

THE CAESAREAN TEXT

'The Caesarean Text of the Gospel of Mark', by Kirsopp Lake and R. P. Blake and Mrs S. New, is the title of a reprint from the *Harvard Theological Review* (vol. xxi, Oct. 1928), which in size and importance is a regular book (pp. 207-404 = 198 pp.). The volume is prefaced by a Table of Contents, which is really needed, so many and so complicated are the subjects handled.

The Caesarean text may be defined as the text of the Gospels as read at Caesarea in the time of Origen. It had been noticed, particularly by Canon Streeter in his book *The Four Gospels*, that in some of Origen's works written at Caesarea in Palestine the Gospel quotations have affinity with variant readings found in certain MSS and versions. An approximate list is:

Codd. Θ-565, 1 &c., 13-69-124 &c., 28, 700; the Georgian, the Armenian. Allied in various ways are: the Old Syriac, codex W, the Palestinian Syriac Lectionary; more occasionally, some Old Latin codices.¹

The editors first collect all the variants for Mark i, vi, xi, in tables; then the singular readings of the various authorities are considered (pp. 213-257). Next follows an analysis of the quotations from Mark in Origen (pp. 258-277), and in Eusebius (pp. 277-285). The Georgian Version and its history is discussed, pp. 286-307 and 358-375: the main point is that this version was made from the Armenian before the Armenian had been revised from the Greek (p. 307). But from what was the Armenian version originally made? This is discussed, pp. 307-312: the editors sum up on p. 310. There we read in the same paragraph: (1) 'it is certain that the original Armenian was a strongly Caesarean text', and (2) 'we are justified in accepting the otherwise more probable view that the Armenian is based on the Syriac'. So far as this judgement is a rejection of F. Macler's theories about the Armenian, I have nothing to say against it, but if we take the propositions (1) and (2) together, must we not conclude that the Old Syriac is a strongly Caesarean text? I shall return to this point later, but it is clear that, if it be accepted, 'Caesarean' becomes only another name for 'non-Byzantine Eastern text[s]'.¹

¹ In this list W is the Washington or Freer Codex (5th cent.), Θ the Koridethi Codex from Tephric, east of Siwas in Armenia (9th cent.), 565 is called by Tischendorf 2^{pe}.

There follows a section on Syriac Versions, first on the Palestinian (or 'Jerusalem') Syriac Lectionary, to which I shall return, and then there is a section called 'Conclusion': on p. 324 is a *stemma codicum*, which will at least shew how complicated is the matter dealt with, as indeed the Conclusion itself points out.

Four valuable Excursuses conclude the work. The Second Excursus, on the Georgian, has been already referred to. The Third is a temperate appeal by Mrs New for reconsidering the date and character of the Harclean version. The theory that more or less holds the field at present is that the version of the four minor Catholic Epistles, commonly printed with the Peshitta and critically edited by Dr Gwynn, belongs to the true Philoxenian, as also does Gwynn's text of the Apocalypse: otherwise the version is lost, or may never have existed. The Harclean Syriac, on the other hand, with its odd over-literal imitations of Greek idiom, is the work of Thomas of Heraclea in the year 616, who also collated some Greek MSS and put some of their readings in the margin. Mrs New brings some interesting arguments to suggest that the 'Harclean' as edited by White *is* the Philoxenian, and that Thomas of Heraclea's part was confined to the various readings in the margin and the asterisks and critical notes. It may be asked whether anything like the style of the 'Harclean' was ever seen in Syriac so early as 550: it seems to me a little difficult to separate the work of Thomas of Heraclea from that of his contemporary Paul of Tella, who produced the Syro-Hexaplaric version of the Old Testament while in exile, like Thomas, near Alexandria. And if the 'Harclean' be the 'Philoxenian' only slightly revised, it will be necessary to date Gwynn's minor Catholic Epistles still earlier, for in those Epistles, as in the Apocalypse, the Harclean as edited by White is based on a text like that originally discovered by Pococke and edited by Gwynn.

The Fourth Excursus is a tentative reconstruction of the 'Caesarean' text of Mark i, vi, and xi, which it was well worth while to make, as it gives the reader an opportunity of seeing what is the general style of that text, as compared with familiar entities like the 'Received Text' or Westcott and Hort.

