

It is, I think, hardly open to doubt that the Council thought Leo was quoting from E. But was it so in fact? On this point there is still room for difference of opinion. The words undoubtedly occur in R, but would Leo have quoted R as authoritative to Flavian or to an Eastern council? Or did he press the claims of his see so far as to make his own baptismal creed a standard equal in authority to the Scriptures over a Constantinopolitan archimandrite? On this point I refer to Dr. Gore: 'Leo appears to make no exact or definite claim over the Eastern bishops. He professes his "universalis cura" for the whole church . . . but when he comes to write his celebrated letter to Flavian . . . he writes in a tone no wise different from that adopted by St Cyril in his letters against Nestorius' (*Dictionary of Christian Biography* vol. iii p. 662). On the other hand Leo had no objection on principle to a conciliar enlargement of N; for his own predecessor Damasus had already adopted that course. And two years later Leo acknowledged E through his legates at Chalcedon.

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## THE PRIMITIVE TEXT OF THE GOSPELS AND ACTS, A REJOINDER.

AMONG the notices of my work upon the Primitive Text of the Gospels and Acts three differ from the rest in that they are signed. The writers are Dr Sanday,<sup>1</sup> Dr Souter,<sup>2</sup> and Sir Frederic Kenyon,<sup>3</sup> three well-known defenders of what Westcott and Hort call the 'Neutral' text. I have always made it a rule not to reply to reviewers, and it is with the utmost reluctance that I break this ordinance. I only do so because it has been represented to me by friends that, if I do not reply, an unfavourable interpretation may be placed on my silence.

Since I cannot expect that my little book may have made its way into the hands of more than a few of those who read this answer, I may be allowed to state my main points. This I will do very briefly.

My work consists of two parts. In the first I treat the Gospels and in the second the Acts. The method followed is different in the two cases, but the result is the same. This is, that the primitive text is to

<sup>1</sup> *Oxford Magazine*, June 4, 1914.

<sup>2</sup> *Review of Theology and Philosophy*, August 14, 1914.

<sup>3</sup> *Church Quarterly Review*, October 1914 (under the title 'Von Soden's edition of the New Testament').

be found in what is generally termed the 'Western' family, viz. the Graeco-Latin Codex Bezae (D) and its allies; not in B<sup>N</sup>, the two oldest Greek MSS. I object to the term 'Western', since this text, which is that quoted by the earliest Fathers, was used in the East as well as in the West, and the Old Syriac versions exhibit a text similar to that found in the Old Latin. As the most obvious difference in them is their length, I shall here speak of them as the longer and shorter texts. My thesis is that it is the longer text which is original, and that the shorter text has been derived from it; not, as is generally held, that the longer is the result of periphrasis and expansion.

In my book I began with the Gospels, though I quote on two occasions Professor Lake's remark that the proper course is to begin with the Acts. I now propose to invert the order and to begin with the Acts, since in their case the problem is more simple.

D is written in lines of irregular length, divided more or less according to the sense. As an example of the formation we may take, e.g.:

- v. 38-39    *και εασατε αυτους  
μη μιαναντες τας χειρας  
οτι εαν η εξ ανθρωπων η βουλη αυτη  
η το εργον τουτο καταλυθησεται*
- 5            *ει δε εκ θυ εστιν ου δυνασεσθαι καλυσαι αυτους  
ουτε υμεις ουτε βασιλεις ουτε τυραννοι  
απεχεσθαι ουν απο των ανθρωπων τουτω  
μηποτε θεομαχοι ευρεθητε.*

While comparing the rival texts I observed a striking phenomenon, viz. that passages found in D, but omitted by B<sup>N</sup>, and as a rule by the other Greek MSS, not infrequently occupy complete lines in D. Thus in the passage quoted above, l. 2, *μη μιάναντες τὰς χεῖρας*, and ll. 6-7, are omitted by all Greek MSS except D. I found forty-one cases of such omission.

In the case of classical MSS a single example where a complete line (of reasonable length) in one MS is omitted by another is considered to be good evidence that the MS which omits the passage was copied from the MS which contained it. Here, however, the conditions are different, since (a) in D the *στίχοι* are 'sense-lines' and (b) they are so short that accidents may easily occur. Here the Gospels come to our aid. In them I find fourteen cases in all where passages, omitted (rightly or wrongly) by B<sup>N</sup>, occupy complete lines or blocks of lines in D. If, however, we take into consideration the length of the Gospels (324 pages in D) as compared with that of the Acts (88 pages in D) we should expect to find about 150 cases in the Gospels. The hypothesis of accident, therefore, does not seem to work.

