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## THE WASHINGTON MS AND THE CAESAREAN TEXT OF THE GOSPELS.

I HAVE lately made a discovery which is in itself, I think, of considerable interest for the textual criticism of the Gospels, and which happens also to have a bearing on the friendly controversy between Prof. Burkitt and myself.

Prof. Sanders in his edition of the fourth-century<sup>1</sup> MS W proves conclusively that for Mk. i 1 to v 30 its text is the Greek equivalent of the Old Latin. But he proceeds, 'In the second part of Mark there is still a decidedly close relationship between W and the Old Latin MSS, but the special Latinisms and the peculiar agreements with MS *e* have mostly disappeared. . . . The most interesting feature . . . is the increase in the number of agreements with fam. 13 (Ferrar group) and the other Syriacizing MSS fam. 1, 565, and 28.'

This description understates both the extent and the importance of the change in the text of Mark at this point, as I discovered after carefully testing several passages of considerable length. W is full of mistakes of spelling and has numerous singular readings. Ignoring these, it appeared in the passages tested that eighty-five per cent. of the readings in which W differs from the T.R. are to be found in at least one (more often in several) of the six chief representatives of what I believe to be the Caesarean text, i. e. Ⓞ, 1 &c., 13 &c., 28, 565, 700. Conversely, W (so far as tested) contains seventy-eight per cent. of the readings in which any two of these six authorities agree together against the Byzantine text. What is even more significant, in some fourteen out of seventeen cases where W does *not* contain the family reading it agrees with the Byzantine text.<sup>2</sup>

From these facts only one conclusion seems possible. We have in W, for Mk. v 31-xvi 8, a MS which has preserved, with a relatively small amount of Byzantine correction, the fundamental text of the Ⓞ family. Accordingly, it is no longer possible to maintain that this 'text' is a purely hypothetical entity. That *all* the non-Byzantine readings found in any member of the Ⓞ family ever stood in one single MS I have never suggested; but I am now able to call into court a witness of the fourth-fifth century in which the great majority of them are actually to be seen.

<sup>1</sup> Prof. A. S. Hunt tells me he inclines to date W late fourth century, but would not rule out early fifth century.

<sup>2</sup> I am giving further details in an Appendix to a new impression of my book *The Four Gospels* now in the press.

Prof. Burkitt's reply to my Note in *J. T. S.* (p. 379) of last July calls for a few observations. The formidable-looking lists of readings which he there prints, though intrinsically very interesting, have no real bearing on the points at issue between us.

(1) The group of 'triple variants' first cited merely proves that members of the © family are occasionally divided against one another in support of non-Byzantine readings. So far from denying this fact, I have myself stated it, and have endeavoured to define its exact significance (both in my book p. 572 f, and in *J. T. S.* p. 375) by pointing out that such differences are of less frequent occurrence than differences between **N** and **B**. Unless it can be shewn that 'triple variants', like those adduced by Prof. Burkitt, occur *more often* than differences between **N** and **B**, my argument is absolutely unaffected.

(2) The second group of five readings is brought forward by Prof. Burkitt as a set of 'instances where 1 &c., with or without subsidiary support, deserts the main "Caesarean" body to agree with Syr. S'. He has, however, failed to notice the all-important fact that in four out of the five instances the members of the © family which differ from 1 &c. exhibit the reading of the Byzantine text, and therefore cease for the time being to represent the characteristic family text. Hence the agreement of 1 &c. with Syr. S in these four instances is merely their agreement against the Byzantine text; it is not against the non-Byzantine element in the other members of fam. ©, and therefore cannot fairly be described as a 'desertion' by 1 &c. of 'the main "Caesarean" body to agree with Syr. S'. (His remaining instance must, I think, have been adduced through some accident, for in the reading cited 1 &c. does *not* agree with Syr. S.) Now these readings are brought forward by Burkitt as 'some notable agreements of 1 &c. with Syr. S against the majority of Streeter's "Caesarean" authorities'. That they are not such I have already shewn; but, seeing that in every instance the only members of the © family which do *not* give the Byzantine reading are found to agree with 1 &c., they can actually, every one of them, be cited in support of my contention that the non-Byzantine readings in those MSS belong to one single text.

The point in regard to which Burkitt and I differ is small compared with those on which we are agreed; but, as he himself indicates, it is of importance to the student of the history of the text.

