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## THE CAESAREAN TEXT OF THE GOSPELS<sup>1</sup>

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OF ALL the thousands of Greek manuscripts of the New Testament, the immediate and exact parentage of only a very few is known. Even those manuscripts which were written in the same *scriptorium* manifest scarcely any tokens of close and direct relationship. But although the available data have thus far resisted practically all efforts to discover immediate filiation, New Testament textual critics have succeeded in establishing with some degree of confidence the more remote relationship of certain groups of manuscripts. These groups of manuscripts, depending upon the closeness of the relationship, may be called families or texts. A family of manuscripts, it may be said, is ordinarily more limited in extent than is a text and discloses much closer affinity among its members than is expected of a text. Consequently it is usually possible to reconstruct the common archetype of a family with but a slight margin of error. To reconstruct a text, however, involves far greater difficulties, and the common ancestor which lay behind its known descendents can often be determined only approximately, particularly when the extant manuscripts have been modified by successive copying or revision. There are, of course, many intermediate stages between the extremes of family and text,

<sup>1</sup> The following are the *sigla* employed in the article to designate periodicals and series which are referred to more than once: *ATR* (*Anglican Theological Review*), *B* (*Biblica*), *BBC* (*Bulletin of the Bezan Club*), *CQR* (*Church Quarterly Review*), *HTR* (*Harvard Theological Review*), *JBL* (*Journal of Biblical Literature*), *JR* (*Journal of Religion*), *JTS* (*Journal of Theological Studies*), *OC* (*Oriens Christianus*), *PO* (*Patrologia Orientalis*), *RB* (*Revue Biblique*), *SD* (*Studies and Documents*), *TS* (*Texts and Studies*), *ZNW* (*Zeitschrift für neutestamentliche Wissenschaft und die Kunde der älteren Kirche*).

but the distinction between the two, though at times apparently only academic, is none the less a valid and helpful one.

During the past seventy-five years the combined efforts of many scholars have succeeded in isolating and analyzing several families of New Testament manuscripts which together constitute, so it has been held, a certain type of text, the so-called Caesarean text. In view of a new turn which this investigation has recently taken, it seems both profitable and necessary first to review the several stages in the isolation of the component parts of this text of the Gospels, then to summarize some of the more important discussions and debates regarding the interpretation of the evidence, and finally to indicate some of the tasks and problems which remain to be investigated.

### 1. *The Beginnings: Family 13*

As far back as 1868 a professor of Latin in Dublin University, W. H. Ferrar, discovered that four medieval Greek Gospel manuscripts, those known as 13, 69, 124, and 346, were closely related textually. His collations were published posthumously in 1877 by his friend and colleague, Professor T. K. Abbott.<sup>2</sup> It was their opinion that these four minuscule manuscripts were descendants of a not very distant uncial ancestor of good character.

It was not long before the interest of the other scholars was aroused. F. H. A. Scrivener noticed that 543 is related to the group and prepared a careful collation of the document.<sup>3</sup> The Abbé J. P. P. Martin pointed out that at least three of the four manuscripts (namely 13, 124, 346) were written in Calabria, in the "toe" of Italy, and concluded that this was the presumed home of the archetype. He was also of the opinion that 348, whose Calabrian origin he thought he had demonstrated, was also a member of the Ferrar group.<sup>4</sup>

<sup>2</sup> *A Collation of Four Manuscripts of the Gospels* (Dublin and London, 1877).

<sup>3</sup> *A Plain Introduction to the Criticism of the New Testament*, 3rd ed. (London, 1883), p. 236. The collation was published posthumously in *Adversaria Critica Sacra* (Cambridge, 1893).

<sup>4</sup> *Quatre manuscrits importants du Nouveau Testament, auxquels on peut ajouter un cinquième* (Paris, 1886), a reprint of his article of the same title in *Revue des sciences ecclésiastiques*, sixième série, tome III, no 313 (Jan. 1886), 5-33

At about the same time a British scholar, J. Rendel Harris, while teaching at Haverford College, published several pieces of investigation concerning this family of manuscripts. First of all, he observed that manuscript 713 is related to the Ferrar group,<sup>5</sup> and further research has proved him to be correct at least for Matthew.<sup>6</sup> Of much more significance was his thorough investigation of the origin of 69.<sup>7</sup> The same author, on the ground of certain affinities with the Old Syriac version, sought to establish a Syriac origin for the most characteristic readings of the group,<sup>8</sup> while in a subsequent study<sup>9</sup> he argued that this Syriac influence had been transmitted via an Arabic medium. More important, however, was his discovery that most of the Ferrar manuscripts are provided with much the same selection of various "helps for the reader," that is, with a menology, the lives of the apostles, a list of the patriarchates, the appearances of the risen Christ, the symbols of the evangelists, and so on.<sup>10</sup>

Meanwhile, one of Harris's pupils, Kirsopp Lake, had examined a number of Italian manuscripts which had been suspected of being related to the Ferrar group and added two more members to the group, 826 and 828.<sup>11</sup>

The next great advance was made by Hermann von Soden who discovered that the Ferrar group, to which he had added other members, falls into three sub-groups: (a) 983 and 1689;

<sup>5</sup> "Cod. Ev. 561 — Codex Algerinae Peckover," *JBL*, 1886, 79-89; and "An Important Uncollated Manuscript of the New Testament," *The Sunday School Times* (Philadelphia), 28 (Nov. 6, 1886), 707.

<sup>6</sup> A. Pott (*Der griechisch-syrische Text des Matthäus, ε351 im Verhältnis zu Tatian, Ssc, Ferrar* [Leipzig, 1912], p. 41) concluded that this manuscript is closely related to the Ferrar group but not so closely as the four Ferrar manuscripts are related to each other. Later Harris defended his opinion concerning the textual affinities of this manuscript (*Expositor*, VIII, xxiii [1922], 120-129) against Kenyon's misrepresentation (*A Handbook to the Textual Criticism of the New Testament* [2nd ed., London, 1912], p. 139).

<sup>7</sup> J. Rendel Harris, *The Origin of the Leicester Codex of the New Testament* (Cambridge, 1887).

<sup>8</sup> *On the Origin of the Ferrar Group* (Cambridge, 1893).

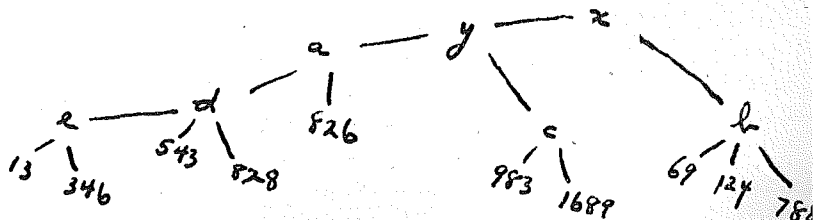
<sup>9</sup> *Further Researches into the History of the Ferrar Group* (Cambridge, 1900).

<sup>10</sup> *Ibid.*

<sup>11</sup> *JTS*, 1 (1899-1900), 117-120.

(b) 69, 124, 174 (only in Matthew), and 788; (c) 13, 230, 346, 543, 826, and 828.<sup>12</sup>

A British vicar, the Reverend E. A. Hutton, in what he called *An Atlas of Textual Criticism*,<sup>13</sup> devoted an excursus to an examination of several manuscripts of the Ferrar group and concluded that, of the five at his disposal,<sup>14</sup> 69 and 124 are the two best representatives of the group. Mr. Hutton's analyses, however, have been altogether superseded by the definitive work of Professor and Mrs. Lake, who have printed the Greek text of Mark according to what was probably the archetype of the group.<sup>15</sup> The stemma which they have worked out is as follows:



In their discussion of the date and provenance of the manuscripts they incline to a slightly earlier date (eleventh century) than that ordinarily assigned (twelfth century, so Gregory and von Soden) to the majority of the documents. Many of their notes on the textual variants are invaluable, and the whole monograph will remain a landmark in the investigation of the Ferrar group.

## 2. Further Advances: Family 1, etc.

Four other Byzantine manuscripts (dating from the twelfth to the fourteenth century) were discovered by Kirsopp Lake to be related in a family which manifests many of the characteristics of family 13. These are manuscripts 1, 118, 131, and

<sup>12</sup> *Die Schriften des Neuen Testaments*, I, ii, (Leipzig, 1906), 1066 ff.

<sup>13</sup> Cambridge, 1911.

<sup>14</sup> Namely, codices 13, 69, 124, 346, and 543.

<sup>15</sup> Kirsopp and Silva Lake, *Family 13 (The Ferrar Group); the Text According to Mark* (SD, XI. London and Philadelphia, 1941).

209.<sup>16</sup> Later, manuscript 1582 (tenth century) was likewise recognized by Lake as belonging to this group.<sup>17</sup> Von Soden also regarded certain others<sup>18</sup> as weak representatives of the same family. But in view of the inaccurate collation of at least one of these manuscripts (codex 22)<sup>19</sup> which he used, other scholars are not inclined to put too much reliance upon the trustworthiness of his opinion either in this case or in other cases where they are unable to check his material.

