Some Manuscripts of Dionysius the PeriegeT

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With admirable dedication, Isavella Tsavari has collated 134 manuscripts of Dionysius the PeriegeT, analysed their relationships in a monograph of 456 pages, and reported in an edition readings from all 44 manuscripts older than the 15th century.¹ Both works give a full stemma at the end. When I reviewed them, however, I found her method of analysis unsatisfactory and the connexion between her stemma and her text opaque.²

Revising Dr. Tsavari's conclusions might seem to require almost as much collation as she carried out herself, less because she made mistakes, though I shall correct some below, than because even in conjunction her two works seldom bring the evidence in particular passages sufficiently into view. Besides reporting no manuscripts later than the 14th century, the edition mostly passes over readings that offend against sense or metre (p. 23); at 147, for instance, it passes over the omission of καὶ πολλάν by νκλατ7ημv2, which she reports at least three times in the monograph (pp. 259, 346, 391). The monograph itself could not have been expected to serve as an apparatus, and indexing passages would have taken a long time; but finding relevant evidence is made harder by her occasional failure to mention things in all the appropriate places, as when she mentions in her analysis (p. 401) but not in her description (pp. 138–39) that N₁ omits 375, or only once that F shares the omission of καὶ πολλάν in 147 (p. 370). In some manuscripts of the 15th or 16th century, moreover, she collated only 1–100, 550–650, 1000–1100 (p. 22), and she does not indicate which they were.

¹ Histoire du texte de la Description de la terre de Denys le PériégeT (Ioannina 1990); Διονυσίου ΄Αλεξανδρέως Οίκουμένης περίγραφες (Ioannina 1990).
² CR 41 (1991) 306–09. In a long rejoinder, Διονυσιακή 1 (1992) 53–75, Dr. Tsavari accuses me of ὑπερωπία, σύγχυση, ἀνακρίβεια, and most unpleasantly of all κακοσμία. Three errors I apologize for, all on p. 307: as she says (pp. 55, 58, 63), I wrote ψ₂ instead of ψ₂₂, W₁ originally had the order of A at 506–12, and “le manuscrit δ” should have been “le manuscrit ο” (the slip occurred in printing, but evidently I failed to spot it in the proofs). None of these, however, affects the substance of my objections, which she has quite failed to answer; and I admit no others, whether of fact or of logic. Rather than defend myself in detail, which I have done by letter without receiving a reply, I will try to break new ground.
Nevertheless, the information that she has provided sometimes allows a different conclusion. For the moment I will confine myself to four areas of her stemma, one early and editorially important, the others late and doubtless unimportant. In the first I use no information of my own; in the second, very little except about printed editions; in the third and fourth, only enough to confirm suspicions already formed.

About the wider context I need only say that apart from A (s. x) and its descendant V9 she derives all the manuscripts from one lost source, Ω3, through four lost descendants, bdφψ, and that she regards the family of Ω3 as riddled with contamination, not least from A.3

\[ b_2 \]

In the family of b Dr. Tsavari postulates 14 lost intermediaries, from b1 to b14. This is her stemma for the seven extant descendants of b2 (p. 275):

Three of these manuscripts are the oldest after A: B (Paris gr. 2771, s. x/xi), m5 (Moscow Syn. gr. 30, s. xi), W1 (Wolfenbüttel Gud. gr. 46, s. xi).

How well has she defined b14, the common source of B and m5 (pp. 270–71)? In the monograph she cites no separative errors of B where m5 is present (278–350, 470–524). Though in her apparatus she ascribes to B γαρ for πᾶσαν at 300, she does not mention this reading in the monograph (p.

