Since the decipherment of the Linear B script by Michael Ventris in 1952, many books and papers have contributed to a better understanding of the Mycenaean texts and their cultural context. F. Aura Jorro has been able to critically review and systematize the richness and variety of the scholarship devoted to the interpretation of Mycenaean in his outstanding Diccionario micénico. This volume honours him with sixteen papers written by scholars working from different perspectives on the Linear B tablets, Mycenaean culture, and related fields. The papers collected in this book provide new insights into a number of various subjects: Linear B script and texts, Mycenaean grammar, lexicon and religion. Some of the papers tackle with open questions concerning the phonetic value of certain Linear B syllabograms, thus making a contribution towards a more refined understanding of this script. The function of particular types of texts, such as the ‘flat-based nodules’, is also dealt with in the volume. Various linguistic questions raised by the study of the Mycenaean texts are the focus of other papers, including certain aspects of Mycenaean grammar in comparison to the situation in 1st millennium Greek and its Indo-European background, as well as onomastics, lexicon, and word formation. Other papers are devoted to the interpretation of individual tablets, thus providing in-depth analyses of some texts. A number of papers deal with particular aspects of Mycenaean religion (gods, cults and rituals), and the volume also makes a contribution to the ongoing debate on the interpretation of the new Linear B texts from Thebes. Finally, some papers are concerned with the interaction of Linear B and Mycenaean with other languages and scripts and other questions of Aegean epigraphy. The volume will be of interest not only for Mycenologists and specialists in the Aegean cultures, but also for scholars working on the history of the Greek language, Greek religion and institutions, and Indo-European.
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*Anthroponymica mycenaea: e-ke-ra,-wo */En-kheriā-wōn/, *ἐγχειρία
y ἐγχειρέω ‘emprender’ (*’poner mano en’), ἐγχείρημα, ἐγχείρησις*
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Mycenaean -pi and pa-ro in the light of TH Uq 434

It is salutary that those of us who theorize about Mycenaean morphology and syntax be reminded from time to time that our grand theoretical edifices are often built on the foundations of the most slender evidence: foundations, indeed, which can be shaken by the discovery of even a single new tablet. TH Uq 434 is just such a tablet, whose publication in the proceedings of the Rome Colloquium (ARAVANTINOS et al. 2008) has much to tell us about the status of the instrumental case in Mycenaean Greek, as well as the preposition pa-ro.

**TH Uq 434**

```
1. pa-ro , te-qe-jo-i₄, qa-si-re-u-pi
2. pa-ro , qe-re-ro    e-te-wa  *152 1
3. pa-ro , o-to-ke-se-we , e-te-wa  *152 1
4. pa-ro , ri-wa-so , e-te-wa  *152 1
5. pa-ro , e-po-ro-jo , u-po-o-pa  *152 1
6. pa-ro , a-e-ri-qo , u-po-o-pa  *152 1
7. pa-ro , su-ko-pu-te-e  *152 1
8. pa-ro , ka-wq-jo , a-mu-ne-u-te  *152 1
9. pa-ro [          ] \j\e-u-pi , ka-[
10. pa-ro , *22-ka-ne , wa-ka-[\*]₄-t\e-we[  \]
11. pa-ro , a-pi-qo-ta[   ]\na-\ra-ro  [  
12. pa-ro , pa-[      ] ka-pa-ti-ja[  
13. pa-jo , [ ] wi-dwo-i-jo  *152[  \]
14. ]*152[  [  
```

inf. mut.

The text of TH Uq 434 as printed by its editors is reproduced above. In lines 2–8 each entry begins with the preposition pa-ro /paro/ ≈ Classical παρά followed by a man’s name, and ends with the ideogram *152, representing an ox hide (the ideogram is strikingly similar in shape to the modern ‘real leather’ logo, and is surcharged with the syllabic sign wi, probably standing for /wrīnos/ ‘ox hide’), and the numeral 1. The editors interpret the document as recording the contribution of these ox hides by the named individuals to the palace. Line 1 appears to function as a heading for lines 2–8 and to sum up their pa-ro formulae in the

---

* I am especially grateful to Lisa Bendall for her perceptive comments on an earlier draft of this paper. Its remaining flaws are the fault of the author.
phrase *pa-ro, te qa jo il, qa si re u pi /paro* Tēgaioihi  wasileuphi, ‘from the Theban *g* wasilewes’. (/g* wasileu/ is the equivalent of Classical βασιλεύς, but seemingly refers to a minor local official rather than a ‘king’, for which the Mycenaean term is *wa-na-ka /wanaks/ ≈ ἄβασις.) The ends of lines 9–14 are lost, but in .9 *pa-ro* seems to be followed by a plural form *je-u-πi*, and so the editors suggest, plausibly in my view, that this forms a second heading parallel to that in .1 which sums up the *pa-ro* formulae in lines 10–14.