With these two families of manuscripts, family 1 and family 13, subsequent study has classed certain other individual codices which possess texts more or less closely related. The full and accurate edition of the Koridethi Gospels (Θ) by Gustav Beerman and C. R. Gregory in 1913 enabled scholars to examine its textual affinities with much more precision than von Soden's earlier (1906) notices of this manuscript had permitted. The most

<sup>16</sup> *Codex 1 and its Allies* (TS, VII. 1) Cambridge, 1902). The Lakes have recently conjectured that the perpetuation of the form of Caesarean text found in family 1 may have been due to Arethas, Bishop of Caesarea in Cappadocia ("The Scribe Ephraim," *JBL* 62 [1943] 267).

<sup>17</sup> K. Lake, *The Text of the New Testament*, 6th ed. revised by Silva New (London, 1928), pp. 20 f.; and *JBL* 62 (1943) 264 and 266.

<sup>18</sup> Namely 22, 205, 206, 687, 872Mk, 924, 1005, 1192, 1210, 1278, and 2193 *op. cit.*, I, ii, 1042 ff.). Still another member of family 1 is 1542, whose kinship with the rest, though recognized by von Soden (it is his E1337; *op. cit.*, I, ii, 1289 f.), was obscured by his grouping and seems to have been generally overlooked by other investigators. The merit of calling renewed attention to its affinities belongs to C. A. Phillips, who drew up a list of the readings it shares with other Caesarean witnesses (*BBC* 10 [Leyden, 1935], 12-19). It may be said here that the fascicules of this Bulletin were privately printed and distributed among the score of members of the Club, which was organized by J. Rendel Harris in about 1925. Through the kindness of two members of the Club, E. A. Lowe and H. A. Sanders, the present writer was granted permission to have a micro-film made of their copies of the twelve numbers of the *Bulletin* (Lowe supplied numbers one to eleven, with an index of the first ten; Sanders supplied the twelfth and final number). Copies of the film are in the libraries of the Divinity School of the University of Chicago and of Princeton Theological Seminary.

<sup>19</sup> Kirsopp Lake, *The Text of the New Testament*, 6th ed., p. 21; and H. A. Sanders, "A New Collation of Codex 22" (*JBL* 33 [1914] 91-117). On von Soden's accuracy in general, see Hoskier's damaging evidence in *JTS* 15 (1914), 307-326.

elaborate of such examinations was the careful study by K. Lake and R. P. Blake in 1923 which showed that in Mark  $\Theta$  is closely connected with families 1 and 13 as well as with 28, 565, and 700.<sup>20</sup>

Another group which von Soden isolated (the  $I^\phi$  group) exhibits certain affinities with the manuscripts just mentioned. In Matthew and Mark the sub-divisions of this group are, according to von Soden, (a) 349, 517, 954, 1424, and 1675; (b) 7, 115, 179, 267, 659, 827, 1082, 1391, 1402, 1606, and 2191; (c) 160, 945, 990, 1010, 1207, 1223, and 1293; (r) M, 27, 71, 692, and 1194. In Luke and John they are (a) 349, 517, 954, 1188, 1424, and 1675; (b) 7, 185, 267, 659, 1391, and 1606; (c) 945, 1010, 1207, 1223, and 1293; (r) M, 27, 71, and 1194.<sup>21</sup>

In 1924 the versatile British churchman and Oxford scholar, Burnett Hillman Streeter, published a brilliant volume on *The Four Gospels* in which, *inter alia*, he came independently to the same conclusion as Lake and Blake regarding family  $\Theta$  and went beyond it in showing that this text group is not confined

<sup>20</sup> "The Text of the Gospels and the Koridethi Codex" (*HTR* 16 [1923] 267-286). The collation of 28 was supplied privately by H. C. Hoskier. This has been newly collated by the Lakes in Appendix I of their *Family 13*. A full collation of 565 (which is Tischendorf's 2<sup>pe</sup> and Westcott and Hort's 81) is available only for Mark: J. Belsheim, *Das Evangelium des Marcus nach dem griechischen Codex aureus Theodora Imperatricis purpureus Petropolitani aus dem 9ten Jahrhundert in Forhandlinger i Videnskabs-Selskabet i Christiania*, 1885, no. 9 (Christiania, 1886); H. S. Cronin did no more than correct Belsheim's collation of Mark (appendix in *TS*, V. 4). Codex 700 is available in H. C. Hoskier, *Collation of Codex Ev. 604* (London, 1890). Von Soden had, indeed, included many other diverse manuscripts in the same group. With D, W, and  $\Theta$  he gathered together 21, 28, 79, 279, 372, 399, 406, 544, 565, 700, 1515, 1542, and 1654. But practically all subsequent investigators have regarded these as far too heterogeneous to be brought under the same *vinculum*. On the other hand, however, H. C. Hoskier maintained that "it is not correct to divide  $\Theta$  and these cursives [family 1, family 13, 28, 565, and 700] from the *Dd* text" ("A Note on 'Eastern' and 'Caesarean' Texts," *BBC* 5 [1928] 14). See also Hoskier's *Codex B and Its Allies; a Study and an Indictment*, I (London, 1914), 136-39, where he cites numerous instances throughout Mark where W, 28, 565, and 700 are supported by Old Latin witnesses; his article "Some Study of P<sup>45</sup> with Special Reference to the Bezan Text" (*BBC* 12 [1937] 51-57) makes no direct reference to the matter.

<sup>21</sup> *Op. cit.*, I, ii, 1109-1147.

to Mark but applies also to the other Gospels. To the already expanding family he added several other witnesses more or less closely related. The primary authorities of this text, according to Streeter, are  $\Theta$  and 565<sup>Mk</sup>; the secondary authorities, family 1, family 13, 28, 700, (W<sup>Mk</sup>),<sup>22</sup> and the Old Georgian; the tertiary authorities, family 1424,<sup>23</sup> 544, N-Σ-O, Φ, and 157; and the supplementary authorities, U, A, 1071, 1604, and the Old Armenian.

Furthermore, Streeter's research suggested to him a name for the text. He believed that he had proved that Origen used a Neutral type of text of Mark at Alexandria but another type, very like  $\Theta$ , after moving to Caesarea. Streeter felt justified, therefore, in calling this type of text Caesarean and thought that Origen found this text first in Caesarea of Palestine.<sup>24</sup>

This opinion, however, was modified slightly by a joint undertaking entitled "The Caesarean Text of the Gospel of Mark" by Kirsopp Lake, R. P. Blake, and Silva New.<sup>25</sup> These scholars corrected Streeter's theory by pointing out that Origen possibly used a Caesarean text before leaving Alexandria, that at Caesarea he at first used an Alexandrian text, but soon reverted permanently to a Caesarean type of text. But in other details their investigation corroborated Streeter's analyses, confirming and supplementing his work. At the conclusion of the monograph they print their reconstruction of the Caesarean text of three sample chapters of Mark, namely chapters 1, 6, and 11.

In the estimation of these three scholars the most significant new contribution to a knowledge of the Caesarean text was the

<sup>22</sup> Streeter discovered later, in time to add an appendix to the second impression of his book (1926), that in Mark two-thirds of W is Caesarean in character (from Mark 5:31 to 16:8). See also Streeter's article in *HTR* 19 (1926) 165-172; and C. A. Phillips' remarks on Streeter's discovery (*BBC* 5 [1928], 9-12).

<sup>23</sup> Streeter's family 1424 is von Soden's  $I^\phi$  group.

<sup>24</sup> Kirsopp Lake had, indeed, so long ago as 1900, thrown out the suggestion that one ought to localize the text of some of these minuscules at Caesarea; see the 1st ed. of his *The Text of the New Testament*, p. 21. He removed the statement from the following editions, restoring it in the last — the 6th, revised by his student, Miss Silva New, 1928 — after Streeter had confirmed the guess.

<sup>25</sup> *HTR* 21 (1928) 207-404.

publication of a small vellum fragment of Mark 11 11-17. It is Berlin Mus., äg. Abt., P. 13416, which was assigned the Gregory-Dobschütz number 0188.<sup>26</sup> Its editor, A. H. Salenius of Helsingfors, found that it goes with E, G, H, 565, certain versions, and Origen;<sup>27</sup> and Lake, Blake, and New, refining his textual analyses, discovered that this short section comes from a codex strongly marked by Caesarean readings.<sup>28</sup> "The fragment," they write, "contains an unusual number of singular readings [four out of 17 variant readings which they cite], and suggests either that the variations within the Caesarean text may have been greater than the existing evidence would indicate, or that the correction of manuscripts to the standard of the Ecclesiastical Text went even further than we had thought."<sup>29</sup>

Another rather recently edited fragment, a papyrus leaf containing Matthew 26 19-52, has been assigned by Père M.-J. Lagrange and others to this same textual stock. The original editor, indeed, had concluded that the text of the fragment belongs to the Western text,<sup>30</sup> but Lagrange had little difficulty in pointing out that his conclusions were based on presuppositions which beg the question regarding the relation of the Western text to other texts.<sup>31</sup> Lagrange showed that the papyrus, P<sup>37</sup>, oscillates between B and D and agrees with Θ as many times as with B; he therefore assigned this early fragment to the Caesarean type of text.<sup>32</sup>

<sup>26</sup> ZNW 23 (1924) 252; and 27 (1928) 219.

<sup>27</sup> ZNW 27 (1928) 98. Salenius dated the fragment in the seventh century, but von Dobschütz (ZNW 27 [1928] 219) dated it in the fourth century.

<sup>28</sup> HTR 21 (1928) 212.