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3 I take the opportunity of mentioning two things about A, both of them unconnected with my arguments below and the second unconnected even with anything that Dr. Tsavari has written. First, in my review ([previous note] 309) I said "at 576–8 I find it hard to believe that A omitted ἐριβρεμέτη Διονύσω," but Dr. Tsavari declares that it παραλείπει πράγματι these words ([previous note] 73–74). I said "omitted," not "omits." I have now inspected A, and 576 ἐριβρομον Εἰρασίαν is in rasura. What stood there before if not ἐριβρεμέτη Διονύσω? Second and more important, 705–17 in their first appearance, after 664, begin not with κείνων but with ἀφεσων, over which a corrector wrote the κεῖ of κείνων. In the exemplar of A, therefore, or a remoter ancestor, ἀφες, "drop," must have been an instruction written above κείνων, and so the scribe of A itself, who mistook it for a correction of κεῖ, cannot have been responsible for the transposition.
In any case, she did not collate $m_5$ herself: her reports of its readings go back to a rare *Programm* published by Matthaei in 1788.

How well has she defined $b_2$, the common source of $b_{14}$ and $W_1$ (pp. 242–43)? She says that $W_1$ abandons $b$ for $\psi$ round about line 385 (p. 225 n. 606 and elsewhere), but in her edition she substitutes 350 for 385 (pp. 32–33). Why not 256? Up to that point the only differences between $B$ and $W_1$ that I can find either in the monograph or in the edition are the separative errors of $W_1$ that she lists in the monograph (p. 270); after that, agreements of $W_1\mu$ against $B$ are common ($\mu$ is the oldest manuscript that she derives from $\psi$ throughout). Where $W_1$ descends from $b$, therefore, I see no reason why it should not do so through $B$.

Provisionally, then, I propose the abolition of both $b_{14}$ and $b_2$. That leaves no intermediary between $B$ and $b$.

$\psi_{26}$

In the family of $\psi$ Dr. Tsavari postulates 32 lost intermediaries, from $\psi_1$ to $\psi_{32}$. This is her stemma for the seven extant descendants of $\psi_{26}$ (p. 415):

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\begin{verbatim}
       \psi_{26}
       / \     \\
  \psi_{27} /  \psi_{28}
 /     \     /   \\
\psi_{29}   \psi_{30}   \kappa_4
 / \   /   \\
U   P   Q
//   //   //
N_1 = Naples Naz. III.E.27 V_{20} = Vat. Ross. 895
U = Paris gr. 2731 V_{12} = Vat. Ottob. gr. 335
P = Paris gr. 3023 \kappa_4 = Bodl. Rawl. G.95
Q = Paris Supp. gr. 36
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N_1 she puts in the 15th century, UPQV_{20}V_{12} in the 16th, \kappa_4 in the 17th. According to her descriptions, V_{12} bears the date 1527 and \kappa_4 the date 1655–56; she accepts the attribution of U and Q to Constantine Palaeocappa and reports Diller's attribution of P to Iacovos Diassorinos.

The evidence that she cites for her stemma appears adequate except in respect of Q and \kappa_4, whose descent from $\psi_{26}$ and its ancestors $\psi_{24}$, $\psi_{22}$, and $\psi_2$, she hardly establishes (pp. 406, 405, 401–02, 360). Her excuse that they

\footnote{According to the *National Union Catalog* CCCLXIX (1975) 575, no. 0339600, there is a copy at Harvard.}
are contaminated and often desert ψ₂₂ (p. 401 n. 749) plays down the fact that neither shares any of the 15 readings by which she defines ψ₂₂. It seems that once she had derived them together with UP from ψ₂₇ she was determined to persevere.

Five of the seven manuscripts reappear in her account of the printed editions (pp. 425–38). There she connects the editio princeps (Ferrara 1512) with ψ₂₆ and says that it shares errors now with P, now with V₂₀, now with PV₂₀, now with PV₂₀N₁V₁₂, and “ne semble avoir servi d’antipagre à aucun manuscrit conservé de la Périégèse, ainsi qu’il ressort des fautes séparatives qu’elle présente.”

I have pleaded elsewhere against separating early printed editions from manuscripts.⁵ In her introduction Dr. Tsavari promises to treat the editio princeps and the Aldine “comme si elles étaient de véritables manuscrits” and remarks that “un intérêt spécial que présentent ces éditions, c’est de voir si l’on peut retrouver en elles les ascendants de certains manuscrits conservés du XVIᵉ siècle” (p. 21); but in the event she dismisses the possibility too lightly, and her analysis of the earlier editions is quite inadequate. Besides the editio princeps, four of these will concern me here: the Aldine (Venice 1513), the edition printed by Tiletanus (Paris 1538), Robertus Stephanus’s edition (Paris 1547), and Henricus Stephanus’s edition in Poetae Graeci princeps heroici carminis (Geneva 1566).

