

On the Transmission of the Greek Lexica

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THE OBSERVATIONS that follow are inspired by the appearance of K. Alpers' *Das attizistische Lexikon des Oros* (Berlin 1981). In this important edition it is made clear that patient sifting of the manuscript evidence can continue to produce valuable dividends, which not only are of use to the specialist concerned with the ancient lexica but add to our knowledge of earlier Greek literature. The first two of my notes are intended to cast doubt on what is now becoming an increasingly widely held view: most scholars emphasize that many of the extant copies of the lexica were not written in Constantinople or the main centres of the Byzantine empire but in the provincial areas of southern Italy and Sicily. I hope to show that statements of this kind are not as well founded on palaeographical or other evidence as might be supposed by the uninitiated. In the other notes I offer a palaeographer's modest contribution to certain other problems connected with the transmission of the lexica.

1. *Barocci 50*

Barocci 50 is a volume of interest to many scholars, since it contains a large miscellany of texts, among which are the oldest copy of Musaeus' *Hero and Leander*, the *Batrachomyomachia*, and grammatical texts such as the *Canons* of Theognostus and the *Orthographia* of Choeroboscus. The date is generally agreed: it belongs in the tenth century, although there has been some difference of opinion whether it should be placed early or late in that period. A more serious difference of opinion concerns the origin of the book. The majority of scholars seem now to favour the view that it was produced in the Italo-Greek area.¹ There are however several strong reasons for thinking otherwise.

¹ The doxography: in favour of Italo-Greek origin are J. Irigoin, *JOB* 18 (1969) 50–51 (=D. Harlfinger, ed., *Griechische Kodikologie und Textüberlieferung* [Darmstadt 1980] 247); he is explicitly followed by P. Lemerle, *Le premier humanisme byzantin* (Paris 1971) 253 n.30; G. Cavallo, *Scrittura e Civiltà* 4 (1980) 168–70. The opposite view, with varying degrees of dissent and reservation, has been expressed by R. Browning, *Byzantion* 33 (1963) 291 (also in *Studies on Byzantine History, Literature and Education* [London 1977]); N. G. Wilson, *Mediaeval Greek Bookhands* (Cambridge

I deal first with a topic on which I am not qualified to pronounce an independent opinion, the decorative ornament. This has been described and studied with care by Dr I. Hutter, who concluded that there is no evidence to make an Italo-Greek origin likely. She has kindly confirmed for me that she still adheres to this view and hopes to return to the subject in the near future.

The second type of argument is based on the contents. One of the authors found in the manuscript, Theognostus (early ninth century), also wrote a history of Sicily. It is true that compositions of a very parochial nature can serve as a pointer to the origin of a manuscript; and a case in point is the preservation of poems by Nicholas of Otranto in the Vienna Aristaenetus (*Phil.gr.* 310). But in the present case I do not think the argument has any weight, for events in Sicily must be seen as having more than merely parochial significance for Byzantine intellectuals. Nor is there much in the idea that Italian provenance is made more likely by the inclusion of the words "in Italy" in the title of the last item in a series of short poems. If anything these words point in the opposite direction; a scribe writing in Italy would not have used them. But there is a further refinement here: a scribe living in Sicily might possibly use them. The historical arguments for and against origin in the island must therefore be discussed. The Arab conquest began in 827 but was not completed until 965 when the last outpost of Greek resistance fell. The demographic consequences were not so great that Greek ceased to be spoken, but it has been noted that some of the Greek monks in Italy in the tenth century are Sicilians by origin.² It is conceivable that a Greek manuscript might be produced in Sicily at that date. On the other hand the likelihood of such a rich collection of literary and scholarly texts being compiled either under Arab rule or in the last stages of resistance against the invader must be reckoned slight.

Thirdly we have to deal with palaeographical features. One that has been frequently mentioned is the ligature of *epsilon* and *rho* known as the 'ace-of-spades'. This is obviously consistent with Italo-Greek origin, but the question is whether it amounts to a proof. There is evidence to suggest that its use was not confined to one region. The following examples are known to me: *Barocci* 26 (ninth century), which there is no reason to place in the western part of the Byzantine world; the newly discovered fragments in St Catherine's monastery

[Mass.] 1972) 16; I. Hutter, *Corpus der byzantinischen Miniaturenhandschriften* I (Stuttgart 1977) 15–16; R. Barbour, *Greek Literary Hands* (Oxford 1981) 27.

² Irigoin (*supra* n.1) n.11.

on Sinai, which are not likely to derive from the west;³ *Vat.gr.* 504 (dated 1105), which seems to come from Athos or the eastern provinces.⁴ Another palaeographical feature is the short note in tachygraphy on folio 7^v. Since this type of tachygraphy is almost always referred to as Basilian or Italo-Greek, it is easy to jump to conclusions. The latest and comprehensive study, however, leaves the impression that there was in fact some knowledge of the system in other areas.⁵

In sum, there seems no compelling argument for a western attribution of *Barocci* 50 with its grammatical and lexical texts.