§1. There are 248 instances of *pa-ro* in the corpus where the final sign of the prepositional argument is legible. In the vast majority of these the spelling is compatible with either dative or instrumental morphology: 22 × -Ca = a-stem dat. /-ai/ or instr. /-a/; 163 × -(C)o = o-stem dat. /-oi/ or instr. /-o/; 47 × -Ce = C-stem dat. /-ei/ or instr. /-e/. In a very small number of cases we see forms which can only be dative: *ka-ke-u-si in PY An 129.7* must be *kbalkeuisi/ ‘smiths’, the dat. plur. of an *ei*-stem; *po-so-pe-re-i in PY Cn 40.2* must be the dat. sg. in /-e-hi/ of an *s*-stem man’s name, /Posi* 'elehil uel sim.; *i-sa-na-o-ti in PY Cn 254.6* looks like the dat. sg. in /-i/ of a C-stem man’s name; and *e-re-u-te-ri in TH Av 100.4b* must be /ereuteri/ ‘inspector’. Prior to the publication of *TH Uq 434* there were by contrast no examples which had to be instrumental. Thus while it remained possible that some of the ambiguous spellings concealed instrumental forms, we might have felt justified to argue on the basis of the unambiguous forms that we were dealing with morphological dative in all instances. That those of us who did so argue were wrong is now shown clearly by *TH Uq 434* which contains two unambiguously instrumental forms: *qa si re u pi* in line 1 and *je-u-πi* in line 9.

The tablet contains another surprise, too. In line 7 the form *su-ko-pu2-te-e* looks like it ought to be an *s*-stem formation from a noun /sukopbutēs/ ‘fig-planter’, either a man’s name (ARAVANTINOS et al. 2008, 26), or possibly an occupational or official title. Now *s*-stem singular forms in -e, while common enough in place names (some ten different forms in -e-e are attested) are exceptionally rare in words denoting persons. Prior to the publication of *TH*
Uq 434 we had two examples: in MY Ge 604.1 we find ke-e-pe, plausibly argued to be an error for ke-pe-e, a datiuus incommodi governed by o-pe-ro /opʰelos/ ‘deficit’ of the man’s name ke-po known elsewhere in the Ge series; and we have ti-ri-se-ro-e as the recipient of olive oil (OLE) in PY Fr 1204 and a gold vessel (AUR *216VAS) in Tn 316.5. This could be the dative of a god’s name or epithet /Trishērōhei/ ‘the thrice-hero’, but it could formally be the plural of a nominative of rubric /Trishērōhes/. Our previous examples of personal s-stem -e were thus marginal and uncertain, but in su-ko-pu2-te-e we have a clear and unambiguous example: but is it dative in /-ehei/ or, now we know that pa-ro can govern the instrumental, instrumental in /-ehē/? A further question of wider significance is whether pa-ro governs different cases in its different senses, dative, presumably, when LOCATIVE and instrumental when ABLATIVE; and, if so, is this evidence of an ABLATIVE sense of the instrumental more generally, and therefore of a syncretism of instrumental-ablative on the one hand and dative-locative on the other?

§2. Before we examine the context of the examples of pa-ro where the governed case is clear, we must deal with a further startling anomaly of TH Uq 434. In line 1 the editors print pa-ro , te-qa-jo-iₐₙ qa-si-re-u-pi. If this reading is correct then the instrumental /gʷasileupʰi/ is qualified by a dative adjective /Tʰēgwaioihi/. Standard rules of concord would require either an instrumental adjective te-qa-jo /Tʰēgwaiois/ or dative noun qa-si-re-u-si /gʷasileusi/. If we assume scribal error is responsible for the failure of concord then the presence of /je-u-pi in line 9 suggests that te-qa-jo-i is the error and the scribe intended te-qa-jo: we are still left, in other words, with the deliberate construction of pa-ro + instrumental. In any case, we should only appeal to scribal error as a last resort; to explain an apparent failure of concord as an error on the part of the scribe is to take the arrogant (and patently false) position that we know more about Mycenaean syntax than the scribe himself. If we reject the idea of scribal error we are forced to conclude that the scribe intentionally construed a dative adjective with an instrumental noun. What does this tell us about the grammatical system?