Dr. Tsavari says that the Aldine corrected some obvious errors of the editio princeps and also drew on ψ₂₆ for a reading found in V₂₀, 1074 Σούτων for Σοῦσων. Apart from this single agreement with V₂₀, however, she offers no evidence that it is anything more than a reprint of the editio princeps, with some proofreading but with new misprints; and her notion that that reading of V₂₀ already occurred in ψ₂₆ conflicts with her stemma. Surely the reading in question, 1074 Σούτων for Σοῦσων, originated as a misprint in the Aldine itself.

Whoever prepared the edition printed by Tiletanus (Paris 1538) started from an earlier edition, she says, but claims to have improved the text innumeris locis by collating a codex vetustissimus. In her edition (pp. 20–21) she describes Robertus Stephanus’s edition as the first after the editio princeps to use manuscripts; but if she doubts the claim made by Tiletanus’s editor, she cannot have collated even a few lines. Incidentally, she also seems to have forgotten her own view that the Aldine editor consulted ψ₂₆.

Robertus Stephanus, she says, followed Tiletanus but “doit avoir utilisé des manuscrits, par ex. le manuscrit Q, qui doit être de quelques années antérieur à son édition et dont celle-ci répète des fautes.” Certainly he often diverges from the editio princeps and the Aldine, and so does Tiletanus; but each diverges in his own way. Between them, they drove out many

readings of $\psi_{26}$ and its ancestors, but Tiletanus’s editor did it by using his codex vetustissimus, Stephanus presumably by using the manuscripts from which he compiled his appendix of variants.

Robertus Stephanus’s son Henricus overhauled the text by drawing on the appendix of variants. Starred variants in the margin provide a ready way of identifying his interventions. Incidentally, he deserves the credit that I gave Papius for numbering the verses. I mentioned Poetae Graeci principes heroici carminis in this connexion, but it had escaped me that that was precisely where he published his text of Dionysius.

These developments in editions are reflected in some of the manuscripts that Dr. Tsavari derives from $\psi_{26}$. Nicholas Lloyd (1630–1680) wrote $\kappa_4$ in 1655–56, first as a scholar of Wadham College, Oxford, and then as a fellow. That he wrote it abroad is neither attested nor likely, and no manuscript at all close to it is known to have been in England, let alone Oxford, at that date. So late a manuscript can be assumed anyway to derive from a printed edition in default of evidence to the contrary. Its source was an edition no older than Henricus Stephanus’s, where at 33 είνεκα first took the place of οὐνεκα in a printed text. Dr. Tsavari twice implies, correctly, that $\kappa_4$ reads είνεκα (pp. 406, 409).

Q at 33 reads οὐνεκα (ibid.), hardly a separative error, as Dr. Tsavari calls it, if it occurs in UPV $\psi_6 N_1 V_1$. In fact Q departs less than $\kappa_4$ from $\psi_{26}$, for the simple reason that it has a close connexion with an earlier edition, Robertus Stephanus’s. All her 11 errors of Q$\kappa_4$ (p. 408) occur there. What then is the connexion between Q and Stephanus’s edition? Checked against both Stephanus’s edition and Tiletanus’s, such information as Dr. Tsavari gives about Q suggests that, far from generally following Tiletanus but occasionally Q, Stephanus hardly diverges from Q. Only four of the 11 errors just mentioned had already occurred in Tiletanus’s edition, and by comparison with previous editions two of them are not errors anyway: 234 ἐπειρήσαντο, 302 νέμονται. Whether the work of collation seen in Stephanus’s edition left its mark independently on Q and the edition, or rather on one by way of the other, I cannot say without collating both. The scribe of Q, Constantine Palaeocappa, wrote it at Paris, and as he arrived there at an undetermined date that may well have been closer to 1552, when he wrote out a catalogue of the royal library at Fontainebleau, than to 1542, when he left Athos, no weight can be put on Dr. Tsavari’s assertion that Q must antedate the edition. In the belief that Stephanus’s edition of Eustathius’s commentary rested on another manuscript written by