From a palaeographical point of view the most interesting case is :

xv 29        *ευ πράξατε φερομενοι*  
*εν τω αγιω π̄νι ερρωσθε.*

So also Irenaeus and Tertullian. The other MSS give *εὖ πράξατε ἔρρωσθε*. The obvious explanation here is that the scribe has passed from one line to the same place in the next line, omitting the intermediate words. No critic would venture to suggest any other explanation if he were dealing with a profane text.

In spite of all this evidence I should have hesitated to draw any conclusion, but for the fact that it is reinforced by a number of cases where the ordinary text seems to have been patched after a loss of one or more lines. I select as examples :

iii 3        *ουτος ατεινισας τοις οφθαλμοις αυτου*  
*και ιδων πετρον και ιωαννην*

So D : *ὃς ἰδὼν Πέτρον καὶ Ἰωάννην cett.*

xv 12        *συνκατατεθειμενων δε των πρεσβυτερων*  
*τοις υπο του πετρον ειρημνεοις*  
*εσειγησαν παν το πληθος*

So D : *ἐσίγησε δὲ πᾶν τὸ πλῆθος cett.*

The hypothesis of expansion here is not so probable as that of contraction. I would ask the reader to consider the parallel phenomena which I point out in Acts xxvii 11-13 (p. 104), as given in the Fleury palimpsest, and in Mark x 46, xii 4, Matt. xiii 14-15, as given in **U**<sup>k</sup>.

I have collected twenty cases in which small alterations appear to have been made in order to remedy omissions. Among these is the famous passage xxi 16 :

*ουτοι δε ηγαγον ημας*  
*παρ ω ξενισθωμεν*  
*και παραγενομενοι εις τινα κωμην*  
*εγενομεθα παρα νασωνι τινι κυπριω*

So D : *ἄγοντες παρ' ᾧ ξενισθῶμεν Μνάσωνί τινι Κυπρίῳ cett.*

Here, according to the shorter text, St Paul goes from Caesarea to Jerusalem, a distance of sixty-eight miles, in one day, while the extra line found in D, *καὶ . . . κώμην*, makes him spend a night in a village. Who can doubt which text is right here ?

Other cases which I discuss in chapter xii are more complicated.

The only conclusion which I can draw from these phenomena is that at a very early date the Acts were arranged in sense-lines, such as those which have been preserved in D. This method of division is very

suitable for purposes of a literal translation, such as might be circulated in Rome.

At the risk of being prolix I venture to give an instance of an abbreviated text of a later author. I refer to the treatise upon the Apocalypse written by Primasius, Bishop of Hadrumetum, cent. vi. This was published in the Bale edition of 1544, and again in Migne's *Patrologia*. Migne omits large blocks of text which appear in the Bale edition, sometimes producing utter nonsense. I have recently examined an Oxford MS, Douce 140, known as D, written in the eighth century, which not only contains the passages omitted in Migne, but also long passages omitted by both editions. Here Migne gives a short text, the Bale edition a longer text, and D one longer still. The omissions are chiefly due to accident, not intention, and exhibit the regular operation of a numerical unit. In one passage, however, the problem is more complicated, viz. Migne 837 c (ed. Basil. n. 3). Here both editions give :

militant. In qua nunc quartam dari praedixit potestatem. Hanc dicit Ecclesiam, qua ex parte consistit variis malorum gladiis exercendam. Quod etiam etc.

D has :

militant (quae una in tribus  
quartis constare nuntiatur siue quia in  
trina unitate credit siue quod trium professi  
onum ordinibus grata uarietate distin  
guitur uirginum uiduarum et coniugum. In  
quibus nunc tribus quartis dari praedicet po  
testatem hanc dixit aeclesiam et una  
est et ex tribus quartis quadrata stabilita  
te consistet cum ad unum capud reperitur  
trina professio super aedificata inquit super  
fundamentum apostolorum et propheta  
rum. Ipso summo angulari lapide xpo ihu hunc  
uariis praedicet malorum cladib; exercen  
dam a gentilitate simulata fraternita  
te et heretica prauitate) quod etiam

I do not think that any one will here deny the authenticity of the longer text.

