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## PREFACE TO THE PRESENT NUMBER

A few words of introduction may seem fit, now our "Bezan Club" is deploring the loss of its Secretary, our much lamented friend, Professor D. PLOOIJ.

One of his best students, our Fellow-member Miss Dr. ADOLPHINE BAKKER has written a memorial for him which comes first in this new issue of the Bulletin. Our Club owes more to Dr. PLOOIJ than we can ever express, and we mention his name here with sincere gratitude and admiration. During the last ten years our Secretary had been enabled to provide us with ten numbers of this Bulletin packed with important matter. We are convinced that its pages have served his intention of providing an adequate means of private communication between us on those matters of textual research in which we are interested, and that they have done so to the satisfaction of all our Fellow-members. The death of Dr. PLOOIJ is a great loss to these studies, and might seriously affect the Bulletin's usefulness.

As it appeared, however, desirable that this part of our work should be carried on, both our President and our Treasurer have requested me to see the present number through the press and to act as Secretary in the future. Accepting this trust I shall try to carry on PLOOIJ's task in this regard with the nearest approach to his spirit of cordial and informal cooperation that I may be able to realize. It depends on our Members to maintain the serviceableness of our Bulletin, and, if possible, even to raise its degree of usefulness by their contributions. We should be very glad if a greater frequency of the issues could be the response.

Our Fellow-member Rev. A. M. COLEMAN has obliged us by the compilation of a very welcome and useful

Index to the numbers I to X. We acknowledge a debt of gratitude to him on behalf of all our members.

The present XI<sup>th</sup> number contains: (1) *Memorial* of Prof. Dr. D. PLOOIJ by Miss A. BAKKER, (2) *Memorial* of Prof. Dr. H. WINDISCH by the Secretary, (3) Note on the *Resurrection of the Bezan Problem* by RENDEL HARRIS, with a prefatory remark by Dr. PLOOIJ, (4) *The Greek New Testament goes West*, by RENDEL HARRIS, (5) *The Latin text of the Epistles of St. Paul* by Dr. PLOOIJ, (6) *On the new editions of the Acts of the Apostles*, I. a Letter of Prof. H. A. SANDERS to Dr. PLOOIJ. II. a Note by Prof. H. VOGELS.

The Index to numbers I to X by Rev. A. M. COLEMAN is added separately.

J. DE ZWAAN  
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† PROFESSOR DR. DANIEL PLOOIJ  
(1877—1935)

On the 5th of July 1935 Dr. D. PLOOIJ passed away at the age of 58 years. By his death the chair of N. T. and Early Christian Literature both at the Utrecht and Amsterdam University fell vacant. The Royal Academy at Amsterdam lost in him a highly esteemed member. A keen and enthusiastic explorer of the wide field of N. T. studies was called home. Love of the N. T. and of the Gospel contained in it was the motive power of his work which aimed at a deeper understanding both of its original meaning and of its history<sup>1</sup>.

It is especially the latter side of Dr. PLOOIJ's studies which was known to the members of the "Bezan Club". Started at the time when Dr. PLOOIJ had just discovered the importance of the text of the Liège Diatessaron for

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<sup>1</sup> In this connection mention should be made of the painstaking study devoted to the chronology of Paul's life: "*De Chronologie van het leven van Paulus*", Leiden, Brill, 1918 (VII + 195 pp. 8<sup>o</sup>).

the textual criticism of the N. T., the "Bezan Club" shared in the development of ideas on this matter by means of many a valuable note or article. The questions raised by this find were discussed in a more elaborate form in "*A Primitive Text of the Diatessaron*" and in "*A further study of the Liège Diatessaron*" (Leyden 1923, 1925). An edition of the Liège text with English translation and a comprehensive apparatus was meant to collect the material essential for a final solution. A few weeks before Dr. PLOOIJ died the 4th volume of this work appeared, completing about half of it.

