The Collected Biblical Writings of T.C. Skeat

Introduced and edited by
J.K. Elliott

SUPPLEMENTS TO NOVUM TESTAMENTUM [113]

BRILL

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NOVUM TESTAMENTUM

VOLUME CXIII

THE COLLECTED BIBLICAL WRITINGS OF T.C. SKEAT

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J.K. ELLIOTT

TUTA SUB AEGIDE PALLAS · 1683 ·

BRILL
LEIDEN · BOSTON
2004

This book is printed on acid-free paper.

Library of Congress Cataloging-in-Publication Data

Skeat, T.C. (Theodore Cressy)
[Works. 2004]
The collected biblical writings of T.C. Skeat / introduced and edited by J.K. Elliott.
p. cm. — (Supplements to Novum Testamentum, ISSN 0167-9732 ; v. 113)
Includes bibliographical references and index.
ISBN 90-04-13920-6 (alk. paper)
1. Bible. N.T.—Manuscripts, Greek. 2. Bible. N.T.—Criticism, Textual. 3. Codicology.
1. Elliott, J. K. (James Keith) II. Title. III. Series.

BS1939.S54 2004
225.4'86—dc22

2004045680

ISSN 0167-9732
ISBN 90 04 13920 6

PRINTED IN THE NETHERLANDS

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PREFACE

This collection of essays by Theodore Skeat on matters related to early Christian writing and writings is intended to show the enduring significance of his work for Biblical studies. The contributions he made were thoughtful, well-considered and thoroughly researched. His meticulous scholarship is in evidence throughout; his shrewd conclusions meant that his voice was heard, and continues to be heard, by later generations interested in codicology and the early history of the Bible. The articles reproduced here were originally published between 1938 and 2001 in a range of different journals and Festschriften. It is to be hoped that this volume will enable his work to be more readily accessible, and to ensure that his richly deserved prominence in this field will be maintained.

Silent corrections have been made where necessary but otherwise the chapters that follow are as Skeat published them. Bibliographical references remain as he gave them. Biblical references have been standardised for consistency. Cross-references to essays found elsewhere in this volume are given *ad loc.*

Permission to reproduce the articles here has been readily granted by Oxford University Press, Cambridge University Press, Koninklijke Brill, the American Society of Papyrologists, and the editors of the *Zeitschrift für Epigraphik und Papyrologie, Aegyptus* and *Hermathena*. Details of the original places and dates of the publications are indicated in the Bibliography of Skeat's writings, following the Introduction. I am most grateful for these permissions.

My thanks are also due to my wife, Carolyn, for her assistance with the preparation of this book; to Stuart Pickering of Sydney for his help with the Bibliography; to Loes Schouten and Regine Reincke of Brill; and to Mr. Jonathan Skeat.

J. K. Elliott
The University of Leeds
January 2004

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INTRODUCTION

Theodore Cressy Skeat (1907–2003) spent his working life as a Librarian at the British Museum (except for a period during the Second World War when he was seconded to the Admiralty) and rose to the position of Keeper, Department of Manuscripts and Egerton Librarian from 1961–72 when he retired.[1]

Shortly after he started his career at the Museum two important acquisitions by the Trustees brought his work to public notice.

The first was the momentous arrival in London of the Biblical Greek Manuscript, Codex Sinaiticus, purchased by public subscriptions and the British government from the Soviet authorities. The manuscript arrived at the Museum at Christmastide 1933 (December 27) and was assigned the catalogue number Add. 43725. It is now on permanent display at the British Library. It was Skeat, who, with H. J. M. Milne and Douglas Cockerell, was given the task of preserving and reading the manuscript. Their collaboration resulted in the publication of the now standard work on the manuscript, *Scribes and Correctors of the Codex Sinaiticus* (London, 1938).[2] Five hundred copies of the book were printed. A certain number were distributed to major subscribers to the national collection for the purchase of the manuscript; the others were sold at 1½ guineas (£1.11.6d). Skeat's diary records that he began work on the manuscript on May 13, 1935 and finished on December 23, 1936. Work on the book started on August 24, 1937 and the last proofs were sent to Oxford University

[1] The bare bones of his biography may of course be seen in *Who's Who* and are summarised in the obituary I contributed to *The Independent* (8th. July 2003): Theodore Cressy Skeat, palaeographer and librarian; born 15 February 1907; Assistant Keeper, Department of Manuscripts, British Museum 1931–48, Deputy Keeper 1948–61, Keeper and Egerton Librarian 1961–72; FBA 1963–80; married 1942 Olive Martin (died 1992; one son); died London 25 June 2003.

[2] Skeat was particularly proud of the very useful 'Table of Concordance' (on pp. 94–112) the idea for which had been his. Among the small errata subsequently marked into his own copy (presented by him to me in 2001) are the following: p. 106 Mark ends at xvi 8; p. 9 read: nos. 107–342 remain unnumbered. . . .; p. 20 Fig. 7: read Fig. 6 (= *OT* 68b l.5); p. 22 note 4 NT 38b col. i (Fig. 7) read OT 46b (Fig. 6).

Press, who printed the volume on behalf of the Trustees of the British Museum, on February 18, 1938. In the year that *Scribes and Correctors* was published Skeat wrote two lengthy articles for the British daily newspaper *The Daily Telegraph and Morning Post* (reproduced in the present collection, chapter B1a and b) which give his first impressions of the manuscript and its importance. [Interestingly, some of the points made by Skeat in those articles re-emerged in another article on Sinaiticus published by Skeat sixty years later (and reproduced below as chapter B7.)]

The other significant publication was the editing in 1935 (with H. Idris Bell) of the newly discovered apocryphal Gospel now known as the Egerton 2 Papyrus. The text was published under the title *Fragments of an Unknown Gospel*. That exciting discovery was given prominence in the press;[3] the *editio princeps* remains the standard work on that text.

Some of Skeat's other books were also co-authored. He collaborated again with Milne in writing the popular and regularly reprinted Museum pamphlet *The Codex Sinaiticus and the Codex Alexandrinus* (although neither author is named in the publication).[4] Much later, in 1983, he co-wrote *The Birth of the Codex* with C. H. Roberts. That is a book referred to in all serious subsequent discussions on the topic. However, Skeat alone was responsible for the editing of a catalogue of the Greek papyri in the British Museum, the editing of the papyri from Panopolis in the Chester Beatty monograph series, and the introduction to a volume of illuminated manuscripts in the British Museum.

A list detailing Skeat's major publications follows this Introduction. In it will be seen that the range of topics covered by Skeat is great. It includes Greek and Egyptian classical and secular writings, Christian texts, as well as English literature and history. Much of this reflects his professional duties as a librarian at a major national archive. Several articles appeared in the *British Museum Journal*. Many other early articles were published in the *Journal of Egyptian Archaeology*. It will be seen also that his publications stretch from 1933 until 2001, that is but two years before his death in his 97th year. A high pro-

[3] *The Times* (23.1.1935) contains photographs of the fragments, a lengthy report (written by Bell) and an editorial leader article.

[4] Skeat alone was responsible for a revised reprint in 1955.

portion of these published writings appeared in his years of retirement and many of those concern matters relevant to the text of the New Testament.

This present volume collects together Skeat's articles on Biblical manuscripts and related matters. Section A, 'Ancient Book Production', includes his papers on matters codicological; Section B, 'New Testament Manuscripts', contains his discussions of individual Biblical manuscripts; Section C, 'Textual Variants', gathers together his papers dealing with text-critical variation in the New Testament.

I maintained a lively and vigorous correspondence with Skeat during his last 30 years. It began with his assisting my proofreading of the drafts of the pages of the critical apparatus to Luke that I was assembling for the International Greek New Testament Project's volumes on Luke (published by Oxford University Press in 1984 and 1987). His eagle eye and prodigious memory were much in evidence as we laboured on this remorseless task.

His involvement with Codex Sinaiticus was with him all his working life. Professor D. C. Parker of Birmingham, my successor as executive editor of the International Greek New Testament Project (now working on the Fourth Gospel), reported to the British committee of IGNTP that Skeat had provided the Project in 2000 with up-to-date material on the correctors of Sinaiticus in John, itemizing every single correction and indicating which hand was responsible.

The centennial issue of the *Journal of Theological Studies* carries an important article by Skeat on the reason behind the production of Codex Vaticanus and Codex Sinaiticus, argued there to have been in fulfilment of the Emperor Constantine's request for fifty Bibles. He concludes that the provenance of these two surviving examples of that request was Caesarea, but that Sinaiticus, because of its format and faults, remained there until the sixth century when it was sent to the newly-founded monastery of St Catherine's, Mount Sinai. Vaticanus was sent to Constantinople. (That article appears in the present volume, chapter B7).

It was as recently as 2000 that he published (in *Novum Testamentum*) a final article on Sinaiticus, entitled appropriately "The Last Chapter in the History of the Codex Sinaiticus" (chapter B8 below). (Unlike some of his articles whose titles bear a question mark, this one, significantly, was seen as a definite final statement.) This article was published over sixty years after his essays on Sinaiticus in *The Daily*

Telegraph and Morning Post. He had long felt that the adverse judgement[5] on Tischendorf's *bona fides* in removing the manuscript from Sinai had overshadowed his real achievement of saving the manuscript from destruction. In a letter to me dated 27.4.1999 Skeat, in anticipation of writing that article for *Novum Testamentum* wrote:

> What, then, happened to the rest of the dump which the monks had collected and had intended to burn? Tischendorf had, before he left Sinai, urged the monks to search for further fragments of Sinaiticus, and it is clear that they did just this, recovering in all further leaves of the Old Testament plus the complete New Testament, the Epistle of Barnabas and the earlier part of Hermas. These the monks attempted to bind. This is the 'second binding' described by Cockerell on pp. 82–3 of *Scribes and Correctors* and, as he showed, a very incompetent job it was—indeed so faulty that it looks as if they abandoned the attempt to complete it by attaching boards. Their efforts did, however, succeed in keeping the surviving leaves together. That this binding was *after* Tischendorf's 1844 visit is proved by the fact that it includes the leaf containing Isaiah 66:12–Jeremiah 1:7 which Tischendorf had been allowed to copy, though he was not permitted to take away. Furthermore, Uspensky claimed to have seen the volume during two visits to Sinai, in 1845 and 1850, and the binding applied by the monks must have been executed between Tischendorf's departure in May 1844 and 1845. This was the same binding which the manuscript possessed when Tischendorf finally found it in 1859 and still possessed when the manuscript reached the British Museum on 27 December 1933 (cf. the plates in *Scribes and Correctors* fig. 1).

A few days later, on 30.4.1999, Skeat wrote another letter to me on this topic:

> Tischendorf found 130[6] leaves put out for burning, of which he was allowed to keep 43 [Codex Friderico-Augustanus, which remains in

[5] Popularized in Ihor Ševčenko, "New Documents on Constantine Tischendorf and the *Codex Sinaiticus*" *Scriptorium* 18 (1964) pp. 55–80 and repeated many times in a range of publications. In Britain, particularly, the views about the way in which the manuscript left St Catherine's are often allied to claims that the Elgin Marbles in the British Museum should be returned to Greece.

[6] 129 appears in the *Novum Testamentum* article. In *Scribes and Correctors*, p. 82 Milne and Skeat correctly state that 129 was merely a guess, albeit a pretty accurate one, based on his estimate that the 43 leaves he had secured represented 'about a third of the whole'. There are 133 leaves extant up to the end of Malachi. (Tischendorf is unlikely to have seen any poetic books. Had he done so he is likely to have noted that these were written in two columns per page.) Then, deducting the two scraps from the Pentateuch (Genesis and Numbers in St Petersburg) and one leaf of Judith, not then available, there were 130 leaves remaining in Sinai. Those were the leaves accessible at the time of Tischendorf's visit in 1844 and (as Milne and Skeat wrote) "[We] can rest assured that after his visit no further destruction took place".

> Leipzig], the monks retaining the remaining 87. After searching through the dump the monks recovered a further 112 leaves of the OT making a total for the OT of 199. They also recovered the complete NT, followed by Barnabas and the beginning of Hermas, in 148 leaves, a grand total of 199 + 148 = 347, and 347 is the number of folios which Sinaiticus in London holds today.

But it was not only Sinaiticus that Skeat worked on. Inevitably, he wrote on that other great and early Greek Bible in the British Museum Library, namely Codex Alexandrinus (Royal 1 D. VIII). His essay (chapter B2 below) discusses the provenance of that manuscript.

Not surprisingly, Skeat was drawn into studying the third great Greek codex extant, namely Vaticanus (Bib. Vat. gr. 1209). Chapter B3 is his study of the later history of Vaticanus. In "The Codex Vaticanus in the Fifteenth Century" Skeat argued how the manuscript ended up in Italy. He claims that it was presented by the Greek delegation to the Reunion Council of Ferrara-Florence of 1438–9. Further to his article we may refer to Ihor Ševčenko on the presentation of manuscripts by the Greeks: "Intellectual Repercussions of the Council of Florence" *Church History* 24 (1955), pp. 291–323.

Skeat's advice to Paul Canart, then Vice-Prefect of the Vatican Library, is acknowledged in the first section of the Prolegomena to the splendid facsimile edition of Codex Vaticanus published in 1999. In "Le Vaticanus Graecus 1209" Canart in his unsigned introduction to the Prolegomena finds Skeat's views on the manuscript in the fifteenth century 'séduisante'. More substantially, Canart in an article in *L'Osservatore Romano*[7] deals sympathetically with Skeat's *Journal of Theological Studies* article on Vaticanus, Sinaiticus and Constantine, (reprinted here as chapter B7). The *JTS* article had been long in the writing and it had been through several drafts before it appeared eventually, but happily in the centennial edition of *JTS*. This was a wise choice for it was in *JTS* that five of Skeat's earlier pieces had appeared—all of them are now reprinted in the present collection.

Skeat was disappointed that the Prolegomena to the facsimile of Vaticanus did not reflect more strongly the opinions expressed in the *JTS* article, as these were views which he had shared with Canart for several years before the publication of either. He was surprised that in the introduction to the 1999 facsimile of Codex Vaticanus

[7] *Sunday, February 27, 2000, p. 3.*

the Vatican had not drawn attention to his point that their great Biblical treasure was one of the Bibles commissioned by Constantine!

One area where he continued to maintain contact with Paul Canart was over the identification of the writer of the cursive additions made to Vaticanus. (This had been something which he had originally raised in his article "The Codex Vaticanus in the Fifteenth Century", (chapter B3.)) Skeat lived in the hope that the name of a Constantinopolitan scribe would emerge and thus prove his case that Vaticanus had reached Italy from Constantinople. Work on this question continues but so far has not yielded the name of an identifiable scribe.

Skeat's *JTS* article of 1999 dealt with the provenance of Sinaiticus, which he argued was Caesarea. He then turned to the question of the provenance of Vaticanus. and claimed, on the basis of, *inter alia*, shared colophon designs found in both codices, that both must have come from the same scriptorium. His final letter to me (written, as most were, on pages from a conventional writing block) was posted on June 13, 2003, less than a fortnight before his death. The topic he wrote about concerned the colophons in Codex Sinaiticus, a subject that he had first become interested in nearly sixty years earlier; the letter was accompanied by a separate sheet of diagrams and calculations. He realised that his case, unassailable as he was convinced it was, would have been even more persuasive had he provided further examples of the common colophon designs. Nearly all the colophons up to Ruth in Vaticanus are of the same type as he had shown in the 1999 article. As well as the example he had given from Mark, he was minded to publish comparable examples of the same design found at the end of Tobit, Judith and 1 Thessalonians in Sinaiticus. Perhaps Skeat's final wish to demonstrate even more fully the shared scribal features within the two codices should be satisfied with further investigations on this topic by a competent scholar.

Following the Vatican's publication of the magnificent new facsimile of Codex Vaticanus to mark the new Christian millennium, the University of Geneva hosted a conference on the manuscript at the Fondation Hardt in Geneva in June 2001. Inevitably, Skeat was invited to address the colloquium. His advanced age and increasing deafness made it impossible for him to attend but he asked the organisers to invite me in his stead. As a consequence, I gave a paper on Skeat's contribution to the question about the writing and provenance of Vaticanus, and for it I drew extensively on Skeat's *JTS* article of

1999. (My paper was given in French,[8] but an English version appears in the present volume as Appendix C below). The published proceedings of that conference are to include, alongside the papers delivered in Geneva, a reproduction of the volume of Introduction that had accompanied the Vatican's facsimile edition. Skeat was very disappointed that the opportunity was not taken at that stage to revise the original opinions and to include an informed decision on his own work. Instead all that was to be printed again was a concluding footnote to the first section (by Paul Canart) merely drawing attention, almost as an afterthought, to Skeat's *JTS* article of 1999.