If my analysis of the Acts is sound, it affects vitally the similar question which arises as between the rival texts in the Gospels. This is of importance, since in the Gospels the evidence is different, and the method which I have followed is, apart from Mr Cronin's analysis of  $\aleph$  in St John, novel. I may say that I had not seen Mr Cronin's paper until my book was being read for press. My work upon the Gospels

falls into two parts. In the first I analyse the most important MSS, and find that all of them appear from internal evidence to be derived from ancestors written in very narrow columns containing on the average 10-11 letters, such as are found in the palimpsest of Cicero *de Re Publica* (cent. iv). The evidence is furnished by omissions, which seem to represent lines of an ancestor, and also by dittographies, dislocations, and corruptions of all kinds. Here the problem is purely palaeographical, and there is no place for any other considerations. I venture to think that the similarity of my results to those which Mr Cronin obtained, when working upon a branch of the subject, is somewhat striking. I postpone remarks upon my arithmetic until I proceed to answer some objections which have been made. It will be seen from my book that I lay great stress upon this analysis of the MSS, and venture to express myself with some confidence.

The second part of my work concerns what I call 'the most delicate and critical point in the inquiry', viz. the consideration of the main variants characteristic of the different families, in the hope that they may throw some light upon the formation of the archetype. I endeavour to put the reader in possession of all the facts, pointing out that the variants are 'a farrago taken from all sources', and that the influence of interpolation, coincidence, and mere chance must be taken into account.

The method which I follow here, as previously in my analysis of the individual MSS, is to arrange the omissions in order of length, and to let the larger figures speak for themselves. The theory is, that in a certain percentage of cases lines have been omitted. If so, we shall expect to find units and multiples of units, and also, when we come to large figures we shall observe numerical relations.

I have recently made a considerable study of old Latin MSS, chiefly patristic, in which a second hand has added passages omitted in the text, prefacing them with omission signs. Sometimes these are few in number, sometimes they are very frequent; thus in one MS I have noted 199 cases. Sometimes the unit is a very large one. Thus in Laud. Misc. 134, cent. ix, containing Augustine *de Spiritu et littera*, there are seven such supplements. One of these seems to have no connexion with the others, and to have been omitted by accident. The others contain the following number of letters :—

- 72 (three cases),
- 76 (one case),
- 77 (one case),
- 78 (one case).

Here it is obvious that 72-78 represents a line of the model. In many MSS I find a regular series of multiples. In such cases it is

always the largest figures which are most important, since in them the average length of the line asserts itself. Thus in Laud. Misc. 121, cent. ix, containing Augustine *de doctrina Christiana*, the largest passages added by the second hand, with omission symbols, contain the following number of letters: 100, 101, 224, 399, 425.

The first two are practically identical, while 399 is almost  $100 \times 4$ . If we subtract 399 from 425 the result is 26. I now call attention to the following multiples:  $25 \times 4 = 100$ ,  $25 \times 9 = 225$ ,  $25 \times 16 = 400$ ,  $25 \times 17 = 425$ .

Among the shorter omissions are one of 75 letters, three of 50-52, seven of 24-27. Thus we have a regular series in an ascending scale. First there is an irregular unit 24-27, while in the multiples the average asserts itself and becomes dominant.

Similar evidence is yielded by various corruptions. Thus on f. 50 r the words *bonorum atque malorum* are inserted in the wrong place. They occur lower down in their correct context, viz.:

intra unum rete piscium bonorum et malorum.

The intervening words between the right and the wrong place contain 647 letters. Here the same multiple appears ( $25 \times 26 = 650$ ). The inference is that the words *bonorum atque malorum* were in the first place omitted by accident, then added in the margin by a corrector, and finally inserted in the text some 26 lines too soon. As 26 lines would be a very suitable number for the content of a page or a column, it is probable that instead of being inserted at a certain place in one page or column they got into another page or column at the same place.

I have a mass of such evidence, gathered from MSS in which omission symbols occur, which I hope to publish before long. Meanwhile I can only say that similar phenomena are normal, and where the unit is sufficiently large the facts cannot be gainsaid.

When we are concerned with MSS written in very narrow columns, e. g. 10-11 letters to the line, the larger numbers cannot be ascribed to the operation of so small a unit without further proof. This is furnished by correspondences which begin to appear between the larger figures. These are *φωνήματα συνερούσιον*. I can give no better example than that provided by Syr.<sup>Sin</sup>. The small figures present what I term a 'welter of confusion'. The largest are 83, 128, 132, 167, 262. Here 128 and 132 are practically identical. If we take an average the result is 130. We have now to notice that  $83 \times 2 = 166$  while  $130 \times 2 = 260$ . The explanation is that, while 83 equals a certain number of lines containing a particular unit, 167 equals twice that number of lines containing the same unit. So if 132 contains a certain number of multiples of that unit, 262 contains twice the amount. The proof, therefore, depends

on the articulation of the larger numbers and their relation to each other, not on the fact that they may be explained as multiples of 10-11 letters. My critics pass over my analysis of this particular MS in silence. I must ask whether they accept it or reject it.