<sup>29</sup> *Ibid.* It may be mentioned that Henry A. Sanders reopened the question of the legitimacy of regarding 0188 as evidence for the Caesarean text, regarding it as basically Western (HTR 26 [1933] 83-87).

<sup>30</sup> Henry A. Sanders, "An Early Papyrus Fragment of the Gospel of Matthew in the Michigan Collection" (HTR 19 [1926] 215-226). He dates it between A.D. 200 and 350.

<sup>31</sup> RB 38 (1929) 161-177; and *Critique textuelle* II, *La critique rationnelle* (Paris, 1935), 157 f.

<sup>32</sup> Compare J. M. Bover's earlier analysis of P<sup>37</sup> in "Dos papiros egipcios del N. T. recientemente publicados" (*Estudios Eclesiásticos*, 9 [1930], 291-320, especially 290-306), where he indicates the decided Caesarean affinities of this papyrus.

### 3. *Oriental Versions and the Caesarean Text*

Not the least valuable part of the monograph on the Caesarean text of Mark by Lake, Blake, and New is the attention given to certain oriental versions, namely, the Old Armenian, the Old Georgian, and the Palestinian Syriac. They regard the Caesarean text as the basis of all three versions according to the following stemma. The relationship is traced from the Caesarean Greek text through a postulated Syriac text to the Palestinian Syriac on the one hand and to the Old Armenian (no longer extant) on the other.<sup>33</sup> The Old Georgian is held to be based on the latter. All extant manuscripts of the current Armenian version and almost all extant manuscripts of the Georgian version have been corrected to a Byzantine standard.<sup>34</sup>

It will be appropriate to mention at this point other investigations concerning these oriental versions. In 1919 Frédéric

<sup>33</sup> F. C. Conybeare was the first to observe that quotations by ecclesiastical writers seem to indicate the existence of an Armenian version whose text differed from that of extant Armenian manuscripts and editions. In the last article he wrote before his death, he advanced tentatively the suggestion that this early Armenian version of the Gospels was in the form of a diatessaron (*JTS* 25 [1924] 232-245). This hypothesis has recently been investigated with greater thoroughness by Paul Essabalian (*Le Diatessaron de Tatien et la première traduction des Évangiles arméniens* [Vienna, 1937]), in modern Armenian with a substantial résumé in French; see the review by R. P. Casey (*JBL* 57 [1938] 95-101). Essabalian and Stanislas Lyonnet ("La première version arménienne des Évangiles," *RB* 47 [1938] 355-382) conclude that there really was an Armenian diatessaron such as Conybeare suggested, based on a Syriac model either identical with or analogous to Tatian's Diatessaron. According to Lyonnet, when it was found to be advisable to have separate Gospels, the primitive Armenian version was worked over by several revisers who used Greek texts of a decidedly Caesarean character and thus introduced into all extant manuscripts of the Armenian version this Caesarean strain (see also Lyonnet in Lagrange, *Critique textuelle*, II, 354, 361, and 386). Lyonnet strengthens these conclusions in an important article in *B* 19 (1938) 121-150, entitled "Vestiges d'un diatessaron arménien." He finds that the *Rituale Armenorum*, published by the Mekhitarists of Vienna, preserves traces of an Armenian diatessaron which was colored by certain Caesarean readings.

<sup>34</sup> *HTR* 21 (1928) 324 ff., and Kirsopp Lake, *The Text of the New Testament*, 6th ed., pp. 44 f.

Macler<sup>35</sup> claimed for the Armenian version certain textual affinities with D and Θ, regarding it, in fact, as a weak representative of that type of text which was later to be termed Caesarean. Macler was also impressed by a very considerable amount of Greek influence still observable in the Armenian and was led to deny the existence of any Syriac stratum whatsoever in the Armenian, asserting categorically that the Armenian was translated directly from the Greek text.

But with reference to this latter view, Merk<sup>36</sup> and Blake<sup>37</sup> take issue and point out Syriacisms which can have come from neither the Evangelion da-Mepharreshe nor the Peshitta, but which (according to Blake) can be explained only by postulating the existence of a third Syriac text akin to the Caesarean.

Concerning Macler's former point, that the Armenian version is only a weak representative of the Caesarean text, Stanislas Lyonnet has little trouble in showing that his textual analyses were much too rough. A more refined and discriminating analysis discloses, Lyonnet maintains, that the Armenian text of Matthew agrees little if any with D, but does show a very decided affinity with Θ and its family.<sup>38</sup> Likewise in Mark the Armenian text is, according to the same scholar, an important witness of the

<sup>35</sup> *Le text arménien de l'Évangile d'après Matthieu et Marc (Annales du Musée Guimet, XXCIII, Paris, 1919), pp. 569-637.*

<sup>36</sup> *B 7* (1926) 40-71 (especially 63-68).

<sup>37</sup> *PO*, XX.3 (1929), 448.

<sup>38</sup> "La version arménienne des Évangiles et son modèle grec: l'Évangile selon saint Matthieu" (*RB* 43 [1934] 69-87). As can be seen from the title, Lyonnet agrees with Macler as against Merk regarding the direct dependence of the Armenian version upon the Greek and not upon any form of Syriac. C. S. C. Williams, "Syriacisms in the Armenian Text of the Gospels" (*JTS* 43 [1942] 161-167), examines afresh fourteen of the sixteen passages from the Gospels that J. Armitage Robinson cited (*Euthaliana, TS*, III.3 [1894], 76 ff.) "in which the Armenian Version offers us a rendering, which is not easily accounted for by supposing it to be a direct translation of any known reading of the Greek text." Williams finds only two of the fourteen instances to be convincing, and concludes that, "while it is probable that there was an early translation from Syriac into Armenian, as opposed to the later revision with Greek manuscripts, the Syriac element remaining is not strong" (p. 167). See also Maries, "Le texte arménien de l'Évangile d'après Matthieu et Marc" (*Recherches de science religieuse* 10 [1920] 28-54).

Caesarean text.<sup>39</sup> Indeed, Lyonnet believes that he has sufficient evidence to prove that not only in Matthew and Mark but also in Luke and John, so far as one can speak of a Caesarean text in these Gospels, the Armenian version is definitely Caesarean in character, "maintaining a happy equilibrium between the Western and Neutral texts."<sup>40</sup>

Another important study of the Caesarean content of the extant Armenian text discloses that, far from having lost its specific Caesarean coloring (as Lake, Blake, and New held), the text of Armenian manuscripts still preserves a very considerable proportion of the typically Caesarean readings. E. C. Colwell tested the amount of Caesarean readings in the text of Mark 1, 6, and 11 in six Armenian manuscripts and discovered that "the large majority of these Caesarean readings are supported by all the Armenian sources studied. The list included about 235 readings; of these 179 are found in all the Armenians. . . . Each of the Mss reads about 205 Caesarean variants; the American Bible Society text contains about 200."<sup>41</sup> Colwell found that the Armenian text agrees most often with Θ, 565, 700; less often with family 1, family 13, 28, and W. He concludes, therefore, that "extant Armenian Mss (and printed texts) contain a text of Mark that is strongly Caesarean in type."<sup>42</sup>

The investigation of the Georgian version is restricted to an

<sup>39</sup> "Un important témoin du texte césarien de saint Marc: le version arménienne" (*Mélanges de l'université saint-Joseph, Beyrouth, XIX.2; 1935*). See the reviews by Mlaher (*Orientalist. Literaturzeitung* 41 [1938] 631 f.) and by Baumstark (*OC, 3te Serie, XI Band [1936], 245-252*). The latter very ardently opposes Lyonnet.

<sup>40</sup> Lyonnet, in Lagrange, *Critique textuelle*, II, 365.

<sup>41</sup> "The Caesarean Readings of Armenian Gospel MSS" (*ATR* 16 [1934] 125). In the following year Lyonnet published in *Handes Amsorya* (49 [1935], cols. 596-603) a discussion of "Le tétraévangile de Moscou et son importance pour l'établissement du texte critique de la version arménienne," in which, on the basis of an examination of selected readings in Mark 1 and 6 in Etchmiadzin ms. 229, he concludes that this Armenian ms. manifests decided Caesarean affinities.

<sup>42</sup> *Ibid.*, p. 132. See also Pres. Colwell's article in *JR* 17 (1937) 48-61, "Slandered or Ignored: the Armenian Gospels," where he concludes that "the Armenian version is one of the strongest witnesses to the Caesarean text" (p. 59).

even smaller number of scholars than the Armenian. F. C. Conybeare's<sup>43</sup> preliminary work on its textual affinities has been largely superseded by the labors of R. P. Blake, who first called Streeter's attention to the significance of a collation of Mark 1 in certain Old Georgian manuscripts. With Lake and New in the Harvard article already referred to as well as in his edition of the Georgian version of Matthew and Mark,<sup>44</sup> Blake finds its text to be definitely of the Caesarean pattern,<sup>45</sup> and holds that it is a better witness of the Old Armenian, from which it was translated and which is no longer extant, than are any of the extant Armenian manuscripts.

Colwell, followed by Lyonnet,<sup>46</sup> agrees with Blake in regarding the Georgian version as Caesarean, but takes exception to ranking it above the extant Armenian version as the better witness to the Caesarean text.<sup>47</sup> Anton Baumstark, however, will not admit that the Georgian version is Caesarean at all. He characterizes it as harmonistic, and traces it back through the Armen-

<sup>43</sup> See his brief account in Scrivener's *Plain Introduction*, 4th ed. (1894), II, 156-158, and his article in the *American Journal of Theology*, I (1897), 883-912.