Among other manuscripts that Skeat wrote on was the Chester Beatty manuscript of the four Gospels and Acts, Biblical Papyrus I, known as P^{45} in the Gregory-Aland register, and considered to have been written in the mid-third century. Skeat's original article was split into two. That was because he felt, with typical modesty, that his "lucubrations on the structure of the codex"[9] should not be included alongside the work he had shared on the manuscript with Brian McGing. Both articles appeared in the Dublin journal *Hermathena*, and are reproduced below as chapters B4 and B5. The first (co-written with McGing), the more 'objective' description of the manuscript, appeared in 1991 and contained a tentative reconstruction of the possible placement of the John fragments; Skeat's stimulating codicological analysis came out two years later. In the latter he calculated (a) where the beginning of Matthew would have come in the codex and how it ended on p. 49 followed by the colophon, and (b) that, because of the way in which the codex was made up—at least in the Gospels (with quires of two leaves organized in such a manner that the side of the papyrus on which the fibres were horizontal formed the inside pages, while the other side where the fibres were vertical formed the outside)—John accordingly began at the top of p. 50, as neither Mark nor Luke fitted in with that layout. In other words this manuscript has the Gospels in the so-called Western sequence, Matthew-John-Luke-Mark.

In his 1993 piece in *Hermathena* Skeat displays the mathematically precise calculations that are to be seen again in his later article "The

[8] In that form it will appear in the proceedings of that conference: P. Andrist (ed.), *Le manuscrit B de la Bible (Vaticanus gr. 1209): Actes du colloque de Genève* (Lausanne, 2004).

[9] As he described his paper in a letter to me (1.5.1991).

Oldest Manuscript of the Gospels?" (dealing with P^4, P^{64} and P^{67}) which was published in *New Testament Studies* in 1997 (B6 below). In this he wisely refrained from commenting on the now discredited and universally dismissed view of Peter Carston Thiede that P^{64} was written in the first century. That opinion was being widely canvassed in the popular press in the period when Skeat was preparing his article, but he decided to avoid commenting on what he considered to be not only wrong and unconvincing but also irrelevant to his own arguments. Skeat's careful detective work has persuaded many New Testament scholars of his conclusions,[10] although the official register of manuscripts[11] has not yet agreed to show that these three fragmentary manuscripts were in fact originally from the same codex.[12] Skeat's *NTS* piece is possibly his most important and stimulating on the history of the codex.[13]

But Skeat was not just a librarian, interested in the finished products. He was very knowledgeable about codicology, papyrology and palaeography—in short, with ancient book production in general. The articles in Section A treat of these matters. In some cases (e.g. chapters A3, A5) Skeat addresses some fundamental and interesting questions, such as the use of adhesive in making a papyrus roll; rerolling a roll; the practicalities of handling a scroll. He was also interested in the cost of book production, as can be seen in his article on the cheapness of papyrus as a writing material or the cost advantage of the codex (chapters A5 and A8). Such papers help correct the misinformed views on such matters by showing that, contrary to what some non-experts were suggesting, papyrus was a relatively inexpensive writing material and one that was serviceable

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[10] Most recently and conspicuously Graham Stanton in his *Gospel Truth?* (London, revised ed. 1997) esp. pp. 16ff., where Stanton acknowledges that Skeat had convinced him.

[11] Kurt Aland, *Kurzgefasste Liste der griechischen Handschriften* (Berlin and New York, [2]1994) (= *ANTF* 1).

[12] K. Aland, the onetime registrar of the list, expressed his doubts in 1966 in his "Neutestamentliche Papyri" *NTS* 12 pp. 193–5 and again in Kurt Aland & Barbara Aland, *The Text of the New Testament* (Grand Rapids and Leiden, [2]1989) pp. 96–100.

[13] Among others who claim to have benefited from Skeat's study is David Runia. In his work on the ms. of Philo, in the binding of which were found the scraps of New Testament now known as P^4, P^{64} and P^{67}, he acknowledges Skeat's work. See David T. Runia, "One of Us or One of Them? Christian Reception of Philo the Jew in Egypt" in James L. Kugel (ed.), *Shem in the Tents of Japhet: Essays on the Encounter of Judaism and Hellenism* (Leiden: Brill, 2002) pp. 203–222 (= *Supplements to the Journal for the Study of Judaism* 74).

and durable. For instance, he picked me up on a query I had raised in a book review. I had asked the rhetorical question "Why did Paul go to so much expense to write his long letter to the church in Rome on costly papyrus?" In a letter (16.5.1991) Skeat reminded me of the state of affairs:

> In the first place it is inconceivable that the Epistle was written on anything other than papyrus. Papyrus was the universal writing material, freely available throughout the Graeco-Roman world, whereas parchment, except in the form of parchment note-books, was little used, at any rate in this period, and although we have absolutely no information about the relative costs of papyrus and parchment, it is very unlikely on general grounds that parchment would have been noticeably cheaper. The myth of the costliness of papyrus is curiously persistent, and it is difficult to see why. It is quite modern, and all started, I believe, with an article by the French historian, Gustave Glotz, in the 1930s, in which (discussing, I think, the building accounts of the Erechtheum) he claims that entries in accounts for 'papyrus' meant a single *sheet* of papyrus. Since the normal length of the commercially produced papyrus roll was 20 sheets joined together, this gave a very high figure for a complete roll. I tried to combat that view in my chapter in the *Cambridge History of the Bible*[14] pointing out that the lavish, and even wasteful, way in which papyrus was often used (e.g. the enormous margins in the Chester Beatty Numbers-Deuteronomy codex) proved that papyrus cannot have been expensive. Furthermore, although the verso (the side with the vertical fibres) could easily be used for writing, as became evident when the codex was introduced, very little use was made of this in practice, any more than the re-use of the recto by wiping off the ink, which is easily done with a sponge. In fact, if papyrus had been expensive, as has been claimed, I am sure a huge recycling industry would have sprung up!
>
> In a very dry article I published in 1982[15] I claimed that Pliny was correct in stating that the (normal) roll was made up of 20 sheets, and that the average price of this was somewhere about 2 dr. I also estimated that the average length of this roll was 340 cm.
>
> In P^{46} Romans occupies 40 columns of writing. The width of the column varies between 11–13 cm. (due to the fact that it is a single-quire codex). If we take 12 cm. as average, and add 2 cm. for space between columns, Romans, written in roll form, would have needed $40 \times 14 = 560$ cm. length of papyrus. If the standard roll of 340 cm. cost 2dr., 560 cm. would have cost $560/340 \times 2$dr. = 3.3dr., (say, 3dr. 2ob.)—not an enormous amount of money. These calculations are of course very rough but they cannot be very far out.

[14] Edited by G. W. H. Lampe (Cambridge, 1969). (Reproduced here, chapter A2.)
[15] Chapter A4 in the present volume.

> I presume that Tertius, who wrote out the epistle, either from dictation or from Paul's rough draft, would not have charged for his services. Indeed as a Christian and as a disciple of Paul it very likely that he would have supplied the papyrus as well, so the cost to Paul would have been *nil*.
>
> Naphtali Lewis, in his magisterial study *Papyrus in Classical Antiquity*, did indeed give full coverage to my views on the relative cheapness of papyrus, but unfortunately did not formally endorse them, so echoes of this absurd theory remain and surface from time to time—of course I mean no reflection on yourself. But I thought you would like to know the facts.

I have since been careful never to broadcast again this 'absurd theory' that papyrus was expensive for the early Christians!

The important and influential essay in the second volume of *The Cambridge History of the Bible* referred to in the letter concerns early Christian book production. That magisterial essay (A2 here) is regularly to be found cited by later scholars. The essay deals with the following topics in this order: the Prehistory of the Christian Book, Papyrus and Parchment, the Origin of the Codex, Christianity and the Codex, and the Supremacy of the Parchment Codex (to take its sub-headings). Here he brought to completion his researches into the topics up to the time that the article was submitted to the editor in the 1960s. How his views developed after this may be seen in other articles in this section (e.g. A4, A5, A7, A8).

Perhaps Skeat's most enduring article in Section A is his "The Use of Dictation in Ancient Book Production" (A1). Among those who have engaged with Skeat on this topic are Klaus Junack (in the 1981 Festschrift for Bruce Metzger),[16] Harry Gamble[17] and, more recently, Kim Haines-Eitzen[18] The writer I associate most with this topic is Günther Zuntz. In his monograph on Lucian of Antioch[19] he accepts Skeat's conclusions on the use of dictation, and also agrees with Skeat's conclusions on the provenance of Vaticanus and Sinaiticus.

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[16] "Abschreibpraktiken und Schreibergewohnheiten in ihrer Auswirkung auf die Textüberlieferung" in Eldon Jay Epp and Gordon D. Fee, *New Testament Textual Criticism: Its Significance for Exegesis* (Oxford, 1981) pp. 277–95.

[17] Harry Y. Gamble, *Books and Readers in the Early Church: A History of Early Christian Texts* (New Haven and London, 1995).

[18] Kim Haines-Eitzen, *Guardians of Letters: Literacy, Power, and the Transmitters of Early Christian Literature* (New York, 2000).

[19] Günther Zuntz, in Barbara Aland and Klaus Wachtel (eds.), *Lukian von Antioch und der Text der Evangelien* (Heidelberg, 1995) esp. pp. 36–40 (*Abhandlungen der Heidelberger Akademie der Wissenschaften, Philosophisch-historische Klasse*, 1995, 2).

The article "The Origin of the Christian Codex" (chapter A7), written in 1994 is a theme he regularly returned to. This piece, published in the *Zeitschrift für Papyrologie und Epigraphik*, is, like many of Skeat's studies, painstakingly argued with that mixture of reticence (so typical of the modest man that Skeat was) and firm convictions which his reasoning led him to. In it he argues that the Christian leaders adopted the codex initially to promote the fourfold Gospel canon, concluding that "The Four-Gospel Canon and the Four-Gospel Codex are inseparable" However, this was merely one way in which he broached the topic. Perhaps rather uncharacteristically he allowed himself the indulgence of a somewhat whimsical dramatization of the events leading up to the decision of the church leaders in the second century to adopt a fourfold Gospel codex. This one-act play appears as Appendix A. When he sent a copy of this to me he wrote in a covering letter: "It is of course totally unpublishable, but I thought it might provide you with some amusement". However no less an authority on the canon than B. M. Metzger, to whom he had also sent a copy of his playlet, replied: "I do hope that the form in which you persuasively sketched the dramatic considerations that must have taken place in the early church will also appear in print. . . . Your highly plausible reconstruction of the debates that may (or must) have taken place in the early church will be useful to readers who do not have access to technical journals" (Letter to Skeat from Metzger dated 3.3.1994).

Another comparable piece of whimsy was Skeat's imaginative description of what could have happened when Constantine received the fifty great Bibles he had commissioned from Eusebius in 330 A.D. That writing was read out (in a French translation) as Skeat's only direct contribution to the Geneva conference of 2001 on Codex Vaticanus. I include the English original here as Appendix B.

Arising from Skeat's lifelong involvement with texts containing Christian scriptures was his inevitable alertness to textual variation. The seven essays in Section C deal with text-critical issues at Matt. 6:28; Mark 3:20–21; 7:3; 16:8; Luke 6:1; Phil. 1:1; 2 Tim. 4:13. He had a great curiosity in the often bizarre readings commonly to be found in edited Greek New Testaments. His piece on πυγμῇ at Mark 7:3 (chapter C3) is an obvious example. Similarly the strange reading δευτερόπρωτῳ at Luke 6:1 caught his attention. There he argued that this was a nonsense word ('ghost-word' was his preferred description) that had arisen on palaeographical grounds from a dittography of βατω (from the end of σαββάτῳ), which was later read as a numeral (β′α′) followed by the definite article to relate the adjective to σαββάτῳ. A differing view to this appeared (Jean Bernardi, "Des chiffres et des lettres: Le texte de Luc 6,1" *Revue Biblique* 101 (1994) pp. 62–6) in which it was argued that the letters βα were wrongly taken to be numbers when they were actually intended to be a Greek transliteration of the Hebrew verb 'to go', which had been inserted into the manuscript by a scribe translating διαπορεύεσθαι in the verse. After I had drawn Skeat's attention to this article, his response in a letter to me dated 16.1.1994 was characteristically scathing:

> What a delightful surprise! Well, we live and learn. So, an "amateur Hebraist" inserted a marginal gloss in Hebrew in Greek letters, and that this somehow found its way into the text, and because it made no sense as such, was transformed into the non-existent Greek word δευτερόπρωτῳ! But how can anyone write—or editors agree to print—such nonsense? It is very puzzling.

It is noticeable that the title of Skeat's own article on the word in 1988 (chapter C5) had claimed that it was 'the final solution'.

Surprisingly, it is his first article on a text-critical variant, "The Lilies of the Field" in 1937 (chapter C1) that has had the greatest impact on recent scholarship. This is due to James Robinson's acceptance of Skeat's conclusions about the original reading of Sinaiticus at Matt. 6:28b and the text of P. Oxy. 655 to support his reconstruction of Q at this point (in James M. Robinson, Paul Hoffmann and John S. Kloppenborg (eds.) *The Critical Edition of Q*).[20] This book gives great prominence to Skeat's arguments (e.g. pp. xciv–ci). Also both sets of endpapers to the book show the photograph of Matt. 6:26–31 in Sinaiticus, originally taken by Skeat using ultraviolet light. Skeat's permission for reproducing this photograph is credited in the book. Skeat also helped with a new reconstruction of P. Oxy. 655. His consultation with Harold Attridge is acknowledged (*Critical Edition* p. xviii); they improve the transcription and translation of the Gospel of Thomas 36.3 in P. Oxy. 655, 10–13 at Q 12:27, 25: κ[αὶ]/ ἕν ἔχοντ[ες ἔ]νδ[υ-]/μα, τί ἐν[. . . . . .]. . αι ὑμεῖς; 'And] having *one* clothing, . . . you . . .?' Robinson has acknowledged and reported on Skeat's work in several articles including, among others, those published in 1998, 1999 and 2002.[21]

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[20] (Minneapolis: Fortress Press, and Leuven: Peeters, 2000) (= *Hermeneia-A Critical and Historical Commentary on the Bible: Supplements*).

[21] (with Christoph Heil) "Zeugnisse eines schriftlichen, griechischen vorkanoni-

As a further indication of the longevity of some of Skeat's writing I noted that an article by Vern Sheridan Poythress in the 2002 volume of *JTS* picked up on Skeat's piece on μάλιστα, "The Meaning of μάλιστα in 2 Timothy 4:13 and Related Verses" (here chapter C7). In it the author refers to Skeat's preferred translation of μάλιστα as meaning 'that is' or 'namely' but finds Skeat's argument and examples unconvincing. The bulk of Poythress' piece is given over to examples that confirm the traditional meaning, 'especially'. When I drew Skeat's attention to the article, he wrote to me as follows (on 3.11.2002):

> I am afraid that Mr. Poythress has completely failed to convince me that my own article was erroneous. It is perfectly true that normally μάλιστα means something like 'mostly' or 'especially'. But, as I tried to show, it can on occasion be used as a means of explaining or clarifying something which has been stated in general terms.
>
> I think the best example of this is 1 Tim. 4:10 ὅς ἐστιν σωτὴρ πάντων ἀνθρώπων, μάλιστα πιστῶν, which is translated in the AV as 'Saviour of all men, specially of those that believe'. RV is virtually identical. [In English 'especially' means 'in the great majority of cases' thereby implying that there is a minority, which in this context are 'non-believers'.] Both these versions suggest that at least some of those saved are not believers, which is nonsense. The Revised English Bible seems to have realised that there is a problem, but its own rendering 'The Saviour of all—the Saviour, above all, of believers' is really no better, because all these imply that there are at least some saved who are not believers.
>
> Of course on many occasions μάλιστα does mean 'especially' but in the cases I discussed it could best be translated as 'i.e.' or 'viz'. There is nothing very revolutionary about this for I have a very ancient Liddell and Scott printed in 1883, in which the question τί μάλιστα; is translated as 'what exactly?'. That is why in my article I suggested the translation ". . . God who gives salvation to all men—that is to say, to all who believe in him". Poythress has very little to say about this passage—perhaps because he realised that the traditional rendering involved difficulties.
>
> It might be useful to quote a non-Biblical example, and I have one from Strabo, *Geography* (vii.3.2), who is discussing the position of the Mysians during the Homeric age. (In the classical period, of course,

schen Textes Mt 6.28b א*, P. Oxy 655 I 1–17 (EvTh 36) und Q 12,27" *ZNW* 84 (1998) pp. 30–44; "A Written Greek Sayings Cluster Older than Q: A Vestige" *HTR* 92 (1999) pp. 61–77; and (with Christoph Heil) "P. Oxy. 655 und Q. Zum Diskussions-Beitrag von Stanley E. Porter" in Hans-Gebhard Bethge *et al.* (eds.), *For the Children, Perfect Instruction: Studies in Honor of Hans-Martin Schenke* (Leiden: Brill, 2002) pp. 411–23 [This discusses Stanley E. Porter, "P. Oxy. 655 and James Robinson's Proposals for Q: Brief Points of Clarification" *JTS* 52 (2001) pp. 84–92.]

