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THE TEXT OF THE GOSPELS.

IN the present position of textual criticism of the New Testament it is necessary to speak of problems which await solution rather than of results which have been reached. From 1882 to the present day Westcott and Hort's theory has held the field in England, and to some extent, in Germany. Every one will remember that this theory is roughly as follows: They thought that the text of the New Testament could be explained as consisting of three main recensions, which were afterwards worked over and so formed the late Syrian or ecclesiastical text of Antioch. To these three recensions the names were given of Neutral, Western, and Alexandrian. Since their time there has been a general tendency up to the present to accept the theory that the late text is based on these recensions, but there has been considerable controversy as to the reconstruction of the recensions themselves. Especially has this been the case with the Western text, which, instead of proving to be the unity which they imagined it to be, has, as it were, disintegrated under the hands of the critics, until it has become difficult to speak any longer of the Western text in any but a purely academic sense, as it is quite certainly not Western in origin and almost equally certainly not a single text but many texts. Still, with this degree of modification the theory of Westcott and Hort has held the field.

We are now faced with the new theory of Professor von Soden of Berlin, which will have to be compared with those of Westcott and Hort and submitted to very close examination before being accepted or rejected. In the present article I do not propose to make any contribution to this examination, but merely to explain the main issues. Von Soden's work falls into two divisions: The first part, which

is purely mechanical, consists of the rearrangement of the nomenclature of MSS. He has given up the old system, which used the capital letters of the Latin, Greek and Hebrew alphabets for uncial MSS. and numerals for cursive MSS., and has in place of this worked out an ingenious system of assigning numbers to the various MSS. in such a way as to tell us at once the approximate age and contents of the MSS. in question. Older scholars who have been brought up under the old conditions are naturally (though not, I think, justifiably) aggrieved at this change, and under the leadership of Dr. Gregory of Leipzig, supported by Dr. Kenyon in this country, are trying to make a stand against this innovation. Yet they themselves have felt obliged to alter the old system in many ways, and for my own part I feel convinced that in the end the Berlin method will be found to have so many advantages over the old one that it will be generally adopted. Even if it were not distinctly better than the old method, it would probably win the day, because in the end we are always forced to use the system of nomenclature employed in the *standard critical edition*. The critical edition of the past and of the immediate present is that of Tischendorf, which uses the old notation, but it is only a matter of a few years before von Soden's edition will be published, and it is difficult to believe that it will not be the standard edition for at least the next fifty years. The reason for this belief is that von Soden was financed by an exceedingly rich Berlin lady and was enabled to send scholars to every library in the world containing MSS. and to investigate the character of every MS. in a manner which surpasses everything which has been done in the past, or which will be possible in the future, unless, indeed, Dr. Gregory or his friends can produce another millionaire and so enrich the world with yet another critical edition using his notation.

The second part of von Soden's theory is concerned with the grouping of the various MSS. His results are these : In the fourth century there were three main recensions of the text of the Gospel. To these he gives the name of *K, I, H*. *K* corresponds roughly to Westcott and Hort's Syrian text, and is connected by von Soden with Antioch, and especially with the recension which is known to have been made by the martyr Lucian. There are, of course, many differing sub-types of *K*, and von Soden has spent much trouble in grouping the various late MSS. into them ; for our purpose none of them are very important.

I, found in many important sub-types, roughly corresponds to Westcott and Hort's Western text, and *H* to Westcott and Hort's neutral text ; but von Soden thinks that the Alexandrian text of Westcott and Hort is nothing more than a subdivision of *H* of no special importance.

The probably correct elements in this theory which will be accepted by almost every one without much dispute are that *H* and *K* really represent definite recensions. Furthermore, no one is likely to dispute that a number of the subdivisions which von Soden traces in *I* really represent actual groupings of MSS. Many of them, indeed, had already been recognised, and some of them had been edited, though in every case von Soden seems to add to the extent of our knowledge.