<sup>44</sup> *PO*, XX.3 (Paris, 1929), and XXIV.1 (Paris, 1933). Regarding Blake's work in *HTR* for 1928, Anton Baumstark thinks that it is far from being the last word on the subject; see his criticisms in "Zum georgischen Evangelientext" (*OC*, 3te Serie, III/IV Band, 1. Heft [1928-29] 117-124). See also G. Peradse, "Die Probleme der georgischen Evangelientübersetzung" (*ZNW* 29 [1930] 304-309), which is a general discussion. Blake's latest publication on the subject is "Notes on the Text of the Georgian and Armenian Gospels," (*Quantulacumque*, ed. R. P. Casey, et. al. [London, 1937], pp. 355-363).

<sup>45</sup> "The Adysh ms. [dated 897] is closely related to the group  $\Theta$ , 565, 700, and in spite of all translational loss is almost as good a representative of the clan as the best mss. A and B, however, [A = Opiza ms, written 913; B = Tbet' ms, written 995] contain a considerable amount of Caesarean material which has been revised out of the Adysh, and in particular they manifest special affinities to family 1 (1, 118, 209), and to the Ferrar group. If one rejects the different K variants found in the three mss., the residuum of readings when combined affords a very pure Caesarean text" (*PO*, XX.3 [Paris, 1929], 447).

<sup>46</sup> In Lagrange's *Critique textuelle*, II, 384 and 386.

<sup>47</sup> *ATR* 16 (1934) 129.

ian (which, in his opinion, is no more Caesarean than its daughter version, the Georgian) to a Syriac model, the Diatessaron.<sup>48</sup>

The Palestinian Syriac version, according to Lake, Blake, and New, discloses the presence of the Caesarean text, and is thought by these scholars to be derived from a "postulated" Syriac version of the fourth century no longer extant.<sup>49</sup>

#### 4. *The Caesarean Text in Lectionaries*

As is well known, the several parts of a Greek Gospel lectionary are far from homogeneous in textual complexion.<sup>50</sup> It is necessary, therefore, to analyze separately the text of each constituent section of the lectionary system. Of the several sections which have been thus far examined, two show signs of Caesarean affinities. The Marcan week-day lections, according to Paul Schubert, are akin to von Soden's  $I\phi$  group, a tertiary authority of the Caesarean text.<sup>51</sup> Again, the Lucan Saturday and Sunday lections, according to the investigations of the present writer, are much closer to the text of family 1 and family 13 than to any other text group.<sup>52</sup>

<sup>48</sup> *OC*, 3te Serie, III/IV, 1. Heft (1929), 117-124; and *ibid.*, XI (1936), 246-252 (see also footnote 33 above).

<sup>49</sup> Simultaneously with the publication of the *HTR* study, Lake advanced some considerations which, he thought, mitigated somewhat the strangeness of the hypothesis that a Syriac version which existed in the fourth century should have disappeared so completely; see his article, "The Text of the Gospels," in *Studies in Early Christianity*, ed. S. J. Case (New York 1928), pp. 41 f. But Colwell offers some trenchant criticisms of this "postulated" Syriac version in his stimulating article "Slandered or Ignored: the Armenian Gospels" (*JR* 17 [1937] 56).

<sup>50</sup> See, e. g., Colwell in E. C. Colwell and D. W. Riddle, ed., *Prolegomena to the Study of the Lectionary Text of the Gospels* (Chicago, 1933), p. 15.

<sup>51</sup> In Colwell and Riddle's *Prolegomena*, pp. 43-56. Streeter classified von Soden's  $I\phi$  group as the most important of what Streeter terms the "poor relations" of the Caesarean text (*The Four Gospels*, p. 84 and pp. 575 ff.)

<sup>52</sup> *The Saturday and Sunday Lessons from Luke in the Greek Gospel Lectionary* (Chicago, 1944). This is vol. II, part 3, of *Studies in the Lectionary Text of the Greek New Testament*, edited by E. C. Colwell.



## 5. Debate

The discoverer of the Caesarean text formulated four main conclusions concerning the characteristics of this text. He believed that (1) so far as minor variants are concerned, the text of family Θ is almost equidistant from both the Alexandrian (Neutral) and the Western texts. The balance inclines slightly, but only slightly, to the Western side. (2) In family Θ are found certain striking additions to the Textus Receptus which the Old Syriac shares with D and the Old Latin, besides others found only in the Old Syriac or the Armenian. (3) As regards, however, the longer omissions from the Textus Receptus found in B and the Sinaitic Syriac, family Θ nearly always supports the shorter text. (4) Family Θ is nearer to the Old Syriac than is any other surviving Greek text, but it is by no means identical; it is frequently supported by the Armenian against the Syriac. Most frequently of all it is supported by the oldest manuscripts of the Georgian version.<sup>53</sup>

But F. C. Burkitt, in an extensive review of Streeter's book, was not convinced that it is correct to speak of a Caesarean text at all. "My chief objection," he wrote, "to speaking of 'the Caesarean text' is that this term gives apparent definiteness and consistence to a set of 'various readings' that remain to me obstinately disparate and amorphous."<sup>54</sup> He likened the unity underlying family Θ to a unity of undenominationalism, as if a man should combine the peculiarities of the Baptist, Congregationalist, and other Protestant bodies into a sect marked by complete dissent from common catholicity.<sup>55</sup> Nevertheless, Burkitt admitted that the sub-groups within the Caesarean family (he calls it the "Caesarean crowd") do exhibit individuality, and thought that the ancestors of family 1 and family 13 could be reconstructed.<sup>56</sup>

Streeter replied to Burkitt's review in an article entitled "The Caesarean Text of the Gospels,"<sup>57</sup> wherein he explains the sense

<sup>53</sup> *The Four Gospels*, pp. 84 f.

<sup>54</sup> *JTS* 26 (1924-25) 284.

<sup>56</sup> *Ibid.*, p. 286.

<sup>57</sup> *JTS* 26 (1924-25) 373-378.

<sup>55</sup> *Ibid.*, pp. 285 f.

in which he used the word "text" (in "Caesarean text"). By it he did not mean "recension" (i. e., *one definite* manuscript), but the majority of readings which are either peculiar to, or only rarely found outside, this group of authorities. That is, most of these readings would have been found in an *average* manuscript used at Caesarea, while of the rest of these characteristic variants, some would have been found in one, some in another manuscript in that locality.<sup>58</sup>

To this reply by Streeter, Burkitt added a note in rejoinder, the gist of which amounted to objecting to dividing the testimony of the Eastern group of witnesses into the Antiochian and Caesarean families. He regards the Sinaitic Syriac as an integral element of the Eastern group, and holds that when "the attestation of any reading of this group does not include syr. S. there is a strong presumption that the reading in question is a later corruption inside the group, not an original feature of it."<sup>59</sup>

The debate, though apparently ended, was carried on *sub rosa* in an article entitled "The Washington MS. and the Caesarean Text of the Gospels," where Streeter urges that the point on which he and Burkitt differ is small compared with those on which they are agreed.<sup>60</sup> He concludes by turning against Burkitt what the latter had written ten years before in an article entitled "W and Θ."<sup>61</sup> In this article Burkitt had (rightly, says Streeter) argued for the general superiority of the B<sup>N</sup> text. "But, if so, are we to say that the larger number of readings in which the Θ family supports B<sup>N</sup> 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.?"<sup>62</sup>

<sup>58</sup> *Ibid.*, p. 375. Lake, Blake, and New admit, also, that "the Caesarean text was never a definite single entity like the Vulgate or the Peshitto, but is analogous to the European Latin. . . . The MSS. of the European Latin can be easily recognized as neither African nor Vulgate; they were used at one period in one place, but they are full of variations" (*HTR* 21 [1928] 326).

<sup>59</sup> *JTS* 26 (1924-25) 380; so too he had expressed himself much earlier in *Evangelion da-Mepharreshe*, II (Cambridge, 1904), 246.

<sup>60</sup> *JTS* 27 (1925-26), 144-147.

<sup>61</sup> *JTS* 17 (1915-16) 1-21, 139-152.

<sup>62</sup> *JTS* 27 (1925-26) 147.

A few years later Burkitt again expressed his view in an extended notice of the Lake, Blake, and New publication. He still held that, while the task of reconstructing the text of the Ferrar group is feasible and practicable, the attempt to reconstruct the Caesarean text is impossible because it was and is non-existent.<sup>63</sup>

Quite similar to Burkitt's position is that of another British scholar, P. L. Hedley. In his thorough and scholarly survey of "The Egyptian Texts of the Gospels and Acts" he has occasion to express his opinion concerning whether it is legitimate to speak of a Caesarean family. He asks the question, "Is it probable that every single important Greek MS. of the Gospels with a non- $\beta$  text (except D) should be descended from a 'Caesarean' ancestor?"<sup>64</sup> The Caesarean family, so Hedley maintains, is really an artificial amalgamation formed by textual critics from diverse texts which "are not descended from any one source, but represent the general treatment of the Gospel text in the East during the first two or three centuries."<sup>65</sup>

At about the same time Henry A. Sanders maintained that so far as fragments of papyri are concerned,<sup>66</sup> the dominant complexion of the Egyptian text of the Gospels and Acts was Western. The Caesarean text, in his opinion, had no great influence on the Egyptian New Testament,<sup>67</sup> being, he is inclined to think, a revision of that type of the Western text which circulated in Palestine.<sup>68</sup>

An unexpected development in textual studies came in 1935 when R. V. G. Tasker, of King's College, London, apparently

<sup>63</sup> *JTS* 30 (1928-29) 347-358. But Burkitt subsequently modified this extreme position, allowing the existence of such a text but observing that "it is easier, from some points of view, to reconstruct the original than some half-way house like the 'neutral' or the 'Caesarean' text that contains some corruptions but not all" (*JTS* 34 [1933] 367).