> they were located on the south side of the Sea of Marmara.) Posidonius, using a quotation from the Iliad, had concluded that in the Homeric age they had been settled in Europe, and had crossed over to Asia only later. He quoted Iliad xiv.5 as evidence for this. This passage depicts Zeus as seated on Mount Ida and looking out over the Trojan plain and beyond that to the sea, and Europe: αὐτὸς δὲ πάλιν τρέπειν ὄσσε φαεινώ/ νόσφιν ἐφ΄ ἱπποπόλων Θρῃκῶν καθορῶμενος αἶαν/ Μυσῶν τ΄ ἀγχεμάχων on which Posidonius observed τὸ γὰρ ῾πάλιν τρέπειν᾿ μάλιστα μέν ἐστιν εἰς τοὐπίσω. ὁ δ΄ἀπο τῶν Τρώων μεταφέρων τὴν ὄψιν ἐπὶ τοὺς ἢ ὄπιθεν αὐτῶν ἢ ἐκ πλαγίων ὄντας, προσωτέρω μὲν μεταφέρει, εἰς τοὐπίσω δ΄οὐ πανύ where obviously μάλιστα does not mean 'especially' but gives a definition. At the end οὐ πανύ conveys, as it sometimes does, a note of sarcasm (προσωτέρω 'forwards' can hardly mean 'backwards'). This is a very good example of μάλιστα meaning something like 'to be exact' or 'to be precise'.

A week later on 10.11.2002 Skeat wrote adding that

> One of the few commentators who I know has accepted my proposals on this verse is A. T. Hanson, *The Pastoral Epistles* (*New Century Bible Commentary*, Grand Rapids, 1982, p. 92) when he discusses 1 Timothy 4:10: "Commentators make valiant efforts to save this sentiment from the obvious charge of appalling ineptitude. In fact, however, a recent suggestion by T. C. Skeat . . . absolves the author from this charge."

Skeat also drew my attention to the correspondence he had had in 1978 with G. B. Caird, then editor of *JTS* and a scholar actively interested in lexicography. Photocopies of letters between Skeat and Caird were sent to me by Skeat. Skeat was then preparing the article which was to appear in *JTS* the following year. Caird had raised with Skeat whether he had considered the attenuated sense of μάλιστα in modern Greek, where, like βέβαια, it had become a synonym for ναί 'sure' 'certainly' 'of course'. [We may also add φυσικά 'naturally'.] Skeat noted that this conversional use was a meaning as old as Aristophanes, and is also in Plato (Gorgias 448D and Meno 80b).

That last point was well made although Poythress' article has drawn attention to some weaknesses in some of Skeat's other examples, and the article needs to be consulted alongside Skeat's piece of twenty-three years earlier. One point that Poythress had made was that Skeat had not discussed μάλιστα at 1 Tim. 5:17 in the 1979 article. That was unfortunate because Skeat had already realised that he had inadvertently overlooked this passage which was one he believed to be an excellent further example in his favour. He was convinced that here too μάλιστα had the same significance as in the other passages he had quoted, namely to identify the meaning of something just written and not to evoke an element of selection. He decided to write this up as a short note for *Expository Times* but, unfortunately, it was never accepted for publication. This is part of what Skeat had submitted:

> At 1 Tim. 5:17 the Revised Standard English Version translates as follows: 'Elders who give good service as leaders should be reckoned worthy of a double stipend, in particular (*malista*) those who work hard preaching and teaching.' I suggest that the real meaning is as follows: 'Elders who give good service as leaders should be reckoned worthy of a double stipend, that is to say those who work hard at both preaching *and* teaching.' In other words, double pay for a double job. The writer of the letter, having said that outstanding leaders deserved double pay realised that this could be a subjective assessment leading to arguments and recrimination, and therefore went on to suggest an objective test by which performance could be measured.[22]

Among other matters pursued by Skeat are two issues in which he assisted recent researchers. One concerns the *nomina sacra*. In 2001 C. M. Tuckett published a piece[23] on the reconstruction of the Rylands John fragment in which he argued that it could well have had the name of Jesus written in full on both occasions where it occurs in the verses found in the fragment, even though the fragment is deficient in the very places where the name itself would have occurred. Skeat wrote to me on 1.1.2002 to say that the name of Jesus was in his experience *never* written in full in New Testament manuscripts.

Larry Hurtado reacted to that article[24] and showed the importance of taking account of all scribal features of manuscripts in attempting to establish probabilities for lacunae. In preparing for that article Hurtado sought advice from Skeat, and his help is duly acknowledged in the final footnote (note 39). Independently of his

[22] Cf. A. T. Hanson (*Commentary op. cit.* p. 101) who had appropriated Skeat's translation of μάλιστα in 4:10 for use at 1 Tim. 5:17: 'Presbyters who preside well should be rewarded, I mean those who labour in preaching and teaching'. This has the advantage that it avoids any suggestion of two groups, a larger one who merely presided, and a smaller who preached and taught.'

[23] C. M. Tuckett, "P^{52} and *Nomina Sacra*" *NTS* 47 (2001) pp. 544–8. See also C. E. Hill, "Did the Scribe of P^{52} use the Nomina Sacra? Another Look" *NTS* 49 (2002) pp. 587–92.

[24] L. W. Hurtado, "P^{52} (P. Rylands Gk. 457) and the Nomina Sacra: Method and Probability" *Tyndale Bulletin* 54 (2003) pp. 1–14.

advice to Hurtado (see Hurtado footnote 38), Skeat also sent me his calculations on the reconstruction of P[52] in a letter dated 6.1.2002, with a later *post scriptum*:

> I enclose[25] the result of my attempt to verify Tuckett's suggestion by calculating the length of the line in P[52] (a) if the *nomina sacra* were contracted and (b) if the *nomina sacra* were written out in full, as proposed by Tuckett, and, as you will see, the result is inconclusive.
>
> My method was very crude, using nothing more than a ruler and a pair of dividers. I first built up an alphabet, for each letter the width of the letter itself plus half the width between it and the preceding letter plus half the width between it and the following letter. Using this alphabet, I calculated the length of the *extant* portions of the lines, and as you will see these agreed very closely with the actual measurements, suggesting that my alphabet was reasonably accurate.
>
> Despite this, I do not see how Tuckett's suggestions can possibly be accepted. If they were, they would be the *only* known NT manuscripts in which the name was written out in full, and the fact that these are *both* in lacunae does strain incredulity.

As an afterthought Skeat wrote to me on 6.21.2001:

> The very earliest known *nomen sacrum* is in the Epistle of Barnabas which is usually dated late first century (cf. Colin Roberts, *Manuscript, Society and Belief*, p. 35) and this is in the form IH. I do not think there is any case of IHΣ which can be dated earlier than the 3rd century. It would thus be quite extraordinary to find in P[52] the name written out in full.[26]

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[25] What was enclosed was the following, with measurements in millimetres:

Line	Extant (actual length in square brackets)	Lost		Total
1	49.6 [49.0]	92.14	=	141.74
2	50.06 [50.0]	86.4	=	136.46
3	45.78 [45.0]	103.28	=	149.06
4	40.9 [40.0]	98.12	=	139.02
5	24.44 [25.0]	99.4	=	123.84
6	24.0 [24.0]	108.0	=	132.0

If in line 2 Ἰησου were written instead of $\overline{\mathrm{IY}}$ the total would be increased from 136.46 to 149.28, only fractionally longer than line 3. If in line 5 Ἰησουν had been written instead of $\overline{\mathrm{IN}}$ the total would have been increased from 123.84 to 141.66, still shorter than line 3.

[26] The whole question of the *nomina sacra* has had a new lease of life thanks to Tuckett. Skeat's responses to Hurtado and me settle Tuckett's suggestions but the whole issue deserves investigation especially when we can see how πατῆρ, for instance, was sometimes abbreviated in manuscripts even where the meaning 'God' is not intended. (See Matt. 2:22 in ℵ C K L M N.) See also Matt. 27:16 where Jesus (Barabbas) is abbreviated (to $\overline{\mathrm{IN}}$) by Θ and 700 or Matt. 27:17 where, again, Jesus (Barabbas) is abbreviated (to $\overline{\mathrm{IN}}$) by Θ 700 579 or Luke 11:24 where P[45] contracts πνεῦμα even though it is the *ἀκάθαρτον* πνεῦμα!

Another example of Skeat's being more than willing to assist enquirers concerns what is meant when Luke reports at 4:17 that Jesus ἀναπτύξας τὸ βιβλίον read from a text in Isaiah. Much depends on the meaning of the verb ἀναπτύξας in v. 17 and πτύξας in v. 20. In 4:17 note *v.l.* ἀνοίξας for ἀναπτύξας. Does the text imply that Jesus opened and read from a codex or did he unroll a scroll and read from that? This question was precisely up Skeat's street, and, although he did not publish on that passage, his help in advising Roger Bagnall who was tackling the meaning of the words is acknowledged in footnote 1 of, and elsewhere throughout, Bagnall's short note "Jesus Reads a Book".[27] Bagnall concluded his article by saying ἀναπτύξας means 'separating two surfaces' and cannot therefore refer to the unrolling of a roll, but must mean the turning of pages of a codex. In a letter to me dated 14.11.2001 Skeat disagrees with Bagnall stating that "I don't see why Luke should not have used πτύξας for bringing the two parts of the roll together just as ἀνοίξας had been used for drawing them apart. And in any case Luke must have been aware that the Jews did not use codices in their synagogues at any time". He also pointed out (letter 17.12.2000) that there are no examples of codices being used in the first century except for the poems of Martial. A day earlier (16.12.2000) Skeat had written "Personally I find Bagnall's attempt to differentiate 'unrolling' and 'unfolding' too fine-spun".

Among some of the papers that Skeat sent to me is his draft of the correspondence he had had with Bagnall in 1998. In one letter Skeat had written to Bagnall: "The real difficulty, as I see it, is that we don't really know whether Luke wrote ἀνοίξας or ἀναπτύξας—both have good authority, and although ἀνοίξας *might* be the commonplace word ousting the more recherché ἀναπτύξας, it might equally be the other way round, with ἀναπτύξας replacing ἀνοίξας to fit in with πτύξας in v. 20. How unfortunate that P^{75} is defective here!" [In his article Bagnall is attracted to the originality of ἀνοίξας (p. 588)].[28]

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27 *JTS* 51 (2000) pp. 577–88.

28 My own interest in the meaning surfaced when I was preparing *The Apocryphal New Testament* (Oxford, 1993). In the Acts of Peter 20 Peter 'rolls up' (*involvens*) the gospel, implying rolls were still in use in the second-century Christian community that produced the Acts of Peter. But perhaps as with the Eusebius' Letter to Carpianus the 'old fashioned' term ἀναπτύξας which Luke may have used with the meaning 'to unroll' was anachronistically used of a codex. That is perhaps comparable to modern English uses of words like 'carriageway' (for modern motorways) 'to dial' (for push button telephones) and 'to sail' (for any sea voyage).

This survey of Skeat's work on the New Testament reveals but one aspect of his academic research. The Bibliography of his literary output that follows this Introduction shows the wider range of his learning, not least his long-lasting interest in ancient calendars. He was also alert to studies in Classical and in English literature. But it was his abiding fascination with the Christian origins of the codex, with the major Biblical uncial manuscripts and with the deciphering of fragmentary New Testament texts that spurred him to write and to share with readers his insights and queries. The present collection of articles demonstrates his skills and expertise in conveying those enthusiasms, and is published now to perpetuate his memory and his scholarship.

J. K. E.

BIBLIOGRAPHY OF T. C. SKEAT'S PUBLICATIONS

[Titles preceded by an asterisk appear in the present volume.]

Heurtley, W. A., and T. C. Skeat, 'The Tholos Tombs of Marmáriane', *Annual of the British School at Athens* 31 (1930–31), 1–55, plates 1–11, 20 figures.

Roberts, C. H., and T. C. Skeat, 'A Sale of ΥΠΟΛΟΓΟΣ at Tebtunis in the Reign of Domitian', *Aegyptus* 13 (1933), 455–471 (including 1 folding plate, opposite p. 456). [Edition of B.M. Pap. 1876.]

Skeat, T. C., 'ΤΕΤΡΑΔΕΡΜΑ', *Classical Review* 47 (1933), 211–213.

Skeat, T. C., 'The Collection of Greek Papyri in the British Museum', in *Papyri und Altertumswissenschaft. Vorträge des 3. Internationalen Papyrologentages in München vom 4. bis 7. September 1933*, ed. W. Otto and L. Wenger (Münchener Beiträge zur Papyrusforschung und antiken Rechtsgeschichte, 19), Munich, C. H. Beck, 1934, pp. 429–435.

Hunt, A. S., T. C. Skeat and J. G. Tait, 'The Greek Ostraka', in *The Bucheum*, ed. R. Mond and O. H. Myers (Egypt Exploration Society, Memoirs, 41), vol. II: *The Inscriptions*, London, Egyptian Exploration Society and Henry Milford, 1934, pp. 75–78.

Skeat, T. C., *The Dorians in Archæology*, London, Alexander Moring, 1934. Pp. iii + 69. 2 plates. ["This study, in an abridged form, was successfully submitted for the Cromer Greek Prize of 1932" (quoted by library catalogue).]

Bell, H. I., and T. C. Skeat (ed.), *Fragments of an Unknown Gospel and other Early Christian Papyri*, London, Trustees of the British Museum, 1935. Pp. x + 63. 5 plates. ISBN 0 714 10438 8.

Bell, H. I., and T. C. Skeat, *The New Gospel Fragments*, London, Trustees of the British Museum, 1935; 2nd ed., 1951; 3rd ed., 1955. Pp. 33, 1 plate. [A more popular work than the edition in *Fragments of an Unknown Gospel and other Early Christian Papyri*. Includes corrections to the text.]

Skeat, T. C., and E. P. Wegener, 'A Trial before the Prefect of Egypt Appius Sabinus, *c.* 250 A.D. (P. Lond. Inv. 2565)', *Journal of Egyptian Archaeology* 21 (1935), 224–247, plate 28 opposite p. 224.

Skeat, T. C., 'A Greek Mathematical Tablet', *Mizraim* 3 (1936), 18–22.

Roberts, C.[H]., T. C. Skeat and A. D. Nock, 'The Gild of Zeus

Hypsistos', *Harvard Theological Review* 29 (1936), 39–88, one folding plate (opp. p. 88). [P. Lond. 2710.]

Skeat, T. C., 'A Forthcoming Catalogue of Nome Strategi', *Mizraim* 3 (1936), 30–35. 'This article was written and set up before I became aware of the imminent publication of M. Henri Henne's Liste des stratèges, which appeared at the beginning of this year. In these circumstances, any early publication of a list such as I have described would be undesirable, though ultimately, when a considerable amount of new material has accumulated, I hope to take up the project once more. T. C. Skeat' (slip pasted to cover [as quoted by library catalogue]).

Skeat, T. C., 'The Reigns of the Ptolemies, with Tables for Converting Egyptian Dates to the Julian System', *Mitzraim* 6 (1937), 7–40. [Expanded and revised in *The Reigns of the Ptolemies* (1954).]

Skeat, T. C., 'The Epistrategus Hippalos', *Archiv für Papyrusforschung* 12 (1937), 40–43.

*Skeat, T. C., 'Four Years' Work on the Codex Sinaiticus', *The Daily Telegraph* (London) 11 January 1938.

*Skeat, T. C., 'Clues and Blemishes in the Codex Sinaiticus', *The Daily Telegraph* (London) 12 January 1938.

Milne, H. J. M., and T. C. Skeat, *Scribes and Correctors of the Codex Sinaiticus*, including contributions by Douglas Cockerell, London, Trustees of the British Museum, 1938. Pp. xii + 112. 43 plates. 23 figures. [Also Oxford, printed by order of the Trustees of the British Museum at the Oxford University Press, 1938.]

