieg and K. find my treatment of these endings *ad hoc* — an assessment which, though correct as far as it goes, is equally applicable to K.'s own proposal. In K.'s view, the history of *-u* and *-y* was determined by two non-trivial changes: first, a raising of **-ōis* to **-ūis* in the nom. pl. of *o*-stems (*prabī*); and second, an exceptional replacement of the partially rounded long diphthongs **ōi* and **ūi* by their fully rounded counterparts **ōu* and **ūu* (i.e., **ūu*). There is no need to discuss the independent merits of this analysis here. For our present purposes it is sufficient to note that, since K.'s rules and the subsequent shortening of **-ōū* to **ou* could all easily have preceded the general raising of **ō̄* to **ū*, nothing in the above account precludes or compromises the proposed derivation of *-y* and *-b* from **-ō̄* and **-ōm*, respectively. To the extent that K. has suggested a viable alternative theory for the development of **-ōi* and **-ōis*, the effect is merely to transfer the discussion back to the gen. pl. and the nom. sg. of *n*-stems, where it belongs.

The evidence for a change of **ō̄* to **ū* in the latter two case forms provides the chief motivation for my assumption of a parallel change of **ē̄* to **i* in *mati*, *dašti*. Despite the obvious difficulties presented by the form *tgde*' (assuming that the equation with Go. *þande* is correct), I continue to believe that a direct derivation of *dašti* from **duktē̄* (=Lith. *duktē̄*) offers significant advantages over K.'s view that *-i* is the phonological reflex of **-ēr*. The latter possibility has often been proposed, but apart from its phonetic unlikelihood, it is badly compromised both by the form of the corresponding Baltic ending and

by the apparent development of *-*ēn* to -*ę* in Slavic. K. offers *ad hoc* arguments of his own to deal with these objections, and it is obvious from his remarks that neither explanation can ever be completely proved or disproved on its own merits. Ultimately, the analysis of *mati*, *da-šiti* must depend on the interpretation of the endings in *-*ō-*, and here, I think, the evidence is quite clear.

A NOTE ON THE ENTITLEMENT OF OTHO

JOSEPH P. WILSON

University of Iowa

In their article on the Roman succession, Bush and McHugh (1) demonstrated that in the imperial period the principal heir to the imperial throne was the emperor's son-in-law or a person with a status reducible to that of a son-in-law or, in some cases, the son of an emperor's son-in-law. To explain this fact they postulated an attenuated form of *manus*, a power which made a man's wife the equivalent of his daughter, as being operative in the free marriages of the empire. (2)

In their article they quite rightly point out that Otho's entitlement is through his connection with Nero, not Balba. Unfortunately they base their case on the betrothal of Otho's daughter to Drusus, the brother of Agrippina who was the mother of Nero (3). This does serve as a link between the two men, but there is a more obvious connection between the two which fits in with the idea of an attenuated form of *manus*. Nero and Otho were both married to Poppaea Sabina. (4) By holding the same woman *in loco filiae* they achieved the same status equivalency which existed between a father-in-law and his son-in-law. This relationship provided Otho with the entitlement which he needed to claim the throne.

The passage in Tacitus (Ann. 13.46) in which the historian recounts Otho's loss of Poppaea to Nero seems to confirm both the presence of an attenuated form of *manus* in the free marriages of the imperial period and the status equivalency which results from the *manus*. Tacitus says "*Otho sine amore incantus laudare formam elegantiamque uxoris apud principem, sive ut accenderet ac, si eadem femina potirentur, id quoque vinculum potentiam ei adiceret . . .*" The phrase "*eadem femina poti-*

(1) Archie C. Bush and Joseph J. McHugh, "Succession to the Throne of Rome through 192 A.D.," *Journal of Indo-European Studies*, 2 No. 3 (1974), 259-277

(2) *Ibid.*, 260-1

(3) *Ibid.*, 272 n19

(4) Tacitus, *Annales* 13.45-46, *Historiae* 1.13. Suetonius alleges that Otho's marriage to Poppaea was a fiction, then seems to contradict himself; see Suetonius, *Otho* 3