<sup>64</sup> *CQR* 118 (1934) 224.

<sup>65</sup> *Ibid.*, p. 38, note 6.

<sup>66</sup> "The Egyptian Text of the Four Gospels and Acts" (*HTR* 26 [1933] 77-98).

<sup>67</sup> *Ibid.*, p. 95.

<sup>68</sup> *Ibid.*, p. 94.

overturned the "assured results" of textual criticism. In that year he published two brief articles dealing with "The Quotations from the Synoptic Gospels in Origen's *Exhortation to Martyrdom*"<sup>69</sup> and "The Text Used by Eusebius in *Demonstratio Evangelica* in Quoting from Matthew and Luke."<sup>70</sup> The conclusions drawn by the author of these two articles were, from Streeter's point of view, quite disconcerting. In the former Tasker argues that because he has shown that "in no case does a member of family  $\Theta$  support Origen's reading to the exclusion of support from the Neutral Text. . . . [therefore] . . . No clearly 'Caesarean' text emerges in Matthew and Luke."<sup>71</sup> In the latter article he concludes that "the text of  $\aleph B$  has more right to be called the Caesarean text used by Eusebius than Family  $\Theta$ ."<sup>72</sup>

These statements did not remain unchallenged. In the same year Canon Streeter contributed to each journal a refutation of Tasker's conclusions.<sup>73</sup> While praising Tasker for his industry in collecting and publishing textual data, he condemns him for methodological errors in interpreting these data. The gist of Streeter's articles is that precisely the same textual phenomena are found in the citations of Origen and Eusebius from Matthew and Luke as from Mark, and since Tasker admits that in Mark these Fathers used a Caesarean text, the same conclusion ought to be drawn regarding the other two Synoptic Gospels. Streeter's characterization of the present state of the Caesarean text is as follows. "All the MSS. which preserve this [Caesarean] text have been heavily corrected to accord with what Griesbach called the 'Byzantine,' what Hort and others the 'Syrian' or 'Antiochian' text; but the different MSS. have been so corrected in different places. Accordingly it is only when MSS. of this family differ from the Byzantine text that we can identify their readings as authentically representing the characteristic family Text."<sup>74</sup>

<sup>69</sup> *JTS* 36 (1935) 60-65.

<sup>70</sup> *HTR* 28 (1935) 61-69.

<sup>71</sup> *JTS* 36 (1935) 64 f.

<sup>72</sup> *HTR* 28 (1935) 67.

<sup>73</sup> "Origen,  $\aleph$  and the Caesarean Text" (*JTS* 36 [1935] 178-180); "The Caesarean Text of Matthew and Luke" (*HTR* 28 [1935] 231-235).

<sup>74</sup> *HTR* 28 (1935) 232.

What explanation does Streeter have for the fact that Origen and Eusebius' readings are supported by Neutral as well as Caesarean manuscripts? He admits that "the great majority of 'Caesarean' readings are to be found either in Hort's 'Neutral' or in his 'Alexandrian' texts or in the 'Western' (D and the Old Latin). The same thing holds of the Byzantine text. The majority of its readings are to be found either in the 'Neutral' or in the 'Alexandrian' or in the 'Western' text. But the particular selection of Neutral, Alexandrian and Western readings in the Byzantine text is totally different from the selection in the Caesarean text. What constitutes the characteristic feature of both these texts is not so much the relatively small proportion of readings peculiar to themselves as the specific pattern, so to speak, in which Neutral, Alexandrian and Western readings are found combined."<sup>75</sup>

Streeter concludes his article by appealing to the Chester Beatty Papyrus as "a dramatic vindication of the critical methods by which it is attempted to reconstruct this [Caesarean] text."<sup>76</sup>

Evidently Streeter convinced (or intimidated) Tasker, because in a subsequent publication on "The Chester Beatty Papyrus and the Caesarean Text of Luke,"<sup>77</sup> he indicates that Streeter "read my original draft of this article and made valuable suggestions, which I have incorporated, for the treatment of the evidence."<sup>78</sup> He concludes that the text of the papyrus in Luke "is akin to, though not identical with, that found in members of fam Θ." Moreover, "there are in proportion more Neutral readings preserved in members of family Θ of Luke than in Mark. In other words, the distinction between the Neutral and Caesarean text in Luke is less than it is in Mark."<sup>79</sup>

In two subsequent articles Tasker examined "The Text of

<sup>75</sup> *Ibid.*, p. 233. To the same effect, Lake, Blake, and New, *HTR* 21 (1928) 257.

<sup>76</sup> *HTR* 28 (1935) 234.

<sup>77</sup> *HTR* 29 (1936) 345-352.

<sup>78</sup> *Ibid.*, p. 345, note 1.

<sup>79</sup> *Ibid.*, p. 350.

St. Matthew Used by Origen in his Commentary on St. Matthew"<sup>80</sup> and "The Chester Beatty Papyrus and the Caesarean Text of John."<sup>81</sup> His conclusion in the former is that "the text used by Origen while writing this portion of his Commentary on St. Matthew at Caesarea was that of family Θ."<sup>82</sup> His conclusion in the latter is that "the remarkable thing about the text [of P<sup>45</sup> in John] is its mixed character, which is similar to the mixed character of the text of family Θ."<sup>83</sup>

Both Streeter and Tasker — as well as all other textual critics — overlooked what is without doubt a most significant analysis of the textual complexion of the Caesarean text. In 1936 James E. McA. Baikie was awarded a M.Litt. degree by the faculty of Divinity at Cambridge University for a thesis entitled, "The Caesarean Text *Inter Pares*." Baikie sought to determine whether or not the Caesarean text is a unity, basing his investigation on analyses of Caesarean agreements with Western, Syrian, and Neutral readings in three test chapters, Matt. 3, Mark 12, and Luke 12. The author's succinct summary<sup>84</sup> of his thesis is as follows.

Statistical tables of agreements for Mark 12 showed "that the Caesarean witnesses (1) did not agree in one family preference for types of variant; (2) did not agree in one family order of domestic preferences; (3) in agreement with the Latins, under certain types of variant, showed almost complete unanimity of preference, but, under others, diversity; (4) in agreement with the Syriac, had, some, an almost constantly high place, others, a uniformly low one, the remainder, a variable one; (5) in agreement with Neutral comparative to Western, displayed, some, a consistent preference for Western, one, a less consistent preference for Neutral, part of the remainder, an almost consistent

<sup>80</sup> *JTS* 38 (1937) 60-64.

<sup>81</sup> *HTR* 30 (1937) 157-164.

<sup>82</sup> *JTS* 38 (1937) 64.

<sup>83</sup> *HTR* 30 (1937) 162.

<sup>84</sup> *Abstracts of Dissertations Approved for the Ph.D., M.Sc. and M.Litt. Degrees in the University of Cambridge during the Academical Year 1935-1936* (Cambridge, 1936), pp. 53 f. The present writer is grateful to Miss Elena F. Mackay for her kindness in calling his attention to Baikie's work.

preference for Western, the rest of the remainder, a varying preference between Western and Neutral.

"From a co-ordination of these tables it was observed that in most cases the Caesareans were in greater absolute agreement with outsiders than with the majority of their fellows.

"Further evidence of Caesarean diversity was afforded by Inter-Familiar Variation, i. e. cases where, when two or more variants occur, the family witnesses are divided against themselves and united with those of other families.

"The Caesarean witness of Origen and Eusebius sometimes agreed with other Caesareans, at times had isolated agreements with outsiders, and also took part in Inter-Familiar Variation. Both are thus typical Caesareans.

"Two explanations of the problem, not mutually exclusive, can be offered. The above divergences are due, either to later and irregular correction, or to varying textual influences. Inter-Familiar Variation would seem to demand the latter, thus denying the Caesarean unity, while the Caesarean 'self-consciousness' observed in agreement with the Latins and the lack of continuity in grouping observed in cases of Inter-Familiar Variation favour the former.

"A final suggestion is made that the Caesarean unity is one of influences rather than origin, and that the Caesarean text, in a measure at least, is really a Textual Process."

Meanwhile, the Caesarean group was assigned not a few additional members. Just before Canon Streeter's tragic death in an airplane accident in the Alps he wrote a brief study for Lake's *Festschrift* (*Quantulacumque*. London, 1937) in which he expressed his belief that most, if not all, of the minuscules which von Soden assigned to his *I* text are really Caesarean. These manuscripts, it is true, have a much larger Byzantine element than do the authorities on which Lake based his reconstruction. But, "the value of these 'weak' supporters to the 'Caesarean' text, including *fam* 1424, is that they appear occasionally to preserve a 'Caesarean' reading which has been revised out of the more important authorities for that text. . . . That some of the readings in the inferior authorities are authentically 'Caesarean' is shown by the fact that they appear in quotations by Origen and Eusebius."<sup>85</sup>

<sup>85</sup> "Codices 157, 1071 and the Caesarean Text" (*Quantulacumque*, ed. R. P. Casey, *et. al.*, p. 150).