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8 E. Gamillscheg and D. Harlfinger, Repertorium der griechischen Kopisten 800–1600 1A (Vienna 1981) 126, no. 225. I do not know who explained away the evidence on which some older works place his death in 1551.
Palaeocappa (U), Diller once asked, “Was Palaeocappa an editor for Stephanus?”, but he later abandoned the belief. I have inspected Q but not had time to collate it, and for the moment I will only say that no reading cited by Dr. Tsavari or noticed by me prevents it from being a copy of Stephanus’s edition. Even if it is not, however, it must be a contaminated descendant of the *editio princeps*.

Despite objecting a moment ago, therefore, I have come out accepting Dr. Tsavari’s derivation of Q and κα from ψ26. They are by no means the only manuscripts, incidentally, that above the lowest levels of descent show few signs of belonging to any of the families in which she places them. V22 (Vat. gr. 121, s. xiii2) provides a striking example, σ (Ambros. G 56 sup., s. xiv1) up to about line 450 another. I doubt whether the explanation is always the same.

A word here about A7 (Athens Nat. 3003) and A5 (Athens Univ., Seminar of Byz. and Mod. Greek 25), which she excludes from her classification. After calling them copies of editions (p. 225 n. 606), she decides that they are just eclectic (pp. 430–31, 456). Coming from someone who has gritted her teeth through all the contamination and classified everything else, this admission of defeat takes one by surprise, especially when both manuscripts are preserved in the same place. Is that how the Greek provinces stand up to their capital? Be that as it may, A7 bears the date 1574 and shares errors with Robertus Stephanus’s edition, from which Diller derives it in Eustathius’s commentary. It will surely turn out to be a copy of an edition after all. A5 she assigns to the 15th century but otherwise veils in mystery, and so I venture no prediction.

In discussing the Aldine above I argued that 1074 Σούτων for Σούσων originated there as a misprint. If so, V20, which has the same reading, should derive from the Aldine. The list of separative errors that she gives for this manuscript includes 1079 ἀλλήλων for ἀλλήλωι (p. 410), and that too, as she mentions in her description of the third edition (Basel 1522), is a reading of the Aldine. I can find in her pages only one reading that prevents V20 from being a copy of the Aldine: 679 Τάναξιν ποταμόν in that order (p. 407, by implication). In fact, however, it reads ποταμόν Τάναξιν. She mentions in her description that one of its watermarks closely resembles one attested in 1524–28 (pp. 191–92). On inspecting the manuscript up to line 460, I found that it has several errors inherited by the Aldine from the *editio princeps*, for instance 33 οὕνεκα, 87 νένευκες, 132 περιβρέχοντας, 169 κυανουγέως, 245 ἁμφοτέροις, 321 νότου, 328 τε for τις, 343 [τε], 363 ὅσῃ, 364 κεῖνο, παραφαίνετο, but not 33 νηκρόν, 396 αὐχή, 404


10 *Textual Tradition* (previous note) 204.

11 Silvia Rizzo very kindly checked for me before I could see the manuscript myself.
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πλατάνοι, 443 ἀπειρεσίην, 452 ἀπ', 460 ἐκεῖνει. A reader of the Aldine could surely have corrected these errors without recourse to a manuscript.

Dr. Tsavari implies that N₁, like V₂₀, has Τάναξίν ποταμόν in 679 and not ποταμόν Τάναξίν with the editio princeps and UPQκ₄. Again, however, she is wrong.¹² In order, therefore, to derive N₁ and its alleged descendant V₁₂ from the editio princeps, one need only move N₁ from the 15th century and suppose that it corrected the misprints of the editio princeps that she reports from U, P, or the Aldine. As I have not seen it, however, this breezy assertion should be taken only as a challenge.