I return to my list of disputed passages in the Gospels, most of which do not appear in the shorter text. There are two possibilities, viz. (1) that there are a fortuitous congeries of interpolations, and (2) that they are fragments of an ancestor. In the first case we should not expect to find any numerical relation between them, in the second we should. The five longest passages contain the following number of letters: 166, 167, 320, 829, 964. Here 166 and 167 are practically identical, while  $166 \times 5 = 830$ . So there is a numerical relation between 320 and 964, while 320 can be explained as  $160 \times 2$ .

My conclusion is that these numbers 160-167 correspond to some division in the archetype of the Gospels. I offer no opinion as to whether this was a page or a column. Columns of this length are found in the palimpsest of Cicero *de Re Publica* (cent. iv) and in a MS of the Gospels (N cent. vi). I point out that such an archetype must have been a codex or book, not a roll, and shew that the vellum codex was used at Rome in the reign of Domitian (A.D. 81-96) for popular books, e. g. Homer, Virgil, Martial, and works of Cicero and Livy.

These are the essential points in my work. It is necessary for me to indicate them since they have been disregarded. None of the distinguished critics concerned have questioned my analysis of the individual MSS, though it is here that the method employed can be best tested. On the subject of the Acts, which is the citadel of my case, Dr Sanday and Dr Souter preserve an *altum silentium*. Sir F. Kenyon does mention the Acts once, but in a signally unfortunate context. He reports me as saying that various omissions in the Gospels *and Acts* consist of 160-168 letters! It is discouraging to find my main point, stated in the Preface as well as in two chapters of my book, thus ignored.

I now turn to some objections which have been raised. I take first those of Dr Souter, since he has put them simply and in a tabulated form. They are

(1) B and N, the MSS which give the shortest text, are the oldest MSS we have, and are connected with Egypt, the home of textual criticism.

(2) The phenomena observed in D and other 'Western' authorities bear witness to the free handling which the texts had to submit to in the West at an early date.

(3) The tendency was for the text to become longer rather than shorter, on the principle that nothing be lost.

(4) In Ciceronian MSS the patching is bad where an omission has occurred. In New Testament MSS the 'added' parts are nearly always complete in themselves, and, in the judgement of many, better absent.

(5) In the case of the New Testament, MSS were from an early date (say about A. D. 175) sacred and of eternal moment to the scribes themselves. Wholesale omission on their part cannot be supposed, since they were particularly careful where the sacred words of the Lord were concerned.

As to (1), although B<sup>N</sup> are the oldest Greek MSS, their evidence is inferior in antiquity to that furnished by the Old Latin and Old Syriac versions, which are said to be not later than 150 A. D. Also, we have the quotations of the oldest Fathers. That the evidence of the oldest versions and the earliest Fathers is in favour of the longer text is not disputed. Further, the fact that Egypt was the seat of textual criticism is in itself suspicious. The oldest papyri of Homer present us with a longer text, from which a number of lines were excised by Alexandrine critics, sometimes on subjective grounds. Jerome reproaches Origen for corrupting the text of the Septuagint by the use of the Alexandrine obels and asterisks. Egypt is the natural place for an early revision of the New Testament, but the primitive text is more likely to have been preserved in Antioch or in Rome.

(2) The theory of 'free-handling' at any early date involves certain difficulties. I venture to combine this subject with that stated under (5).

The defenders of B<sup>N</sup> maintain, as they are bound to do, that the ancestor, or ancestors, from which they are descended, remained pure from such interpolations. Thus the scribes are divided into the sheep and the goats, or possibly into one sheep and a number of goats. I find no such dichotomy between scribes at other periods. They seem to me honest and simple folk who were subject to various infirmities, but did their best at all times. The assumption that scribes after about A. D. 175 looked upon a casual omission as a deadly sin to be avoided at all hazards does not agree with the facts. I have remarked (p. 57) that 'the writer of B<sup>N</sup> was a helpless victim to *δμοιότης*, while Syr.<sup>sin</sup> omits continually, with freedom for which I know few parallels'. Scrivener says that B<sup>N</sup> has 115 examples of omission due to *δμοιότης* in the New Testament. Similar instances will be found in k, and Dr Sanday frequently describes its text as 'abridged'. But if it was a sin to omit, it was a sin to abridge, and still more to interpolate. Yet the theory obliges us to suppose that at a very early date, going back to subapostolic times, men interpolated with the utmost freedom, without thinking that they thereby endangered their souls. The hypothesis of omission is from a psychological view far simpler.