The most recent investigator of the affinities of P<sup>45</sup> with the Caesarean text, C. C. Tarelli, strikes out in a different direction.<sup>86</sup> Tarelli alleges that Kenyon's analysis of P<sup>45</sup> is vitiated by two errors of methodology. In the first place, Kenyon has unfairly cited, claims Tarelli, agreements of P<sup>45</sup> with members of the Caesarean group even when they agree with Byzantine text. But in this regard it is no doubt Tarelli's judgment and not Kenyon's which is at fault. For if the Byzantine text (which, for most purposes, may be identified with the Textus Receptus) disagrees with, say, the Neutral and Western texts while agreeing with the Caesarean, the evidence of P<sup>45</sup> in agreeing with the Caesarean text may quite fairly be presumed to be significant, and the agreement of the Textus Receptus may be explained on the hypothesis that the Textus Receptus, an eclectic text, has absorbed a Caesarean reading.

The other charge of error in methodology which Tarelli levels against Kenyon is more serious. He points out that in Kenyon's statistics "the 323 agreements [of P<sup>45</sup>] with the [Caesarean] group are obtained by adding together the agreements with its individual members, so that an agreement with W and Θ counts as two, an agreement with Θ, *fam.* 1 and *fam.* 13 as three, and an agreement with all four as four agreements. The total number of agreements with the group is in fact more than twice as many as the total number of variants noted. This is an obvious oversight, and the question of the Caesarean affinities of the papyrus evidence requires reconsideration. As, moreover, 28, 565, and 700 are equally regarded as belonging to the family, their agreements should also be taken into account, and the agreements of the whole group and its various combinations should be properly weighed."<sup>87</sup>

Nevertheless, even after making necessary adjustments in the

<sup>86</sup> "The Chester Beatty Papyrus and the Caesarean Text" (*JTS* 40 [1939] 46-55).

<sup>87</sup> *Ibid.*, pp. 46 f. Tarelli fails to mention, however, that Kenyon employs exactly the same procedure in evaluating the non-Caesarean, pre-Byzantine elements of P<sup>45</sup>. This largely counter-balances the otherwise excessive weight which would be allowed the Caesarean witnesses by Kenyon's method of counting variants.

evaluation of the textual affinities of P<sup>45</sup>, Tarelli confesses: "It must be frankly admitted that the papyrus falls very readily into Streeter's pattern, at least so far as it has extremely few agreements with  $\aleph$ , B, or A, and none at all with D, which are not supported by some members of the W  $\Theta$  group. This does not mean, however, that the P<sup>45</sup> text is identical with 'the text which results from the purely objective process of deducting Byzantine readings from members of Family  $\Theta$ .' The text resulting from that process in Prof. Lake's tables (*Harvard Theological Review*, 21 [1928] 216-246) is so uncertain that it is necessary in many cases to assume 2, 3, 4, and sometimes even 5 possible family readings, and, as we have seen, there are only two non-Byzantine readings in P<sup>45</sup> which do not find the family in conflict, nor are the opposing members of the family invariable in agreement with the Byzantine text."<sup>88</sup> Yet, "P<sup>45</sup> has no Byzantine reading which is not supported by some members of the Caesarean group, and all but 14 of such readings are supported by family 13, and all but 17 of these by W also."<sup>89</sup>

After raising many important issues regarding the Caesarean text and asking not a few questions difficult to answer, Tarelli concludes that "what underlies P<sup>45</sup> and its supporters is not a local text but rather such a text as von Soden imagined under the name IHK, containing elements of all later texts 'in their pregnant causes mixed'. No doubt actual mixture of different lines of transmission took place at all periods of the MS. tradition, but it is highly probable that the fundamental cause of the 'mixed' texts of which P<sup>45</sup> is our earliest example is the fact that the original text itself was 'mixed' in this sense of combining elements found in all of its descendants."<sup>90</sup>

<sup>88</sup> *Ibid.*, p. 50.

<sup>89</sup> *Ibid.*, p. 52.

<sup>90</sup> *Ibid.*, p. 55. In two other articles dealing with P<sup>45</sup>, Tarelli touches upon but does not deal explicitly with the problems of the Caesarean text; the articles are "The Chester Beatty Papyrus and the Western and Byzantine Texts" (*JTS* 41 [1940] 253-260), and "Some Linguistic Aspects of the Chester Beatty Papyrus of the Gospels" (*JTS* 43 [1942] 19-25).

### 6. A New Orientation

In 1933 Sir Frederic G. Kenyon published a papyrus of the Gospels which, like an acid, was to dissolve the unity of the elaborately constructed Caesarean text.<sup>91</sup> At first sight the text of the papyrus seemed to confirm all that had been accomplished in the isolation of the several parts of this text. Both its editor and Streeter<sup>92</sup> hailed it as possessing strong affinities with the Caesarean group. But then gradually the realization began to dawn on scholars that if this papyrus, which its editor dates in the first half of the third century, agrees with the Caesarean text, then that text can scarcely be called any longer the Caesarean text. In other words, the text which had been localized at Caesarea in Palestine is discovered to antedate its introduction there; it must be traced to Egypt.

It is interesting to observe the slowly shifting opinion. In 1924 Streeter was confident that the Caesarean text was found by Origen when he moved from Alexandria to Caesarea in Palestine. But the investigations of Lake, Blake, and New cast doubt upon the certainty of this opinion, showing that perhaps Origen may have known it in Alexandria. They have, that is, no certain answer to give to the question they raise, "Was it found by Origen in Caesarea or brought thither by him? On that question we have no firm convictions."<sup>93</sup>

It was Père Lagrange who spoke with more assurance in this matter. As early as 1929 (i. e. four years before the publication of P<sup>45</sup>) he wrote, "The family [Caesarean] is not necessarily the type of a recension made at Caesarea. It is certainly not the work of Origen. . . . Its origin in Egypt appears to us to be more probable [than its origin in Caesarea]."<sup>94</sup> Similar too was the

<sup>91</sup> *The Chester Beatty Papyrus I* (London, 1933).

<sup>92</sup> Streeter's concurrence is to be found in the preface to the fifth impression of his *The Four Gospels*, dated 1936. See also his enthusiastic remarks in *HTR* 28 (1935), 234, quoted in the text above, at note 76.

<sup>93</sup> *HTR* 21 (1928) 324.

<sup>94</sup> "Le groupe dit césarién des manuscrits des Évangiles" (*RB* 38 [1929] 507).

tenor of Lagrange's brief notice of the discovery of the Chester Beatty papyrus,<sup>95</sup> and in his subsequent article, "Les papyrus Chester Beatty pour les Évangiles,"<sup>96</sup> he writes with great positiveness: "Nothing prevents the recognizing in this type [of text] a product of Alexandrian criticism, as we have suggested earlier." These same conclusions were likewise embodied by Lagrange in his excellent treatise on textual criticism.<sup>97</sup>

There were, of course, other estimates of the significance of P<sup>45</sup>. Leo Vaganay, believing that the Western text was the current primitive text, regards P<sup>45</sup> as a witness to this primitive text which appears in much the same form in Alexandria as well as in Caesarea prior to the time when revisors began methodically to work upon it.<sup>98</sup> He proposes, furthermore, another term for the group ordinarily designated "Caesarean." Since he thinks that the textual characteristics of the several members of the group (he does not include P<sup>45</sup> in the group) are due to the recension of Pamphilus of Palestine, who, indeed, may have worked upon Origen's Palestinian text, he would call the text the Palestinian or Pamphilus Recension.<sup>99</sup>

But none of these generalized statements can be compared with a methodical and painstaking study which has apparently been generally overlooked but which certainly deserves to be better known. In 1935 Teófilo Ayuso, professor in the Roman Catholic Seminary at Segovia (Spain), published an article whose title indicates the trend in criticism, "¿Texto cesariense o pre-cesariense?"<sup>100</sup> He comes to the following conclusions: (1) At

<sup>95</sup> *RB* 41 (1932) 453 f.

<sup>96</sup> *RB* 43 (1934) 23.

<sup>97</sup> *Critique textuelle*, II, 166, "There is no reason to say that the Caesarean text is Caesarean in origin."

<sup>98</sup> *Initiation à la critique textuelle néotestamentaire* (Paris, 1934), p. 101 (Eng. trans. [London, 1937], p. 119). See too, P. L. Hedley, *CQR* 118 (1934) 224, note 35.

<sup>99</sup> *Op. cit.*, pp. 86 f. (Eng. trans., pp. 114 f.).

<sup>100</sup> *B* 16 (1935) 369-415; the subtitle is "su realidad y su trascendencia en la crítica textual del Nuevo Testamento." In his footnotes Ayuso mentions that he had previously contributed an article entitled "El 'Texto cesariense' del papiro de Chester Beatty en el Evangelio de San Marco" to *Estudios*

Caesarea Origen and Eusebius used a "Caesarean" text. (2) This text did not originate at Caesarea but was carried there from Egypt. It did not, however, come from Alexandria, where a Neutral text was in use. (3) It originated in a locality off the beaten track so far as scholarship was concerned; it came from the region of Gizeh and the Fayyum. Evidence for localizing it here is to be found in W, which Charles L. Freer bought of an Arab dealer in Gizeh in 1906; in P<sup>37</sup>, Berlin P. 13416, and especially P<sup>45</sup>, all of which came from the Fayyum, or nearby, and which exhibit a "Caesarean" type of text.