In PY Jn 829, a record of bronze contributed by the officials of the sixteen towns of the two provinces of Pylos, two place names which are demonstrably instrumental, pa-ki-ja-pi in line 7 and e-ra-te-re-wa-pi in line 17 stand in parallel with the demonstrably dative-

---

2 Throughout this paper, in order to distinguish the labels of case forms from those of case functions, the latter are printed in SMALL CAPS.
locative *e-re-i* in line 19. This suggests a degree of overlap between the functions expressed by the instrumental and the dative-locative case forms.

In KN Ld(2) 787.B we find *pa-we-a₂* / *o-re-ne-ja* *161 ki-to-πi*. Now *pa-we-a₂* is clearly /pʰarweha/ ‘pieces of cloth’, and *o-re-ne-ja* an adjective in agreement with it, perhaps denoting some kind of decorative motif. The form *ki-to-πi* appears to be the instrumental plural of the noun /kʰitōn/ and has very plausibly been interpreted as meaning ‘for khitons’ (Milani 1958, 108; Doria 1968, 771-772; Killen 1979, 171). This would be an example of an instrumental form with final DATIVE function.

We thus have examples of instrumentals overlapping in function with dative-locative case forms, to the extent that the instrumental can apparently stand in a core DATIVE function, and this constitutes evidence at least of a partial functional syncretism between instrumental and locative case forms. We ought not, then, to be surprised if an adjective which was formally dative could qualify a noun which was formally instrumental. This might suggest that the syncretism was fairly advanced. We could imagine, for example, that elements of morphology which formerly expressed two or three distinct cases (dative-locative and instrumental; or dative, locative and instrumental) synchronically expressed one syncretic case, which we label ‘dative’ and which carried DATIVE, LOCATIVE and INSTRUMENTAL functions (and we could add ABLATIVE functions into the mix if we follow the hypothesis that the instrumental and ablative cases had already syncretised). Each set of morphs would be operating in free variation to express the same syncretic case, and eventually we would expect one member of each set to oust the others. This is exactly what happened in the later dialects, where the ‘dative’ case is expressed by morphs some of which have their origins in the IE dative, others in the IE locative, but which carry DATIVE, LOCATIVE and INSTRUMENTAL force.

If this is indeed the situation then the questions posed at the end of §1 become irrelevant: the scribe did not construe *pa-ro* here with an instrumental rather than a dative, but with a dative(-locative-instrumental) which here happens to be expressed by a morph which formerly belonged to the instrumental. Before we jump to this (I confess, to me, attractive) conclusion, however, we should observe that the reading *te qa jo i₃₃ qa si re u pi* is far from certain. Although that is their preferred reading, the editors admit that *te qa jo qa si re u pi* (i.e. both words instrumental) is also possible. What are their reasons for preferring the syntactically more difficult reading?
The reader is referred to the photograph of the tablet and Godart’s facsimile drawing in the original publication (ARAVANTINOS et al. 2008, 32-33). The first three signs of the second word of line 1 are clear, but the sign which the editors restore as \( \mathit{i} \) exists only as a small vertical mark which touches the horizontal rule and is slightly skewed to the top left. Although the editors concede that this may in fact be a word divider, they are firmly convinced that this reading should be rejected. The reason—and it is the same reason that they read the next sign, also surviving only as a vertical mark touching the rule, as a syllabogram—is their claim that the divider, where present, is on this tablet always significantly raised above the rule: any stroke which touches or crosses the rule must therefore belong to a syllabogram. In fact, from Godart’s drawing, this claim does not appear to be true. The first word divider in .2 separating \( \mathit{pa-ro} \) and \( \mathit{qe-re-ro} \) impacts the horizontal rule.

There are furthermore good reasons for wanting to see one of the two strokes between \( \mathit{jo} \) and \( \mathit{si} \) as a divider rather than a syllabogram. First, the only clear example where a divider is missing in this tablet is between the words \( \mathit{qe-re-ro} \) and \( \mathit{e-te-wa} \) in .2, but there is a large gap here, equivalent in width to the sign \( \mathit{we} \) of the line beneath. If one restores the \( \mathit{jo} \) and the proposed \( \mathit{i} \) and \( \mathit{qa} \) (using \( \mathit{i} \) and \( \mathit{jo} \) from .13, scaled to the same height as the signs in .1, and \( \mathit{qa} \) from earlier in .1, and using the existing traces as a guide to placement) it becomes apparent that, unless the ductus of these signs differed considerably from that of the remainder of the document, restoring both \( \mathit{i} \) and \( \mathit{qa} \) leaves no gap at all between \( \mathit{te-qa-jo-i} \) and \( \mathit{qa-si-re-u-pi} \). Rather, these words would be written as \textit{scriptio continua}. Furthermore, again, unless the ductus of these three signs was significantly different, whereas elsewhere signs are drawn neatly spaced from one another, these three would be so cramped as to be touching one another, or nearly so. Finally, the slope of the first of the two traces is pronounced, and resembles that of the divider between \( \mathit{pa-ro} \) and \( \mathit{e-po-ro-jo} \) in .5 rather than that of the vertical shaft of any of the syllabic signs. All of these anomalies can be removed by the reading \( \mathit{te-qa-jo} \), \( \mathit{qa-si-re-u-pi} \), i.e. /\( \mathit{Thē} \)gwaiois g\( \mathit{g} \)asileuph\( \mathit{i} \)/ with both adjective and noun instrumental plural.