The doubtful points require a somewhat longer statement. In the first place the reconstruction of *I* is exceedingly doubtful. von Soden's method is somewhat as follows : He reconstructs a number of sub-groups belonging in the main to *I*. To these he gives the names of *H^r, Φ, J, I^a*, and so on. Perhaps in some details his work will be criticised, but in the main this part is probably correct. Then he reconstructs from a comparison of these groups the original text of *I*, and here he has to deal with

our old friend, Codex Bezae. It is common knowledge that the Codex Bezae which is now at Cambridge is a Graeco-Latin MS. of the sixth (or, I think, more probably of the fifth) century, with a very remarkable text which Westcott and Hort regarded as the chief authority for the Western text. It frequently agrees with the old Latin version and sometimes with the old Syriac. Now von Soden regards *D* as one of the authorities for his sub-group *I^a*. The other members of this group are the MSS. which are generally described as 28, 565, 700, and a new MS. to which he gives the number of 050 and Gregory the symbol Θ . But he does not think that Codex Bezae is in any way a pure representative of the type, but has been contaminated by the Latin and Syriac versions. Thus his reconstruction of *I* leaves out a great many of the passages which Westcott and Hort regarded as typically Western, and *I* is as a whole a much less bold and remarkable type than the Western text of Westcott and Hort. I imagine that in the future we shall hear a great deal more of this point. There are the two possibilities: either Codex Bezae really represents an original Greek text contaminated by Latin and Syriac influences (which is the theory of von Soden) or it is a tolerably good representative of the same type of text as was made use of by the translators who produced the Latin versions. That has been up till now the dominant theory, and for myself I am not disposed to abandon it. If this second theory be adopted, it is plain that Codex Bezae is not a representative of *I* at all, but of an earlier text which may have been known to *I* but was rejected by it in its main features.

Another point on which von Soden's theory will meet with severe criticism is his rejection of Westcott and Hort's Alexandrian text. The point is this: No one doubts that there is a close connexion between 6 or 7 MSS., of

which the best known are \aleph B C L Ψ Δ Ξ 33, supported more or less by the Egyptian versions. But between these MSS. the chief difference is that which separates \aleph B from the others. Westcott and Hort's theory was that \aleph B represent the earlier stratum, and that the others are a more or less literary recension made in Alexandria. The frequent agreement between \aleph B and the Egyptian version was explained by the theory that the makers of the Egyptian version used early MSS. of the same type as \aleph B. von Soden rejects this theory, regards the other MSS. as frequently preserving the true text of *H*, and explains the peculiar characteristic of B as largely due to the influence of the Egyptian version on B. It is impossible to speak with certainty as yet, and the last word in this matter will probably have to be spoken by some one who has an intimate knowledge of the Egyptian dialect, a knowledge which is not often found, and is still more rarely employed in the interests of New Testament criticism. But with the greatest diffidence and reserve I must confess that the examples which von Soden gives in support of his theory seem to me singularly unconvincing. What is required is the proof that Egyptian idiom has produced readings in B which are not Greek, and almost all that he has produced so far seem to me to be readings which are indeed found both in B and in the Egyptian version, but are perfectly good Greek and may quite as well have originated on the one side as on the other.

Thirdly, there is room for considerable doubt whether von Soden is right in regarding *K* as entirely independent of *I* (supposing that *I* is a real entity) and *H*. Of course, if his theory holds good in other respects, this must be the case; for he believes that he can connect *H* with the recension said to have been made by Hesychius and *I* with a recension made or at least used by Eusebius. If he be

right in these two points, *K*, the recension of Lucian, cannot be derived from the combination of the two *I* and *H* in the way in which Westcott and Hort believed. It is, however, necessary to be careful not to be unfair to von Soden on this point. The temptation for English scholars is to say that Westcott and Hort demonstrated by the argument from conflation that *K* is the resultant of the two other texts, and that therefore von Soden's theory is clearly wrong. Westcott and Hort certainly proved that *K* is the resultant of a combination of earlier texts, but the weak point in the attack on von Soden is that although *I* and *H* *roughly* correspond to Westcott and Hort's Western and Neutral texts they do not do so completely, and (according to the Berlin School) are much later than the two recensions postulated by Westcott and Hort.

In this way von Soden reconstructs his three recensions *I H* and *K*. he then goes on a step further and reconstructs the *I-H-K* text, which he thinks was the common origin of the three. It is worth while to consider carefully the importance of the general hypothesis underlying this assumption. It is that before the fourth century, at some period which is not accurately defined, there was in existence a single text of the Gospels which was universally used, and that the recensions found in the fourth century, and represented by our MSS., are deviations from an originally common source. As I shall show later, I believe that it is this hypothesis which is the weakest point in von Soden's theory, but for the moment let me continue to describe his hypothesis. He believes that *I-H-K* was used in a tolerably pure form by Origen, and that he can trace its use in other earlier writers of Greek. But he has then to surmount the difficulty that the oldest authorities for the text, namely the Latin and Syriac versions, differ very widely from *I-H-K*, and that the Church fathers who used these ver-