Furthermore, on the basis of elaborate analyses of variant readings, Ayuso divides the "Caesarean" group into two parts: P<sup>45</sup>, W, fam 1, 28, and fam 13, calling this group primitive and pre-Caesarean; and Θ, 565, 700, Origen, Eusebius, Sinaitic Syriac, Old Georgian, and Old Armenian, calling this group recensional and Caesarean proper. The former was localized in Fayyum-Gizeh and belongs to the "subgrupo occidental"; the latter was localized in Caesarea and represents the "subgrupo oriental."

The implications of this analysis are sufficiently startling, and at the conclusion of his article Ayuso says that he anticipates repercussions. Whether all his conclusions will stand remains to be seen, but certainly they must be given serious consideration by future textual critics of the New Testament.

Slowly a few other investigators began to sense that a new phase had been reached, but these scholars were apparently unaware of Ayuso's studies. One of Kirsopp Lake's students, Norman Huffman, published part of his doctoral dissertation in which he suggests a pre-Caesarean stage of that text. He observes that "Caesarean readings are very frequently found also in other texts which are perhaps older than the Caesarean. . . . There may have been in Egypt in the second and third centuries a text, as yet unidentified, which was the ancestor of the old Syriac version, of the Caesarean text, and of the type of text

*Biblicos* 6 (1934) 268-282. Although he canvassed most of the large libraries in this country, the present writer has been unable to discover either the periodical or anyone who had ever seen it. Any information will be gratefully received.

found in Pap.<sup>45</sup> and W — in short, an 'Egyptian text' — antedating the earliest texts which we know."<sup>101</sup>

In a tribute to the memory of Père Lagrange, who died in 1938, Kirsopp and Silva Lake find occasion to express their revised views regarding the Caesarean text.<sup>102</sup> Without referring to Ayuso (and therefore, one can assume, with independent confirmation of his studies) they express themselves in similar terms regarding the bipartite division of the Caesarean family. They recognize that with the acquisition of the evidence in P<sup>45</sup> the textual critic must separate Θ, 565, and 700 (whose text accords closely with the citations in Origen and Eusebius) from W and fam 13 (with which P<sup>45</sup> is in close agreement). Obviously, too, the text of an Egyptian papyrus which antedates Origen's hegira to Caesarea cannot be called Caesarean. This term, therefore, must be reserved for the text identified by the citations of Origen and Eusebius and for the text of 700. On the other hand, P<sup>45</sup>, W, and fam 13 may be regarded as witnesses of a text on which the "Caesarean" was to be established, and P<sup>45</sup> and W suggest that this more ancient text was Egyptian before it was Caesarean. It is from P<sup>45</sup> that something of the nature of this pre-Origenian, Egyptian text can be learned. Here the Lakes leave the matter to take it up again in the fascicle entitled *Family 13 (The Ferrar Group)*.<sup>103</sup>

In this study of one part of the Caesarean text the Lakes reconstruct the Marcan text of family 13 as a preliminary step in the reconstruction of the Caesarean text of Mark, a task which they contemplated since about 1924.<sup>104</sup> In their discussion of the textual affinities of family 13 they acknowledge that

<sup>101</sup> "Suggestions from the Gospel of Mark for a New Textual Theory" *JBL* 56 [1937] 356.

<sup>102</sup> "De Westcott et Hort au Père Lagrange et au-delà" (*RB* 48 [1939] 497–505). A similarly orientated discussion by the same authors had appeared in *Religion in Life*, 5 (1936), 90–95.

<sup>103</sup> It may be mentioned here that more than once in her monograph on *Family II and the Codex Alexandrinus*, Mrs. Lake had thrown out remarks looking in this direction, though postponing a full discussion of the matter for a more suitable occasion (*SD V* [London, 1937], p. 4, note 5; p. 55; pp. 61 ff.)

<sup>104</sup> *HTR* 21 (1928) 210.

readings of W, family 1, family 13, and 28, must not be considered (as both Streeter and they had done) to be witnesses of a bad Caesarean text. They are not Caesarean at all, but, as the discovery of P<sup>45</sup> (with which they agree so closely) made plain, they represent "a pre-Origenian text which was revised into the true 'Caesarean.'"<sup>105</sup>

By way of summary, it must be acknowledged that at present the Caesarean text is disintegrating. There still remain several families — such as family 1, family 13, the Armenian and Georgian versions — each of which exhibits certain characteristic features. But it is no longer possible to gather all these several families and individual manuscripts under one *vinculum* such as the Caesarean text. The evidence of P<sup>45</sup> clearly demonstrates that henceforth scholars must speak of a pre-Caesarean text as differentiated from the Caesarean text proper.<sup>106</sup> Future investigators must take into account two hitherto neglected studies, namely Ayuso's very significant contribution to *Biblica* in 1935, in which he sets forth fully the compelling reasons for bifurcating the "Caesarean text," and Baikie's M.Litt. dissertation in 1936, the implications of which suggest that the "Caesarean text" is really a textual process.

### 7. Tasks and Problems

Although no little time and energy have been expended in investigating the "Caesarean text," many more tasks and analyses remain for future scholars. The following are some of the problems — not all of the same magnitude — which presented

<sup>105</sup> *Family 13*, p. 7. This statement represents no doubt the real opinion of the authors. The statement on p. ix ("Fam 13 is associated with Θ, W, fam 1, 28, 565, 700 and the Georgian version, as a representative of the type of text used by Origen in Caesarea in Palestine") contradicts the one cited above, but presumably ought to be regarded as somehow in harmony with it. In a recent article the Lakes suggest that the type of text found in family 1 be called either pre-Caesarean or Old Egyptian ("The Scribe Ephraim," in *JBL* 62 [1943] 267).

<sup>106</sup> When it is necessary to refer to both the pre-Caesarean text and the Caesarean text proper, the combination will hereafter be designated by enclosing the words *Caesarean* or *Caesarean text* in quotation marks.

themselves to the writer in the survey of the work already accomplished.

1. Manuscript 565, which agrees so frequently in Mark with Θ, ought to be collated carefully in the other Gospels as well. Its editor, J. Belsheim, thought it good to cite only the more weighty variants from the Textus Receptus in Matthew, Luke, and John.<sup>107</sup>

2. The "Caesarean text" of Mark has been investigated with some care in three sample chapters (chapters 1, 6, and 11). Similar investigations should be undertaken in the rest of the Gospel as well as in the other three Gospels.<sup>108</sup>

3. The textual affinities of the several sections of Greek Gospel lectionaries ought to be analyzed. The present writer discovered that the lessons from Luke appointed to read on Saturdays and Sundays are predominately pre-Caesarean in textual character.<sup>109</sup> Other sections of the lectionary system remain to be investigated in the light of the new orientation of the "Caesarean text"-*Forschung*.\*

4. Lake, Blake, and New report that the Palestinian Syriac version is "Caesarean" in its textual affinities, and think that it derived its "Caesarean" coloring from a Syriac version of which no trace remains. According to Père Lagrange<sup>110</sup> and

<sup>107</sup> See footnote 20 above.

<sup>108</sup> In looking farther afield one could wish with the author of a recent paper on textual criticism that someone would do for the Pauline corpus what Streeter did for the Gospels (though here perhaps some of the necessary materials are lacking; there is no Θ, no ante-Nicene Syriac version); see W. Arndt, "A Definite Need in the Field of New Testament Textual Criticism" (*Concordia Theological Monthly* 16 (March, 1945) 180-186). It would no doubt be profitable to investigate more completely the textual affinities of codex 1739, a tenth century manuscript of the Acts and Pauline Epistles whose text is connected with Origen (*Six Collations of New Testament Manuscripts*, edd. Kirsopp Lake and Silva New [Cambridge, 1932], pp. 141-219; cf. K. and S. Lake, *JBL* 62 [1943] 263 ff.) but is not "Caesarean" (so Lagrange, *Critique textuelle*, II, 470). See also Heinrich Seesemann, "Die Bedeutung des Chester-Beatty Papyrus für die Textkritik der Paulusbriefe" (*Theologische Blätter* 16 [1937] cols. 92-97).

<sup>109</sup> See his *The Saturday and Sunday Lessons from Luke in the Greek Gospel Lectionary* (Chicago, 1944), pp. 37-55 and 66 f.

<sup>110</sup> *Critique textuelle*, II, 167, note 4.

Pres. Colwell<sup>111</sup> this hypothetical Syriac version was postulated without adequate reason. The grounds for assuming such a Syriac intermediary between the "Caesarean" Greek text and the Palestinian Syriac version need to be reviewed.

5. In a recent article on "Syriasms in the Washington Text of Mark,"<sup>112</sup> C. S. C. Williams concludes that there is "a strong streak of Syriac in the Caesarean texts and they and Sys are more closely affiliated than even the diagram in *Harvard Theological Review*, 1928, 324 f. would suggest." Detailed research in this field would certainly be fruitful.