Milne, H. J. M., and T. C. Skeat, *The Codex Sinaiticus and the Codex Alexandrinus*, London, Trustees of the British Museum, 1938. Pp. 35. 6 Plates. Reissued, 1951. 2nd ed., reset, revised by T. C. Skeat, 1955. Pp. 41. 6 plates. Reissue of 2nd ed., 1963. [According to the Preface to the 1951 re-issue, the pamphlet was first published in 1934 under the title *The Mount Sinai Manuscript of the Bible*.]

*Skeat, T. C., 'The Lilies of the Field', *Zeitschrift für die neutestamentliche Wissenschaft* 37 (1938), 211–214.

Skeat, T. C., et al., 'Bibliography: Graeco-Roman Egypt: Papyrology, *Journal of Egyptian Archaeology* 20–25 (1934–1939).

Skeat, T. C., 'An Epitaph from Hermopolis', *Journal of Egyptian Archaeology* 28 (1942), 68–69.

Skeat, T. C., 'The Macedonian Calendar during the Reign of Ptolemy Euergetes I', *Journal of Egyptian Archaeology* 34 (1948), 75–79.

Skeat, T. C., 'A Letter from Philonides to Kleon Revised (P. Lond.

593 = Crönert *Raccolta Lumbroso* 530 = Sammelbuch 7183)', *Journal of Egyptian Archaeology* 34 (1948), 80–81.

*Skeat, T. C., 'St. Mark xvi. 8: A Modern Greek Parallel', *Journal of Theological Studies* 50 (1949), 57–58.

Skeat, T. C., 'Manuscripts Acquired During the Years 1941–50', *British Museum Quarterly* 15 (1949–50), 18–33, plates 9–13.

Skeat, T. C., 'Britain and the Papyri (P. Lond. 878)', in Siegfried Morenz (ed.), *Aus Antike und Orient: Festschrift Wilhelm Schubart zum 75. Geburtstag*, Leipzig, Harrassowitz, 1950, pp. 126–132.

Skeat T. C., 'Two 'Lost' Works by John Leland', *English Historical Review* 65 (1950), 505–508.

Skeat T. C., 'The British Museum: The Catalogues of the Manuscript Collections. 2. Manuscripts' *Journal of Documentation* 7 (1951), 18–60. Offprinted in a number of editions. Cover title, *The Catalogues of the Manuscript Collections in the British Museum*, London, Trustees of the British Museum, 1953. Pp. 43. Rev. ed., 1962. Pp. 45.

Skeat, T. C., 'The Egmont Papers', *British Museum Quarterly* 16 (1951–52), 62–65.

Skeat, T. C., 'Note-books and *Marginalia* of S. T. Coleridge', *British Museum Quarterly* 16 (1951–52), 91–93.

Skeat, T. C., F. G. Rendall and H. M. Nixon, 'Manuscripts and Printed Books from the Holkham Hall Library, 1. The Library, 2. The Manuscripts', *British Museum Quarterly* 17 (1952), 23–40.

Skeat, T. C., 'Two Byzantine Documents', *British Museum Quarterly* 18 (1953), 71–73.

Skeat, T. C., 'The Last Days of Cleopatra: A Chronological Problem', *Journal of Roman Studies* 43 (1953), 98–99.

Glanville S. R. K., and T. C. Skeat, 'Eponymous Priesthoods of Alexandria from 211 B.C.', *Journal of Egyptian Archaeology* 40 (1954), 45–58.

Skeat, T. C., 'An Early Mediaeval "Book of Fate": the Sortes XII Patriarcharum. With a note on "Books of Fate" in general', *Mediaeval and Renaissance Studies* 3 (1954), 41–54.

Skeat, T. C., 'Appendix' to A. H. M. Jones, 'Notes on the Genuineness of the Constantinian Documents in Eusebius's *Life of Constantine*', *Journal of Ecclesiastical History* 5 (1954), 196–200, at p. 200; and reconstructed text on pp. 198–199 [see p. 197 n. 3].

Skeat, T. C., *The Reigns of the Ptolemies* (Münchener Beiträge zur Papyrusforschung und antiken Rechtsgeschichte, 39), Munich, C. H. Beck, 1954. Pp. 43. Repr., 1969. Pp. 43. [Cf. 'The Reigns of the Ptolemies . . .', *Mizraim* 6 (1937), noted above.]

*Skeat, T. C., 'The Provenance of the Codex Alexandrinus', *Journal of Theological Studies* 6 (1955), 233–235.

*Skeat, T. C., 'The Use of Dictation in Ancient Book-Production', *Proceedings of the British Academy* 42 (1956), 179–208.

The Codex Alexandrinus (Royal MS. 1 D v–viii) in Reduced Photographic Facsimile (British Museum), *Old Testament*, Part IV: *1 Esdras—Ecclesiasticus*, London, British Museum, 1957. Introduction signed T. C. Skeat.

Skeat, T. C., 'Letters from the Reign of Henry VIII', *British Museum Quarterly* 21 (1957–59), 4–8.

Neugebauer O., and T. C. Skeat, 'The Astronomical Tables, P. Lond. 1278 (IId C. A.D.)', *Osiris* 13 (1958), 93–112. [Note by Skeat pp. 112–113: 'The Palaeography of the Fragments']

Skeat, T. C., 'A Receipt for *Enkyklion*', *Journal of Egyptian Archaeology* 45 (1959), 75–78 and plate VIII (facing p. 79).

Wace, A. J. B., A. H. S. Megaw and T. C. Skeat, *Hermopolis Magna, Ashmunein: The Ptolemaic Sanctuary and the Basilica* (Alexandria University, Faculty of Arts, Publications, 8), Alexandria, Alexandria University Press, 1959. Pp. xv + 82. 24 plates.

Skeat, T. C., 'The Cult of the Ptolemies after the Roman Conquest of Egypt', in A. J. B. Wace, A. H. S. Megaw and T. C. Skeat, *Hermopolis Magna, Ashmunein: the Ptolemaic Sanctuary and the Basilica* (1959), pp. 12–16.

Skeat, T. C., 'A Letter of Robert Burton', *British Museum Quarterly* 22 (1960), 12–16, plate I.

Skeat, T. C., 'The Case of the Missing Three-Quarter', *British Museum Quarterly* 22 (1960), 54–56.

Skeat, T. C., 'Notes on Ptolemaic Chronology' I 'The Last Year which is also the First', *Journal of Egyptian Archaeology* 46 (1960), 91–94.

Skeat, T. C., 'Notes on Ptolemaic Chronology' II 'The Twelfth Year which is also the First: The Invasion of Egypt by Antiochus Epiphanes (incl. ed. of P. Lond. 1974)', *Journal of Egyptian Archaeology* 47 (1961), 107–112.

Skeat, T. C., 'Papyri from Panopolis in the Chester Beatty Collection', in *Proceedings of the IX International Congress of Papyrology, Oslo, 19th–22nd August, 1958/Forhandlinger ved den IX Internasjonale Papyrologkongress, Oslo, 19.–22. August, 1958*, ed. L. Amundsen and V. Skånland (Association Internationale de Papyrologues; Universitet i Oslo, Klassisk Institutt), Oslo, 1961, pp. 194–199.

Skeat, T. C., 'Letters of Charles and Mary Lamb and Coleridge (Add. Ms. 50824)', *British Museum Quarterly* 26 (1962–63), 17–21, plates VIII–IX.

Skeat, T. C., 'Kubla Khan', *British Museum Quarterly* 26 (1962–63), 77–83, plates XXX–XXXI.

Skeat, T. C., 'Notes on Ptolemaic Chronology' III 'The First Year which is also the Third: A Date in the Reign of Cleopatra VII (incl. ed. of P. Lond. 827)', *Journal of Egyptian Archaeology* 48 (1962), 100–105.

Skeat, T. C. (ed.), *Papyri from Panopolis in the Chester Beatty Library, Dublin* (Chester Beatty Monographs, No. 10), Dublin, 1964. Pp. xliv + 194. 3 plates.

Skeat, T. C., 'The Caxton Deeds', *British Museum Quarterly* 28 (1964), 12–15.

Reproductions from Illuminated Manuscripts, Series V (British Museum, Department of Manuscripts), London, Trustees, 1965. Pp. 30. 50 plates. Note to fifth series signed T. C. Skeat.

Skeat, T. C., 'A Fragment on the Ptolemaic Perfume Monopoly', *Journal of Egyptian Archaeology* 52 (1966), 179–180.

Skeat, T. C., 'Sir Harold Idris Bell', *Journal of Egyptian Archaeology* 53 (1967), 134–139.

Catalogue of an Exhibition of Poetry Manuscripts in the British Museum, April–June 1967, by Jenny Lewis [later called Jenny Stratford], with contributions by C. Day Lewis, T. C. Skeat and Philip Larkin, London, Turret Books for the Arts Council of Great Britain & the British Museum, 1967. Pp. 68. 8 plates. [Title of exhibition: 'Poetry in the making' (on half-title and cover).] Introduction by T. C. Skeat.

Skeat, T. C., and E. G. Turner, 'An Oracle of Hermes Trismegistos at Saqqâra', *Journal of Egyptian Archaeology* 54 (1968), 199–208, plates XXXII–XXXIII (between pp. 204–205).

*Skeat, T. C., 'Early Christian Book-Production: Papyri and Manuscripts', in *The Cambridge History of the Bible*, vol. 2: *The West from the Fathers to the Reformation*, ed. G. W. H. Lampe, Cambridge, Cambridge University Press, 1969, chap. III (pp. 54–79, 512–513). Italian translation: T. C. Skeat, *La produzione libraria cristiana delle origini: papiri e manoscritti*, trans. M. Manfredi, Florence, Istituto Papirologico "G. Vitelli", 1976. Pp. 41, 2 plates.

Skeat, T. C., 'Notes on Ptolemaic Chronology. IV. The Sixteenth Year of Ptolemy Philopator as a *Terminus ad quem*', *Journal of Egyptian Archaeology* 59 (1973), 169–74.

Skeat, T. C., *Greek Papyri in the British Museum: Catalogue with texts*, vol. VII: *Zenon Papyri*, London, British Museum Publications, 1974. 5 plates (folding). ISBN 0 714 10486 8. Pp. viii + 235.

[Variant edition: *Greek Papyri in the British Museum (now in the British Library)*, vol. 7: *The Zenon Archive*, London, British Museum Publications. Pp. viii + 345].

Skeat, T. C., 'A Note on P. Lond. 854', *Journal of Egyptian Archaeology* 60 (1974), 259–260.

Skeat, T. C., 'ΟΚΝΟΣ', in *Le monde grec. Pensée, littérature, histoire, documents. Hommages à Claire Préaux*, ed. Jean Bingen, Guy Cambier and Georges Nachtergael (Université Libre de Bruxelles, Faculté de Philosophie et Lettres, 62), Brussels, Éditions de l'Université de Bruxelles, 1975, pp. 791–795.

Skeat, T. C., 'Another Dinner-Invitation from Oxyrhynchus (P. Lond. Inv. 3078)', *Journal of Egyptian Archaeology* 61 (1975), 251–254.

Skeat, T. C., 'A Letter from the King of the Blemmyes to the King of the Noubades', *Journal of Egyptian Archaeology* 63 (1977), 159–170, plate XXVII, and a fold-out sheet (opposite p. 168).

Davis, G. R. C., *Magna Carta*, 3rd reprint (revised), London, British Museum Publications for the British Library, 1977. Pp. 39. 7 plates. ISBN 0 714 10473 6. First published: 1963. Preface by T. C. Skeat. [The Making of Magna Carta. The Text of Magna Carta, 1215. The Documents of Magna Carta exhibited in the British Library.]

Skeat, T. C., review of K. Aland, *Repertorium der griechischen christlichen Papyri* I and J. Van Haelst, *Catalogue des papyrus littéraires juifs et chrétiens, Journal of Theological Studies* 29 (1978), 175–186.

Skeat, T. C., 'A Table of Isopsephisms (P. Oxy. XLV 3239)', *Zeitschrift für Papyrologie und Epigraphik* 31 (1978), 45–54.

Skeat, T. C., 'The Date of the Dioiketes Theogenes', *Ancient Society* 10 (1979), 159–165.

*Skeat, T. C., ''Especially the Parchments': A Note on 2 Timothy iv. 13', *Journal of Theological Studies* 30 (1979), 173–177.

*Skeat, T. C., 'Two Notes on Papyrus', in *Scritti in onore di Orsolina Montevecchi*, ed. Edda Bresciani, Giovanni Geraci, Sergio Pernigotti and Giancarlo Susini, Bologna, Cooperativa Libraria Universitaria Editrice, 1981, pp. 373–378. 1. Was Re-rolling a Papyrus an Irksome and Time-consuming Task? 2. Last Word on the Question: Was an Adhesive Used in the Manufacture of Papyrus?

Skeat, T. C., 'Manumission and Tax-receipt', in *Papyri Greek and Egyptian, edited by various hands in honour of Eric Gardner Turner on the occasion of his seventieth birthday* (Egypt Exploration Society, Graeco-Roman Memoirs, 68), London, 1981, no. 19 (pp. 93–99), plate VIII.

Skeat, T. C., 'A Note on Tebtunis Papyrus 8', *Bulletin of the American Society of Papyrologists* 18 (1981), 141–144.

*Skeat, T. C., 'The Length of the Standard Papyrus Roll and the Cost-Advantage of the Codex', *Zeitschrift für Papyrologie und Epigraphik* 45, 1982, 169–176.

Roberts, C. H., and T. C. Skeat, *The Birth of the Codex*, London, Oxford University Press for the British Academy, 1983. Pp. ix + 78. 6 plates. ISBN 0 197 26024 1, 0 197 26061 6. 2nd ed., 1987. Pp. ix + 78. ISBN 0 197 26061 6.

Skeat, T. C., 'The Augustan Era in Egypt: A Note on P. Oxy. xii. 1453', *Zeitschrift für Papyrologie und Epigraphik* 53 (1983), 241–244, plate VI.

Skeat, T. C., 'Gospel of John 18: 36–19: 7', in *The Oxyrhynchus Papyri*, vol. L (Egypt Exploration Society, Graeco-Roman Memoirs, 70), London, 1983, no. 3523 (pp. 3–8), plates I–II. [P90.]

*Skeat, T. C., 'The Codex Vaticanus in the Fifteenth Century', *Journal of Theological Studies* 35 (1984), 454–465 (plates on pp. 459, 460, 462).

Skeat T. C., unsigned Introduction to *The New Testament in Greek* III *The Gospel according to Saint Luke* Part One: Chapters 1–12, edited by the American and British Committees of the International Greek New Testament Project, Oxford, The Clarendon Press, 1984, pp. v–xvi. ISBN 0198261675

Skeat, T. C., "Carte blanche", *British Library Journal* 13 (1987), 25–32, 2 figures (pp. 30, 31).

*Skeat, T. C., 'The "Second-First" Sabbath (Luke 6:1): the Final Solution', *Novum Testamentum* 30 (1988), 103–106.

*Skeat, T. C., 'A Note on πυγμῇ in Mark 7:3', *Journal of Theological Studies* 41 (1990), 525–527.

*Skeat, T. C., 'Roll versus Codex—A New Approach?', *Zeitschrift für Papyrologie und Epigraphik* 84 (1990), 297–298.

*Skeat, T. C., and B. C. McGing, 'Notes on Chester Beatty Biblical Papyrus I (Gospels and Acts)', *Hermathena* 150 (1991), 21–25, 2 plates (between pp. 22–23).

*Skeat, T. C., 'Irenaeus and the Four-Gospel Canon', *Novum Testamentum* 34 (1992), 194–199.

Skeat, T. C., *The Reign of Augustus in Egypt. Conversion Tables for the*

Egyptian and Julian Calendars, 30 B.C.–14 A.D. (Münchener Beiträge zur Papyrusforschung und antiken Rechtsgeschichte, 84), Munich, C. H. Beck, 1993. Pp. vii + 44. ISBN 3 406 37384 4.

*Skeat, T. C., 'A Codicological Analysis of the Chester Beatty Papyrus Codex of Gospels and Acts (P45)', *Hermathena* 155 (1993), 27–43.

*Skeat, T. C., 'The Origin of the Christian Codex', *Zeitschrift für Papyrologie und Epigraphik* 102 (1994), 263–268.

Skeat, T. C., 'The Beginning and the End of the Κρίσαρος Κράτησις Era in Egypt', *Chronique d'Égypte* 69 (1994), 308–312.

*Skeat, T. C., 'Did Paul Write to "Bishops and Deacons" at Philippi? A Note on Philippians 1:1', *Novum Testamentum* 37 (1995), 12–15.

*Skeat, T. C., 'Was Papyrus Regarded as "Cheap" or "Expensive" in the Ancient World?', *Aegyptus* 75 (1995), 75–93.

*Skeat, T. C., 'The Oldest Manuscript of the Four Gospels?', *New Testament Studies* 43 (1997), 1–34.