sions show no signs of knowing any other type of text, and he tries to do this by a liberal use of the hypothesis that the Diatessaron of Tatian was almost universally known in the third century. In other words he reduces all the textual variation of the third century to the formula *I-H-K plus Tatian*. In different localities the proportions of the mixture were different, but in the main it is not unfair to von Soden to say that if a reading is neither *I-H-K* nor an obvious error he ascribes it to Tatian if he possibly can. This is the most serious part of the hypothesis, and it is probably worth while to spend a few minutes in discussing the material which is available for controlling it.

You will remember that Tatian was an Eastern by birth who became a pupil of Justin Martyr in Rome. He afterwards went back as a missionary to the Churches of Mesopotamia, and he also became a heretic. It is doubtful whether he became a missionary first and a heretic afterwards, or became a missionary because he found it impossible on account of his heresy to remain in Rome. Neither possibility can be excluded, and historical parallels could be produced for both. In any case in Mesopotamia he made use of a harmony of the four Gospels which is commonly called the Diatessaron. Whether he made this harmony himself in Syriac, or brought it with him in Greek and translated it into Syriac is doubtful. The activity of Tatian in Mesopotamia may be dated somewhere in the last thirty years of the second century, and for another 300 years the Diatessaron was the authentic Gospel of the Syriac Church. Probably the Syriac Church also possessed the four Gospels translated separately, and this translation is what we call the old Syriac. It was not, however, the official authorised version and never displaced the Diatessaron. But at the beginning of the fifth century Rabbula, bishop of Edessa, filled with a desire to make Syriac Chris-

tianity agree more closely with that of the Greek Church of Antioch, displaced the Diatessaron by a new translation, to which the name of Peshitta was afterwards given, and the Diatessaron not only fell into disuse but was destroyed whenever it was met with by the orthodox. The result is that there are no copies in existence of the Syriac Diatessaron. In that case, it may be asked, how does von Soden reconstruct the text of the Diatessaron with sufficient accuracy? The answer is that we have in an Armenian translation copious quotations from the Diatessaron in the commentary of Ephraim, and a large number of quotations in the Syriac writings of the fourth century Aphraates. These quotations really give us considerable information about the text of the Diatessaron, and the English school of students of this question, headed by Professor Burkitt, maintain that we have no other source which is reliable for the text of the Diatessaron. There are, however, two other documents which in a certain sense contain translation of the Diatessaron. One is the Codex Fuldensis, written by Victor of Capua in the sixth century, which is clearly based on the Diatessaron, but is textually useless because the compiler has merely copied the text of the Vulgate and followed the order of the paragraphs in the Diatessaron, so that the manuscript is an authority for the Diatessaron only so far as the order of the paragraphs is concerned, and is textually an authority—an exceedingly good authority—for the text of the Vulgate and not for the Diatessaron. Besides this there is an Arabic translation of the Diatessaron; but here also it is stated that the compiler did almost exactly the same as the compiler of the Codex Fuldensis, that is to say, he translated the ordinary Syriac text and only followed the order of the paragraphs in the Diatessaron. Probably, therefore, the Arabic version represents, not a translation from the original Diatessaron, but a translation from a

copy of the Syriac Diatessaron in which the text, as distinct from the order of the paragraphs has been replaced by that of the Peshitta. *There are, however, a few places in which it would appear that the writer who adapted the text was influenced by the original Diatessaron.* Now, the question between von Soden and other scholars is likely to turn very largely on the extent to which this is true. My impression is that von Soden thinks that there is a considerable amount of original Diatessaron text to be recovered from the Arabic, and that Professor Burkitt is inclined to the opinion that the Arabic is textually worth little more than the Codex Fuldensis. You will see that we have here a subordinate problem which will call for a good deal of controversy before it can be settled. We cannot really discuss von Soden's theory until we have made up our minds about the text of Tatian. And I fear that von Soden himself has assumed the solution of this problem somewhat too lightly. My own examination of the passages which he attributes to the influence of Tatian suggests that many of his examples are open to grave doubt, and that it is not impossible that when the whole question has been properly investigated we shall be forced to the conclusion that the number of passages in which the influence of Tatian is a really probable hypothesis is so small that the whole theory collapses. Most people who have written at all about von Soden have discussed this point, and those who read German will find an excellent statement of almost the latest criticism in Nestle's last edition of his *Einführung in das Griechische NT.*, Ed. 3, 1909 (not translated).¹

