

(cf. however below), or that a whole sermon is lost, or again that two sermons occurred here on the same subject, the end of the seventh and the beginning and the end of the eighth being lost. In favour of this last supposition is the inordinate length of this sermon if it is all *one*, but as I have said there is no obvious break in the argument which might indicate a new sermon. If there were two sermons the second perhaps began on the leaf lost after fol. 123.

Fol. 131. Λόγος Ἰ. Τοῦ αὐτοῦ εἰς τὸ ῥητὸν τοῦ αὐτοῦ εὐαγγελιστοῦ δηλονότι τὸ φάσκον· “καὶ οὐδεὶς ἐπιγινώσκει τὸν υἱὸν εἰ μὴ ὁ πατήρ, οὐδὲ τὸν πατέρα τις ἐπιγινώσκει εἰ μὴ ὁ υἱὸς καὶ ᾧ ἔαν βούληται ὁ υἱὸς ἀποκαλύψαι” (Mt. xi 27), καὶ κατὰ Ἀρείου καὶ Σαβελλίου καὶ Μακεδονίου τῶν παραπλήγῳν καὶ δυσσεβῶν αἰρесиάρχῳν, ἔτι γε μὴν καὶ κατὰ τῶν ἀγνοητῶν<sup>1</sup> προσφόρως ὀνομασθέντων ὡς ἀγνοίαν κατηγορησάντων τοῦ κυρίου τῶν γνώσεων. Inc. Καλῶς τῷ ὄντι καὶ σοφῶς ἄγαν τὸ παροιμακὸν λόγιον ἔφησε. It is interesting to note that though the text is from St Matthew the Evangelist is called τοῦ αὐτοῦ. This strengthens the case for supposing that either a whole sermon is lost or two separate sermons came between fols. 99b and 131.

Fol. 140. Τοῦ αὐτοῦ εἰς τὸ ῥητὸν τοῦ κατὰ Ματθαίου εὐαγγελίου τὸ φάσκον· “εἰσὶν εἰνούχοι οἵτινες ἐκ κοιλίας μητρὸς ἐγεννήθησαν οὕτως, καὶ εἰσὶν εἰνούχοι οἵτινες εἰνούχισθησαν ὑπὸ τῶν ἀνθρώπων, καὶ εἰσὶν εἰνούχοι οἵτινες εἰνούχισαν ἑαυτοὺς διὰ τὴν βασιλείαν τῶν οὐρανῶν” (Mt. xix 12). Inc. Ἀπειροκάλων ἀνθρώπων ὁ νοῦς οὐδὲν τῶν ὀνησιφόρων καὶ συντελούντων πρὸς ἀφθαρσίαν εἶθε γεωργεῖν. Ends (complete) fol. 151b.

In conclusion, I may add that nothing is known as to the history of the MS except that it was bought from the monastery of Caracalla on Mount Athos by Robert Curzon in 1837. The binding is recent; very possibly the volume was unbound when it was acquired, as the first and last pages are dirty and a little rubbed. Several single leaves are lost from various parts of the volume, and a whole quire after fol. 54.

H. I. BELL.

## THE CAESAREAN TEXT OF THE GOSPELS.

IN my book *The Four Gospels* I submitted evidence for the view that the very numerous non-Byzantine (or, as Hort would have called them, ‘pre-Syrian’) readings found in the Koridethi MS © and certain cursives (especially 1 &c., 13 &c., 28, 565, 700) are survivals of the Gospel text in use at Caesarea before A.D. 231—the date when Origen transferred himself to that city. Prof. Burkitt in the April number of

<sup>1</sup> MS ἀγνοητων.

this JOURNAL devoted several pages to a criticism of this conclusion. The remarks which Prof. Burkitt makes about my book as a whole are couched in terms of approbation far more generous, I am sure, than I deserve, and it is therefore in no sense to defend myself or my book, that I write this article. I do so because the question whether this group of MSS does or does not preserve the text of the Gospels dominant in Caesarea about A.D. 230 is one of general interest to scholars.