We are at one in distinguishing an 'Eastern' text (preserved in the Old Syriac and in the non-Byzantine readings of © 1 &c., 13 &c., 28, 565, 700) from the 'Alexandrian' (**N** B L &c.) and the 'Western' (D Old Latin). I differ from him in maintaining that a further distinction exists between the text presupposed in the Old Syriac and that of the non-Byzantine element in © and its supporters—a distinction com-

parable to that between the 'African' and 'European' families of the Western text. I maintain that the text of the © family is a kind of half-way house between the Old Syriac and the Alexandrian; and that it is not unreasonable to connect this with the fact that Caesarea is geographically intermediate between Antioch and Alexandria.

Burkitt in his Note in *J. T. S.* p. 378 ff repeatedly emphasizes the close connexion of Codex 1 with the Old Syriac. If, then, I make that MS the test case, I am meeting him on ground he has himself chosen; and if I produce statistics from Lists of readings compiled twenty years ago by another scholar, I cannot be suspected of 'cooking' my figures. Fortunately in the Introduction to Prof. Lake's edition of Codex 1 (*Texts and Studies*, vol. VII) there are ready to hand classified Lists of the readings of that MS which make possible the test desired.

List C gives 'Readings (of Cod. 1) for which the Old Syriac version is the chief authority'; thirty-two variants are noted. In List D, which gives 'Readings which are found in both the Old Latin and Old Syriac versions, but not in  $\aleph B^1$  or in the Antiochian (= Byzantine) text', there are fifty-seven. In List E, which gives 'Readings for which  $\aleph B$  are the chief ancient authorities', seventy-two variants are noted. That is to say, the agreements of Codex 1 with readings specially characteristic of the Alexandrian text are nearly twice as numerous as its agreements with readings specially characteristic of the Old Syriac. And even if one adds together the figures in Lists C and D—as one must in order to get a fair idea of the relation of the ground text of 1 to  $\aleph B$  and the Old Syriac respectively—the proportion is as seventy-two to eighty-nine. This surely justifies my contention, so far as Codex 1 is concerned—in which MS according to Burkitt the connexion with the Syriac is specially notable—that the text of fam. © is markedly distinct from that of the Old Syriac, and is roughly speaking midway between this and the Alexandrian type.

The occasional instances of cross-grouping, shewn in Burkitt's list of 'triple variants' above, in no way invalidate my main argument. They are exactly comparable to the occasional cross-groupings found in the chief authorities for the African and European types of the Western Text. Thus  $\epsilon$ , our second most important authority for the African Latin, has been shewn by Prof. Burkitt elsewhere to have quite a number of readings characteristic of the European Text. Indeed, with the possible exceptions of B k and Syr. S, no uncial MS exists which has escaped entirely from some mixture with an alien type of text. Even  $\aleph$ , as Hort pointed out, has a not inconsiderable

<sup>1</sup> I think  $\aleph B$  is an error for B alone, since in the List several instances are given where  $\aleph$  deserts the Alexandrian group and has a 'Western' reading.

Western mixture. What I am claiming for the non-Byzantine element of the © family is, not that it preserves an absolutely pure text, but that its deflections from the type are less frequent than the deflections of **Σ** from the Alexandrian or of *e* from the African Latin type.

Another point should be borne in mind. The uniformity of the Byzantine Text as it is represented in the numerous surviving uncials of the eighth, ninth, and tenth centuries is not the absolute uniformity of a printed text. Every one of these predominantly Byzantine MSS has (besides a few readings peculiar to itself or found with small support elsewhere) sporadic readings, now of the Alexandrian, now of the Western type. Presumably, then, the MSS used by the various scribes who revised the several ancestors of © and its supporters had a few such readings. Hence the occasional occurrence, on which Burkitt lays stress, of readings in some one member of the © family agreeing with **Σ**B or D Latt. against other members of the family is quite as likely to be due to the text used by the 'Byzantine corrector' as to survival from earlier MSS of the Caesarean text. At any rate, sporadic mixture is a phenomenon which occurs in all uncials (and in all cursives of which the readings are quoted in any Apparatus Criticus). Hence to group MSS into families, we must consider averages holding over large numbers and—provisionally at least—ignore occasional exceptions. If the method of averages is regarded as valid by the Physicist when he investigates the constitution of the atom, or by the Zoologist when he tests Mendelian theories, the textual critic need not blush to call in its assistance.

Burkitt concludes as follows: 'When the attestation of any reading in this (sc. Eastern) group does not include Syr. S, there is, I think, a strong presumption that the reading in question is a later corruption inside the group, not an original feature of it'. In an interesting article on 'W and ©' in *J. T. S.*, October 1915, Burkitt argued at some length for the general superiority of the **ΣΣ** text. I think he proved his case. But, if so, are we to say that the larger number of readings in which the © family supports **ΣΣ** against Syr. S are 'later corruptions'; or are they evidence that the © family is, not only independent of, but at times superior to, the text found in Syr. S?

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