During these years of prolonged research the initial hypothesis of a wide-spread influence of the Syriac Diatessaron on the Latin Gospel tradition and its descendents had grown into a strong conviction that the clue to the solution of the problem of the Western text had to be looked for in this direction. In an article on the Bezan problem in Bulletin IX this hypothesis is even extended to the text of Acts. The question of the original language in which Tatian wrote his Harmony, revived by the discovery of a Greek Diatessaron fragment in Dura, occupied Dr. PLOOIJ's attention during the last months of his life. Unfortunately he has not been able to publish his opinion on this important subject in a definite form. A preliminary study in the *Expository Times* of June 1935 which tends to the conclusion that the Dura fragment is not sufficient proof for a Greek original, shows that this find did not affect his fundamental view on the Diatessaron question. Thus, notwithstanding the fact that he did not live to complete his work, Dr. PLOOIJ left us a clear-cut vision on matters of textual criticism, which no doubt will prove of great value for its further development.

In the hearts of those who had the privilege of coming into closer touch with Dr. PLOOIJ, respect for the work done by him and for the way in which he did it goes with a feeling of deep gratitude, which can only be expressed more or less adequately by trying to follow in his track.

Ad. H. A. BAKKER.

†. PROFESSOR DR. HANS WINDISCH  
(1881—1935)

Another of our members, Dr. H. WINDISCH, was taken away from us on Nov. 8<sup>th</sup> 1935. From 1914—1929 he occupied the chair of N. T. and Early Christian Literature in the University of Leyden, afterwards at Kiel in Germany and at Halle. The great enterprise of the "Corpus Hellenisticum", planned by HEINRICI, carried on in succession by VON DOBSCHÜTZ, has lost in him its third leader.

WINDISCH's New Testament work was to a large extent influenced by his vivid interest in the struggles of Liberal Christianity in Holland and in Germany. His "*Present state of the Christ-problem*" (2<sup>d</sup> ed. 1925), "*Der Sinn der Bergpredigt*" (1929), "*Paulus und das Judentum*" (1934) and, appearing this year, his posthumous "*Brunner's Mediator and the New Testament*" show this characteristic tendency. He remained faithful to a very mitigated form of the theories of the "later Tübingen School" and never quite appreciated the significance of SCHWEITZER's work. His peculiar force appeared in the discussions of the "Neutestamentler-" and "Theologentage" in Germany in which he took part; and he is probably seen at his best in his well known Commentaries in LIETZMANN's "Handbuch zum N. T." and in the surveys of New Testament and Early Christian Literature which he wrote for the "Theologische Rundschau". As a specimen of the research work of this fine scholarly mind one must consult "*Die Orakel des Hystaspes*" of 1929, a contribution to this most intricate problem which was published in the "Verhandelingen" (Letterkunde, N. R. XXVIII, 3) of the Royal Academy at Amsterdam. In this study he pursued the origins of this "prophecy" into Persia and down to the 5<sup>th</sup> century, making a good case for its non-Jewish origin. By the way he produced a vast amount of information and a number of important views on a host of questions which are of profound interest to the student of Christian origins.

The Semitic side of early Christianity did not attract him; Greek philology and the technical side of textual

research he seemed to view as mere means to a higher end. But his conscientious and painstaking work as an expositor of the New Testament, and his leading functions in the preparation of the "new Wetstein" sustained the interests which he professed by his membership of our "Bezan Club".

J. DE ZWAAN

#### RESURRECTION OF THE BEZAN PROBLEM.

Dr. RENDEL HARRIS sends me the following admirable note as an introduction to another Bulletin of our Club. On account of the importance of the subject, I thought it advisable to send a preliminary copy of this introductory note to our Members hoping that it may serve as a stimulus for sending us their observations and suggestions on the problems raised by the important new publications. We again wish to emphasize that the informal way in which the pages of our Bulletin are put at our disposal for friendly discussion, is perhaps the most effective method for real cooperation in solving the fascinating problem of the 'Western' text.

† D. PLOOIJ.