Excursus I is not for the general reader, but it is most instructive to the worker. It is an attempt to exhibit the actual variants found in the *K*-text of Mark xi among the MSS of Sinai, Patmos, and Jerusalem (pp. 349-357). It may perhaps be not out of place to explain why Professor Lake thought it worth while 'to do so much to learn so little' (p. 345, note). What von Soden called the *K*-text is variously known as the *κοινή*, the Constantinopolitan or Byzantine, and the 'Textus Receptus'. A further name given by Lake to one branch of it, defined as 'the most popular text in MSS of the 10th to the 14th century'

(p. 340), is the 'Ecclesiastical' text. The *K*-text has had few defenders since the late Dean Burgon as representing the apostolic autographs, but in one form or another it was the dominant text in the Middle Ages: if an ancient or aberrant codex were being corrected in late times it would be to the *K*-type that it would be more or less assimilated. That, in fact, is the method adopted in the reconstruction of the archetype of the 'Ferrar-Group', and also of the Caesarean text: the extant MSS are compared with the *K*-text, and the reading which disagrees with the *K*-text is taken to be the reading of the special group. But which type of *K*-text should be taken as the standard? The 'Received Text' as printed by Stephanus and Elzevir was not a bad representation of *K*, but in quite a number of instances it had an eccentric reading, and in a number more it had been corrected by Erasmus to agree with good MSS and so, though perhaps nearer the 'true' text, it did not represent *K* correctly.¹

Professor Lake's very full collation of Mk. xi—over 50 MSS at Sinai, 20 at Patmos, over 20 at Jerusalem—makes several things clear. In the first place von Soden's recensions or groups of *K* (called by him *K*^a, *K*^x, *K*^r, &c.) do not emerge. 'There is extraordinarily little evidence of close family relationship between MSS even in the same library. They have essentially the same text with a large amount of sporadic variation' (p. 341). It may be added that this 'sporadic variation' consists of small graphical errors, such as the omission of words by single MSS or small changes of tense or order: that is to say, they are the sort of variations made by scribes who are copying a text of which they understand the grammatical meaning.²

But the collation also shews that what Lake calls 'the Ecclesiastical text' is a real entity. Not that it is a 'recension' properly so called, i. e. a conscious edition of the text: it is rather of the nature of a composite photograph, an average from which no member of the group differs very much. Its value to modern scholars is that it *is* the average mediaeval Greek text: the differences of any given MS or group of MSS from this 'Ecclesiastical' average text are significant for characterizing the textual character of the MS or group. Lake points out on p. 340 the almost inevitable errors into which von Soden's collaborators fell by collating their single MSS on a 'good' modern critical text instead of using this average text—or, failing that, its near relative the *Textus Receptus*.

¹ Examples are: 'daily' in Lk. ix 23 is omitted by most Greek MSS; in Mk. xi 1 most have the spelling Βηθσαγαη.

² The scribe who does not understand what he is writing makes graphical errors due to misreading the *ductus litterarum*, but he does not make small sense-variations like 'that' for 'which' in English, or σκεῖν μίαν for σκεῖν (Mk. xi 13).

It should be noted that while this average mediaeval text is a sort of growth, the original form of *K*, whether now represented accurately by any extant group of MSS or not, must have been a 'recension' in the strict sense. Some at least of Hort's 'conflate readings' are a real putting together by an editor of two rival readings. It must have been a conscious emender who first in Mark xi read διὰ τοῦ πέραν for καὶ πέραν (or πέραν). It must have been a conscious emender who substituted the correct forms Ἀσά and Ἀμών for Ἀσάφ and Ἀμός in Matt. i 7, 10, or first let ἡ γὰρ ἐκεῖνος stand for παρ' ἐκεῖνον in Lk. xviii 14. And this emending editor must have been the person who also admitted (or let stand) in his revision that rather capricious selection from the *plus* or *minus* of the ante-Nicene texts,¹ and the equally capricious selection of the readings now called 'neutral' or 'Caesarean' or 'Western', which is found in the vast majority of Greek codices.

It is much to be hoped that Professor Lake will be able to carry out his intention to edit the 'Ecclesiastical' text of the Gospels in a handy form, for it will be a great help towards the scientific collation of Greek MSS.

The 'Jerusalem Syriac Lectionary'.