It has also often been said that von Soden pays too little attention to the Latin and Syriac versions. This is partly

¹ I may also refer those who wish for more detailed information as to von Soden's grouping of MSS. to my *Professor von Soden's Treatment of the Text of the Gospels*, published by O. Schulze & Co., Edinburgh, 1908.

true. I believe it to be a real defect in his book that he has dismissed the history and the text of these two versions with insufficient study. But the criticism is partly based on a misconception which is very unfair to von Soden, who, let me repeat, has in any case done more for the text of the New Testament than any other living man. The reason for this misconception is once more the fact that his *I* text does not entirely correspond to Westcott and Hort's Western text, and that people talk as if it did. According to von Soden the *I* text is later than the great versions, which represent an altogether different type. His point is that all Greek MSS. represent Greek recensions, based on *I-H-K*, and that the versions represent independent use of this original text, contaminated by Tatian. I very much doubt whether this theory is right, but that does not take away the fact that on his own theory von Soden is justified, and has not neglected the versions in reconstructing his three recensions for the simple reason that the versions have nothing to do with them.

It is plain that the complete criticism of von Soden will call for many years and a whole series of special studies. Until they have been made it is idle to do more than explain the points at issue and to express a tentative opinion as to the results of a superficial examination. Even superficial examination of a book containing about 2,000 pages, many of which are closely printed tables of Greek variants arranged with an insufficient indication of the place where one really ends and another begins is, in my own experience, a matter of months rather than weeks. But there is another line of criticism which is legitimate. One may ask whether von Soden's theory is historically probable. That is to say, is it historically probable that there was originally a single text of the Gospel, that this was contaminated by Tatian, and that the recension of the fourth century represents deviations

from an original text? I am prepared to argue that it is historically one of the most improbable that can be conceived.

Let us consider the probable history of the Gospels after they came into existence in roughly their present form. I take it that there is a general agreement that the four Gospels existed as documents at the beginning of the second century. There is also a tolerably general agreement that they did not become "Holy Scripture" for at least another fifty years. What happened during this fifty years? The Gospels had at this time a value, not because they were sacred books, but because they related the sacred history. Later on they were important for themselves as well as for their contents. The result must have been that in each Church any one who was possessed of a copy of one of the Gospels was inclined to value it in proportion as he believed that it contained all the facts. If he heard of a new fact, from whatever source, he put it in, and added it when he made a new copy. In this way every scribe was more or less a redactor. We must remember that it is unlikely that at this period each Church possessed four Gospels; more probably one locality had one and another locality had another. Gradually various localities came to have two, and then three, and finally four. We can prove that the "four-Gospel canon" came into existence in this way by the method of accretion, because in the earliest authorities we find no agreement as to the order of the Gospels. The result would be that during this fifty years the text of each individual Gospel had a local history of probably greater variation than can be found in the next 1,500 years. The next stage in the process was the attribution of sacredness to the text. From that moment the tendency to create variation was checked, and the ultimate standardisation of the whole became inevitable. At first, no doubt, each Church, though it accepted the four-Gospel canon, held to its own local text,

and the result was that there came into existence copies, no longer of the single Gospel but of the four Gospels, which perpetuated these strongly marked local texts. There was, no doubt, a text of this type in Africa, from which the African Latin version was made, and probably this took place in the second century, though it is just possible that it may not have been before the third. In the same way, Tatian either made his Diatessaron from the Greek text of the four Gospels current in Rome or from a Syriac text current in Antioch, or in the alternative from the Greek text current in Antioch. For reasons which have been given by Professor Burkitt the probability is that in the main he translated the Greek text which was used in Rome, and that this explains the undoubted resemblances between the European Latin and the Syriac text, as well as the fact that you do not get anything like the same resemblance between the African text and the Syriac.