The pith of Prof. Burkitt's criticism lies in his contention that the set of 'various readings' which von Soden groups as the I Text 'is far too disparate and amorphous . . . to be called a recension at all, or to be properly grouped under any single sign. Dr Streeter has tried to reduce von Soden's I into a manageable entity by cutting off from it the Old Syriac texts on one hand and D and the Old Latins (together with W, the "Freer" MS now at Washington) on the other. But the Latin Vulgate is properly called a *recension* because there was once a MS which contained the Gospel-text of the Latin Vulgate as Jerome wrote it, and this MS can practically be reconstructed: I am not certain that one MS ever contained the peculiarities of © 565 and their friends'.

Everything in the above quotation has my hearty assent—except the reference to my own procedure. This happens to express exactly the reverse of the line which my investigations actually took. So far from starting from von Soden's conception of an I text, derived from a Jerusalem recension, I started with the conviction that this was pure moonshine; and I had fortified this view by writing to an eminent textual critic in Germany from whom I extracted the answer that no one in that country believed in the existence of von Soden's I text. It was the fresh study of the facts, which I began after reading Lake's article in the *Harvard Theological Journal*, July 1923, that convinced me that von Soden was not quite so mad as I had previously supposed. In Appendix II of my book I give reasons for supposing that certain other MSS—especially NΣ and 1424 &c.—which von Soden assigns to his I text should be classed with the © group. But I expressly warn the reader (cf. p. 580) against 'classing as authorities for the text of *fam* © all MSS—merely excepting D W<sup>Mk</sup>, Old Lat., Old Syr.—cited in von Soden's *Apparatus* as authorities for the I text. They must be scrutinized again in every case. And this caution is the more necessary as von Soden is over-anxious to enlist MSS in support of the I text'.

I conceive, moreover, that von Soden is wrong, not only in grouping D, the Old Latin and the Old Syriac along with © and its allies, but also in using the term *recension*, which as Prof. Burkitt points out,

implies that there was once at some date (in the third or fourth century) a *single* manuscript which contained *all* the peculiar readings in question. But when one speaks of the 'Alexandrian *text*', one does not mean that *all* the readings in  $\aleph$  B C L,  $\Delta^{\text{Mk}}$ , the Egyptian versions and the citations of the Alexandrian Fathers, ever co-existed in any single MS. One means that the majority of the readings which are either peculiar to, or only rarely found outside, this group of authorities would have been found in an *average* MS used at Alexandria, while of the rest of these characteristic variants some would have been found in one, some in another MS in that locality. A 'local text' is not the same thing as a 'recension'. For that reason I also demur to von Soden's description of what he calls the H text as a 'recension'. Even if, as is quite possible, B represents pretty accurately the recension of Hesychius, then, I think,  $\aleph$  must be regarded as a typical pre-Hesychian text or as such a text partially corrected by a Hesychian MS. But whatever be the actual connexion between B and  $\aleph$ , scholars for the last fifty years have habitually spoken of the 'B  $\aleph$  text', and when I use the phrase 'Caesarean text', I use the word 'text' in that sense and not in the sense of 'recension'.

To come now to the actual phenomena of the  $\Theta$  group, or rather of the non-Byzantine readings found in these MSS. So far from being, as Prof. Burkitt says, 'a set of various readings that remain obstinately disparate and amorphous', what to me seems remarkable about them is the extent of their homogeneity. In the first chapter of St Mark there are 102 cases in which one or more members of the  $\Theta$  group give a non-Byzantine reading, but there are only five cases in which members of the family differ from one another in a non-Byzantine reading. Now in this same set of variants B and  $\aleph$  differ from one another sixteen times. So far then as this particular chapter is concerned the non-Byzantine variants in the  $\Theta$  family shew a far smaller range of variation than do B and  $\aleph$ , which are notoriously more closely allied than any two other authorities of the first five centuries. If, then, we are justified in using the phrase 'the B  $\aleph$  text', *à fortiori* may we apply the term 'text' to the characteristic element in this group of MSS. Now I make no pretence to have tested more than a limited number of passages in the Gospels, but every passage which I have tested has yielded a similar result, viz. the range of variation between the readings of the six main members of the  $\Theta$  group is *less* than that between B and  $\aleph$ . Indeed it is so small that, bearing in mind (a) the late date of most of the MSS concerned, and (b) the way that MSS accumulate sporadic variants in the margin which may get into the text of MSS copied from them, it would be quite compatible with