*Skeat, T. C., 'The Codex Sinaiticus, the Codex Vaticanus and Constantine', *Journal of Theological Studies* 50 (1999), 583–625, plates 1–2 (pp. 623–624).

*Skeat, T. C., 'The Last Chapter in the History of the Codex Sinaiticus', *Novum Testamentum* 42 (2000), 313–315.

Skeat, T. C., 'A Forgotten Factor in the Debate on the Calendar in Augustan Egypt', *Zeitschrift für Papyrologie und Epigraphik* 132 (2000), 240.

*Skeat, T. C., 'Αρτον φαγειν: a Note on Mark iii. 20–21', in *Essays and Texts in Honor of J. David Thomas*, ed. T. Gagos and R. S. Bagnall (American Studies in Papyrology, XLII), Oakville, Conn., American Society of Papyrologists, 2001, pp. 29–30.

Skeat, T. C., 'The Egyptian Calendar under Augustus', *Zeitschrift für Papyrologie und Epigraphik* 135 (2001), 153–156.

A

ANCIENT BOOK PRODUCTION

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1

THE USE OF DICTATION IN ANCIENT BOOK-PRODUCTION[1]

1. History of the Dictation Theory to 1913: the Work of Birt and his School

It is a striking fact that of all the numerous handbooks of palaeography, barely a handful pause to consider in a practical manner the technique of ancient book-production. As an example, I may quote Monsignor Robert Devreesse's *Introduction à l'Étude des manuscrits grecs*, published in 1954—an excellent book, I would hasten to add, and one particularly germane to the present inquiry, since it deals not so much with palaeography as with the externals of ancient books, rather on the lines of the infant science of codicology. Thus, he devotes whole chapters to ancient writing-materials, writing-implements, the structure of manuscripts, and such topics on the one hand, and with the literary content of the manuscripts on the other; but the essential process linking the two, the means whereby the materials and implements are used to produce the manuscripts, is wholly ignored.

The principal aim of the present inquiry is to consider the theory that books, when produced on a commercial scale in the ancient world, were commonly—I do not think even its most extreme advocates would say exclusively—produced by means of a number of scribes copying simultaneously from dictation. Historically, the theory is not of any great antiquity. Kurt Ohly, in his *Stichometrische Untersuchungen* (1928), a book of which I shall have much to say later on, traces it back to W. A. Schmidt, of Berlin, who in 1847 published a work entitled *Geschichte der Denk- und Glaubensfreiheit im I. Jhdt. der Kaiserherrschaft u. des Christentums*. But in fact Schmidt does not either directly or indirectly discuss the question of dictation; all he does (op. cit., pp. 130–1) is to quote a passage from the Life of

[1] The substance of two lectures delivered in the University of London in March 1956, as the Special University Lectures in Palaeography.

Atticus by Cornelius Nepos, in which Nepos describes the household of the great publisher as including slaves of the highest education (*pueri literatissimi*), first-class readers (*anagnostae optimi*), and a large staff of copyists (*plurimi librarii*). Nepos does not define the duties of these *anagnostae*, and he does not state—nor does Schmidt discuss the point—that the copyists worked from dictation. Nevertheless, it is important to quote the passage now, since it brings us face to face, at the outset, with a most serious difficulty in terminology. Ἀναγνώστης in Greek, like *lector* in Latin—or 'reader' in English—is a neutral term applicable equally to a reader dictating to a scribe or scribes; to a reader assisting in the collation of a newly written manuscript by reading it back while another followed in the exemplar (as we should say today, a 'reader to the press'); to a reader reading aloud to entertain his master; or, finally, to a reader reading to himself. The *anagnostae* of Atticus might have been in any, or all, of the first three of these categories. In itself, therefore, the passage in Nepos proves nothing, and there is accordingly no reason to father the dictation theory on W. A. Schmidt. It is, in fact, a good deal older than his time, for F. A. Ebert, in his *Zur Handschriftenkunde*, published at Leipzig in 1820, refers to the dictation theory as having been propounded 'in neuester Zeit'; and he quotes as his authority J. F. Eckhardt, *Exercitatio critica de editione librorum* (Jena, 1777). This latter work is inaccessible to me, but to judge from Ebert's comments Eckhardt's statements appear to have been little more than guesswork, and I do not think it would be profitable to pursue the history of the question back into the eighteenth century.

We may now pass on to a name which, more than any other, is inseparably linked with the dictation theory—that of Theodor Birt. His *Antike Buchwesen*, published in 1882, is an enormous, if uncritical, collection of materials; and both Birt himself in his later publications, and other writers too numerous to mention, have constantly referred to this book as containing an exposition of the dictation theory. But though I have searched carefully through the pages customarily quoted, and elsewhere in the book, I have not succeeded in discovering any references to dictation, with the exception of an imaginative (or should one say imaginary?) picture, on p. 362, of an ancient publishing house in action, with its hordes of slaves busily writing from dictation, and a casual reference, in a footnote on p. 356, to the effect that two hours would be sufficient time to write the second book of Martial from dictation. No arguments of any kind are brought forward, or even considered necessary, but the use of dictation is taken for granted. So, too, when Birt comes to consider the passage from Cornelius Nepos' Life of Atticus to which reference has already been made, Birt translates 'sehr gute Verleser und sehr viele Buchschreiber', but only the words 'sehr viele Buchschreiber' are printed in *Sperrdruck* as being relevant to the matter in hand, and Birt's views on the functions of the *anagnostae* therefore remain obscure.

Although Birt's book turns out on examination to contain no critical discussion of the problem, his unqualified references to dictation appear to have convinced many of his contemporaries that the matter had been settled once and for all. Thus Usener, in a paper on the text of Plato, printed in the *Nachrichten* of the Göttingen Academy (1892, pp. 194, 197, 199), follows Birt in virtually assuming the general use of dictation, despite the fact that the only ancient authorities he can quote are very ambiguous references in Varro and St. Jerome. Usener does, however, deal directly with the passage in Nepos' Life of Atticus, and instead of evading the issue, like Birt, he unhesitatingly identifies the *anagnostae* as the professional readers who dictated, each to a roomful of scribes writing simultaneously. But here again we miss—surprisingly in the case of so great a scholar and critic—a proper formulation of the theory and an awareness of the arguments which can be advanced for or against it.

The same uncritical dogmatism marks, though in a less acute degree, a discussion of the problem by Karl Dziatzko, in a privately printed pamphlet which also appeared in 1892 (*Zwei Beiträge zur Kenntnis des antiken Buchwesens*, p. 13, n. 1). Here he attempts to define accurately the functions of the *anagnostes*. Accepting, apparently without question, the theory of dictation, he stresses that no less important than the original reading of the text in the course of dictation to the copyists would be the reading through of the finished manuscript in order to detect and correct scribal errors. That this latter step was in fact taken by reputable publishers in the ancient world before they offered their books for sale we know from the complaint of Strabo (xiii. 1. 54) about 'inferior' booksellers, both in Rome and Alexandria (γραφεῦσι φαύλοις χρώμενοι καὶ οὐκ ἀντιβάλλοντες), who threw their wares on the market without having first taken this essential precaution. Dziatzko makes the further point that in a large publishing firm it is unlikely that the same reader who dictated to a roomful of scribes would subsequently read through all the numerous copies so produced, since the time taken would have been considerable, and in the meantime further production would have been at a standstill; he therefore supposed that ancient publishers kept a distinct staff of 'readers to the press', and that it was the number of such readers rather than the number of scribes which was the limiting factor in the size of an edition.

These speculations are necessarily inconclusive, but they are interesting as they reveal a practical appreciation of the problems involved; and the question of collation of the finished manuscript is a very important one, to which more attention will be given below.

Before we quit the nineteenth century, it may be of interest to quote the views of a modern publisher who has studied the methods of his predecessors of the ancient world, G. H. Putnam. In his *Authors and their Public in Ancient Times* (New York, 1894), p. 222, he declares his belief in the dictation theory in the words: 'It seems probable that in no other way would it have been practicable to produce with sufficient speed and economy the editions required, and I find myself in accord with Birt in the conclusion that dictating was the method generally followed, at least in the most important establishments and for the larger editions.' Coming from a practical publisher these views deserve to be treated with some respect.

In 1907 Birt published *Die Buchrolle in der Kunst*, a great collection of iconographic material, covering not only representations of the papyrus roll, as the title implies, but also its use in reading and writing. Birt begins his book with some representations of scribes at work taken from ancient Egyptian monuments, which are of special interest to us, since in some instances they undoubtedly show scribes, and sometimes several scribes simultaneously, writing from dictation (Birt, pp. 10–12). Nevertheless, what these scribes are writing are almost certainly documents—accounts, lists, letters, and the like—and the copying of literary texts in Pharaonic Egypt is a separate issue. In 1937 A. Volten, of Copenhagen, discussed the numerous copies of Middle Kingdom literary works produced in the Ramesside period, and concluded that, with very few exceptions, these had been written as exercises by pupils working under a schoolmaster who dictated the text to them (*Studien zum Weisheitsbuch des Anii* [*Kgl. Danske Vid. Sels., hist.-fil. Medd.* xxiii. 3]). More recently B. Van de Walle has re-examined Volten's conclusions, and suggests that, though dictation was certainly widely employed, visual copying also existed (*La Transmission des textes littéraires égyptiens*, Brussels, 1948). Professor J. Černý, in his inaugural lecture, *Paper and Books in Ancient Egypt*, 1952, p. 28, states his view as follows: 'Comparison of various manuscripts of the same text has suggested that there is a class of errors which can only be explained as arising from dictation, but we have no proof that dictation to several scribes was ever employed so as to produce concurrently several copies of the same work, *as in the Roman scriptoria* [the italics are mine].' To sum up, although dictation was certainly employed in Pharaonic Egypt for the writing of documents which were required in several copies, there is as yet no agreement among scholars as to the extent, if at all, it was used for the multiplication of literary texts.

Nevertheless, this subject has another aspect which deserves our attention, namely the position adopted by the scribe for writing. Normally (Černý, op. cit., pp. 13–14) he sat cross-legged, with his short kilt tightly stretched between his thighs, which thus formed a sort of substitute for a table or writing-desk on which the open section of the roll of papyrus lay. While the right hand wrote, the left steadied the unwritten and still rolled-up portion of the roll, and unwound it as necessary. Other representations show a different posture, in which one knee was raised in front of the writer to form a sloping support for the open section of the roll, which then rested on the knee and thigh. Tables or desks are never used. These facts are of special interest, since there is evidence to show that professional scribes of the classical period wrote in the same (to us) most awkward position. And to forestall incredulity, I may perhaps recall that in the classical world, as in the Near East generally down to comparatively modern times, life is marked by a general absence of such conveniences as chairs and tables: I will quote but one random example—an extract from the diary of Edward Lear whilst travelling in Albania in 1848: 'Bitter cold saluted me at rising—if that may be called rising which, in this chair-less land consists of a perpetual scramble on the floor, reminding the performer of such creatures as swallows and bats, of which naturalists relate that their difficulty in leaving the ground, when once there, is extreme.' An excellent example of the adoption of this very ancient writing position is furnished by Papyrus 136 in the British Museum, a roll containing the third and fourth books of the *Iliad*, written in the first century A.D. At the end of the roll, on a separate sheet of papyrus, is the following remarkable colophon:

Ἐγὼ κορωνὶς εἰμὶ γραμμάτων φύλαξ
κάλαμος μ' ἔγραψε, δεξία χεῖρ καὶ γονύ,
ἄν τινί με χρήσῃς, ἕτερον ἀντιλάμβανε,
ἐὰν δὲ μ' ἀλείφῃς, διαβαλῶ σ' Εὐριπίδῃ
ἄπεχε.

(H. J. M. Milne, *Cat. of Literary Papyri in the British Museum*, 1927, p. 22, no. 11; Wifstrand, *Hermes*, lxviii, 1933, pp. 468–72; B. Olsson, *Zentralblatt für Bibliothekswesen*, li, 1934, pp. 365–6). It is noteworthy here that the pen, the writing hand, and the *knee* are all mentioned as collaborating in the physical production of the manuscript. Somewhat similarly, in the opening lines of the *Batrachomyomachia*, in a burlesque address to the Muses, the author appeals to them to aid him in his poem ἣν νέον ἐν δέλτοισιν ἐμοῖς ἐπὶ γούνασι θῆκα, though here it is the author's writing-tablet, not the papyrus roll of the copyist, which finds its support on the writer's knee. I have noticed that in an edition of the *Batrachomyomachia* by C. Marsuppini Aretinus, published in 1509, the editor comments on this very line as follows: '*super genibus*: hoc nostro tempore observant graeci quod vidimus venetiis et alibi.' When, however, we turn from these literary references to the monuments chronicled by Birt, the result is disappointing; according to Birt's own statement (op. cit., pp. 204–5) there is only one clear representation of a man writing a manuscript on a papyrus roll, a sarcophagus in Rome showing a figure seated on the ground with the left knee raised higher than the right and partly supporting a roll of papyrus, the ends of the roll being grasped by the left hand while the right holds a pen above the open section of the roll. This, like nearly all the comparatively rare reproductions of writing in classical antiquity, probably represents an author composing, and is therefore of no more relevance to the present investigation than are, for example, the familiar Evangelist pictures in medieval manuscripts of the Gospels. Nevertheless, the relief is of interest as illustrating the common writing position with the knee used as a desk.

The remarkable scarcity of representations of writing is in itself something of a mystery, especially when contrasted with the numerous Egyptian monuments of this kind. Probably Birt is right in suggesting that the reason is that whereas in Egypt writing was an honourable profession and a passport to high office—one thinks, for example, of the instance of Horemheb, last Pharaoh of the XVIIIth Dynasty, portrayed as a scribe in a statue now in the Metropolitan Museum of New York—in the Graeco-Roman world, on the other hand, it was a characteristically banausic occupation carried on predominantly by slaves and unworthy of pictorial representation.

I do not think the foregoing digression is wholly irrelevant to our purpose. We cannot indeed argue, as Birt appears to do, that because writing from dictation was used in Pharaonic Egypt, occasionally even to make multiple copies (though this seems to be true only of non-literary writings), it necessarily follows that it would be the method adopted by publishers of the classical world. But the question of the scribal writing position, which seems to have been broadly the same both in ancient Egypt and in the classical world, may prove to be an important factor in the evaluation of the dictation theory, since the position in question is one which would be admirably suited to scribes copying from dictation, but much less so in the case of visual copying, since the scribe would have nothing on which to rest his exemplar, nor would he have two hands free with which to manipulate it. This point has in fact been stressed by Volten in dealing with the question of dictation in Ramesside Egypt.

In this later book of Birt's which we have been considering, and which, like its predecessor, is often quoted as evidence of his support for the dictation theory, there is actually no direct discussion of dictation, much less any reasoned argument. It was only in 1913, in his *Kritik und Hermeneutik nebst Abriß des antiken Buchwesens*, published as part of Iwan Müller's *Handbuch*, that Birt's views find anything like definite exposition. The result is most disappointing. He quotes his own two earlier books, neither of which, as we have seen, seriously discusses the matter, and attempts to bolster up his bald assertions about dictation with a few miscellaneous quotations, most of them quite inconclusive since they refer to authors dictating their own literary works, which is, of course, a commonplace of every age and civilization, and has absolutely no bearing on methods of commercial book-production. The only really telling point he makes is that which has just been mentioned, viz. that the writing position normally adopted would have made *visual* copying very difficult, if not impossible; but this is very far from proving the dictation theory.

In the same year, 1913, appeared the invaluable *Companion to Classical Texts* of F. W. Hall, in which is to be found what is, I think, the only discussion of the dictation theory in the English language. Hall denies that dictation was ever widely used, adducing in the main the familiar objections of earlier writers. Any saving of time, he argues, would be cancelled out by the (in his view) inevitable increase in the number of errors to be corrected. He then claims as significant the fact that copying by dictation is not represented in Greek or Roman art—but immediately removes all force from this contention by adding that there are in fact no representations of copying by *any* method. If copies were required in haste, he opines, following a suggestion of Schubart's, that the exemplar could have been cut up and parcelled out among a number of scribes for simultaneous copying. The phonetic errors so commonly found in manuscripts are, he says, due to the scribe reading the exemplar to himself. Hall's remaining objections to dictation relate in general to medieval manuscripts—for example, he stresses the pen-trials sometimes made by a scribe on the margins of his exemplar, which certainly prove visual copying, since with dictation the scribe would not have access to the exemplar; and he quotes the rules enforcing silence in the monastic scriptoria.