This does not, of course, rule out the possibility that the dative-locative and instrumental had syncretised and that /\( \mathit{paro} \ldots \) g\( \mathit{g} \)asileuph\( \mathit{i} \)/ had the same force as /\( \mathit{paro} \ldots \) g\( \mathit{g} \)asileusi/ would have had. But it is the case that the scribe has, apparently, deliberately chosen instrumental morphology for both \( \mathit{pa-ro} \) formulae where the case is unambiguous. In the remaining instances the case morphology could be instrumental or dative-locative; but it
is curious that in line 7 he has used the rare -e-e ending in su-ko-pu₂-te-e which could be instrumental, rather than the common -e-i which could not. Is this choice significant? Can we, in other words, detect a difference in function when pa-ro is followed by unambiguous dative-locative and when it is followed by unambiguous instrumental morphology?

§3. It is generally held that pa-ro has two senses, one ABLATIVE ‘from X’, the other LOCATIVE ‘chez X, apud X’. In those instances where the case morphology following pa-ro is unambiguous, is there a correlation between morphology and sense?

If Aravantinos et al. are correct to interpret TH Uq 434 as a record of the contribution of ox hides from the named individuals to the palace, we have here two examples of unambiguous instrumental morphology (i.e. in qa-si-re-u-pi and lj-e-u-pi; su-ko-pu₂-te-e is ambiguous) in what must be ABLATIVE pa-ro constructions. They argue plausibly that the tablet is compiled from a number of sealings (Aravantinos et al. 2008, 27-29), and on sealings, which are documents intended to accompany contributions of goods being sent to the centre, the sense of pa-ro is highly likely to be ABLATIVE (Piteros et al. 1990, 177).

It is conceivable, I suppose, that the tablet might list hides which have been despatched from the centre to individuals for processing, and so record their current location (LOCATIVE sense) prior to their return to the palace; but this is difficult. The most obvious form that such a process might take would be tanning, but it seems very unlikely that single skins would be sent to individual tanners, and we might expect a reference to tanners in the ‘heading’ in line 1. Furthermore, if the editors are right to interpret u-po-o-pa in lines 5 and 6 as /hypo hopās/, ‘because of hopā’ (Aravantinos et al. 2008, 25), and if Killen is correct that /hopā/ denotes the ‘finishing’ or ‘renovation’ of existing goods rather than the production of new goods from raw materials, for which the term is ta-ra-si-ja /talansiā/ (Killen 1995), we would have to imagine that in lines 5 and 6 the ideogram *152 represented already-tanned leather undergoing finishing or renovation, while in the other entries it represented raw hide undergoing tanning.

There is plenty of evidence to link objects described as o-pa to storage records (Killen 1995, 331). KN Sd 4403, Sf 4420, Sd 4422 and So 4430 all record chariots and wheels, qualified by o-pa, which are in store. Sealing KN Ws 1704 has the ideogram JAC(ulum) on side .α and the noun pa-ta-ja /paltaia/ ‘darts’ on side .γ, with o-pa on .β, and was found in the arsenal in association with a box of arrows. It is quite plausible, then, that
**TH Uq 434** records hides in store, in which case the *pa-ro* formulae would have to refer to their contributors.

**KILLEN** (1995, 332-334) argues that in pastoral contexts /hopā/ denotes the process of fattening animals prior to sacrifice and/or consumption. He further suggests (1995, 335-336) that the sealing **PY Wr 1332** (below) might record the ‘skin of an animal which had been involved in an *o-pa* operation (i.e. … a fattening phase prior to slaughter/sacrifice), but which had died in the course of this operation, and whose skin is now being returned by the herder to the central authorities’, and he cites Ancient Near Eastern parallels for herders being required to return to their owners the body parts of animals which had died while in their charge under contract.