U and P cannot derive entirely from an edition, if only because they incorporate in their text the four lines added after 214 by V₁₂ and several relatives (pp. 151–52, 161–62); but the editio princeps surely underlies them. P shares with it ὑπὲρ for ὑπὲρ in 598, σερμήκετος for σερμήκετος in 599,¹³ and ἐπὶ θεῖα for ἐπὶ θεῖα in 612, and UP share with it ἀλέκοντας for ἀλέγοντας in 210, νήσος for νήσοι in 457, and σιδήροι for σιδήρῳ in 476. Most of these readings look like misprints. I have already mentioned that U, like Q, was written by Constantine Palaeocappa, and no doubt P is equally late, whether or not written by his associate Ιακωβος Διασορίνος. Both appear in the Fontainebleau catalogue of 1550.¹⁴

A further argument applies equally to all the descendants of ψ₂₆: the eight readings by which Dr. Tsavari defines ψ₂₆ (p. 406) include two that could well have originated as misprints, 33 νηκρόν for νεκρόν and 132 περιβρέται for περιβρέμεται.

Once ψ₂₆ has been reduced to the editio princeps, its relationship to Y and ψ₂₅ must be reassessed. This is the relevant part of her stemma:

She herself, however, describes Y (Paris gr. 2854) as a contaminated descendant of ψ₂₂ (p. 401 n. 749), and it may not be a coincidence that ψ₂₆ and ψ₂₅ both omit 375. In any event, the earliest descendants of ψ₂₂ all descend from ψ₂₅: one was written in 1468 by Antonios Damilas and two by

¹² Albio Cassio very kindly checked for me.
¹³ Dr. Tsavari says that the Aldine corrected this (p. 428), but the copy that I consulted, Cambridge U. L. Sel. 6.36, has σερμήκετος.
¹⁴ Diller, Textual Tradition (above, note 9) 203–04.
Michael Apostolios, whose activity cannot be traced after 1474.\textsuperscript{15} Zacharias Callierges wrote \( Y \) \( \varepsilon \nu \gamma \eta \rho \alpha \omega \varsigma \omicron \upsilon \delta \omicron \varepsilon \nu \) '\( \Pi \omega \mu \eta \) and so not before 1515;\textsuperscript{16} from it in 1523 \( \varepsilon \nu \) '\( \Pi \omega \mu \eta \) (p. 185) he copied \( V_{21} \) (Vat. Ottob. gr. 193). Though I have collated \( Y \), however, I cannot at the moment see a way through the contamination that lies behind it.

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d_3
\]

In the family of \( d \), much the largest, Dr. Tsavari postulates 42 lost intermediaries, from \( d_1 \) to \( d_{42} \). These are the upper levels of her stemma:

\[
\begin{align*}
  &d \\
  &\downarrow \\
  &d_1 \\
  &\downarrow \\
  &d_3 \quad d_4 \\
  &\downarrow \\
  &d_2
\end{align*}
\]

Whereas the family of \( d_2 \) includes \( V_{16} \) (s. xiii/xiv) and the family of \( d_4 \) several manuscripts of similar age, the family of \( d_3 \) does not emerge until the end of s. xv. Dr. Tsavari delineates it as follows (p. 289):

\[
\begin{align*}
  &d_3 \quad d_5 \\
  &\downarrow \\
  &\Omega_3 \\
  &\downarrow \\
  &H_7 \quad I \quad \beta \quad R_1 \\
  &\downarrow \\
  &\downarrow \\
  &\downarrow \\
  &\downarrow \\
  &\downarrow \\
  &K \\
  &\downarrow \\
  &\Pi_1
\end{align*}
\]


\textsuperscript{16} E. Mioni, Dizionario biografico degli italiani XVI (1973) 750–53; E. Gamillscheg and D. Harlfinger, JÖB 27 (1978) 306–07.
I have collated three of these manuscripts: \( \kappa_3 \) (Bodl. Auct. F.4.5), l (Cambridge U. L. Kk.6.29), and E\(_1\) (Eton 146).