6. Since the publication of Streeter's investigation in 1924, the Berlin Kirchengväter-Commission has edited several additional volumes of Origen's works. The Gospel quotations in these critically established texts should be investigated to determine whether they confirm, modify, or contradict the current opinion regarding Origen's New Testament text.<sup>113</sup>

7. In Kirsopp Lake's admirable handbook, *The Text of the New Testament* (6th ed.) — a book which must be described as *multum in parvo* — the hint is thrown out that the text of the Persian version may be "Caesarean" in character. This hint should be explored.

These seven problems presuppose that the methods of textual criticism employed heretofore are valid. But certain other problems clamor for attention concerning basic methodology. Embarrassing questions rise; questions to which there are no satisfactory answers.

8. The discovery of P<sup>45</sup> revealed that what had been accounted as one text must, in fact, be divided into a pre-Caesarean text and a Caesarean text proper. Suppose P<sup>45</sup> had not been found;

<sup>111</sup> *JR* 17 (1937) p. 56.

<sup>112</sup> *JTS* 42 (1941) 177 f.

<sup>113</sup> Neither Streeter nor (it appears) anyone else has availed himself of the information conveniently presented by Ernest Hautsch, *De quattuor evangeliorum codicibus Origenianis* (Diss., Göttingen, 1907). Although only four volumes of the Berlin corpus containing Origenian treatises had appeared before Hautsch wrote, he was able to exhibit a surprising array of Origenian readings from all four Gospels. See also Erwin Preuschen, "Bibelcitate bei Origenes" (*ZNW* 4 [1903] 67-74).



scholars would no doubt have continued to combine diverse families which ought to be separated. Was there a fundamental flaw in the previous investigation which tolerated so erroneous a grouping? It may be, of course, that in the nature of the case scholars could not have been expected to make allowances for evidence only subsequently brought to light. But on the other hand one wonders why textual critics could not have distinguished the presence of two groups of manuscripts even before the discovery of P<sup>45</sup>, as astronomers were able to predict the existence of another planet before they had a telescope powerful enough to see it.

9. A glance at the critical apparatus in Kenyon's edition of P<sup>45</sup> reveals how very frequently fam 1 and fam 13 are opposed to each other. Why should this be so? In the same vein, when Origen and Eusebius differ, which is to be regarded as presenting the Caesarean text? Or, if as not infrequently is the case, Origen and Eusebius are on one side and the oriental versions (Armenian, Georgian, and Palestinian Syriac) are on the other, with the Greek manuscripts of the family divided between them, what explanation are we to give? The only answer Lake, Blake, and New can offer is the suggestion that the "Caesarean text" was never a definite single entity like the Vulgate or the Peshitta but is analogous to the European Latin.<sup>114</sup> Is this a legitimate and helpful analogy or an evasion of a disturbing question?

10. Is it licit to reconstruct the ancient "Caesarean text" from oftentimes late documents merely by pooling the non-Byzantine variants? Lake, Blake, and New, for instance, include in the "Caesarean text" of Mark 1, 6, and 11, 168 variants from the Textus Receptus which are read by only one of eight "Caesarean" witnesses.<sup>115</sup> Is it reasonable to maintain that originally most or all of the witnesses had the "Caesarean" reading but that this common reading was supplanted by the Textus Receptus in all but one "Caesarean" witness? In very many of these 168 instances the variant read by only one "Caesarean"

<sup>114</sup> *HTR* 21 (1928), 326. See also note 58 above.

<sup>115</sup> That is, by one of these eight: Θ, fam 1, fam 13, 28, 565, 700, W, and the Georgian.

witness is found also in Neutral or Western witnesses. Why should such readings not be called simply Neutral or Western? Moreover, the accepted method of determining the "Caesarean text" cannot but fail to discover all "Caesarean" readings, for certain "Caesarean" readings have undoubtedly passed into the Byzantine text and therefore are not disclosed when "Caesarean" manuscripts are collated against a Byzantine text. On the other hand, must it not be acknowledged that we have but Hobson's choice in the matter; is any other more satisfactory method for determining the "Caesarean text" at our disposal?

11. Is it possible to determine whether the "Caesarean text" is a correction of the Western text by the Neutral (as the Lakes,<sup>116</sup> H. A. Sanders,<sup>117</sup> and, with certain reservations, P. L. Hedley<sup>118</sup> believe), or a correction of the Neutral text by the Western (as Lagrange maintained<sup>119</sup>), or an independent text, co-ordinate in value with the Western and Neutral texts (as Streeter<sup>120</sup> and apparently Kenyon hold<sup>121</sup>)? Obviously the answer given to this question will determine one's attitude toward the usefulness of the "Caesarean text" in discovering the elusive text of the autographs. For, if the "Caesarean text" is a correction of the Western text by the Neutral, or a correction of the Neutral by

<sup>116</sup> *The Text of the New Testament*, 6th ed., p. 84; also "The Text of the Gospels" in *Studies in Early Christianity*, ed. S. J. Case (New York, 1928), pp. 30 f. Yet compare their letter (with R. P. Blake) to *The Times* (London), Dec. 29, 1931, p. 13, just after Kenyon's preliminary announcement of the discovery of the Chester Beatty papyri (*ibid.*, Nov. 19, 1931), in which they pose the question of whether the Neutral text is not rather to be explained as a revision of the "Caesarean," (so too Miss Silva New in a note sent to *JBL* 51 [1932] 73 f.). The present writer does not know, however, of any other publication of theirs in which this hypothesis, made before the text of P<sup>45</sup> was published, is explored, confirmed, or rejected.

<sup>117</sup> *HTR* 26 (1933) 94.

<sup>118</sup> *CQR* 118 (1934) 225.

<sup>119</sup> *Critique textuelle* II, 165.

<sup>120</sup> *The Four Gospels*, p. 106.

<sup>121</sup> *Recent Developments in the Textual Criticism of the Greek Bible* (The Schweich Lectures of the British Academy, 1932. London, 1933), pp. 70 f. and 85; "The Bible Text and Recent Discoveries," *University of Toronto Quarterly*, 5 (1936), 315; *The Text of the Greek Bible* (London, 1937), pp. 210 ff.

the Western, it may be disregarded as Hort as taught us to disregard the Syrian or Byzantine text.

12. Finally — and to swing into a much wider orbit — is it possible to analyze the textual complexion of a given document by utilizing all variants, large and small? Is the principle, employed with such persuasive force by Hort,<sup>122</sup> that “Identity of reading implies identity of origin,” really applicable in analyzing the affinities of the “Caesarean text” on the basis of very minor variations from the Textus Receptus?

On the one hand, if it is licit to employ all variants in determining family consanguinity, the full possibilities of the method have been largely neglected. The proper method of determining the relation of a hitherto unknown manuscript to the Neutral, Western, “Caesarean,” and Byzantine families is not merely to count how many of its variants from the Textus Receptus (or from any given norm) agree with B,  $\aleph$ , D,  $\Theta$ , W, etc. Such a procedure is indeed necessary and not un instructive, but the only really satisfactory method is to reconstruct the text of each of the major families and to determine precisely what proportion of variants from the Textus Receptus in such a reconstructed text is also present in the manuscript to be analyzed. For obviously it is of slight value in determining family relationship to know only that in a certain area a given manuscript agrees with, say, B and  $\aleph$  ten times in differing from Textus Receptus. If B and  $\aleph$  should in addition differ from the Textus Receptus in ninety instances, the Neutral element in the given manuscript would be slight indeed. It is necessary, therefore, assuming that identity of reading implies identity of origin, to determine exactly what proportion of the total number of variants of each type of text (and/or of each manuscript) is present in the manuscript to be analyzed.

But, on the other hand, is it really legitimate to utilize all variants, large and small, to determine the relation between manuscripts? Manifestly, a spectacular variant, such as the presence of the *pericope de adultera* after Luke 21 38 in all of the

manuscripts of family 13, has real significance in disclosing the textual affinities of a given manuscript. But it seems to the present writer that the possibility of chance coincidence among manuscripts in agreeing in small variations (involving, *inter alia*, word order, common synonyms, the presence or absence of the article, the aorist for the imperfect or historical present) has not been sufficiently taken into account. Is the fact, for instance, that in Mark 6 55 manuscripts W and 565 agree in reading  $\delta\tau\iota$ , whereas all other witnesses read  $\delta\pi\omicron\upsilon$ , significant in tracing family relationship? Or again, does the fact that in Mark 6 27 manuscripts 565 and 700 agree in reading  $\acute{\alpha}\lambda\lambda\acute{\alpha}$ , while all the other “Caesarean” witnesses read  $\kappa\alpha\iota$  with the Textus Receptus, prove that  $\acute{\alpha}\lambda\lambda\acute{\alpha}$  was the original family reading (as Lake *et al.* suppose)?<sup>123</sup> Or did the scribes of 565 and 700 just happen independently to use  $\acute{\alpha}\lambda\lambda\acute{\alpha}$  instead of  $\kappa\alpha\iota$ ? If one hundred people today were to transcribe independently from a common text, how often would they agree fortuitously in their errors? The point is that in many instances it is exceedingly difficult to decide with finality whether a given variant present in four or five manuscripts is significant or insignificant in determining genealogy. The conclusion one must draw is that many of the variants which are commonly utilized to show relationship among the members of the “Caesarean text” (or almost any other text) are not really capable of turning the scales in either direction.

<sup>123</sup> HTR 21 (1928) 230.

<sup>122</sup> *The New Testament in the Original Greek, Introduction and Appendix* (2nd ed., London, 1896), p. 46, §58.