It is very necessary to realize at this point how little weight many of these objections carry. When it is argued that dictation is inconceivable because of the flood of errors which would have resulted, the answer is that, as we shall see, there are indeed manuscripts which contain errors on this allegedly inconceivable scale, but that this is not *necessarily* the inevitable result. The other points made by Hall will be discussed later on; and I would conclude this section by looking forward a few years to a review by Professor F. Zucker of K. Ohly's *Stichometrische Untersuchungen*, printed in *Gnomon*, viii, 1932, p. 384: 'Ich möchte überhaupt grundsätzlich bemerken, daß wir im Buchwesen in weit größerem Ausmaß als man vielfach anzunehmen scheint, auf die Erwägung von Möglichkeiten angewiesen sind. Das Material ist gefährlich ungleichmäßig, in mancher Hinsicht überaus reich, in mancher überaus dürftig. Vor allem muß man davon warnen, Lücken unserer Kenntnis auf Grund gewisser allgemeiner Vorstellungen auszufüllen, die uns selbst-verständlich erscheinen.'

2. New Trends: Balogh and Ohly

We now come to two works, published almost simultaneously, which have profoundly affected the history of the dictation theory, and have produced, or hastened, a general revulsion against Birt's position. We have seen that, from the start, critics had dimly realized the possibility that phonetic errors—errors due to mispronunciation—might have been introduced by the scribe copying visually and pronouncing aloud the text before him, thus, in effect, dictating to himself. In recent years this has grown to be the major argument against the dictation theory, and the reason for its rise to prominence is not far to seek. In 1927, after fourteen years of study and research, Joseph Balogh published an article in *Philologus* entitled 'Voces Paginarum', in which he established once and for all that in the ancient world all readers, whether of books or documents, normally pronounced aloud the words as they read them, and that the silent reading which is so universal today was then looked upon as something phenomenal. This position continued until about the fifth century A.D., after which various influences, such as the rise of monasticism, with its ascetic ideal of silence, gradually turned the scale in favour of the silent reading which we know today.

The importance of this discovery—for discovery it justly deserves to be called—is obvious. If in fact the scribe, while copying a manuscript visually, pronounced aloud each word as he read it in his exemplar, the sounds so produced must inevitably have influenced, or indeed determined, what he put on paper. This process we may call self-dictation, and it is now no longer viable, as some earlier writers have thought, to use the existence in manuscripts of phonetic errors as in itself a proof that the manuscript was copied from dictation. Equally, however, it does not, as a number of more recent writers appear to believe, necessarily disprove the dictation theory.

In the year following Balogh's article, 1928, appeared another work which has greatly, though with far less justification, affected the history of the dictation theory. This is the *Stichometrische Untersuchungen* of Kurt Ohly, published as a *Beiheft* of the *Zentralblatt für Bibliothekswesen*. Even in the sphere of its professed subject the book is by no means comprehensive—it omits, for instance, all reference to the stimulating book of Rendel Harris on *Stichometry*, published in 1893—while the discussion of the dictation theory is remarkably superficial, extending to barely a couple of pages (123–5, 131) near the end of the book. The account which Ohly gives of the history of the dictation theory is scanty and unsatisfying: for instance, he speaks of the views of Birt as having obtained 'fast allgemeine Geltung', whereas in fact there has always been a strong undercurrent of criticism and opposition to Birt. The argument against the dictation theory which Ohly claims as his original contribution to the debate is as follows: the existence of stichometry, he says, presupposes that scribes were paid according to their individual output, i.e. on a piece-work basis. But under the dictation theory, all the scribes working under one dictator would have produced the same amount of work in the same time, and they must therefore have been paid on a time-work, not a piece-work basis. As, however, stichometry presupposes piece-work, the dictation theory is false. Ohly then produces some ponderous calculations designed to show that visual copying was just as quick as copying from dictation. Assuming that a certain work could be copied in six hours, thirty scribes working to the dictation of one reader would produce thirty copies at the expense of 186 man-hours (180 by the scribes and 6 by the reader). If, on the other hand, visual copying were employed, six hours would be needed to produce the first copy from the exemplar; copy and exemplar would then be handed on to two other scribes, who would each make one copy, raising the total to four; these four would be handed out for four other scribes, and so on, the number produced doubling each time. In the end, as Ohly shows, visual copying could produce the same number of copies for the same expenditure of man-hours as copying from dictation, indeed there would be a slight advantage in favour of visual copying, which would dispense with the services of the reader. Ohly then shows that similar results would be obtained if, instead of making a complete copy of the whole manuscript each time, the exemplar were divided into sections and parcelled out among a number of scribes who would each copy out that particular section. Apart from these purely mathematical considerations, Ohly claimed certain incidental advantages for visual copying: in visual copying each scribe would proceed at his own fastest speed, whereas with dictation speed for all would be limited to the capacity of the slowest writer in the scriptorium. In addition (and here we have a familiar argument) manuscripts produced by visual copying would need little correction, whereas those produced by dictation would swarm with errors. Ohly concludes triumphantly: 'So ist damit der positive Beweis erbracht, daß im griechisch-römischen Altertum die Herstellung der Bücher ausschließlich auf dem Wege der Abschrift erfolgt ist.'

The fragility of Ohly's arguments hardly needs demonstration, and their deficiencies have for the most part been pointed out by Zucker, in his review in *Gnomon* already quoted, and by the veteran but still hard-hitting Birt (*Berliner Philologische Wochenschrift*, 50, 1930, cols. 297–317), supported in a brief article by W. Weinberger, *Zur Diktat-Theorie*, in *Hermes*, lxvi, 1931, pp. 122–4. To take Ohly's four points in succession: first, the existence of stichometry as disproving dictation. It is mere assumption on Ohly's part to say that copying from dictation must necessarily have been paid for as time-work. Even in industry today there is no hard and fast line between time-work and piece-work, and jobs may change their category from time to time. In any case, as Birt himself points out, the mass-production scriptoria of the big publishers of the ancient world, such as Atticus, were staffed by slaves and the question of payment as such does not arise.

Ohly's elaborate calculations are equally beside the point, since they demonstrate nothing more than the staggeringly obvious fact that, however the task of copying an edition of a given size is split up, the total man-hours involved in the writing of it remains the same. Ohly completely overlooks the one really significant factor, namely that by simultaneous dictation to a number of scribes, the largest number of copies were produced in the shortest time—a matter of vital importance to a publisher handling a work for which there was a large and insistent demand. Thus, in the hypothetical case quoted by Ohly, under his scheme of copying and recopying visually until the full number was reached, the edition would be completed in twenty-four hours, whereas by dictation it could be produced in six hours. Thirdly, the argument that speed of dictation would be limited by the slowest writer in the scriptorium is easily disposed of, since obviously any scribe consistently falling behind his fellows would be replaced. Ohly gives no weight to the contention that dictation is *per se* the faster method, since there must always be some saving of time through the scribe not having to glance backwards and forwards to his exemplar. Lastly, the argument that dictation would let in a flood of errors, the correction of which would nullify any saving of time in the original copying, is again a mere assertion. As I hope to show later, the fact would seem to be that, although dictation *may* be productive of error, it is not necessarily so, and that in the last resort, irrespective of whether visual copying or dictation is employed, it is the education and attention of the scribe which is really the governing factor.

Zucker's general conclusion, in reviewing Ohly's work, was that both visual copying and dictation might well have existed side by side. Birt, besides traversing Ohly's objections, did adduce some new pieces of evidence in favour of dictation, notably a scholium of Pseudo-Acro on Horace, *Ep.* i. 1. 55: 'dictata propria dicuntur quae pueris a librario dictantur', that is: 'dictated is the term properly applied to what is dictated by a *librarius* [here = publisher] to his slaves.' Another scholium, on *Ep.* i. 20. 19, contains the words 'secundum morem librariorum et magistrorum', i.e. 'after the manner of publishers and schoolmasters'. As Birt points out, the common characteristic of these two occupations is that both dictate to their *pueri* (= slaves in the case of the publisher, pupils in the case of the schoolmaster). These references are certainly impressive.

3. The Present Impasse

To bring the history of the dictation theory down to modern times, I pass on to another landmark, M. A. Dain's book, *Les Manuscrits*, published in 1949, and containing within its brief compass an extraordinary wealth of information and acute observation by an acknowledged master in the field of ancient manuscripts and textual transmission. M. Dain, though he admits that dictation may have been used for the urgent reproduction of important texts, rejects it as a normal practice. His objections (pp. 19–21) are, however, not always easy to follow since the reader is sometimes left in doubt whether he is referring to medieval manuscripts or those of an earlier age. Thus he quotes as disproof of dictation what he calls 'ces multiples représentations de nos manuscrits, grecs, latins ou orientaux, nous montrant le copiste écrivant en se reportant à son modèle devant lui', which obviously refers to medieval manuscripts. M. Dain further objects that no manuscript mentions, and no monument represents, dictation as a means of book production. But, as we have seen, the absence of representations of dictating in monuments of the classical world is of no significance in the virtual absence of representation of copying by *any* method. Next, M. Dain states that only visual copying would enable the scribe to arrange his text properly on the page. This argument may be of some weight in the case of medieval manuscripts with their carefully drawn margins and lines, but surely not in literary papyri where normally no such exactness and symmetry is found. M. Dain follows this up with some questionable assertions, that visual copying is quicker than copying from dictation, that dictation to a number of scribes simultaneously would not effect any saving of time (here he presumably echoes Ohly's argument which we have dealt with in detail above), and that anyway time was no object (here again we are surely back in the monastic age).

It must be admitted that the case made out by M. Dain for rejecting the dictation theory is not wholly convincing. Certainly it has not convinced G. Pasquali, who reviewed the book in *Gnomon* (xxiii, 1951, p. 233); 'nach meiner Erfahrung', he goes on, 'stimmt es jedenfalls nicht, daß kopieren schneller geht, als unter Diktat zu schreiben'.

M. Dain's book has another interest for us, since it is one of the very few works to discuss the writing position of the scribe. Basing his conclusions primarily on Greek manuscripts, he points out that writing on the lap or knees was the normal practice until the very end of the Middle Ages; and though he does not in so many words trace the practice back to the classical period, his references to rolls of papyrus leave us to infer that he accepts the knee-rest position as the norm for that age also. The importance of these observations is emphasized by Pasquali in the above-mentioned review.

Nevertheless, so far as the dictation theory is concerned, the position as left by M. Dain is disappointing. It really seems as if, after a century and a half of intermittent effort, the debate on the dictation theory, based mainly on the evidence of archaeology and literary criticism, had reached an impasse. It is therefore, perhaps, not surprising that E. Kuhnert, writing the section on ancient book-production in the new edition of Milkau's *Handbuch der Bibliothekswissenschaft* (i, p. 862, and n. 5), takes up an agnostic position, merely remarking: 'Ob in der Antike die Vervielfältigung ausschließlich durch Abschrift oder auch nach Diktat erfolgt ist, ist eine Frage, die ebenfalls verschiedene Beantwortung gefunden hat.' Is there any hope of further progress, either by reassessment of our existing knowledge, or by some wholly new line of approach?

4. The Codex Sinaiticus

In attempting to answer this question, I propose to turn back to the year 1938, in which my colleague Herbert Milne and I published a volume entitled *Scribes and Correctors of the Codex Sinaiticus*, in which (Chapter VII: Orthography and the Dictation Theory, pp. 51–59) we propounded, perhaps with more enthusiasm than knowledge, the theory that that famous manuscript had been written from dictation. The relatively small attention which this work attracted will, I hope, be accepted as an excuse for repeating a good deal of the arguments there given. But besides the fact that it was the writing of this book which first attracted my attention to the dictation theory, I have another reason for referring to it here, because in it, for, I believe, the first time, the arguments for and against dictation are not treated as a mere academic debate, but are set against the background of the detailed study of an actual manuscript. I do not claim any special merit for this departure from tradition, since it was the manuscript which led me to the consideration of dictation and not vice versa; but it does exemplify the principal point which I shall attempt to establish, namely that it is only by the painstaking collection and classification of the evidence of the manuscripts themselves that further progress in the matter can be hoped for.

I shall therefore recall that the types and frequency of error vary very considerably between the three scribes of the Codex Sinaiticus, but are relatively constant throughout the sections written by each. Thus Scribe D, whom we may conveniently take first, is a very correct writer, his lapses being limited to interchange of ι and ει in medial and final positions, producing, e.g., δυναμι for δυναμει, ταπινος for ταπεινος, κρειναι for κριναι, ισχυει for ισχυι. Confusions of ε and αι, as in εξελουμαι for εξελου με, or ρυσασθε for ρυσασθαι, are not so much solecisms as mistakes of verbal forms. Scribe A reproduces the same errors, but on a much wider scale, while interchange of final αι and final ε, as θησομε for θησομαι, επαραται for επαρατε, is also very frequent. With Scribe B full rein is given to the interchange of αι and ε, even in the commonest words, as κε, τές, αιν, σαι, for και, ταις, εν, σε. Confusion by Scribe B of liquids or stops is also common, as βερτιων for βελτιων, εσπελας for εσπερας, γλυδων for κλυδων, κιτωνος for χιτωνος. The aspirated letters are another source of confusion to Scribe B, as, e.g., εκμαλωσια or εγμαλωσια for αιχμαλωσια, εκθρος for εχθρος, θυπης for τρυφης, Ιωσηφ ως for Ιωσηπος. Omission, or wrong insertion of γ, already at this period a glide rather than a stop, produces φοιιν for φευγειν, λει for λεγει, λεγοντας for λεοντας, and so on. Errors of purely visual origin, as απαται for αγιασαι, are conspicuous by their rarity. These phonetic errors are supplemented, in the case of Scribe B, by blunders of all kinds—omission (e.g. υωρ for υδωρ), insertion (προιηση for ποιηση), haplography (ειππον for επι ιππον), dittography (παιδαδα for παιδα), metathesis (πλολω for πολλω), to give only one example for each of the main definable categories.

Two reviews of outstanding merit, namely by Professor Kirsopp Lake in *Classical Philology*, xxxvii, 1942, pp. 91–96, and by Professor H. A. Sanders, in *American Journal of Philology*, lx, 1939, pp. 486–90, unite in rejecting the suggestion put forward by Milne and myself that the Codex Sinaiticus was written from dictation. I would admit that we had not given sufficient weight to the possibilities of 'subconscious dictation'; but even so it seems to me hard to believe that errors on the limitless scale indulged in by Scribe B in particular can be produced by such means. Milne and I did indeed think that we had identified one positive proof of dictation in the manuscript—the extraordinary reading in 1 Maccabees 5:20, where, instead of the correct reading ὀκτακισχίλιοι, which would be written as a numeral thus: ͵H, we find the seemingly meaningless jumble H̄ ϛ; H̄ ϛ̅—. Of these four symbols, the second is the normal form of the numeral 6, the fourth the normal form for 3,000; and we suggested that the two remaining letters should be read not as numerals but as words, ἢ . . . ἢ. The explanation we suggested was that the dictator failed to read with certainty the original numeral ͵H in the exemplar, and called out 'ἢ ἓξ ἢ τρισχίλιοι', 'either six or three thousand', which the scribe wrote down word for word! Professor Sanders has indeed attempted to explain away the passage on the ground that the whole expression was originally a marginal gloss, written in the exemplar by someone who had failed to read the numeral there; but no one going to the trouble of entering a marginal note would have put down, even as a possibility, a reading so patently absurd in this context as 'six'; and on the whole I am still inclined to believe that the solution proposed by Milne and myself has the balance of probability in its favour.

There is another feature of the Codex Sinaiticus (or rather the detached portion which is known as the Codex Friderico-Augustanus) which Milne and I did not mention in our book, but which is nevertheless of some interest to the present inquiry. I have already mentioned the importance of a copy being compared with the exemplar, and have quoted the technical term, ἀντιβάλλειν, used to denote the process of collation. None of the passages in which this word occurs, however, enables us to form any clear picture of how this was done, with the exception of two notes which have been inserted, perhaps in the sixth or seventh century, in the Codex Sinaiticus. These notes, at the end of Ezra and Esther respectively, record that certain books in the Sinaiticus had been collated with another exceedingly ancient manuscript which was in the autograph of Pamphilus, one of the successors of Origen in the great Christian school of Caesarea. This autograph manuscript of Pamphilus, the notes go on to say, had at the end the following inscription:

> μετελήμφθη καὶ διορθώθη πρὸς τὰ Ἑξαπλᾶ Ὠριγένους ὑπ' αὐτοῦ διορθωμένα. Ἀντωνῖνος ὁμολογητὴς ἀντέβαλεν, Πάμφιλος διόρθωσα τὸ τεῦχος ἐν τῇ φυλακῇ διὰ τὴν τοῦ Θεοῦ πολλὴν καὶ χάριν καὶ πλατυσμόν. [καὶ εἴγε μὴ βαρὺ εἰπεῖν, τούτῳ τῷ ἀντιγράφῳ παραπλήσιον εὑρεῖν ἀντίγραφον οὐ ῥᾴδιον.]