(3) The statement that the tendency was for the text to become

longer may be true in certain cases, at certain periods, or in a certain sense. Thus a reviewer of my work in *The Times* says that this is so in the development of the Vulgate. I accept the statement unreservedly. I should suspect, however, that this is due to the conflation of variants, a fact which is indisputable in itself, though some of Hort's examples are questionable in the case of the New Testament. In Latin MSS such conflations become most noticeable in the period following on the Caroline Renaissance, when it became a common practice to enter a variant from one MS into another. Also, the ravages of accident must always be taken into consideration. Thus, a number of passages omitted by the first hand are added by a corrector with omission marks in such MSS as, e.g., the Lindisfarne Gospels (cent. vii-viii), or Harl. 1775 (cent. vii), both of which contain the Vulgate. The cod. Rehdigeranus (Old Latin) cent. vii is interesting in this connexion. The writer was exceedingly conscientious, and, when he found that he had omitted something, he erased on occasions a number of lines and rewrote the passage. In spite of all his care he failed to notice a number of omissions, which were added by a second hand. One of these additions consists of two columns with 20 lines in each. As this happens to be the number of lines in the Rehdigeranus itself, it is clear that the scribe omitted one page of an ancestor written in the same formation.

It is impossible to insure against such risks. A text is like a traveller who goes from one inn to another, losing an article of luggage at each halt. I would add that such omissions are especially likely in the case of MSS written in narrow column, since this arrangement is very confusing. For proof I refer to Van Buren's transcript of the *de Re Publica* palimpsest.

(4) The objection that so many of the disputed passages are complete in themselves is a grave one, and in my judgement the most important which has been brought against my theory. I wish to treat it with great respect.

I would remark—

(a) When Dr Souter says that 'in Ciceronian MSS' the thought is disturbed where an omission has occurred, I presume that he is referring to instances quoted in my book, which are chiefly chosen to illustrate a rent in the text. I could easily provide him with hundreds of cases in which there is no such rent. Perhaps the most striking example is Cic. *Phil.* ii 93-96, where a long self-contained passage *sunt ea quidem . . . acta defendimus* is omitted by D (i. e. all MSS except V). So in the same speech § 3 a complete passage of some length *sed hoc . . . fuisse* is omitted by D.

(b) The *pericope de adultera* in St John vii 53-viii 11 is not really a separate incident, since the story is preceded by the verse (vii 53) καὶ

ἐπορεύθησαν ἕκαστος εἰς τὸν οἶκον αὐτοῦ. It is difficult to see why an interpolator should have added these words as a preface to the story.

(c) The various MSS exhibit the same phenomena in the case of passages which are undoubtedly genuine. I would refer once more to the larger omissions of Syr.<sup>Sin.</sup> consisting of 83, 128, 132, 167, 262 letters. The authenticity of the passages is beyond dispute, and their numerical relation shews that they represent lines in the exemplars. They are, however, in all cases self-contained.

This being so, we have to ask why it is that these omissions, if such they are, so frequently form complete sentences?

In the first place we must remember that in the Gospels the sentences are very short, quite unlike the Ciceronian period. If short sentences are written in narrow columns, there is considerable chance that the end of the sentence may coincide with the end of the line. Secondly, there is a tendency in some MSS, e.g. **ℵ** from time to time, and in the Old Latin MSS generally, to adopt a system of paragraphs, in which the last line is slightly shorter than the others so as to begin a new sentence or new subject with a fresh line. This slight reduction in the length of the last line, i. e. of 4 or 5 letters, would not affect the average shewn by the figures. Thirdly, and this is the reason to which I attach most weight, there is a great difference between the transmission of the Gospels and that of classical texts, especially in Latin. In ordinary pre-Caroline MSS the mistakes shew that the copyists were grossly ignorant, and the correctors little better. Consequently, bleeding wounds in the text were unnoticed and untended. In the case of the Gospels the scribes knew what they were writing, and corrected errors when they were obvious.

Thus in Matt. x 14 the model of B seems to have had

καὶ ὅς ἂν  
μὴ δεξιῆται υ (10)  
μας

The first hand omits *μὴ δεξιῆται υ*-, but the second hand repairs the loss.

So Luke xvi 1, the model of B seems to have had

δια  
σκορπιζὼν τα υπ (13)  
αρχοντα αυτου (12)  
και

The first hand omits *αρχοντα αυτου*, but the omission is again rectified. If, however, the rent was not so obvious, the error might not be noticed.

Thus in Mark xiii 8 the model of **ℵ** seems to have had

και  
βασιλεια επι (11)

βασιλειαν ε (10)  
 σονται σεισμοι (13)  
 5 κατα τοπους ε (11)  
 σονται λιμοι (11).