#### THE ACTS OF THE APOSTLES

There can be little doubt that the study of the early history of the Christian Church, as outlined in the Acts of the Apostles, has received a great impetus from the simultaneous appearance of two great works dealing with the subject, in each of which a fundamental study of the Greek text is involved. On the one hand we have the two new volumes by Professors LAKE and CADBURY, with some cooperation from other quarters; on the other hand we have the monumental edition by Prof. A. C. CLARK, of Oxford, in which the text is re-edited from the MS. known as the Codex BEZAE (and its allies). The points of view are almost diametrically opposite. CLARK claims the Bezan text as the original, following in this the lead of BLASS that Luke published two successive editions of the

Acts, of which the Bezan is the earlier. Prof. CLARK does not accept BLASS' theory of two editions published by Luke. LAKE and CADBURY with the text as edited by themselves and their late colleague Prof. ROPES before them, have made very slight concessions to the Bezan text, except in occasional details, and have almost entirely set on one side those expansions of the Bezan text, to which has been given the name of the Western Glosses. CLARK very properly objects to both of the terms here involved. He points out that they are not Western, nor will he allow the Codex Bezae itself to be Western, even in a geographical sense, nor glosses to be interpreted in any sense of their being secondary to an original text. The epithets are what is known as 'question-begging', which I suppose is a translation of 'petitio principii'; and CLARK does his best to get rid of them by a change of notation. It is, however, too late to do this without a wholesale massacre of standard works. It was HORT who emphasised the nomenclature, and in part invented it, by a companion term of 'Neutral' for the Vatican text; and one of the first things that CLARK does is to set HORT in evidence against himself, and so making him sponsor for the superior antiquity of the Western readings which the system actually rejects.

A few observations on my own part may serve to reopen the discussion of the relative value of D and B in the Acts.

The whole question has been re-opened; and we are back again at the time when BLASS, then my guest at Cambridge, was occupied with the Lucan text, and examining with his practised eye the Codex Bezae itself in the University Library. What has happened since then to make a re-statement of BLASS' position possible?

Very much has happened. On the one hand there is a certain amount of evidence that has been withdrawn. The supposed Western evidence of the Greek text of the Codex Laudianus at Oxford (Cod. E) is now admitted to be Greek (at certain points at least) only by a process of re-translation from Latin, and is no longer entitled to a capital letter for any such re-translated passages. ROPES has stated this part of the argument with sufficient clear-



ness. We say, then, that there is some reason to suppose that the evidence for a Western Greek text of the Acts has been overrated. It is natural to ask whether it is not also true that some traces of similar reflex action may not be observed in the Bezan Greek text itself. Indeed I have sometimes asked myself whether under this kind of criticism, based on supposed Latin and Syriac re-translations, there will presently be any Greek text left, as differing materially from the canonical editions. At this point we are sharply checked. First of all, and here CLARK is emphatic and cannot be contradicted, the Bezan text itself is much older than it has been thought to be. The actual MS. is older. Fifty years ago we used to say that the Codex Bezae was a sixth century MS. It is now admitted to be of the fifth century at latest, two members of the Bezan Club, the late Professor BURKITT and Dr. LOWE bearing the same witness on this point. In the next place, the paleographic peculiarities of the Codex Bezae are indicative again of an early transmission. It has long been known that the short, broken lines in which D is written, were in the copy which the scribe had before him. Consequently when we push back the Bezan text, we push back its ancestry. I think it will be felt that this is difficult to follow.

Is it really true that an early text of the Acts could be written in these broken verses instead of in uniform lengths, such as are more properly called *στίχοι*? Here is one of the first places for a division of opinion. CLARK tells us plainly that the history of the development of a MS. tradition as to how a book should be produced is largely at fault; and even the supposition that a shorter text must be an earlier text should be discarded. His argument under this head is very powerful, based upon much original work, and must be attended to. It bears at once upon the glosses. They may look like additions, but there were artists in book-publishing, who knew how to use, what we call editorially, 'the blue pencil'. The next thing to notice is that the Bezan text not only pushes back its own ancestry, but also the ancestry of those

versions, such as the Syriac, which share its peculiarities and argue convincingly its antiquity.

Next we see that the evidence for antiquity and for geographical diffusion in antiquity, has been powerfully increased. There is, for instance, the Koridethi MS. of the Gospels from the Caucasus, which is as far as can be from the West, and has Bezan affinities; then there is the Washington MS. of the Gospels, which declines to be called either Neutral or Alexandrian, though found and written in Egypt; and lastly, perhaps of as much importance as all the others, there is the Michigan Papyrus of a portion of the Acts, including certain characteristic glosses (excuse the phrase, Prof. CLARK); and it is quite possible that the papyrus itself may go back to the end of the third century, and be the oldest known text of the Acts. More might be said about the evidence of the versions; but we are not writing an introduction to the textual criticism of the New Testament. It is evident that we are far back in the second century, and if age implies respect, the Codex Bezae is in the very front row of honour. So we are face to face with the old problem, viz., the so-called Bezan glosses.