To come now at length to the criticism of the 'Caesarean text', it is obvious that our conclusions as to its nature will very much depend upon the role and character assigned to the Oriental Versions concerned. And first, a word or two about the 'Jerusalem Syriac Lectionary'. The editors refer to my article in this JOURNAL, vol. ii: I wish they had made some use of my article called *The Old Lectionary of Jerusalem* (*J. T. S.* xxiv 415-424). A great deal of that article deals with the diplomatic reconstruction of a MS at Leningrad and other such out-of-the-way things, but the result (p. 423) is a proof that 'the Palestinian-Syriac church originally followed the ritual customs of Jerusalem very closely, and that the divergences from it in the later documents are due to the general decay of Jerusalemite influence which followed the Mohammedan conquests and to the ever more preponderating authority of Constantinople over all Orthodox communities'. This, indeed, is also the view of Prof. Lake and his colleagues; and, as a practical result, I fully agree with them that the Palestinian (or 'Jerusalem') Lectionary has been 'conformed to the late Ecclesiastical standards' (p. 314). But this is not equally true of some of the fragments of complete Gospel codices. So unconventional indeed was the text of

¹ Hence Matt. xvi 2, 3 (the Face of the Sky) is found in all Greek MSS but a few, but the addition after Matt. xx 28 is only found in one Greek MS in addition to Codex Bezae. Yet their origin must have been similar.

one fragment that Land (*Anecd. Syr.* iv, p. 188) had said, 'Videtur e Diatessaro nescio quo petatum esse', but on the same page of my article (p. 423) I shewed that it was the text of Mk. xiv 49^b-51^a, and I quoted it in an English translation. Short as it is, it is of some textual interest: I repeat it here, with some 'Caesarean' variants.

Mk. xiv 49^b-51^a (= Land, *A S* iv 217).

“... that the Scriptures of the Prophets might be fulfilled. ⁵⁰ And then all the disciples left him and fled. ⁵¹ [And a] certain youth was [following] after him, clothed . . .

49 of the Prophets (= Matt. xxvi 56)] W @-565 13&c. arm, N syr^{hl*} : *om. rel.* 50 And then (= Matt. xxvi 56)] Then W @-565 13&c. arm, N syr.vg syr^{hl} lat.vg : 'And' *rell.* the disciples (= Matt. xxvi 56)] W @-565 13&c. arm, N (syr. S-vg) syr^{hl} lat.vg : *om. rel.*

The agreement of W @-565 13&c. arm makes it quite clear that the three assimilations to Matt. xxvi 56 all belong to the 'Caesarean' text.¹ Turning now to the Georgian we find that both *G*¹ (the Adysh Gospels) and *G*² (codd A and B) have the addition in *ver.* 49, but in *ver.* 50 the 'Caesarean' readings are only supported by *G*².

I have given these readings in full, because they really do raise most of the problems in a concrete form. First of all, the little scrap edited by Land is strongly Caesarean, as it should be. Secondly, the Caesarean reading, however ancient, is clearly wrong: *τότε* in narrative is characteristic of Matt., not characteristic of Mk. Thirdly, what account are we to give of the genesis of *G*¹? The Armenian and *G*² are in agreement: if *G*¹ has come to have the pure text in *ver.* 50 it must be either by not having been revised at all, or by revision from a Greek MS (*via* a hypothetical Armenian source). It seems to me more likely that it has not been so revised, and that the 'his disciples' of syr. *S* is an independent context-supplement (i.e. not really characteristic of all the MSS of the *Ev. da-Mepharreshe*). The reading is of no importance in itself, but it raises the question how far *G*¹ is really 'Caesarean'. Or, if we put the matter in another way, and regard the whole problem as a study in the progressive deterioration of texts current in the East (including the Old-Syriac), we shall put syr. *S* and *G*¹ high up on the slope, while W @-565 13&c. arm and *G*² have slipped much farther down.²

The Georgian MSS and their textual affinities.

Professor Blake's edition of Mark in Georgian is based on three MSS, viz. Adysh (*Ad* = *G*¹), Opiza (*A*), and Tbet' (*B*). *A* and *B* are often

¹ Tischendorf adds the Sahidic, but the words are absent from Balestri's text.