Here  $\aleph$  omits l. 2 and ll. 5-6, and the mistake is not rectified. The explanation, therefore, seems to lie in the survival of the fittest. When the passage omitted formed a complete clause or sentence, its absence was not so likely to be detected.

Dr Sanday attacks the numerical method which I have employed, though with some qualifications. Thus he says of my unit 10-11 letters, 'the favourite hypothetical unit of length, in the narrow columns of the earliest MSS, is—*quite naturally and rightly*—ten letters to the line'. This is an important admission. His contention, however, is that the numerical test is too elastic to be of any practical use. I am thoroughly aware of the difficulties involved where the unit is so small, and so far from concealing them, I have done my best to point them out. Thus when speaking of Syr.<sup>Sim</sup> I noted that certain omissions of 14-18 letters, which I thought too frequent to be due to mere chance, 'might possibly represent lines in the model before the writer', as contrasted with the shorter unit which appears in the bulk of the omissions. So when dealing with  $\aleph^k$  I shew that there was an intermediate Latin ancestor with 14-15 letters to the line, speaking here with confidence.

Dr Sanday says, 'if we have the decimal system for one set of archetypes, we have for another (say) 15 or 16 letters to the line'. This is a singular use of the word 'archetype'. The correct sense of the term, so far as I am aware, is the exemplar from which all existing MSS are derived. There can be *ex hypothesi* only one archetype, though there may be a large number of intermediate MSS.

So, too, in chapter ix, where I put together the disputed passages, I have made no effort to arrange the evidence in a form favourable to my view. I say 'I feel that the reader has a right to have all the evidence before him, even if the result is at first sight chaos'. The method which I follow here and elsewhere is to let the figures speak for themselves, and this the larger figures do, and with no uncertain voice.

My contention is not that all omissions are to be explained in the same way. Chance is always an important factor, and any omission of any length may be due to *δμοίωτης*. What I maintain is that a certain proportion of omissions are due to a definite cause, viz. the loss of lines in a previous MS, and that where the curve of variations shews recurring bulges, as we rise from multiple to multiple of the unit, while the larger numbers are correlated and support each other, we are justified in considering that something more than blind chance is at work.

Dr Sanday owns that in this case 'the largest figures are the most

dazzling', and he proceeds to deal with the largest, viz. 964 (St Mark xvi 9-20). This he calls 'the key-stone of the arch'. As I have always endeavoured to point out difficulties in the way of my conclusions, I have in a note remarked, 'It is, I admit, remarkable that the last words of the Gospel should come exactly at the end of a page or folio'. This Dr Sanday terms a 'curious note', and proceeds to point out that in  $\aleph$  and D the various writings which they contain do not end at the foot of a page or a column, and that the unoccupied space is left blank or partially filled by a subscription. He asks, if there was any blank at the end of the archetype of St Mark, how can we be sure that the figure 963 represents any unit or multiple of a unit of any length? The answer to this question is, because it is in an obvious relation to the preceding figures 166, 167, 320, 829. I can only conclude that the archetype did end here, unless indeed something, possibly a few words which came after v. 20, has been lost. And now I must say a few words as to the likelihood that the end of St Mark did coincide with the end of a folio or a page or a column in the archetype.

It is, of course, beyond dispute that a document may end anywhere. In a MS which contains 16 lines to a page the chances are 15 to 1 against its ending in the last line of a column. In the case of B, which has 42 lines to the page, the odds are, I believe, 41 to 1 against the coincidence. In it, however, the Gospel of St Luke ends on the 41st line of column 2; and, if the first hand had not accidentally omitted *μεγάλης* in v 52, it would have ended in the last line. This is fairly near, but a better case is to be found in the Epistle to the Philippians, which in B ends with the last line of column 2. A still better example occurs in the cod. Augiensis (29 lines to the page), where the same epistle ends with the close of a folio. In the same MS the Epistle to Philemon, preceded by the title, occupies exactly one page. In the cod. Rehdigeranus St Mark ends with the last line of the page, a very exact parallel. It will be seen, therefore, that such coincidences, though unlikely, do actually occur. On the other hand, I must ask what the mathematical chances are against such sequences as 83 and 167, 132 and 262, or 166, 167 and 829, 320 and 964, coming out, unless some common cause is at work?

I cannot agree with Dr Sanday that this passage is the 'key-stone of the arch'. If I had to select an architectural metaphor I would call it the 'coping-stone'. It crowns the edifice, but, if it is removed, the fabric still stands firm.