2. Manuscripts of the *Etymologica*

The two leading witnesses of the *Etymologicum Genuinum* are *Vat.gr.* 1818 and *Laur. San Marco* 304. In a recent edition they are attributed to southern Italy.⁶ The only evidence adduced is a palaeographical detail in *San Marco* 304, the pattern of lines ruled to guide the script; the authority of J. Irigoin is cited in support of this proposition. He had argued that the ruling pattern in question confirmed the Italian origin of a different manuscript, the *Stobaeus* in Vienna.⁷ I have serious doubts about the validity of this argument. Very unusual ruling patterns might of course be characteristic of one scribe or centre of book production, but the extremely simple pattern in question is one of the commonest, as a recent survey has shown.⁸ That being so, more evidence is required, and my own re-examination of some specimen pages of the two manuscripts has found nothing to suggest a provincial origin for either. This expression of opinion is not meant to cast doubt on the view that later in the middle ages *Vat.gr.* 1818 was the property of a reader who added marginalia in a hand typical of the Terra d'Otranto.⁹ The question at issue at present is the origin of the books.

The position is very much the same with regard to the leading witness for the *Etymologicum Gudianum*, *Barb.gr.* 70. Once again Las-

³ L. Politis, *Scriptorium* 34 (1980) 5–17 with plate 9d.

⁴ C. De Vocht, *Byzantion* 51 (1981) 628–30.

⁵ N. P. Chionides and S. Lilla, *La brachigrafia italo-bizantina* (Studi e Testi 290 [1981]) 42–44.

⁶ F. Lasserre and N. Livadaras, *Etymologicum magnum genuinum* I (Rome 1976) vi–vii.

⁷ Irigoin (*supra* n.1) 50 n.49.

⁸ J. Leroy, *Les types de réglure des manuscrits grecs* (Paris 1976) appendix 2. His type 00C1 = Lake's type I 1c.

⁹ P. Canart, *Codices Vaticani graeci: codices 1745–1962* II (Vatican City 1973) xxxix, and *Les Vaticani graeci 1745–1962* (Studi e Testi 284 [1979]) 79–81.

serre and Livadaras nail their colours to the mast by describing it as “scriptura Hydruntinorum stilo subsimili exarati.”¹⁰ This is either not true or not proven. The most that can be said is that A. Jacob has associated the book with some others that come from the Terra d’Otranto and may have been written there.¹¹ In fact his statements indicate that he is putting forward a hypothesis with due caution and that the Barberini codex exhibits some features not found in the other books mentioned. Unfortunately he does not go into detail, and at this stage of the debate I can do no more than record my feeling that the case for Italo-Greek origin has still to be made.

This is perhaps the appropriate place to observe that the attribution of *Vat.gr.* 1708 to the Italo-Greek area is dubious. The book is not of any great importance except that it is an early copy of what Reitzenstein called the lost *codex Cretensis*. A. Colonna drew attention to it as containing an additional source of the *Etymologicum Genuinum* and asserted without any hesitation that it is an Italian product.¹² This view was not upheld by C. Giannelli when he came to describe it for the printed catalogue.¹³ Although the parchment is of poor quality and there is some use of yellow wash to make the lemmata more prominent, the hand itself does not include features that suggest origin in an Italian milieu. It is essentially a neat and well controlled script, in which the *kappa* is slightly enlarged. The question of origin must remain open. A date in the twelfth century is generally agreed.

I must conclude that a western origin of certain important witnesses to the lexica is unproven.

3. The Transmission of Hesychius

The origin of the unique manuscript of Hesychius (*Marcianus gr.* 622) is a problem. K. Latte in his edition pointed out that the interpolations from the so-called *Onomasticon sacrum* of *Vat.gr.* 1456 suggest an origin in the Italo-Greek area, because the Vatican manuscript is clearly a product of that region, as had been shown by G. Mercati.¹⁴ A complication has been added by the discovery of O. L. Smith that the *Marcianus* of Hesychius was written by the same scribe as the manuscript containing Demetrius Triclinius’ recension of eight plays of Aristophanes with scholia (*Oxford Holkham gr.* 88).¹⁵

¹⁰ *Supra* n.6: vii.

¹¹ In J. Bompaire and J. Irigoien, *La paléographie grecque et byzantine* (Paris 1977) 270.

¹² *BPEC* 13 (1965) 9–13.

¹³ *Codices Vaticani graeci: codices 1688–1744* (Vatican City 1961) 69–70.

¹⁴ *RhM* 65 (1910) 332.

¹⁵ *Maia* 27 (1975) 205.