They must be carefully considered one by one. The critics must tell us whether there were seven steps to the prison at Jerusalem, and if not, why should anyone say there were. They must tell us why the normal text takes St. PAUL from Caesarea to Jerusalem in a single day! Was NESTLE right when he said the school of Tyrannus was empty between 11 a. m. and 4 p. m.? Was I right in suggesting that the reason for 'tarrying at Trogyllium' was that they could not get *pratique* or past the Customs House before sunrise? and so on, and so on.

I will not express a final opinion; what I am saying is that every question that has to do with the Bezan Acts is re-opened. The Bezan Club must look to the matter; we are no longer in a backwater. Prof. CLARK will see that we do not get into one again. The chief centres of discussion will, I expect, be the Michigan papyrus and the so-called glosses.

How far have LAKE and CADBURY conceded the correctness of the 'glosses'? How far have they established that they were originally produced in Greek?

RENDEL HARRIS.

#### THE GREEK NEW TESTAMENT GOES WEST

The headline which I have written for this note does not mean anything sepulchral; it is not to be read or interpreted in the language of the Great War; so far from being consigned to the oblivion which they call death, it is very much alive, and progressively alert. The only thing that has happened is that the Greek New Testament has migrated. Our study table is in evidence for the accuracy of the statement.

Fifty years ago, when we realised first that the centre of gravity of the world had gone West, a mocking mentor at our side said, "No, not the centre of gravity, but the centre of grocery!" The jest seems strangely out of date at the present time, for here before us is a volume containing collations of six important MSS. of the New Testament, never before collated, or, if collated, accurately and adequately collated; and amongst the six, one of the most important comes from the University of Utah. We should as soon have expected the Moon to drop from her sphere, as for a New Testament study to be announced from Salt Lake City. We did not even know that there was such a University. Certainly Galileo was a prophet as well as a scientist, when he said "Eppur si muove". The study of the New Testament has moved, and the MSS. of the New Testament have moved also. Where the books themselves could not be moved, the art of photography has taken accurate copies of them; and one is tempted to predict that the next Greek text of the New Testament will come not from Berlin, nor from Stuttgart, nor from Cambridge (England), but perhaps from Cambridge (Mass.). It is evident that there is a driving force behind this publication, which is finding a

response all over the United States. This driving force (to give credit where it is due) is clearly the personality of Professor KIRSOPP LAKE, to whose many labours must be added the power to enlist the cooperation of men and women of very diverse religious appellations. Prof. LAKE has always been in the ranks of the great collators; and we believe he holds with ourselves that to be a sound critic one must be experimentally an accurate collator. The title of the volume to which we are referring is *Six Collations of New Testaments Manuscripts*; it is published by the Harvard University Press, as an extra number of the Harvard Theological Review. We should like to make some observations on these six Mss. to which Prof. LAKE here draws attention — but our collating days are over. We will only make one remark, viz., that we are glad the collations are made on a standard issue of the Textus Receptus, and not on any modern edited text.

The next remarkable proof of our statement of the westerly movement of New Testament textual criticism will be found in another Harvard volume. This is the work of my friend Dr. HATCH, who was with me on my last visit to Mt. Sinai. HATCH saw the importance of a renewed study of *dated* Biblical Mss. both for the palaeographer and the **theologian**. So he set himself to work on photographic reproductions of those Mss. in Mt. Sinai and Jerusalem to which the scribes had attached trustworthy notes of time; in this way he produces columns which will be authoritative for Byzantine art as well as calligraphy. We understand that it is his intention to proceed further with these reproductions, and to issue a dated series of Syriac New Testaments. This is, perhaps, a more difficult task; the dating of Syriac Mss. has pitfalls of its own: we are glad to think that this branch of study is in such careful hands.