the view that the non-Byzantine element does go back to a single MS of the fourth century—though that is not the view I myself hold.

But this—I venture to think rather remarkable—degree of homogeneity in the non-Byzantine element in this very diverse set of MSS, cannot be considered apart from the surviving evidence as to the text used by Origen. Of the 157 variants in the text of Mark<sup>1</sup> quoted by Griesbach from the latter part of Origen's *Commentary on John*, the *Commentary on Matthew*, and the *Exhortation to Martyrdom*, no less than 116 are readings of *fam* Ⓞ. It follows that Origen must have possessed a MS which contained 75 per cent. of the readings found in this group of MSS. And since of the forty-one readings in which Origen differs from *fam* Ⓞ, fifteen do not occur in any MS and may well be either slips of memory or scribal errors, while twenty are Byzantine and therefore open to the suspicion of being scribal corrections, it is probable that this percentage much understates the actual truth. These works of Origen we know were written after he moved to Caesarea. But the text of Mark used in the earlier books of the *Commentary on John*, which we know was begun in Alexandria, has a 90 per cent. agreement with the  $\aleph$  B C L  $\Delta$  text. Since then there are many other reasons for connecting the text of these MSS with Egypt, we naturally infer that the change of text was due to a change of residence and, therefore, that a text similar to that of *fam* Ⓞ was prevalent at Caesarea in A. D. 231.

I do not gather that Prof. Burkitt wishes to dispute my facts. So far as I understand, it is merely the description 'Caesarean', as distinct from 'Eastern', to which he objects. 'It is some way', he writes, 'from these things to Streeter's Caesarean text distinct from the Old-Antiochian text.' Now in regard to the Old-Antiochian text I am a humble disciple of Prof. Burkitt. Practically everything that I know about the Old Syriac is derived from his works, and in my book (p. 74) I say, 'Burkitt was, I believe, the first to work out in any detail the suggestion that the Greek text underlying the version of the Old Syriac preserved in Syr. S. was derived from the older text of Antioch'. The arguments which I adduce in support of this theory are avowedly, in the main, a summary of his. Has Prof. Burkitt abandoned this view of his? If so, I wonder why.

At any rate the facts about the text of the Sinaitic Syriac may be

<sup>1</sup> I concentrate on the figures for Mark for three reasons: (a) the text of *fam* Ⓞ is far better preserved for this Gospel, (b) for reasons noted in my book (p. 63 f), research into the pedigree of a MS should always begin with the text of Mark, (c) for the other Gospels it would only be possible to quote a selection of the evidence. But the set of variants noted by Griesbach is probably fairly exhaustive, and at any rate is *his* selection, and not one made by me to illustrate my own argument.

roughly summarized by the description of it as 'a kind of half-way house between the text preserved in *fam* Ⓣ and the geographically Western group of texts preserved in D and the Old Latin'. The distinction within the 'Eastern texts' which I draw between the types represented by *fam* Ⓣ and the Old Syriac respectively, appears to me to be comparable to the accepted distinction between the African and the European branches of the Old Latin. At any rate the distinction exists; and there is no reason why we should interpret it apart from the facts that (a) at least one manuscript with a text very like the ground text of *fam* Ⓣ was used by Origen when at Caesarea, (b) there is a fair presumption that the Greek manuscript used by the translator of the Old Syriac was obtained from Antioch. That being so, it seems to me a real gain in clarity of thought to conceive of the one text as 'Caesarean' and the other as 'Old Antiochian'. But, of course, we are not therefore entitled to assume either (a) that there was a fixed uniform text current in either of these districts, instead of merely a predominant type; or (b) that *every* reading in the scanty authorities we possess was to be found in the majority of the MSS in common use in these cities at the beginning of the third century.