He also refuses to recognize the validity of the other large figures on the ground that the authorities for the omission or addition vary. Thus, e.g., while St Mark xvi 9-20 'is found in all the leading uncials except  $\aleph$  B, the addition to St Luke v 14 has the solitary support of D'. He

says 'the groupings are quite different', and infers that the readings belong to different strata and came in at different times. I can only reply that all arguments about groupings are highly subjective, while mine is an objective test. I have indicated (p. 48) some singular evidence furnished by the omissions in Syr.<sup>Sin.</sup> and k, which seem to cast light upon the relation of the Old Syriac and Old Latin versions to each other. When such questions have been decided, it will be possible to discuss groupings. For the present I prefer to survey the wood from an eminence, not to lose my way among the trees.

Dr Sanday adds that I stake my all on 'a single line of proof, and that line is purely mechanical'. I must demur to this charge. I have nothing at stake. My interest in the problem is palaeographical. I have no axe to grind. What I have done is to apply a new method to a subject of the greatest complexity which has been studied from every other point of view. I have kept carefully aloof, as I state (pp. 106, 108), from more ambitious enquiries, and borne in mind the maxim that 'a cobbler should stick to his last'. Such a method, I believe, is that which Aristotle calls ἐξ ἀφαιρέσεως, when the investigator leaves out of sight those facts which belong to other inquiries and restricts himself to the particular points which he has in view.

The most formidable objection raised by Sir Frederic Kenyon is one on which he is a recognized expert, viz. the formation of the archetype to which the Gospels (not the Acts, as he adds) seem to go back. The references which I give are taken from Sir E. Maunde Thompson's work. Sir F. Kenyon repeats them, and acknowledges that vellum books, as opposed to rolls of papyri, were in use at Rome as early as A. D. 81-96. He attempts, however, to turn my flank in a singular manner. He says that out of the two vellum leaves, written in the second century A. D., which have come down to us, one, in verse, contains 26 lines to the page, while the other, which is in prose, is written in double columns of 36 lines with 24-29 letters each. He concludes that such a formation as 16 lines to a page with 10-11 letters to the line is not yet a *vera causa*, so far as the second century A. D. is concerned.

This really seems unreasonable. When we have only two extant specimens of a second-century book, one of which, being in verse, is scarcely germane to the discussion, it is too much to expect that the other will afford a specimen of any particular formation. It is not long ago since Birt, following up the stichometrical speculations of Graux, talked about a line of 35-37 letters as 'normal' in papyri. Since then the discovery of papyri in large quantities has shewn that the theory of a normal line is a figment. The papyri are of all shapes and sizes, according to the caprice of the writer and the character of his material. The columns vary very greatly in size. From some notes which I made,

when looking through the theological and classical papyri published by Grenfell and Hunt, it appeared to me that the most frequent sizes contain an average of about 18, 22, 24, 28 letters to the line. The 'normal' line of 35-37 is found, e. g., in Oxyrhynchus 418, 668, 880, 884, but is not common. On the other hand, we have examples of such narrow columns as 10-12 letters, and occasionally abnormally broad ones, e. g. in Ox. 1251 (Cicero *pro Caelio*), where the lines contain some 70-80 letters. Since Sir F. Kenyon says that in papyri the columns are hardly ever so narrow as to contain only 10-12, I would mention the following specimens among the above-mentioned papyri:—

- Ox. 883 Demosthenes, 3rd cent.
- 1010 Ezra, 4th cent.
- 1084 Hellanicus, 2nd cent.
- 1093 } Demosthenes, 2nd cent.
- 1182 }
- 1176 Satyrus, Life of Euripides, 2nd cent.
- Greek Papyri (Grenfell), vol. i 6 Zechariah, 5th cent.
- 8 Protevangelium, 5th/6th cent.

It will be noticed that three of the examples belong to the second century. The narrowest columns which I noticed are in Ox. 1176, where the average is, I think, less than 10 letters.

In Latin these narrow columns are frequent, e. g. in the palimpsest of Cic. *de Re Publica*. In this the average length is 10-11 letters, but there are narrower columns with an average of 9. We find the same formation in the Ambrosian palimpsest of Cic. *pro Scauro*, &c., and I am in a position to prove that the Vatican palimpsest of the Verrines was copied from a model written in this way. The evidence of the most ancient Latin Gospels shews that this was the favourite formation. I refer to the Vercellensis, cent. iv; the Palatinus, cent. v; the Veronensis, cent. v/vi, as well as to others of later date. Also, I must point to my analysis of  $\aleph$  B, Syr.<sup>Sin.</sup> and k, and ask whether Dr Kenyon accepts, as apparently Dr Sanday does, the contention that they are derived from a succession of MSS written in this formation. In the case of  $\aleph$  I would request that Mr Cronin's analysis should be compared with mine.