**PY Wr 1332**

\[
\begin{array}{ccc}
\alpha & WI & \\
\beta & o-pa & \\
\gamma & uacat & \\
\end{array}
\]

**TH Wu 76**

\[
\begin{array}{ccc}
\alpha & BOS & \\
\beta_1 & a-e-ri-qo & \\
\gamma & o-pa & 171 \ 30 \\
\beta_2 & uacat & \\
\end{array}
\]

If this is indeed correct, one possibility in **TH Uq 434** is that *e-po-ro-jo* and *a-e-ri-qo* have returned the skins of oxen who had died during the /hopā/ fattening procedure, and that these skins have been put together with ox hides contributed by the other individuals under other arrangements. And if **ARAVANTINOS ET AL.** (2008, 29) are correct that the *a-e-ri-qo* of **TH Wu 76** and the man in **Uq 434.6** are the same, we have evidence which might confirm that he was involved in the fattening of oxen under /hopā/ arrangements.

We now turn to other instances of unambiguous morphology following *pa-ro*.

**PY An 129**

(S129–H22)

\[
\begin{array}{l}
.1 \quad \text{[pa-ro, ti-ki-jo} \\
.2 \quad \text{a-ta-ro-we} \quad \text{VIR} \ 1 \\
.3 \quad \text{pe-re-wa-ta} \quad \text{VIR} \ 1 \\
.4 \quad \text{za-mi-jo, pu-ro-jo} \quad \text{VIR} \ 10 \\
.5 \quad \text{to-ro-wo, ri-na-ko-ro} \quad \text{VIR} \ 1 \\
.6 \quad \text{ka-nu-ta-jo, a-so-na} \quad \text{VIR} \ 1 \\
.7 \quad \text{pa-ro, ka-ke-u-si,} \\
.8 \quad \text{we-ro-ta} \quad \text{VIR} \\
.9-10 \quad \text{uacant}
\end{array}
\]
PY An 129 lists 15 men under the headings pa-ro, ti-ki-jo and pa-ro, ka-ke-u-si. The former phrase contains a man’s name, the latter the dative plural /kʰalleusī/ ‘smiths’. There are further connections with the bronze industry: An 340, in the same hand and stylus, lists at least 19 men under the heading pa-ro, a-ta-o. In Jn 431 a-ta-o is the name of an /atalansios kʰalkeus/, and two of the names listed under him in An 340, wo-ti-jo in line 8 and a-no-ra-ta in line 12, appear as those of /atalansioi kʰalkekēwes/ in Jn 832. In An 129 five of the men are listed individually, but ten are grouped together under the designation za-mi-jo, pu-ro-jo. Whether or not za-mi-jo represents /zāmioi/ ‘forced levies’  ueI sim. (cf. Classical ζημία ‘penalty’), pu-ro-jo is surely the genitive of the place name Pylos. Two of the other entries have words parallel with pu-ro-jo: the man’s name to-ro-wo in line 5 is qualified by ri-na-ko-ro, and ka-nu-ta-jo in line 6 by a-so-na, which may be place names or occupational titles (Ventrīs – Chadwick 1972, 535, 580).

In line 4 pu-ro-jo can scarcely be the destination to which the za-mi-jo are being sent, both because the allative sense is inappropriate to the genitive case, and because no destination is given for a-ta-ro-we, pe-re-wa-ta or we-ro-ta in lines 2, 3 and 8. We cannot assume an implicit ‘Pylos’ in these entries, because then the explicit pu-ro-jo in line 4 would be unmotivated. Presumably, then, the men in line 4 are the za-mi-jo ‘of, i.e. from, Pylos’. The same considerations apply to ri-na-ko-ro and a-so-na if these are place names.

The two pa-ro formulae could indicate where the men listed are currently located, or by whom they have been sent. If the latter, their destination or current location is not recorded, so we would assume Pylos itself. But if so, what sense does pu-ro-jo make in line 4? The evidence is circumstantial, but suggests that the locative interpretation of the pa-ro formulae is the more likely.