With what Prof. Burkitt says as to the marked superiority of the B Ⓝ text of the Gospels I agree (though I am not at all certain that the same thing holds true of the Acts), but I am not sure that I regard it as altogether remarkable. Alexandrian scholarship was traditionally interested in textual criticism, and this fact would I think have inevitably reacted on the standard of accuracy exacted from the ordinary scribe. Antecedently, then, one would expect the text to be better preserved in Alexandria. It is true that Clement of Alexandria uses a very mixed text. But many authorities believe that Clement's extant writings were written after he left Alexandria. We have seen evidence that Origen, after leaving Alexandria, was content to use the text current in the Church in which he wrote. Clement, who had far less interest than Origen in textual criticism, would presumably do the same. His works then afford only precarious evidence against the obvious view that the B Ⓝ type of text was already dominant in Egypt in his time.

Later on, in discussing my theory that the minor agreements of Matthew and Luke against Mark are due to corruptions in the B Ⓝ text, Prof. Burkitt seeks to discredit the theory of the existence of a Caesarean text by impugning its quality. 'But the point I wish here to raise is that in *only one* [the italics are mine] of these thirty-two cases of alleged textual corruption does Dr. Streeter venture to claim that his 'Caesarean' text, unsupported by Ⓝ B or D latt or syr vt, preserves the true reading.' Incidentally I may remark that I make that claim

in *two* cases (Mk. xiv 62, Mt. xxvii 60) where the Caesarean text has no other support, besides one case (Mt. xii 4) where it has the support of a single Old Latin MS. But surely in the parenthetic words 'unsupported by' &c. there lurks fallacy. If in any particular passage the Caesarean text preserves a correct reading, then it *does* preserve it; it is no discredit to the Caesarean text that D or the Old Latin or Old Syriac should happen to support it in this meritorious action. Moreover even if the *fam* <sup>Ⓞ</sup> text were 'essentially bad' it would not follow that it was not in use at Caesarea about 230. If the B <sup>Ⓝ</sup> text is as pre-eminently good as Prof. Burkitt holds, then 'essentially bad' texts were current everywhere outside Egypt at that date.

Prof. Burkitt concludes his main argument with these words: 'Any reading, for instance, found in  $\Gamma$  has an off-chance to be a genuine survival, just as any reading in  $\xi$  or in Syr. S . . . has an off-chance to be a survival. But it is an off-chance, and no geographical word such as Caesarean adds to the authority with which it speaks'. This seems to me tantamount to saying that *external* evidence as to the date or range of circulation of any non-Byzantine reading is entirely without value. I should agree with Prof. Burkitt that in the last resort, and in clear cases, internal evidence must be given the greater weight. But most cases are not clear; and, where that is so, the number of different localities in the third century in which the reading is found to have been current is a fact which the critic is bound carefully to weigh. The identification, therefore, of local texts is the first condition of any scientific attempt to test the comparative value of different types of external evidence as such. This does not mean that external evidence can ever rule out internal, but at least it may provide us with a means of controlling it.

B. H. STREETER.

#### NOTE ON THE PRECEDING NOTE.

By way of explanation let me state what was in my mind when I wrote my strictures on Canon Streeter's Caesarean text.

1. There are a number of variants in our MSS and versions that are neither geographically 'Western' nor 'Alexandrian', i. e. attested neither by D-latt nor <sup>Ⓝ</sup>B. Let us call these 'Eastern'. The chief MSS of this group, or groups, are

Ⓞ 565 700 13-69-124&c 1&c 28 and syr.SC.

2. It happens only rarely that all the members of this group, or groups, agree together against the rest. When they are divided the