² In the immediate context of the passage just discussed it is worth notice that in Mk. xiv 68 *καὶ ἀέκτωρ ἐφώνησεν* is omitted by *G*¹ with NBLW *c* syr. *S* boh, against *G*² and arm.

in agreement against G^1 : their agreement is called G^2 and seems to represent a form of the Georgian version later and more revised than G^1 . It is supposed that the Georgian version was made from a form of the Armenian version earlier than now known to survive, which in turn was made from the Old Syriac. The Old Syriac in Mark is represented by the Sinai Palimpsest (syr.*S*), but the extant parts of Cureton's MS (syr.*C*) shew that there were extensive variations between the MSS of the Old Syriac, just as there were among the MSS of the Old Latin. It is not therefore surprising that G^1 often differs from syr.*S*.

The first thing is to see whether there is any evidence for the direct influence of Greek MSS. Here it seems to me that the spelling of Capernaum is a most important indication. G^1 has *καφαρναῶμ*, *A* has *καφαρναουμ*, *B* has *καπερναουμ*. The existing Armenian has *Kapharnaum*, so far as I know without variant. The Syriac has *ܩܪܢܝܘܨ*: the Nestorians, who alone distinguish between *u* and *o*, seem to be in doubt about the final vowel, for the American N. T. has *-nāhom* while B. M. 12138 has *-nāhum*. But the Syriac tradition is solid for the vocalization of the syllable *-phar*. Moreover the fact that the word is spelt with an initial *Δ* (not *ϐ*) shews that the name is translated, not transliterated: strictly speaking, the Syriac form corresponds to 'Nahumsthorpe' rather than to 'Capharnaum'.¹ Still, all this may perhaps be explained without direct Greek influence. But what evidence is there for *Καπερναουμ* except in Greek? It seems to me that the occurrence of 'Capernaum' in *B*, every time it occurs, points to the influence of a Greek MS with a *K*-text. Or rather, as this seems to be excluded by the general evidence, we must allow in *B* for the occasional influence of the Georgian Vulgate. This is said to be a revision made by Georgians connected with Mt. Athos about 1040 (see *Caesarean Text*, p. 293), but it may have been preceded by a previous Byzantinized text.

To what sort of linguistic influence do the forms of the Proper Names in the Georgian version point? For 'David' we get *Davit*, which is obviously an Armenian spelling (*դաւիթ*). In Mk. iii 17 *Banereges* agrees with the Armenian, also with *Βανηρεγες* 565 and with *Βανηρεγες* 700. The Syriac is *Bnai Rgesh* (*ܒܢܝ ܪܓܝܫ*).² 'Salome' follows the Armenian and the Greek: here the Syriac has *Shalom*. 'Jairus', again, in the Georgian does not follow the peculiar Syriac transliteration *Yō'arash*. In all these cases, if we adopt the view that the Armenian was originally rendered from the Syriac, we have to ask by what channel these forms of Proper Names have gone back to Greek forms.³

¹ The Nestorians pronounce the Prophet's name *Nāhom*.

² The Nestorians say *Bnai Ragsh*.

³ It is worth notice that neither in G^1 (*Belzebul*), nor in G^2 (*Berzebul*) is there any trace of the Syriac *Beelzebul*.

It may be noticed that *G*¹ always spells 'Sabbath' *Shap'at'* (not *shabat'*): this is one of the few words where the Armenian has the correct *sh* as a transliteration instead of simple *s*.

In *Golgot'a* (xv 23) and *elmana* (xv 34) we seem to see in *G*¹ the influence of the Syriac *gāgūthā* and *lmānā*.

In Mk. xv 42 *παρασκευή* is in the Greek, and the Georgian is *paraskevi*: this is the ordinary word for Friday, but it should be noticed that the corresponding term in Armenian is *urbat'* and in Syriac '*rubthā*'. In the next verse the Georgian has 'of Arimathaea'—not 'from Ramtha', as the Syriac has.

The indications are not very clear, but believing as I do that the Old Armenian was derived from Syriac and the Old Georgian from the Old Armenian, it seemed worth while to point out that the Old Armenian at least shews the influence of the Greek in most of its transliterations.

Illustration from Mk. xi 13.

On p. 212 the editors remark: 'The variant *ὡς εὔρησων* for *εἰ ἄρα εὔρησῃσει* is specially characteristic'. This is apropos of a small vellum fragment of the 7th century, containing Mk. xi 13-17, which was purchased at Eshmunein in Egypt and is now P. 13416 in the Berlin Museum. At this point (the story of the Barren Fig-tree) there are three readings. Jesus, seeing a Fig-tree, came

- (1) if perchance he might find something on it.
- (2) to see if there is anything on it.
- (3) as if about to find something on it.