For the sake of clarity I give a translation:

> This volume has been transcribed from, and corrected by, the Hexapla of Origen, as corrected by his own hand. Antoninus, the confessor, collated (ἀντέβαλεν), and I, Pamphilus, corrected (διόρθωσα) the volume in prison, by the favour and enlargement of God. [And if it be not presumptuous so to say, it would not be easy to find a copy equal to this copy.]

This inscription in the autograph manuscript of Pamphilus, which has, of course, itself long since disappeared, must have been written about the year 309, since Antoninus was martyred on 13 November of that year, and Pamphilus on 16 February 310. It will be seen that the collating of the manuscript with the Hexapla needed two persons, and since we are told that it was Pamphilus who made the actual corrections in his manuscript (and thus naturally writes the inscription in the first person), it follows that Antoninus assisted by reading the manuscript against which it was being collated—the original Hexapla. Here, then, we have a certain example, not indeed of dictating for the purpose of copying, but of something very nearly akin—dictating for the purpose of checking a copy already made. Moreover, if dictation was used for the purpose of collating, there is, I think, a certain presumption in favour of its having been used *in the original writing of the text.* It will be noticed that Pamphilus records that his manuscript was not merely collated with, but had originally been transcribed from, the Hexapla. If Antoninus played the part we have supposed in the process of collation, is it not inherently probable that he also dictated from the Hexapla when the manuscript was being written in the first place?

Be this as it may, this inscription is of great interest, as it disposes of one of the commonest arguments urged against the dictation theory, namely the extreme inaccuracy which would result. In the case of the Pamphilus and Antoninus manuscript, we are dealing with a volume which was the work of two Christian scholars of great learning and reputation, working in one of the most illustrious centres of Christian erudition. Is it probable that they would have employed, for the collation of their manuscript, a process which has been stigmatized as liable to inaccuracy?

It does not of course follow that collation of this kind was always done by two persons. Obviously there is no practical difficulty in one and the same scribe collating and correcting at the same time; and such work by a single person seems to be hinted at in the colophon to one of the works of Irenaeus, preserved to us by Eusebius (*Hist. Eccl.* v. 20. 2):

> ὁρκίζω σε τὸν μεταγραψόμενον τὸ βιβλίον τοῦτο, κατὰ τοῦ κυρίου ἡμῶν Ἰησοῦ Χριστοῦ καὶ κατὰ τῆς ἐνδόξου παρουσίας αὐτοῦ, ἧς ἔρχεται κρῖναι ζῶντας καὶ νεκρούς, ἵνα ἀντιβάλῃς ὃ μετεγράψω, καὶ κατορθώσῃς αὐτὸ πρὸς τὸ ἀντίγραφον τοῦτο ὅθεν μετεγράψω, ἐπιμελῶς, καὶ τὸν ὅρκον τοῦτον ὁμοίως μεταγράψῃς καὶ θήσεις ἐν τῷ ἀντιγράφῳ.

Taken together, these two passages suggest that Zucker is right in holding that both visual copying and copying from dictation existed side by side.

Before we leave the Codex Sinaiticus, there is one further point to which I wish to direct attention. We have seen that there is evidence which suggests that the manuscript may have been written from dictation; can we conjecture *why* dictation was employed for the production of this particular volume? Normally, this would be a hopeless question to which to expect an answer. But in the present instance there is, as it happens, an explanation which is at least plausible. In the year 332 Constantine wrote to Eusebius of Caesarea, ordering him to supply fifty vellum Bibles, written by experienced professional scribes, for use in his new churches in Constantinople (Eusebius, *De Vita Constantini*, iv. 36. 37). From the time of Tischendorf onwards critics have suggested that the Codex Sinaiticus (and perhaps the Codex Vaticanus also) may have formed part of this historic consignment. Milne and I did indeed adduce some evidence tending to show that the Sinaiticus was written in or near Caesarea, but hitherto a serious objection to identifying it with one of the Constantine Bibles has been the fact that, so far as we can judge, it has never been near Constantinople, for it was almost certainly in Caesarea in the sixth or seventh century.

Is it possible that the manuscript was written in Caesarea to Constantine's order, and for some reason never dispatched? Here we must reflect on the general circumstances of the case. Constantine was a military man, accustomed to instant obedience to his orders; he was moreover liable to sudden fits of anger, in which he committed appalling cruelties. Perhaps, therefore, we can sympathize with Eusebius' feelings on receiving the Emperor's letter, and noting that the fifty

Bibles were to be prepared 'as soon as possible' and conveyed to the capital in two specially commandeered wagons? Faced with the task of getting scribes to copy something like 200,000,000 uncial letters, he would no doubt have impressed every scribe whose hand would pass muster, and put them to work almost day and night.

In this connexion we may re-examine the words which Eusebius applies to the finished Bibles: τρισσὰ καὶ τετρασσά. All sorts of explanations have been put forward for these apparently mysterious terms; thus it has been suggested that the words mean that the Bibles were written with three (like the Vaticanus) or four (like the Sinaiticus) columns to the page; that they were copied in quires of three or four double leaves; that they were multi-volume Bibles, in three or four parts each; or—even more fantastic—that they were polyglot Bibles in three or four languages—or harmonies of 'three or four' Gospels! A good summary of these competing explanations was published in 1939 by Carl Wendel in *Zentralblatt für Bibliothekswesen*, lvi, pp. 165–75 (but without mention of Kirsopp Lake's article in the *Harvard Theological Review*, xi, 1918, pp. 32–35).

Hitherto, critics have been baffled because they expected the words to represent some mysterious technical terms of ancient bibliography; but as Monsignor Devreesse has pointed out in his *Introduction à l'Étude des Manuscrits grecs*, p. 125, all the explanations so far suggested have been too ingenious. The word τρισσός, which occurs frequently in papyri in such phrases as τρισσὴ γραφεῖσα, means a document written 'in three copies'; but in the case of Eusebius the verb in connexion with which the words occur is not γράφειν; the exact phrase is: τρισσὰ καὶ τετρασσὰ διαπεμψάντων ἡμῶν. The meaning is thus '*dispatched* in three or four copies', which can only mean 'sent off three or four at a time'.

We can now see why Eusebius mentions the fact. Constantine clearly expected all fifty Bibles to be sent off together. Why then should they have been dispatched 'in threes and fours'? The answer, I think, lies in the urgency of the occasion. Knowing the Emperor's character, and realizing—as the Emperor doubtless did not—the enormous task before him, Eusebius may well have sent off the volumes as and when they were completed, instead of waiting for the last one to be finished and risking an outburst of the Emperor's wrath.

All these circumstances fit in perfectly with the signs of haste, and changes of purpose, which characterize the Codex Sinaiticus. And if it was never sent to Constantinople, it was no doubt because for some reason it was not considered up to standard—it is in fact in many respects unfinished. Possibly the spelling of Scribe B, when detected, was alone sufficient to ensure its rejection!

The foregoing is, of course, purely speculative, and it is unlikely that we shall ever be able either to prove or disprove the connexion of the Codex Sinaiticus with the order of Constantine. But at least we have been able to envisage a situation in which, above all others, dictation is likely to have been used to enable the manuscripts to be produced in the shortest possible time.

5. The Morgan Iliad

Our next task must be to see whether the lessons we have learned from the Codex Sinaiticus can be applied to other manuscripts of the ancient world. At first sight, especially with the thousands of literary papyri which have been recovered from Egypt, there would seem to be a fruitful field for investigation. The result is, however, disappointing. The number of manuscripts which can be examined on the same scale as the Codex Sinaiticus is restricted, for their evaluation depends upon the assessment of a very large number of individual passages. The manuscripts chosen must therefore be of considerable extent, which at once rules out the great majority of surviving literary papyri; and they must *not* be correctly written, since such works could be the result of accurate copying by either dictation or visual copying. Another obvious requirement is that the manuscript must have been completely published, or at any rate collated, since a collation which omits phonetic and other seemingly unimportant errors deprives us of precisely the type of information required.

One of the few manuscripts on papyrus which does fit the requirements is the codex in the Pierpont Morgan Library, containing Books 11–16 of the *Iliad*, written early in the fourth century A.D. by a scribe whose hand varies from a rough literary type to an almost pure cursive. A description and collation of the papyrus was published in 1912, in the *Sitzungsberichte* of the Berlin Academy, by Wilamowitz and Plaumann, and the latter then gave it as his opinion that the codex had been written from dictation, on account of the phonetic errors with which it abounds. These errors include: wholesale interchange of unaccented α, ε, ο, and ι, e.g. νηος for νηας, αρπαξαντα for αρπαξαντε, ειλετε for ειλετο, κοινομενοιο for κινυμενοιο, ητηρ for ητορ, γενωτ' for γενετ', ερεξατο for ορεξατο. Interchange of ο and ω is rampant, also of ο and ου, as το περ for του περ, ουδε for ο δε. What is commonly called itacism, i.e. confusion of η, οι, ι, ει, and υ, is of course very frequent, as ητοι for ηδη, ρηιδιοι κρημνη for ρηιδιη κρημνοι, νηω for νειω, ηποντι for ειποντι, οιτε for ειτε, κλοιναντες for κλιναντες, εσσητ' for εσσυτ', σοιμενιν for σημαινειν. This last example also shows the confusion of ε and αι which is so marked a feature of the work of Scribe B of the Codex Sinaiticus. Similarly, we find καιβριονης for κεβριονης, επινυ for αιπεινη, εταιρη for ετερη, μενειτε for μαινηται, τεδεισθαι for τ' αιδεσθε, and countless other examples. More unusual is the occasional interchange of ε and η, as πηλεξ for πηληξ, ρινης for ρινες. The diphthong αυ was already weakening at this period, hence we find ατε for αυτε, ναμμαχα for ναυμαχα, and so on. Other changes, such as υ and ω in νυνυμνους for νωνυμνους, or ου and οι as in επι ου for επι οι, ουδε for οι δε, του or τω for τοι, are less explicable phonetically, and may be partly due to visual error on the part of the dictator. Confusion of liquids provides some striking examples, as ερυ for ελη, πολ for περ. Mistakes in aspiration, or the reverse, are responsible for εξαπαποιτο for εξαπαφοιτο, εγχεσφαλος for εγχεσπαλος, παμβαινον for παμφαινον, αφ Ασκαλαφου for απ Ασκαλαφου, πλοχαμους for πλοκαμους, τριγλωγιν for τριγλωχιν, χοχ αριστον for κ' οχ αριστον. Lack of distinction between σ and ζ produces ρισαν for ριζαν, χεζονται for χασσονται, and others.

As already pointed out, some of the foregoing errors may derive from misreadings of the exemplar by the dictator. Other examples perhaps so explicable are παχυν for ταχυν, περα for τα ρα, νεμεληγερεταο for νεφεληγερεταο, φοιβης for φορβης, αυται for αυταρ, ηθεντον for ηθελετον, θεμινα for θεμελια. Confusion of pairs of basically similar letters, such as ε and θ, α and λ, give errors like και δεχθι for και δ' εχει, or αλλεν for λααν.

The above is only a much abbreviated selection, and does not exhaust the types of error. Some can only be classed as blunders pure and simple: δαιφρονα for δικ φιλον, πεπονηκοταθ' ιππους for πεπονητο καθ' ιππους, απαβειμενος for απαμειβομενος, οπισθεν for ολεσθαι, χοος πολ εων for θοος περ εων, and similar monstrosities.

I do not think there can be much doubt that Plaumann was right in conjecturing the manuscript to have been written from dictation. The extent and depth of the errors and corruptions are so great that it is difficult to conceive a scribe so transforming a text by the mere process of transcription. We may, I think, reasonably regard it as a mass-produced copy designed for sale to the semi-educated Graeco-Egyptians of whom Dioscorus of Aphrodito is so notable an example two centuries later.

It is possible that similar results might result from the examination of Coptic papyri. The great Coptic scholar, Amélineau, has pointed out (*Œuvres de Schenoudi*, i, 1907, p. xxix) that Coptic scribes of the present day customarily write from dictation, and he considers that many medieval Coptic manuscripts were similarly produced. Not being a Coptic scholar myself, I do not feel able to express an opinion, but I may perhaps refer to two papyri in the British Museum for which at least some data are available. The first is the famous fourth-century papyrus codex containing the books of Deuteronomy, Jonah, and the Acts of the Apostles. Budge, in his edition (*Coptic Biblical Texts in the Dialect of Upper Egypt*, pp. xxxi–xxxviii), notes that in Acts phonetic errors become very frequent. The main classes of error are the coalescing of similar or near-similar sounds, as in ⲛⲉⲩⲟⲉⲓϣ for ⲛⲉⲟⲩⲟⲉⲓϣ, ⲉⲩⲏⲛ for ⲉⲩⲟⲩⲏⲛ, ⲧⲟⲧⲟⲩ for ⲧⲟⲟⲧⲟⲩ, ⲛⲉϩⲟⲟⲩ for ⲛⲉⲓϩⲟⲟⲩ, ⲁⲩⲁ for ⲟⲩⲁ, ⲉⲩⲟⲩⲇⲁⲓ̈ for ⲉⲩⲓ̈ⲟⲩⲇⲁⲓ̈, ⲧⲁⲡⲟⲗⲟⲅⲓⲁ for ⲧⲁ ⲁⲡⲟⲗⲟⲅⲓⲁ. Loss of a guttural at the end of a syllable results in ϩⲁⲧⲏⲩ for ϩⲁϩⲧⲏⲩ (four times), ⲡ ⲙⲁϣ ⲙⲟⲩⲛ for ⲡ ⲙⲉϩ ⲙⲟⲩⲛ. Interchange of stops is found: ⲧⲉϥⲡⲓⲡⲉ for ⲧⲉϥϯⲡⲉ, ϩⲁⲡ for ϩⲁⲧ, ⲟⲩⲙⲡⲟⲛ for ⲟⲩⲙⲧⲟⲛ, ⲁⲩⲧⲱϣ for ⲁⲩⲡⲱϣ, ⲉⲧⲟⲗ for ⲉⲃⲟⲗ. Lastly, there is interchange of ϫ with ϣ on the one hand (ϣⲙ̅ⲡϣⲟⲙⲧⲉ for ⲇⲙ̅ⲡϣⲟⲙⲧⲉ) or ϭ on the other (ⲉϭⲱ for ⲉϫⲱ, ⲁϥϫⲱ for ⲁϥϭⲱ).

It will be seen that the variety and extent of error here found is nothing like so widespread as in the Greek documents quoted, and clearly more evidence is required before dictation can be assumed.

The second papyrus I have in mind is also a papyrus codex, Or. 5001, containing a collection of homilies by fathers of the church, in Sahidic, written probably in the sixth century. Budge has edited the texts, but unfortunately gives no details of the orthography. Crum, however, in his *Catalogue of Coptic Manuscripts in the British Museum*, p. 64, remarks that 'the peculiarities of orthography, which are found throughout the whole manuscript, may be taken as an indication of a single scribe writing probably from dictation. They are: (1) continual interchange of ⲕ and ϭ, ⲃ and ϥ; (2) the use of ⲁ for ⲉ; (3) the doubling of vowels, ϣⲁⲁ, ⲙⲏⲏⲧⲉ, ⲟⲩⲟⲟⲙ; (4) ϣⲱⲥ for ⲥⲱϣ; (5) ⲥⲁ for ⲛⲥⲁ; (6) ⲙϣⲁ for ⲙⲡϣⲁ and some other similar phenomena.

These do not, indeed, add up to as impressive a total as do the corresponding statistics of the Greek manuscripts, but the tendency is the same; and the extraordinary conservatism of Egyptian technical processes makes it probable that, if dictation was common in the sixth century A.D., its history goes back centuries before that date.

6. The Early Middle Ages

W. A. Lindsay, in his *Palaeographia Latina*, ii, pp. 28–29, speaks of 'the few eccentric persons' who still believe that manuscripts (i.e. in the early Middle Ages) were written from dictation. He demolishes what has been claimed as literary evidence for the practice of dictation by pointing out that 'dictare' means 'to compose', not 'to dictate', while 'legere' means 'to read' (a text for the purpose of revision, not to dictate a text to a scribe or scribes). As a general principle, Lindsay was no doubt right; but it does not therefore follow that all manuscripts, in all centres, at all times during the period, were visually reproduced. Thus Lindsay (op. cit., p. 10, n. 1) speaks contemptuously of Conway and Walters, the editors of Livy for the Oxford Classical Texts, for daring to suggest the possibility of dictation. One manuscript which Conway and Walters had in mind was certainly the famous Mediceus of Livy. I will not repeat here the extraordinary wealth of phonetic and other errors which characterize the manuscript, for Conway and Walters (vol. i, pp. viii–x) have given an admirable summary of the chief types. We may perhaps notice the editors' suggestion that the dictator suffered from a stutter, because twice in the space of a few lines (iv. 7. 7 and iv. 12. 4) we find the scribe of the Mediceus writing *ante tribuni* for *an tribuni*, while in between (iv. 10. 1) we are given *rerepente in obsonium* for *repente inops omnium*.