RENDEL HARRIS.

THE LATIN TEXT OF THE EPISTLES OF ST. PAUL

Our Club is concerned with the very earliest traditions of the New Testament text. Whatever may be the ultimate result of our researches with regard to the relation of the Syro-Latin tradition to the "genuine" text, we shall scarcely be able to deny that generally the ancestry of the Syro-Latin text can be traced at least two centuries higher than the text of our great Uncials, even into the Greek texts of Irenaeus and Clement of Alexandria. When Latinisms can be discovered not only in the Bezan bilingual but also in the Washington Codex and in the Koridethi Gospels, when it is true, as Dr LOWE has told me, that the Codex Bobiensis *k* shows palaeographical marks of having been copied from a 2<sup>d</sup> century papyrus, well then, I think there is scarcely one subject of textual criticism of greater importance than the study of our Latin tradition. It is for this reason that I wish to draw attention to the study published by Prof. HANS VON SODEN in the *Festgabe für Jülicher*, under the title „*Der Lateinische Paulustext bei Marcion und Tertullian*” (MOHR, Tübingen 1927). Since HARNACK's *Marcion* (1921, pp. 42\*—50\*, 135\*, 160\*—163\*) the thesis that Tertullian used a Latin text of the Marcionite New Testament seems undisputed. Lietzmann has argued that the Catholic translators of the Epistles used an "African" text. Whether this was a genuine "African" text or not is difficult to decide; at all events both translations seem to be independent. This is the general result, rather roughly sketched, of Prof. VON SODEN's valuable study, based upon numerous and very exact collations.

Prof. VON SODEN has confined his researches to the Pauline Epistles, on account of the greater simplicity of the problem here compared with the Gospels and Acts. I doubt very much whether this simplicity is not really delusive. One factor, it is true, harmonistic influence, is pretty well absent from the text of the Epistles; and it is a very disturbing factor indeed. On the other hand, the simplicity might very well be compared to that of the textual tradition of the Classics, which is only a deceptive sim-

plicity for want of documentary evidence. As a matter of fact the evidence for the Gospels has a much wider range, and for that reason the currents of textual tradition may be followed much more clearly and easily than in the Pauline Epistles. It may be true that the "Western" text of the Epistles does not show traces of genuine Marcionite readings, while the Old-Latin tradition of the Gospels and even the Vulgate text simply teems with them. So the problem seems more complicated than the result of Prof. VON SODEN's study tends to indicate. Personally I not only agree with VOGELS that the Catholic translators used the Marcionite text as a help to their translation, but I am persuaded that the Marcionite text is at the base of the Old-Latin Gospels. (Perhaps I may be allowed to refer to a paper by myself in the *Or Chr.* III, Bd. I, 1927, pp. 201—222 on "*Die heutige Lage des Diatessaronproblems*" — especially p. 207). But certainly all these problems deserve our full attention; and it is for this reason that I would draw the attention of our Fellow-members to the interesting paper.

† D. PLOOIJ.

#### ON THE NEW EDITIONS OF THE ACTS OF THE APOSTLES

##### I.

The suggestion that we send in comments on the works of CLARK and LAKE—CADBURY on Acts, came to hand but was mislaid in the pressure of work. I hope these few notes do not come too late to be used in some slight part, if any seem important.

I am thoroughly in sympathy with the work of CLARK. His defence of the Bezan text is right in principle and method. I believe he does not appreciate quite fully the value of the Old Latin for establishing the original Greek. I am quite sure that he undervalues the text value of the special variants of *d*. He omits to mention some of these because his edition gives the Greek text only. I have recently put a pupil at work on a 12th century Ms of Acts in our Library. It is in Latin and written in England. As my hasty survey showed before we bought

it, the text is filled with excellent Old Latin readings, though in many places accommodated to the Vulgate. Here we find for the first time support for many of the longer additions of *Dd* and *harkl*. But just as striking is the affiliation with *gig* etc. and even with *h* and the other African witnesses. It seems clear that the text is derived through the Old-Latin of England and Ireland from the original missionary Bible of those islands. It will be for Acts what *r* and *r*<sub>3</sub> are for the Gospels.