The date of the two books is *ca* 1430, as is shown by the watermarks in the paper. The script, however, shows no sign whatever of Italo-Greek origin, and the hypothesis that it is the work of a Byzantine refugee in Italy suffers from the chronological objection that very few of the refugees had established themselves in the west by that date. There are examples, such as Girard of Patras and Peter the Cretan, who worked at Mantua, but they are the exception rather than the rule.¹⁶

It would be too much to suppose that the unknown scribe's transcription of Triclinius' work can serve as a pointer to the provenance of the Hesychius manuscript. But curiously enough this far-fetched idea receives support from a piece of evidence that has not so far been correctly interpreted. Hesychius appears to be mentioned only once by later Byzantine scholars, in a scholium on Aristophanes' *Clouds* 540, known from a late Cambridge manuscript (Ct2, University Library ms. 2626 = Nn. iii 15, part 2 66^r). Although the hypothesis and scholia to each play are ascribed in the manuscript to Thomas Magister, when Latte collated other copies of Thomas' recension of Aristophanes he failed to find this scholium.¹⁷ The mystery is explained when we realise that the manuscript contains the Triclinian recension, with the usual acknowledgement to Thomas. There are Triclinian metrical notes and metrical signs, and other marks of Triclinian work such as the recommendation at *Frogs* 342 that the word *τινάσσω* be deleted. Investigation shows that the scholium mentioning Hesychius occurs in other famous representatives of the Triclinian recension, *Holkham gr.* 88 and *Vat.gr.* 1294.¹⁸ Does this mean that Triclinius' search for old manuscripts was rewarded by the find of a Hesychius? His operations are not known to have extended beyond Salonica and the capital.

4. The *Codex Galeanus* of Photius' *Lexicon*

Since the Cambridge Photius codex remains of great importance even after the discovery of the complete copy at Zavorda, it is perhaps worth while making two palaeographical observations about it.

First of all the date. The generally received opinion is that it was written in the eleventh or twelfth century, probably at a date not far

¹⁶ I have published a provisional study of Girard in *RHT* 4 (1974) 139–42; one may now consult the entries in E. Gamillscheg and D. Harlfinger, *Repertorium der griechischen Kopisten* IA (Vienna 1981).

¹⁷ K. Latte, *Hesychii Alexandrini Lexicon* I (Copenhagen 1953) xxi.

¹⁸ The late scholia on the *Clouds* have now been edited by W. J. W. Koster, *Scholia recentiora in Nubes* (Groningen 1974).

removed from the year 1100.¹⁹ I should be inclined to place it early rather than late. None of the numerous scribal hands needs to be brought down to a date as late as *ca* 1100; the middle of the eleventh century is more likely to be correct.

Secondly a curious palaeographical puzzle. On six folios there is a siglum in the margin which is clearly an abbreviated word beginning with *lambda*. The abbreviation is not identical in each case. In four of the first five occurrences (28^r, 35^r, 38^r, 39^r) it is seen in its fullest form, with *lambda*, *epsilon*, *omicron*, and *tau*. The basic form is:

The lemmata to which it is applied are *καινόφιλον*, *κατεχορδεύθη*, *κεστόν*, *κηραίνει*, *κημᾶν*. The last two examples are less explicit, because the scribe no longer needed to give so much detail. On 36^v he writes *lambda* with only *epsilon* and *tau* beside the entry for *κενοφροσύνη*, and on 64^r he gives only *lambda* and *epsilon* by the entry for *μηρεύεται*. To the palaeographer who possesses some experience of abbreviations and monograms the natural interpretation of the siglum is *Λέοντος*. The difficulty is that I cannot think of any person called Leo who plays a part in the history of the Greek lexica, and Dr K. Alpers has kindly confirmed for me that he finds exactly the same difficulty. Nevertheless the palaeographical fact, or what looks like one, should be stated in the hope that an explanation will eventually be found.

5. A Quotation from Diogenian

K. Latte in the preface to his edition of Hesychius stated that Diogenian appears not to have been used in the seventh and eighth centuries: “per saecula VII/VIII. qui Diogenianum adhibuerit, neminem novi.”²⁰ A point which escaped his notice is that there is a fragment in the scholia on letter 9 of pseudo-Dionysius the Areopagite (Migne, *PG* 4.569). Traditionally this corpus of scholia is attributed to Maximus the Confessor (580–660), but recently it has been suggested that they are for the most part the work of John of Scythopolis, composed probably between 532 and 548.²¹ On checking the text in a few manuscripts that happen to be readily accessible in Oxford I find that three of them add a detail of some interest. In *E. D. Clarke* 37, *Canonici gr.* 97, and *Lincoln College gr.* 14 the refer-

¹⁹ K. Tzantzanoglou, *Tò Λεξικὸν τοῦ Φωτίου* (Ἑλληνικά 17 [Thessaloniki 1967]) 38; cf. K. Alpers, *BZ* 64 (1971) 72.

²⁰ Latte (*supra* n.17) xliv.

²¹ B. R. Suchla, *NachrGöttingen* 1980.3, 33–66.

ence to the source is given in the form οὕτω φησι Διογενιανὸς ἐν τῇ Λέξει, whereas the existing printed text omits the last three words. If the latest proposal about the date of the scholia wins general acceptance, Latte's position remains intact. Even so it is worth recording a small find.

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