PY Cn 40

| .1  | wa-no-jo , wo-wo , pa-ro , ne-ti-ja-no-re , pa-ra-jo | OVISm 140 |
| .2  | wa-no-jo , wo-wo , pa-ro , po-so-pe-re-i , wo-ne-we | OVISm 75 |
| .3  | wa-no-jo , wo-wo , pa-ro , zo-wi-jo , a-ko-so-ta-o | OVISm 70 |
| .4  | wa-no-jo , wo-wo , pa-ῳ , po-ru-qo-ta , we-da-ne-wo | OVISm 60 |
| .5  | e-ko-me-no , pa-ro , pa-ta , pa-ra-jo | OVISm 80 |
| .6  | e-ko-me-no , pa-ῳ , [ jma-te-we , we-da-ne-wo | OVISm 70 |
| .7  | a-ne-u-te , pa-ro , ma-ri-ti-wi-jo , a-ko-so-ta-o | OVISm 83 |
| .8  | ma-ro-pi , pa-ῳ , ro-ko , pa-ra-jo | OVISm 150 |
| .9  | ma-ro-pi , pa-oro , ka-da-ro , we-da-ne-wo | OVISm 85 |
| .10 | ma-ro , pa-ro , tu-ri-ta , a-ke-o-jo | OVISm 80 |
.11 re-pe-u-ri-jo , pa-ro , e-zo-wo , a[-ko-so-]tə-ọ
.12 ma-ro , pa-ro , ma-u-ti-jo , a-ko-ṣọ-ta-o
.13 a-ne-u-te , pa-ro , ka-ta-wa , a-ko-so-ta
.14 a-te-re-wi-ja , pa-ro , e-wi-te-we , a-ke-ojo
.15–16 uacant

PY Cn 599

(S4–H21; S719-H1 in .8)

.1a pa-ro

.1 wa-no-jo , wo-wo , ne-ti-ja-no a-ke-o-jo
.2 a2-ne-u-te , pa-ro , ka-so , a-ko-so-ta-o
.3 a2-pa-ti-wo-te , pa-ro , a-ke-ra-wo , a-ke-o-jo
.4 a2-pa-tu-wo-te , pa-ro , ru-we-ta , a-ke-o-jo
.5 a2-pa-tu-wo-te , pa-ro , a-wo-i-jo
.6 wa-no-jo , wo-wo , pa-ro , ke-re-no , a-ke-o-jo
.7 a2-pa-tu-wo-te , pa-ro , e-zo-wo
.8 e-ko-me-no , pa-ro , ti-ri-po-di-ko

PY Cn 600

(S4–H21)

.1 o-re-e-wo , wo-wo , pa-ro , ke-we-no
.2 o-re-e-wo , wo-wo , de-kọ-to
.3 o-re-e-wo , wo-wo , ke-ro-u-te-u
.4 o-re-e-wo , wo-wo , e-te-re-ọ
.5a -jo
.5 o[re-e]wo , wo-wo , mi-ka-ri-jo , a-ke-o-
.6 re-qa-se-wo , wo-wo , wa-ra-wo-ọọ , ọ-ke-o-jo
.7 ti-mi-to , a-ke-e , te-[ ]ovis♂ 80
.8 ti-mi-to , a-ke-e , a[ ]-u , we-da-ne-wo
.9 re-qa-se-wo , wo-wo , ka-wi-ta
.10 re-qa-se-wo , wo-wo , wi-ja-te-we
.11 ti-mi-to , a-ke-e , o-pe-se-to , a-ke-o-jo
.12 ti-mi-to , a-ke-e , e-te-wa-jo
.13 ti-mi-to , a-ke-e , a-no-ze-we
.14 ti-mi-to , a-ke-e , qe-ta-ko
.15 ti-mi-to , a-ke-e , *82-de[ ]

PY Cn 254

(S4–H21)

.1a pa-ra-jo

.1 a-ṣi[-ja-ti-ja pa-]rọ , tu-ru-we-u ,
.2 a-si-ja-ti-ja , pa-ro , ti-tu[ ]ovis♂ 180

*Mathematical notation*
PY Cn 40, 599 form a single document recording flocks and herds of single sex and species associated with various places in the Hither Province. Cn 600, 254 are a comparable document, in the same hand and stylus, for the Further Province. Animals are listed in the order OVIS\textsuperscript{m}, OVIS\textsuperscript{f}, CAP\textsuperscript{m}, CAP\textsuperscript{f}, SUS\textsuperscript{m}, SUS\textsuperscript{f} (although there are no SUS\textsuperscript{m} on Cn 40, 599). Each entry begins with a place name followed by the name of a herdsman, usually after the preposition pa-ro (in lines 2–15 of Cn 600 the preposition is missing, and at least one of the names is demonstrably nominative). The entry ends with the animal ideogram and a numeral. Between the herdsman’s name and the ideogram there may appear (i) nothing; (ii) the name of a ‘collector’ (usually) in the genitive case (e.g. a-ko-so-ta-o in Cn 40.3); (iii) the adjective pa-ra-jo /palaioi/ ‘old, last year’s’ describing the animals; or (iv) the term wo-ne-we, which perhaps stands for /wornēwes/ ‘lambs’. KILLEN (1984, 51) observes that these entries are similar in structure to those of the Knossos Da–Dg series, which he demonstrates record standing flocks involved in the production of lambs and wool (KILLEN 1964); mutatis mutandis he argues for a similar function for these Pylian records. If so, the pa-ro formulae would record the herdsmen under whose care the animals currently are, at the various places listed; and the sense of the pa-ro formulae, and of the place names, would be LOCATIVE. The majority of the herdsmen’s names are of indeterminate dat.-loc. or instr. case, but po-so-pe-re-i in Cn 40.2 is an unambiguous s-stem dat.-loc., and i-sa-na-o-ti in Cn 254.6 is an unambiguous C-stem dat.-loc. We may note en passant, however, that one of the place names (ma-ro-pi in Cn 40.8–9) is instrumental,\(^3\) and another (ti-mi-to , a-ke-e in Cn 600.11–15) is an s-stem form in -e-e which could be instrumental.