In the republishing of Mich. Pap 1571 of Acts both CLARK and LAKE made some errors. LAKE's treatment is found in *Beginnings of Christianity*, vol. V, p. 263, note on line 2: he doubts *διακ[ατηλέγ]χετο* because the line is made too long, 38 letters, but he is wrong. One must consider the size of the letters. In that line there are 6 *Iotas*, 5 *Sigmas*, 4 *Omicrons*, and five other narrow letters (*γ, ε, ρ*), and only 15 broad letters, while line 4 has 18 broad letters and two abbreviations, which take extra space; yet it has 33 letters. The decisive proof, however, is that it is necessary to supply only 6 letters at the end of l. 2 (4 of which are broad), while at the end of line 3 one must supply 7 letters (4 of which are broad). Yet the papyrus is broken off quite squarely at the ends of these lines, though it was impossible to keep the small fragments containing them exactly in place when photographing.

As to l. 10, LAKE quotes me as thinking that the papyrus reads *δ'* instead of *δε*. I now accept WILCKEN's emendation except that I read *οιδαπεκρεια[ντοπρο]σαντο*. There are to be sure slight traces of ink that might pass for the apostrophe, but other similar ink spots must be disregarded in this and other lines. They are due to offsets when the fragment was closely crumpled. There is no room for a letter between *δ* and *α*. On p. 264, l. 4. the Pap. does not read *παντα* as LAKE suggests from the photograph; *παντα* is clear, but the fiber on which *Nu* was written had split and the left half containing *Δ* had bent to the left, making it look like *Alfa*. The facsimile does not show the bending of the fibers.

In ll. 10 and 11 LAKE's reading [σνευ] / ἰα[ ξ̄ ιου]δαίου is impossible, but his reference to the reading of Alexandrinus (σνευῖα) is in point. The corresponding line on the recto may be supplied ουσι τι, or 4 narrow letters and 2 broad; even with the *Nu* included it is only a little more than 5 average letters. This is in accord with line 9 on the verso, which has exactly 5 letters in *lacuna* (4 of them broad). The *Nu* must be reckoned as in the *lacuna* for only the first leg of it shows on the very edge of the preserved papyrus. The fragment has spread to the left making the *lacuna* seem broader at the top than it should. Line 10 has a little longer *lacuna* than line 9, yet even then the maximum in *lacuna* must be 5 letters or at most 6, if several are narrow. With the *Alpha* before and the *Delta* after LAKE would force in 6, all broad except 2, and one of these a numeral, which would take double space. Furthermore [σνευ]ῖα cannot be read. Nearly half of an *Omicron* after *Iota* is visible in l. 11. In this hand *Alpha* always has a rather pointed left loop. It could never look like an *Omicron*. The letter partly visible is either ο or ω or σ. Of these only ο is explainable. It is best to read as I did at first [.....] / ἰο[υ ἰου]δαίου, though I then thought it a dittography, which was corrected. However I have now conceded to LAKE the possibility of crowding in extra letters at the end of the previous line. In line 3 we must supply 5 broad letters: there is as much or more space here. Therefore 7 letters, 5 of them narrow, should not be considered impossible, though perhaps not easy. I now suggest that the reading be construed υ[τοι σνευ] / ἰο[υ ἰου]δαίου. This is an imitation of the spelling in the Alex. but with the older form for the genitive; cf. Mayser, *Gramm. d. gr. Papyri aus d. Ptol. Zeit* p. 251, where the form in -ου is amply defended. In Josephus and the New Testament the gen. in -ᾱ becomes more common, but that in -ου survives. Note also the variant form Σκαίουιος in Dio Cassius 37.53 (cf. Scaevi, C. I. L. II, 396). As ε is the later equivalent for αι, the form of Scaeva in the Alex. and here may be due to confusion with the similar name. Both are variants of the common Roman name Scaeva,



and so it is not original in Acts: it is an insert made under "Western" influence. Luke could not have given a Latin name to a Jewish priest.

The above should be used in correcting CLARK's edition of the fragment. Also one may note that C. has misunderstood HUNT's suggestion for correcting line 19 of the recto. It should read *[ημων ιην χεν εισ α]φειν*. The form CLARK suggests is unusual and two letters too long.