The question therefore narrows itself down to the point whether such a MS as the *de Re Publica* palimpsest (cent. iv), which exhibits the particular formation which I desire, viz. 16 lines with an average of 10-11 letters, could have been written in the second century. I do not see how it is possible to prove that it could not. I had used the term *edition de luxe* of the MS of the Gospels (N), cent. vi, a *codex purpureus*, of which Sir F. Kenyon says in his handbook that 'the writing is in silver, with gold for the abbreviations of the sacred names, and the

letters are unusually large', and on this account hesitated to compare it with any supposed archetype. Sir F. Kenyon transfers the description to the palimpsest of the *de Re Publica*, which I supposed to be written in the ordinary style of the period, and not to be an *édition de luxe*. At any rate it is not written on purple parchment or in letters of silver and gold.

Sir F. Kenyon also finds flaws in my arithmetic. He says that of such a unit as 10-12 any number over 40 can be a multiple. I am fully sensible of the difficulty. When treating the individual MSS I have shown how the evidence is buttressed by constant correspondences which transcend the limits of coincidence. When putting together the variants gathered from all sources, I have drawn no conclusions, except in five cases where there was further evidence, and I have left the larger figures to speak for themselves. After they have spoken, then, if I interpret their evidence aright, a presumption arises in favour of the longer text as compared with that of  $\aleph B$ , but the individual cases must necessarily remain a matter of doubt.

In the same way he attacks some larger numbers which I give when discussing Spitta's theory about dislocations in St John's Gospel. I hold no brief on behalf of this theory. It was first mentioned to me after my own work was completed, and I speak of it in a very tentative way, saying that 'I do not profess to have sounded the question to its depths' (p. 70). Here there are four figures concerned, viz. 167 (ch. v 4), 997 (ch. vii 1-14), 4372 (ch. vii 15-24, and v), and 5540 (ch. vi). I point out that  $166 \times 6 = 996$ ,  $168 \times 26 = 4368$ ,  $168 \times 33 = 5544$ . Sir F. Kenyon remarks that the figure 33 is not an even number, and implies a dislocation of  $16\frac{1}{2}$  folios. This is a sound argument, and, whether the unit be a page or a column, which I leave undecided, the difficulty remains, and has to be set against the coincidences which I mention. He suggests that I should make it  $163 \times 34$ , and then remarks that with so fluctuating a base as a unit of 160-168 any figure higher than 3200 can be shown to be a product of some such combination. It is undoubtedly true that  $160-168 \times 21 = 3360-3528$  and  $160-168 \times 22 = 3520-3696$ , but I must, very gently, point out that in the two large numbers I have allowed myself no such liberty, but have employed the same unit, viz. 168, with the result that in each case the multiple is exact except for four letters. His criticism, therefore, is directed upon a position which he invites me to take, not one which I occupied.

There is another point in which I seem not to have made my meaning clear. Sir F. Kenyon, after speaking of the archetype which I suggest, says that I attempt to 'account for the majority of the phenomena which differentiate the Neutral Text from the Western by the omission of whole lines, pages and leaves of *such a MS*'. What

I say (p. 57) is, 'I do not suggest that all the omissions in any of the important MSS are due to the negligence of a single scribe. It is more probable that they represent the sum-total of omissions made by a series of ancestors written in columns of similar breadth.' If this is true of the individual MSS, it is still more true when the whole tradition is taken into account. There must have been a number of intermediate MSS, traces of which are occasionally embedded in our texts. I point out one such case in St Luke xxii 17-22, where there is a curious series of dislocations and omissions peculiar to the Syriac and Old Latin. Here, obviously, a special ancestor is concerned. Sir F. Kenyon remarks 'some cases remain recalcitrant, for these he discovers yet another unit of about 152 letters'. I really cannot sin against all the laws of textual criticism by ignoring the existence of intermediate ancestors.

Both Dr Sanday and Sir F. Kenyon have an 'alternative plea'. This is that my method is not new. Dr Sanday says that 'the phenomena to which he calls attention and the hypothesis by which he would explain them have been recognized as a *vera causa* for some time'. Sir F. Kenyon sums up the case by saying that 'what is true is not new, and what is new is not true', a somewhat well-worn remark. I do not think that a case is strengthened by such 'alternative pleas'. I do not wish to claim novelty for my method, especially if there is a necessary antinomy between novelty and truth. I would only remark that, when I began my enquiry upon the text of Cicero, I had no guides, and that I have carefully recorded in my work any anticipations of my method which I came across. I shall be grateful for any additions, in the hope that I may be able to acknowledge them at a future date.

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