\(^3\) cf. RUIGH (1967, 368-369); the form ma-ro in Cn 40.10, 12 is surely nominative of rubric, rather than an abbreviation of ma-ro-pi, pace Chadwick (VENTRIS – CHADWICK 1972, 559). As Ruijgh notes, we would be
Mycenaean -pi and pa-ro in the light of TH Uq 434

TH Av 100

supra mutila

.1 ] uestigia
.2 ] po-te-we , si-to , ku-na-ki-si GRA 2 V 2 Z 2
.3 ] so , / si-to GRA 3
.4a ] VIR 1 MUL 1
.4b ] no pa-ro , zo-wa , e-re-u-te-ri
.5 ] wi-ri-ne-u VIR 1

TH Av 101

supra mutila

.1 ] uest.[
.2 ] da-ro VIR 1 uest.[
.3 ] po-me-ne VIR 2 da[
.4 ] a-ko-da-mo VIR 2 T 6 V[]
.5 ]V 2 ma-di-je T 6 V 4 ko-ru-we T 2[;
.6a ] ku-su-to-ro-qa
.6b ]-te / si-]/ to-pa-pa-ro-i[;

KILLEN (1999, 219; 2001, 442-443) has argued that the Thebes Av tablets of hand 304, including Av 100, 101 above, record ‘allocations of foodstuffs to persons participating in religious festivals’, and are thus similar to the Fn series from Pylos. The reasons seem compelling. First, the amount (presumably of barley, HORD) allocated to ma-di-je on Av 101.5, viz. T 6 V 4, is the most common quantity allocated on PY Fn 79, which records allocations of HORD for those attending a five-day festival. Then, several of the names and occupational terms on this pair of tablets are associated with cult. An /ereutēr/ ‘inspector’ is found on PY Cn 3 ‘in the context of the supply of oxen, almost certainly for sacrifice’. The names ma-di(-je), a-ko-da-mo and ko-ru recur on the Thebes Fq tablets, which are also records of allocations of cereals in connexion with religious festivals. The occupational term to-pa-po-ro-i[ ‘can plausibly be identified as a cult title’, /torpāpʰoroih/ ‘basket-carriers’, for which we can compare the classical κανηφόροι. And it is possible that the /poimenes/ ‘shepherds’ and /wrīneus/ ‘tanner, leather-worker’ were directly involved in the supply of animals for sacrifice and the subsequent processing of the hides of the victims.

dealing with an alternation between a singular and a plural form, as indeed with pa-ki-ja-na vs. pa-ki-ja-ne, pa-ki-ja-si, pa-ki-ja-pi.
If Killen’s interpretation is correct, and I believe that it is, then the context here is one of disbursements from the palace, not of contributions to it; in Av 100.4b the phrase pa-ro, zo-wa, e-re-u-te-ri, in which the final noun is unambiguously a C-stem dat.-loc., is most likely then to record the current location of the woman (MUL) in line 4a, and possibly the tanner in line 6, perhaps for the purpose of allocating their rations.

§4. In those instances where the morphology following pa-ro is unambiguous we therefore see the distribution given below (Table 1). Although we cannot be completely certain about the sense involved, it does appear that there is a correlation between ablative sense and instrumental morphology on the one hand, and locative sense and dative-locative morphology on the other.

<table>
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<td>pa-ro, i-sa-na-o-ti</td>
<td>—</td>
</tr>
<tr>
<td>TH Av 100</td>
<td>LOCATIVE</td>
<td>pa-ro ... e-re-u-te-ri</td>
<td>—</td>
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Table 1. Sense and unambiguous morphology in pa-ro phrases.