There are objections to Dr. Tsavari's stemma. First, she has not established the existence of \( \delta_3 \), because five of the six errors by which she defines it (p. 282) also occur in \( \kappa_3 \), three of them in the text (5, 39, 1024) and two as variants (638, 1033). Second, over half the errors by which she defines \( d^* \) (p. 280) are absent from l. Third, \( \kappa_3 \), l, and E\(_1\) often disagree, and in many such passages both readings are attested elsewhere (I give first the reading that Dr. Tsavari prints in her edition):

At first sight, these readings suggest that Dr. Tsavari is wrong to connect the three manuscripts. On the other hand, both \( \kappa_3 \) and l have numerous variants or corrections, of which those in \( \kappa_3 \) tend to agree with l or E\(_1\) and those in l with \( \kappa_3 \) or E\(_1\):

So it continues through the poem. Subsequently I inspected three further manuscripts that Dr. Tsavari assigns to the family of \( d_3 \), namely \( \beta \) (Vat. Pal. gr. 319), K (Paris gr. 1411), and z (Vat. Pal. gr. 154), and found that they too drew variants from the same stock as \( \kappa_3 \), l, and E\(_1\); adding their evidence
in detail here would serve little purpose. These variants suggest that if related after all, as Dr. Tsavari holds, the six manuscripts share a corrected ancestor, a possibility that will also account for the disagreements in the first list.

A manuscript that appears to meet the conditions for being that corrected ancestor is lurking in the family of d₄, namely S₂ (Escorial Σ.II.7, s. xv). One of its ancestors in the family of d₄ was d₁₁, whose errors included 271 [Λιβόης], 358 ἀγνὸν for ἀγνης, 418 Λυκαόνων for Λακωνων, 1087 παρ’ for πρὸς (pp. 292–93). E₁ omits Λιβόης and κ₃ expunges it; κ₃IE₁ read ἀγνὸν; E₁ reads Λυκαόνων; and παρ’ appears in the text of l and E₁ and as a correction in κ₃. If one follows S₂ down through the family of d₄ (pp. 290–98), it almost always turns out to have been corrected, and in listing the errors of d₃ Dr. Tsavari reports that several occur in S₂, usually as variants but twice in the text: 78 <τ’> Ἄδσονης, 1019 Ἀτρακατηνοῖ.

Obviously not much would need to be wrong with Dr. Tsavari’s collations for my hypothesis to be reversed and the corrections in S₂ to be derived from d₃. I also know nothing about the date of the corrections, which go unmentioned in her description (p. 110). Nevertheless, my hypothesis not only provides κ₃IE₁βKz with a suitable ancestor but also does away with the implausible independence of so late a family.

This is Dr. Tsavari’s stemma for the 15 extant descendants of d₃₅, a manuscript notable for adding after 214 four lines about African rivers (pp. 344, 329):
Apart from $V_{16}$, no member of the family antedates the second half of the 15th century.

Apostolios and Callierges, mentioned above on $\psi_{26}$, meet again in the family of $d_{37}$: Apostolios wrote $s_3$ and most of $H_5$, Callierges $p$, most of $L_1$, and the rest of $H_5$. Now not only did Callierges according to Dr. Tsavari make two copies of $d_{37}$, namely $p$ and $H_5$, but according to Diller he corrected the text of Eustathius’s commentary in $V_{16}$.\footnote{Textual Tradition (above, note 9) 185, 202. Dr. Tsavari (above, note 2) 68–69 objects that $V_{16}$ belongs not to s. xv as Diller said but to s. xiii/xiv. Her logic baffles me, and anyway it was the text of Dionysius, not of Eustathius’s commentary, that Diller assigned to s. xv.} As it seemed to me an unlikely coincidence that in so broad a tradition he should have encountered both $V_{16}$ and one of its closest relatives, I decided to test Dr. Tsavari’s stemma by inspecting $p$ and $H_5$, which both happened to be within reach. I inspected $H_5$ first.