HENRY A. SANDERS.

## II

Für kein Buch des Neuen Testaments ist die Erforschung der Textgeschichte so zielbewusst und erfolgreich in Angriff genommen worden wie für die Apostelgeschichte, und unter den Sachkundigen ist man sich darüber einig, dass es gilt, für eine der beiden alten Formen sich zu entscheiden, entweder für eine durch die stark überwiegende Mehrzahl der griechischen Zeugen gebotene Form, die wenigstens gegen Ende des 4. Jahrh. auch auf lateinischem und syrischem Boden, desgleichen in den ägyptischen und anderen Versionen sich durchsetzt, oder für die Gestalt, die im doppelsprachigen Codex Bezae, bei Irenaeus, in den lateinischen Übersetzungen vor Hieronymus, in den syrischen vor der Peschittha bezeugt ist, dann im Laufe der Zeit mehr und mehr verdrängt wird, ohne jemals restlos zu verschwinden, die vielmehr in der Harclensis, bezw. deren Randlesarten eine neue und unerwartete Auferstehung feiert. Jenes Entweder — Oder wird freilich kein Verehrer des  $\alpha$ -Textes so verstehen, dass nicht gelegentlich auch einmal in  $\beta$  das Richtige zu finden wäre, und umgekehrt wird niemand so auf  $\beta$  schwören, dass er in keinem einzigen Fall der  $\alpha$ -Lesart beipflichtete.

Der heute ziemlich allgemein üblichen Bevorzugung der  $\alpha$ -Form, als deren kundiger Anwalt zuletzt J. H. ROPES, *The beginnings of Christianity*, Part I, Vol. III, 1926, auftrat, stellt A. C. CLARK, *The Acts of the Apostles*, Oxford 1933,

die These entgegen,  $\beta$  biete das Ursprüngliche. Ein schon um die Mitte des 2. Jahrh. in Sinnzeilen geschriebener Text ( $Z$ ) sei teils durch unabsichtlichen Ausfall einzelner Stichen, in der Hauptsache aber durch bewusste Kürzung zu der Form umgearbeitet worden, wie sie die Masse der griechischen Hss ( $\mathbf{I}$ ) bezeugt. Mit dieser These würde die Beza-Hs wieder in den Mittelpunkt der Forschung rücken; mehr als mir richtig scheint. Sie ist ja weder die einzige Vertreterin der  $Z$ -Form, noch hat sie deren Gestalt besonders rein bewahrt; vielmehr werden die Zitate bei Irenaeus, Cyprian und solche bei Augustin, ferner das Palimpsest von Fleury ( $h$ ) den Vorrang vor ihr beanspruchen dürfen; und bis aufs Wort wird jene Gestalt vermutlich niemals wiederhergestellt werden können, sicherlich nicht in den Sinnzeilen, wenn diese wirklich in so hohes Altertum zurückreichen sollten, wie CLARK glaubt.

Aber schon Hieronymus (*Praef. in libr. Isaiae*, ML 28, 771) ist einer solchen Annahme wenig günstig. Sein Wort *novo scribendi genere* klingt doch so, als wenn man jedenfalls im Abendland von einer stichischen Schreibung der Bibel kaum vernommen, und man müsste schon mit CLARK annehmen, dass  $D$  in Aegypten geschrieben sei (die Schreibung in  $D^{\text{paul}}$ , mit jener des Cantabrigiensis eng verwandt, weist auf das Bestimmteste zum Westen!), wenn nicht seine These an diesem Hieronymuswort bereits zu Schanden werden soll. Zwar lehrt das Studium jeder beliebig herausgegriffenen Doppelseite, dass die beiden Fassungen  $D$  und  $d$  schon einige Zeit lang miteinander verkoppelt sein müssen; denn sowohl hat der primäre  $D$  auf  $d$  eingewirkt, als auch — dies letztere unterschätzt CLARK auffällig —  $d$  auf  $D$ . Auch die Zeilenschreibung und was mit ihr zusammenhängt, d. h. die Einteilung des Textes in grössere Absätze, das Einrücken der Zeile bei alttestamentlichen Zitaten u. dergl., stellt offenkundig Überlieferungsgut dar, ist nicht für diese Hs. vorgenommen worden. Die grosse Frage ist nur, ob wir der Arbeit ein 300-jähriges Alter bewilligen dürfen, und auch dann wäre noch zu untersuchen, mit welcher Treue sie weitergetragen wurde. So, wie sie in der Hs geboten wird, ist sie jeden-