The attestation is

- (1) *εἰ ἄρα τι εὔρησει ἐν αὐτῇ* NB AC W 1 & c. 13 & c. 28 syr. S. arm.
- (2) *ἰδεῖν ἐὰν τι ἔστω* D *b c f f i k gat.*
- (3) *ὡς εὔρησων τι* ⑤ 565 700 *afg* Orig.

It would be difficult on general principles to decide between (1) and (2), but my impression is that (2) is nothing more than the Old Latin rendering of (1), and that D only has it by retranslation from the Latin. On the other hand it is obvious that (3) is a Greek stylistic paraphrase of (1): *ὡς* with a future participle is good Greek, but alien from Mark's style.

The question is, how far are we justified in supposing that this particular 'corruption'—for such it is—ever formed part of the text of W, 1 & c., 13 & c., 28, or the original Armenian? The Adysh MS (*G*¹) is here unfortunately illegible: of the two MSS of *G*², one (*A*) has 'if' and the other (*B*) has 'that'. The editors, unaccountably to me, reckon *B* to (3) and *A* to (1). For note, that at Mark iii 2, where the Greek has *εἰ* without variant, and Adysh and *A* have 'if' (*ukue'tumca*) as in

xi 13, *B* has 'that if', the word for 'that' being the same as in xi 13. As there is no change of tense in *B*'s text of xi 13, or any attempt to imitate the participial construction, I do not think one is justified in supposing that ὡς εἰρήσων is the Greek underlying *G*².

In itself this is a small matter, but it seems to me typical of a good many readings of the 'Caesarean text'. I am not sure whether the method of reconstructing an ancient local type of text practised by the editors is altogether sound. A phrase now and then used by Dr Hort 'Western, of limited range', may perhaps explain what I mean. I should describe ὡς εἰρήσων as 'Eastern, of limited range'. I do not feel at all sure that ὡς εἰρήσων ever stood in the texts from which *W*, 1 &c., 13 &c., 28, or the original Armenian, or the Georgian, were respectively descended.

There are some admirable remarks by Professor Lake on p. 326 about 'the Caesarean text'. He says it 'was never a definite single entity like the Vulgate or the Peshitto, but is analogous to the European Latin, which is not only contemporary in time but remarkably similar to it in character (though not in detail)'. Quite so; but the method Lake uses to construct it is to pick out the non-'Ecclesiastical' readings of (*W*) ① 565 1 &c. 13 &c. 28 700, the Armenian and the Georgian, and to combine them into a synthetic text. The result is a convenient repertory of 'various readings' exhibited in their contexts, but is it a historical entity as Jerome's Vulgate or Rabbula's Peshitto is? If we constructed a Latin text of the Gospels from *abcffinr* by always choosing the reading which differed from the Vulgate, should we get a text which ever had a local habitation?

I have a great difficulty in expressing what I mean without seeming to cavil at the admirable and indeed pioneer work that has been done in the book under review. I do not think Professor Lake and Professor Blake are under any illusion, either as to the complexity of the evidence or as to the positive authority of their reconstructed text. The point I wish to make is that there is a difference of character between constructing the text of the 'Ferrar Group' (= 13 &c.) and that of 'the Caesarean Text'. The late Professor Ferrar's work was published so long ago as 1877, and it is now out of date because fresh material has turned up. But he knew exactly what he was aiming at, and his method was perfectly sound for his purpose. He saw that the minuscules 13-69-124-346 were not only akin in textual character, but were connected by such definite peculiarities, and mistakes of writing,¹ as could only be explained on the supposition that they were all, directly or indirectly, copies of a single codex. This codex is as concrete a historical entity as the non-extant fair copy of Jerome's Vulgate or the autograph of

¹ e. g. ὁδηγὸν (*sic*), Matt. xv 14.

Mark, and it is a perfectly scientific aim to reconstruct it. Codd. 13-69-124-346 were all written after the 11th century, when the 'Ecclesiastical' text (i. e. practically, the 'Received' text) was well established. Prof. Ferrar constructed his text by accepting the reading of a majority of his MSS, and where two were divided against two he put in the text that reading which differed from the 'Received' text. Where only one MS disagreed with the family he only recorded the reading in his margin. Thereby it is possible that in a few cases (e. g. *ἐπικεφάλαιον*, Mk. xii 14) he may have reconstructed a more commonplace text than the common original really was, but more often these singular readings were mere errors of the single MS. In any case the reading was recorded.