In $H_5$ Callierges wrote only the bifolium ff. 1 + 8. When? Another Cretan, George Trivizias, wrote $u_2$ (pp. 196–97), and his death, mentioned in a papal bull of June 4th 1485,\footnote{G. S. Ploumides, Θησαυρίσματα 7 (1970) 236–37; cf. P. D. Mastrodemetres, Θησαυρίσματα 8 (1971) 59.} provides a terminus ante quem for $s_3$ and $H_5$ if that part of Dr. Tsavari’s stemma holds. The terminus ante quem is earlier if Apostolios himself wrote no manuscripts after 1474. Callierges first appears in 1499.\footnote{Reperitorium (above, note 8) 80.} I therefore suggest that the bifolium in $H_5$ was a later replacement. My attempts at proving or disproving this suggestion by peering through the paper of ff. 1–8 came to nothing.

$H_5$ has lost before f. 42 the three leaves that contained 1088–1166. Dr. Tsavari does not mention this in her description (p. 121), but she does mention twice the omission of 1088–1166 (p. 333 n. 703, p. 456). Were its descendants copied from it before or after the loss? Surely before: as $H_5$ has 13 lines to a page and 1088–1166 make 79 lines, it must have omitted a line, and so it cannot be a coincidence that 1091 is missing from the manuscripts that she regards as descendants of it.

On collating $H_5$, I found that it shares many of the errors by which she defines $d_{39}$ (p. 338): 34 άλλα (ante corr.), 92 εύρυνθείσαν (ante corr.), 140 ίδον, 241 άγνωκα (ante corr.), 431 ήπο (μετά mg.; according to her edition, $V_{16}$ also has ήπο), 875 Λυρνησσός τε, 1186 είν άνταξιος. It also omits 1184 μὲν with $d_{39}$, whether or not the error goes back to $V_{16}$ as she rightly says it may, and in 518 reads δ’ Άσιν, an error that she reports from both

\begin{itemize}
  \item $p$ = Bodl. Holkham gr. 85
  \item $H_5$ = B. L. Harl. 1814
  \item $s_3$ = Escorial R.I.6
  \item $u_2$ = Rome Casanat. 424
  \item $L_1$ = Leiden B. P. G. 74F
  \item $V_{16}$ = Vat. gr. 1910
\end{itemize}
V₁₆ and d₃⁹ (p. 333). If she is right, therefore, to derive d₃⁹ from V₁₆, H₅ too should derive from V₁₆, and it should take the whole family of d₃⁷ with it.

As a first test of this conclusion, I collated p. Dr. Tsavari cites only four errors of H₅ that p avoids: 472 εἰνεμόσσα for ἕνεμόσσα, 670 ὀπόταν for ὀπόταν, 842 Διονύσσιο for Διονύσσιο, 1071 ποταμὸν for ποταμοῖ. By implication, a fifth is the omission of 1091, which she does not report from p. I found that before correction p read εἰνεμόσσα and ποταμοῖ and omitted 1091. Its readings in the other two passages, Διονύσσιο and ὀπόταν, are mere matters of spelling and prove nothing. Of the passages cited in the last paragraph, it agrees with H₅ everywhere but at 34 and 241. Plainly it derives from H₅. Where H₅ is missing, it shared with d₃⁹ before correction 1140 τῆς for τοῖς. Incidentally, the single letters that she reports as absent from it (p. 334) all begin lines and are present, written in red; they must have failed to show up on microfilm.

Perhaps, then, all the other descendants of d₃⁵ derive from V₁₆. Agreement has not been reached about its date, but the view accepted by Dr. Tsavari makes it easily the oldest member of the family (s. xiii/xiv). Many of the errors by which she defines V₁₆ + d₃⁹ as a family have been corrected in V₁₆ (pp. 333–34).

Four complications will have to be taken into account when my hypothesis is put through further tests. First, as I have said, ff. 1 + 8 of H₅ seem to be a replacement, and so the original text of H₅ in 1–25 and 182–207 may need to be reconstructed. Second, it certainly needs to be reconstructed in 1088–1166. Third, V₁₆ has lost everything after 1056, and manuscripts that derive from it up to that point may not derive from it after that point. Fourth, the descendants of d₃⁹ omit 1082–1113.

My provisional conclusions about these four areas of Dr. Tsavari’s stemma lead me to suspect that anyone who did all her work again might achieve very different results. How such results might affect the editing of Dionysius I do not know, because apart from expressing trust in A (Paris Supp. gr. 388, s. x) she does not explain how her own results affect it.

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