falls ein höchst mangelhaftes Werk, um Vieles weniger gut als jene des Codex Claromontanus; dabei will noch bedacht werden, dass die Aufteilung der Paulinischen Briefe in Sinnzeilen einen Herausgeber vor ganz andere Aufgaben stellte, als die Arbeit, den einfachen, leicht verständlichen Text der Apostelgeschichte zu gliedern. Die Sinnzeilen im Claromontanus sind so treu überliefert, dass sich die Störungsstellen, durch nachträgliche Überarbeitung hervorgerufen, als solche aus den Stichen vielfach noch deutlich erkennen lassen. Auch im Text von *Dact* sind zahlreiche Fälle zu verzeichnen, wo die Formen **Z** und **Γ** nebeneinander stehen oder ineinander geschoben sind, aber dass die nach CLARK uralte Sinnzeile von diesem Vorgang Zeugnis gäbe, lässt sich kaum irgendwo beobachten. Wohl sichert gelegentlich der Stichos eine bestimmte Textform, wie 1,25, wo es nicht mit CLARK heissen dürfte *ἐκ τούτων των δυο / ενα λαβειν*, sondern mit der Hs. *ἐκ τούτων των δυο / αναλαβειν*, wenngleich das letztere natürlich nichts anderes ist als ein sehr alter Fehler. Und gelegentlich protestiert die stichische Schreibung gegen eine durch CLARK vorgenommene Ergänzung, so z. B. 16, 12; 2, 31; 5, 38: *η το εργον τουτο καταλυθησεται / η δυναμις αυτου*, oder 13, 45, wo es natürlich heissen müsste: *και αντελεγον τοις λογοις <τοις> / υπο του παυλου λεγομενοις*.

Wichtiger ist es mir zu sagen, dass jene Textausgabe in Sinnzeilen, die CLARK im Anschluss an den Cantabrigiensis S. 2—171 bietet, den Tatbestand nicht objektiv vorführt, sondern so färbt, dass er der These dienstbar wird. BLASS hatte in seiner, übrigens an manchen Stellen der Verbesserung bedürftigen ed. Romana (1896) die Abweichungen des Z-Textes sämtlich im Druck hervortreten lassen, hatte selbst ein Minus dieser Fassung gegenüber dem anderen Text durch ein Zeichen angemerkt. Auch CLARK sagt Pref. p. XI: „My chief object is to put before the eyes of the reader the extra matter furnished by *D* and its allies. All such additions, apart from a few monosyllables, are printed in blacker type”. Aber es handelt sich hier keineswegs allein um ein gelegentliches Plus, sondern auch um ein Minus und um vielfach ganz

anders geformten Text. So wäre z. B. in 10, 48 natürlich auch *παρεκαλεσαν* (statt *ηρωτησαν*) und *διαμειναι* (statt *επιμειναι*) herauszuheben. Ferner wäre der Stichos *ημερας τινας ακουστον δε εγενετο* genau in der gleichen ganz unmöglichen Abmessung zu bringen, wie die Hs. ihn bietet, und endlich müsste im Druck herausgearbeitet werden, dass das Stück: *ακουστον δε εγενετο τοις αποστολοις και τοις αδελφοις οι (!) εν τη ιουδαια* Sondergut der Z-Form ist. So würde in zahlreichen Fällen das Bild wesentlich anders erscheinen und für die These CLARK's weniger werben.

Aufmerksames Studium verdienen die Darlegungen S. 273—335: „Witnesses of the Z-Text“ und die „Notes“ S. 336—388. Ebenfalls der Anhang (S. 389—408) mit interessanten lexikalischen Untersuchungen über Lukasevangelium und Acta, wenngleich mir die These, der Verfasser des 3. Evangeliums sei ein anderer als jener der Apostelgeschichte nicht als diskutabel erscheint.

Bonn

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