But an ancient local text, as Lake perceives (see above, the passage quoted from his p. 326), is different in character from that of a concrete non-extant codex. It is not only more diverse at any given date, but also to a certain extent it differed at different dates. To take again the case of Mk. xi 13, we do not know the date at which some one for the first time wrote *ὡς εἰρήσων* for *εἰ ἄρα εἰρήσει*. Does it belong to the earlier, or a later, stage of the Caesarean text? Was *ὡς εἰρήσων* corrected out of 1 &c. and W through the influence of the NB-text? If so, what are we to make of the curious and important readings of W and 1 &c. shared by syr.S but not by NB?

It is easy to ask these questions, much harder to supply any satisfactory answers. Yet I cannot help asking another, of which I certainly do not know the true solution. At the end of p. 336 Lake says we must insist on subjective rather than objective criticism, i. e. we must always give due place in textual criticism of the Gospels to internal evidence. The question that I ask myself again and again in considering various readings is, why is Westcott and Hort's text so good? In other words, how do B and N come to have so good a text? No doubt there is a gleaning of improvement to be got from 'Western' and 'Eastern' authorities, but in at least five cases out of six (if not more) B is the constant element in the attestation of the better reading. What is the historical interpretation of this fact?

I should like to take here the opportunity of saying one or two things about some minor textual problems. What Professor Lake says about Family 1 (= 1 &c.) on p. 327 should be noted, as counsel has been darkened by von Soden's quite misleading investigations on this subject. From von Soden's § 213⁴ (p. 1059, first par.) one would infer that the 40 peculiarities of ε 183 and ε 1131 there listed were not shared by δ 254 = cod. 1! The facts about Family 1 are simple. With one exception cod. 1 itself is so much better than the other members of the family that they can be neglected, except for the purpose of detecting

mere scribal errors. The exception is Vatopedi 747 = von Soden's ϵ 183 = Gregory's 1582, which professes to have been written in A. D. 949, but is perhaps of the 11th century. Where 1 and 1582 agree there can be little doubt that the readings are those of Family 1, and where they differ 1 still seems to be rather the better representative. Cod. 22, judging from the collation in the *Journal of Biblical Studies* xxxiii 91 ff, is a poor relation of 1 and 1582, but valuable from the fact that it has an interesting text in Matthew, where so many 'Caesarean' documents are heavily Byzantinized.

Finally, any fresh reconstruction of the Ferrar-group (= 13 &c.) must take account of the half-dozen new MSS, and also have some theory to account for the peculiar element in 124. This MS is not apparently Calabrian. It has often been assimilated to the 'Ecclesiastical' text, where the others have retained the Family reading; on the other hand its preservation of *ἐπικεφάλαιον* for *κῆνσον* in Mk. xii 14, referred to above, is by no means an isolated case. When working at the group some time ago I felt inclined to put 124 into one sub-family of the group and all the rest together into the other. I cannot feel that any special light on this curious and interesting MS has been shed since Ferrar's own investigation.

I hope that the length of these remarks will be correctly interpreted by my readers as a testimony to the interest of the very stimulating book which is the immediate occasion of them.

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AN ARMENIAN MANUSCRIPT OF THE GOSPELS

IN the small collection of Armenian manuscripts at the Cambridge University Library is one (Add. 2620) of unusual interest.¹ Tetra-evangelium. Oriental glazed paper. Size $9\frac{3}{4} \times 6\frac{1}{2}$ in. Text $6\frac{3}{4} \times 4\frac{1}{2}$ in. In two columns of 20 lines each. Binding, modern English half-morocco with covers. Illuminated headings, capitals, and marginal decoration richly executed in gold and colours. No miniatures. Writing, bologir. The spelling appears to be archaic throughout.

¹ My thanks are due to the Librarian of the Cambridge University Library for permission to use the manuscript, and to Prof. F. C. Burkitt and Mr A. D. Nock for many helpful suggestions. There are six other Armenian manuscripts of the gospels at the University Library. One, badly damaged and containing only Matthew, Mark, and the first few lines of Luke (Add. 2619), is assigned to the fourteenth century, but the others are dated between the sixteenth and eighteenth centuries.