Before the publication of TH Uq 434 there was no reason to suppose that more than one case form followed pa-ro, whatever its sense. Although in the majority of instances the spelling was compatible with either instr. or dat.-loc. morphology, all of the unambiguous instances were clearly dat.-loc. On that basis I have argued elsewhere (THOMPSON 2000), following MORPURGO DAVIES (1966), that the use of pa-ro + dat.-loc. with ablative sense is similar to the construal of ablative prepositions with the dative in Arcado-Cypriot, and, in fact, that the Arcado-Cypriot phenomenon may constitute an isogloss inherited from Mycenaean or a close relative. In Arcado-Cypriot the ablative use of the dative is restricted to prepositional constructions. In other constructions Arcado-Cypriot, like the other dialects, uses the genitive with ablative force. I therefore argued that, like other dialects, both Arcado-Cypriot and Mycenaean had syncretised the inherited ablative case with the genitive and not, as some have supposed, with the instrumental. (In Arcado-Cypriot, so the other argument goes, the instrumental’s subsequent syncretism with the dat.-loc. causes the
ABLATIVE force of the dative; but the use of the genitive with ABLATIVE force in non-prepositional constructions tells against this.) Now, however, as the table above shows, we apparently have evidence not only for a difference of case following *pa-ro* which correlates with a difference of sense, but also of clear ABLATIVE use of the instrumental. On the face of it, then, we have evidence which counters the analysis of THOMPSON (2000) and supports those who argue for a complete syncretism of instrumental and ablative in Mycenaean.

Before accepting this, however, we must be cautious. The evidence on which it is based is slight indeed. In fact, it rests solely on TH Uq 434. The discovery of a single tablet with LOCATIVE sense but instrumental morphology, or with ABLATIVE sense but dative-locative morphology would render it instantly invalid, and because the number of clear instances which we currently possess is too small to allow us to conduct statistical analyses of significance, we cannot be confident that such a tablet will not come to light. But future discoveries aside, this slender evidence in favour of an ablative-instrumental syncretism faces more serious and more immediate problems, because it ignores the counter-evidence which suggests a syncretism of instrumental with dative-locative already discussed in §2 above. And as MORPURGO DAVIES (1985, 100) has noted, if there is an alternative *o*-stem gen. sg. in -ο/-ό/ it would have to continue the inherited abl. *-ōt*, and this would entail a syncretism of ablative with genitive (as we would expect from comparison with every other Greek dialect) rather than with instrumental. But much more seriously—fataliy, I think—the ablative-instrumental theory is in fact internally inconsistent.

Consider PY Cn 40, 254. Here *pa-ro* + dat.-loc. expressions stand after place names which are themselves of indeterminate morphology (*wa-no-jo*, *wo-wo* and *a-si-ja-ti-ja*) but which stand in parallel with unambiguously instrumental *ma-ro-pi* (Cn 40.8.9). Now, if the instrumental-ablative theory is correct *ma-ro-pi* and the other place names must have ABLATIVE force, and so must the *pa-ro* formulae. But in that case we have examples of explicit dat.-loc. morphology with ABLATIVE force and the clear correlation between morphology and force, and thus the reason for supposing an instrumental-ablative syncretism, is lost. If, on the other hand, *pa-ro* + dat.-loc. is LOCATIVE, then *ma-ro-pi* too must be LOCATIVE and, again, there cannot be an instrumental- ablative syncretism.

I can see only one coherent solution which resolves this dilemma, and that is the one presented in THOMPSON (2000). In summary, the instrumental and dative-locative had begun to syncretise such that while elements of instrumental morphology remained in use (and were perhaps still the only exponents of true INSTRUMENTAL-COMITATIVE and ORNATIVE force)
they could also express functions which properly belonged to the dative-locative. Now, in non-prepositional contexts the dative-locative (and thus also instrumental) expressed LOCATIVE force; but, by a simplification of the case government properties of prepositions identical to (perhaps even the ancestor of) that which resulted in Arcado-Cypriot ABLATIVE prepositions governing the dative (and similar to that which in Attic resulted in some prepositions which originally governed three cases dropping the dative and using the genitive even in LOCATIVE function), Mycenaean pa-ro + dative-locative (and thus also instrumental) could have either LOCATIVE or ABLATIVE force. Thus on TH Uq 434 pa-ro + instrumental has ABLATIVE force, not because the instrumental per se has ABLATIVE force but because the syncretic dative-locative-instrumental can have ABLATIVE force after prepositions; and although this tablet at first sight provides evidence in favour of an instrumental-ablative vs. dative-locative split, in fact it supports only a dative-locative-instrumental syncretism. Nor should this come as any surprise, because this is exactly the same syncretism we observe in all of the other Greek dialects, including Arcado-Cypriot.

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