In this way, then we have to imagine that the end of the second century, just at the moment when the "Four-Gospel Canon" became "Holy Scripture," saw the maximum amount of textual variation. I admit that this is merely hypothesis: but I would also maintain that it is an hypothesis which is extraordinarily probable in itself, and that it explains all the facts which we know (though, unfortunately, we do not know very many) about the text of the second century. In the third century the growing intercourse between the various Churches necessarily led to a comparison of texts and the beginnings of standardisation. Accepting, as I am inclined to do,¹ von Soden's view, that in the fourth century there were the three types, *I*, *H*, *K*, we have to see in these not, as he thinks, three forms of deviation from *I-H-K*, but three attempts, in three great centres, to standardise the almost infinite variety of local texts, in

¹ With some reservations as to *I*.

exactly the same way as Jerome did a century later with the equally infinite variety of Latin texts. The ultimate issue of this process was, of course, the production of the standard Greek text of the Church of Constantinople, a process which reached its last refinement in the edition published by the patriarch of Constantinople a few years ago.

I submit that this theory is a reasonable alternative to that of von Soden, and it is worth remembering that his *I-H-K* is at least as much an hypothesis, unsupported by direct documentary evidence, as is the suggestion of a series of local texts which were gradually brought into agreement.

I should like to occupy the remainder of my time in a somewhat broader question. Textual criticism is a desperately dull subject for all but a few specialists, but the public has a right to ask what is its general importance. To this question I propose to give an answer, and it is most important to notice that the answer given is different from that which would have been returned even by Westcott and Hort. I take it that Westcott and Hort would have said that the object of textual criticism is to restore the autograph of the Gospel with a view to writing the Life of Christ. The various recensions were merely lamentable corruptions. How do we stand now? We should say, I take it, that if we want to write the life of Christ, the reconstruction of the original text of the Gospels is insufficient, and that the recensions are in some ways quite as important as the original text; because, if we compare the various recensions of different localities, we can use them to illustrate, and even to explain, the various developments of doctrine and practice in the various Churches, and thus attain as it were a kind of parallax, which helps us to reconstruct the original "point of view." Now, if we want to write the life of Christ, nothing is more important than to understand the "point of

view" of the people who wrote the Gospels. We cannot get behind the documents unless we know the sort of thing which is likely to have influenced the writer, and anything which tells us what they believed helps us to understand this.

But one must say more than this. Supposing we could reconstruct the original text of the four Gospels, it would not have the same value to us as it had to Westcott and Hort for the study of the life of Christ. Between them and us falls the shadow of the Synoptic question. You will be generally aware that the results of a century's work at this problem are at last beginning to reach some sort of definiteness. There is an almost common agreement that Mark is one of the sources of the two other Gospels, though there is less agreement as to whether Mark itself is a composite document. Think what that means;—it means that for the purpose of writing the life of Christ we have in the Marcan sections not three but one primary document of the first century, and that so far as Matthew and Luke cover the same ground as Mark they must be regarded not as parallel accounts but as two early commentaries on Mark. They have their own very great use, but no historian who has the original source thinks of building on a commentary, however excellent it may be. Therefore we may say that while, so far as the study of the life of Christ is concerned, the reconstruction of the text of Mark is really important, the reconstruction of the text of the Marcan passages in Matthew and Luke has, comparatively speaking, only a secondary importance.

Moreover, it is plain that any one who wishes, I will not say to write the life of Christ, but to write something about His life, must base all his conclusions not on the text of the Gospel, but on the reconstruction of the sources of the Gospel. It may be said that this leaves us a very insecure foundation. That may be so: possibly the result of research may

be to show that it is for us just as impossible to know Christ after the flesh as St. Paul believed that it was for him inexpedient. But even if this be so, it is better to know it. A builder is not absolved from examining the ground on which he is going to build, because he will have to alter his plans if it prove to be unsuitable.

Textual criticism combined with source criticism has taken away from us for good the old idea of the "holy original." It has given us in its place a series of documents which enable us to trace the history of early Christianity. The value of that result depends entirely on the way in which it is used. If it be treated from the standpoint of Western civilisation and of the nineteenth century, which contented itself with labelling this practice as magic and that account as legendary, its value will be small; but, on the other hand, for those who take the trouble to get behind documents, however corrupt, and practices, however foolish, and try to understand something of the spirit which animated the men who wrote and the deeds which are described, its value can scarcely be overestimated; for I venture to think that it will make clear that what made Christianity a great power in those days was neither a complicated theology nor an elaborated cultus, but the personal experience of individuals, which, though expressed differently, was essentially the same as our own, and it will be possible to see that the obscure phraseology of the theologian, which differs in every age and is soon forgotten, is only the attempt to express permanent facts which in themselves are few and simple, even though they surpass thought and defy language.

KIRSOPP LAKE.