However, instead of concentrating on this well-documented case, I would prefer to take an author where the question of dictation has not, so far as I know, been even suggested. The *Historiae adversum Paganos* of Orosius is extant in an enormous number of manuscripts, only a selection of which are quoted by Zangemeister in his edition in the Vienna *Corpus*. The two manuscripts which stand out at once from the rest are L, the Laurentian, an uncial manuscript of the sixth century, and D, the Donaueschingen manuscript, written in the eighth. Both are full of phonetic and other mistakes, but D in particular (like the Medicean Livy) is marked by a large number of singular errors. The scribe seems to have had no visual impression of Latin, and mistakes of cases, genders, conjugations, &c., are innumerable. One finds, for instance, *habire* for *habere, fecirunt* for *fecerunt, templas* for *templa*, false concords like *isto iudicium, mare nostro, ab Adam primum kominem, ab initium, sine dubium*, and so on. In the phonetic field the scribe seems utterly indifferent to the use of *o* or *u*, writing *oxoro* for *uxorum, nus* for *nos, urbis* for *orbis, tutius* for *totius, ocio* for *otium, uciano* for *oceano*—even *ot* for *ut*.

In some words, like *insola, pericolum, o* seems to have permanently replaced *u*. Confusion of *e, ae*, and *i* are almost equally common: *hospetaletas* for *hospitalitas, poenae* for *paene*, and the various spellings of *Italia*, to quote only a few. But it is the more deep-seated corruptions which are significant; these include cases where the scribe has (on the dictation theory) misheard a sound, has inserted or omitted syllables, wrongly divided or connected words, or achieved other more complicated deformations. Such cases are: *qui aliud tota lique cursu* for *qui tali ortu talique cursu, a circione urum* for *a circio in eurum, audisse molent* for *aut dissimulent, magimae* for *maxime, refugiunnontium* for *refugia montium, filio melae* for *Philomelae, crucientius* for *cruentius, malos suorum* for *Molossorum, fame a et uita* for *fame ac metu ita, secundam* for *se quondam, uiri* for *tueri, caera* for *cetera, per dito nouerat* for *perdetonuerat, adistis sacrisque* for *adytis arisque, etcentissimo* for *sescentesimo, culo funi* for *Colophonii, uergigen tor ex* for *Vercingetorix, Caesaria nomirum* for *Caesarianorum, magnine potest* for *Magni nepotes*, to quote only a selection.

One error which, at first sight, suggests visual copying rather than dictation is the following: *sine uictore captiuitatem, sine crimine exilium, sine uictore dominatum*, where the correct reading is *sine* bello *captiuitatem*, &c., indicating that *uictore* has been imported from the third member of the clause. While this would be explicable on the basis of visual copying, it is equally possible that the eye of the dictator slipped down a line or two, and that he either did not notice his mistake, or at any rate failed to make the scribe understand what had happened. There are actually a large number of visual errors, generally arising from mistakes in the reading of *i, m*, and *n* separately or in combination. E.g. *inox* for *mox, agi inne* for *agmine, ni emium* for *Memmium, ad omnis* for *a dominis, Dainasyppus* for *Damasippus, atronie* for *at Romae, ima ni* for *imagini*. Misreadings of letters or syllables probably explain *Brotiiquae* for *Boeotiique, Canenser* for *Cannensis, uictuae* for *uictoriae, nanci* for *pauci, recoruisque* for *securosque*. Some especially interesting cases indicate where the reader of the text had difficulty in deciding whether adjacent vowels were a diphthong, or whether a new word began between the two letters. Cases where he decided wrongly are: *demersa & triginta* for *demersae triginta, Cinna egerunt ffriga* for *Cinnae gener in Africa*. But the most illuminating, to my mind, is *arminia et postquam arminii postquam* for the correct *Armeniae postquam*. Here it would seem that the dictator first divided the words incorrectly (or at any rate took the *e* of *Armeniae* for an *et*) but then realized his mistake and re-read the text correctly, while the scribe, not realizing what had happened, wrote down both versions. A similar explanation may underlie the extraordinary *mo dolam ineihui huitans*, for *modulamine inuitans*; the wrong division between *dolam* and *ineihui*, and the repetition of *hui*, indicate that here again the dictator made a second shot at the phrase, and that the scribe put down both, introducing some modifications of his own into the bargain.

To turn to a different class of literature, we may cite the *De Re Rustica* of Columella. The text of this rests mainly upon two early ninth-century manuscripts, S and A, which are very closely connected and must descend from the same archetype. Both manuscripts are marked by an extraordinary profusion of orthographic and other errors which render them specially useful for our purpose. Fortunately, too, there is now available not only an exhaustive critical edition, but also a valuable study of the manuscripts by Å. Josephson of Uppsala (*Die Columella-Handschriften, Uppsala Universitets Årsskṛift*, 1955, 8). Josephson himself (p. 37, n. 40) rejects the dictation theory as applicable to either S or A, but I do not think the question can thus be settled out of hand. The fact seems to be that, just as fifty years ago an editor stumbling on a text full of phonetic errors automatically concluded that it had been dictated, so nowadays he, equally automatically, attributes the phenomenon to 'self-dictation' by the copyist. In other words, there has been not an increase in knowledge but merely a change in fashion.

I will quote here a small selection of readings which, in my view, suggest either that both S and A were copied from dictation, or that their common archetype had been so produced. It is therefore desirable that, where they differ, I should quote both. The correct reading is given first:

Paestique. S: festaque. A: festi.
potanti ueniat. SA: potati ueiat.
locis temperatis. S: locis terrae ratio. A: locis p̄ ratio.

is pasceret. S: ipsasciret. A: ipsas geret.
iuniperus. S: erius. A: imperius.
celeritate bestiam. S: celeriter autem bestii. A: celeriter atem bestii [this error arises from a reduplication of -ate at the end of celeritate].
constrata. A: constra. S: contra.
pabulatur. A: rabulatur. S: ambulatur.
uapores. S: pores. A: tempores.
iubae. S: be. A: uel.
decedere. S: decetere. A: de cetera.
ab hora secunda. A: arbor asecunda.
cuiuslibet uasti alitis. S: cuiuslibet uastialitas. A: cuiuslibet bestialitas.
generosissimaeque. S: generosis eque. A: generosis seque.
grati apibus. SA: gratia pius.
ab latere talea. S: alba ter et alea. A: alua teret alea.
contumaciter. S: coitu aciter. A: coitur aciter.
ab opere. SA: alueo fere.
cum deinde conspicere desiit apem tum. SA: deinde conspicere possit apertum.

Even when all possible allowances are made for such factors as self-dictation, misreading of letters in archaic or unfamiliar script in the exemplar, &c., it seems difficult to believe that such perversions as the above can in all cases proceed from visual copying.

7. The Later Middle Ages

As time advances, references to, and representations of, scribes at work gradually increase in frequency, and leave us in no possible doubt that during this period visual copying was the normal, if not the only method employed for the production of manuscripts. Wherever, therefore, evidence appears which seems to suggest dictation, we should expect this to be coupled with some unusual circumstance which led to the rejection of visual copying.

Everyone knows of the instant and extraordinary fame achieved by the Divina Commedia, and the vast number of manuscripts of it which sprung up, almost overnight. Here, then, we have a work for which there was an insistent demand on a vast scale, and for which speed of production, if not actual mass-production, would naturally become all-important. There is, in fact, an anecdote of an Italian who produced a hundred copies single-handed for the purpose of providing dowries for his daughters. We are not told that he produced the copies by dictation, or, if so, who did the dictating (by rights the work should have fallen to the daughters), but in the circumstances resort to dictation would not be in the least surprising. It is worth noting that Edward Moore, in his *Contributions to the Textual Criticism of the Divina Commedia*, p. xix, remarks: 'the great frequency of such blunders in some manuscripts raises the suspicion that they were written from dictation'; and he singles out especially the manuscripts denoted E, K, and M as possible examples of dictated texts. Of E, a Canonici manuscript in the Bodleian, he writes,

> I have suspected, more strongly than in almost any other MS. oral dictation in this case. This seems to be indicated by: 1. The very great frequency of dialectic forms and peculiarities, which would at least be likely to be multiplied by such a method. 2. Strange and unintelligent blunders in specially difficult passages, proper names, or unusual words. 3. Lines are frequently defective from the omissions of words, especially in the middle of the line, but without any indication of omission, as if the copyist had not always kept up with the dictator. 4. Extreme irregularity and variability of orthography. 5. Some special blunders which seem most naturally explicable on the theory of dictation, e.g. *de contristi* for *dicon tristi, torvi a Firenza* for *toglier via Fiorenza, ela era abbarbicata* for *ellera abbarbicata, pur sol* for *pesol, del tuto* for *di liuto, perche nui for per cenni.*

Moore adds that in the last example quoted *ch* was sounded as soft in the dialect of Venice, in the neighbourhood of which the manuscript is known to have been copied.

I would also quote a general observation of Moore's on what he calls the 'short-sightedness' of scribes, who, when in a difficulty, rarely looked beyond the limits of a single line, and constantly introduced verbal emendations which could at once have been seen to be unnecessary had they taken a larger view; though Moore does not say so, this is obviously a state of affairs which would have been fostered by dictation, since the dictator would naturally read out the text a line at a time.

My other example of a possibly dictated text comes from the other end of the Mediterranean—a Greek Gospels of mid-thirteenth-century date, probably written in Egypt or Syria to judge from the Arabic notes found in it. In general appearance this manuscript, now in Paris, offers nothing unusual, and this impression seems to be confirmed by the contents, since the Gospel of St. Matthew reveals the common Byzantine text with no special peculiarities. The remaining three Gospels, however, are a complete contrast; not only does their basic text show numerous affinities with the Neutral family exemplified by the Codices Vaticanus and Sinaiticus, but every page is disfigured with countless faults of orthography. A. Schmidtke, who has made an elaborate study of the manuscript (*Die Evangelien eines alten Unzialcodex*, 1903), came to the conclusion that it was copied, by dictation, from a very ancient uncial which was either too badly damaged or too unfamiliar in script to be easily deciphered, so that resort was had to the unusual course of copying it from dictation. The types of errors represented are fully analysed by Schmidtke, and I need only give a few typical specimens here. Itacism in the ordinary sense is of course exceedingly common, resulting in such readings as ποιησις σημειον for ποιεις συ σημειον. Interchange of ε and αι is also the order of the day, a striking example being και for κ̅ε̅, the dictator having missed the overline denoting the *nomen sacrum*, and thus dictated 'κε'. Omission of γ produces πιημη for πυγμη, κηρυμα for κηρυγμα. Often an initial vowel is duplicated, as ηιησους for ιησους, or οιεισιν (pronounced ιισιν) for εισιν. Sometimes whole syllables are duplicated, as ταταπεινωσιν, αβρααμαμ, διδιδασκαλος. Final ν is omitted, as χρονο, οινο, or is wrongly added, as φοβουν, ουν, for φοβος, ου. Beginnings or ends of words are sometimes omitted: εχον(τι), οσ(α), ε(γω), (ειπ)εν, (αε)τοι, (νου)ου. Pure blunders of all kinds occur, as δοθησεται for αρθησεται, διαπανησας for δαπανησας, δημασθαι for δαμασαι, ποτηριον for σωτηριον, πολιν for παλιν, αλαλαζων for αλαλον. The foregoing, I would repeat, is only a brief selection, and does not cover the hundreds of purely visual errors, presumed to be due to misreading of the exemplar by the dictator.

8. The Modern Age

The invention of printing did not immediately bring to an end the copying and circulation of manuscripts. In the Near East, for example, Greek manuscripts continued to be copied down to the late nineteenth century. And even elsewhere special circumstances sometimes necessitated the circulation of works in manuscript. One example of this is the works of English Catholics after the Reformation, for though many of them appeared in print from continental or clandestine presses, not all achieved this immortality, and it is to one of these latter that I wish to draw attention. This is the *Life of Sir Thomas More* written by Nicholas Harpsfield, the Catholic cleric and controversialist, who died in the Fleet Prison in 1575. This *Life*, printed in 1932 for the Early English Text Society in a magnificent critical edition by R. W. Chambers and Dr. Elsie Hitchcock, is extant in eight manuscripts, one of which, Yelverton MS, 72, was recently acquired with the rest of the Yelverton collection by the British Museum. Written in a bewildering number of different hands, it is also remarkable for a large number of singular variants which suggest that it may have been written from dictation. Examples are: *Right Chancellour* for *highe Chancellour; sonnday* for *suddayne; Christianes* for *Christmas; after* for *often; parlyament* for *payment; thenten* for *the intent; suing nothing* for *answering nothing; hapton* for *hampton; receyue* for *recite; lettere* for *latine; died* for *did; course and* for *cursed and; incease* for *incense; wise* for *wayes; according* for *awarding; matter* for *maner; speak* for *spreade; horeibly* for *honorably* [a very striking error]; *comforted* for *conformed; as sewer any* for *a sure ayme; hurt* for *hitt; most* for *woorst; toung* for *though; writtin* for *wittie; and Estomachin* for *our Eustochium; holy* for *whole* (and vice versa); *biggine* for *begging; St. Talbons* for *St. Albans; latine* for *letanie; burdened* for *burned; mone* for *boone; shot* for *sought; naturall* for *materiall; wreathe* for *reache; bookes* for *bones*; and *Calline* for *Cateline*. All these, I would repeat, are variants which occur in *no other manuscript*, and which suggest to me at least the strong possibility that they are the outcome of dictation. The manuscript is now numbered Add. MS. 48066.

9. Conclusion

It now remains to attempt to form some conclusions, or at least to show the ways and means of further investigation which would seem most likely to produce fruitful results. Before so doing, however, I should like to make one general observation which is important and which nevertheless may not be thought self-evident. Because a manuscript is full of errors due to dictation, it does not follow that it gives us a bad text; on the contrary, in most of the manuscripts which we have been considering in detail, the misspellings and other blunders are purely superficial, and when they are stripped off, the resultant texts are very good indeed. There may even be some truth in the seeming paradox that a manuscript copied from dictation can actually be *more* accurate than one visually copied.

If it is asked whether it is of importance to know if a manuscript has been visually copied or dictated, I would reply that in my view there is a great gulf fixed, both practically and psychologically, between the two methods. The scribe copying visually can range over the exemplar at will, he can gain a complete image of a passage, and look either forwards or backwards in search for a clue to the meaning; nor is he troubled by any need to keep up with the speed of the dictator. The scribe writing from dictation, on the other hand, is in a fundamentally different predicament; he depends entirely on a single, fleeting, auditory image for the production of his text, and if he mishears the chances of his rectifying, or even realizing, the mistake are small. All he has before him at any one time is the small section of text with which he is currently concerned: he cannot look forward to see what is coming, and as he must keep up with the dictator, he has little or no time to see what has gone before.

It might be thought from the foregoing that the two systems, visual copying and dictation, being so fundamentally different in character, would produce two readily separable types of error, so that we could tell after a very short examination the method by which a particular manuscript had been reproduced. But this is not the case. The scribe copying visually may commit visual errors through misreading the exemplar, or audible errors through self-dictation. The scribe copying from dictation may reproduce visual errors of the dictator, or himself commit phonetic errors through faulty hearing. In short, both types of copying are liable to both species of error. But are they then indistinguishable?

To this vital question, I would suggest that the answer is, that over and above the types of error which are common to both systems, there is a third type due to what we may call lack of liaison between dictator and scribe, and which is peculiar to the dictated manuscript. In this article we have tried to identify characteristic examples of these misunderstandings, or rather the textual errors resulting therefrom, and if the interpretations suggested are correct, we have a positive clue to the identification of a dictated manuscript.

Much work, however, remains to be done. I have stressed the fact that for far too long the question has been debated in the absence of the manuscripts. Nor can progress be easy or rapid; on the contrary, my view is that only by the patient accumulation of small details can we hope, over an extended period, gradually to build up a picture. And in such investigations the manuscripts must be treated as individuals—it is useless, for example, to collect errors of a given type from a variety of manuscripts and then lump the results together, as in palaeographical manuals of the type of Louis Havet's outstanding *Manuel de critique verbale*.

Are there any clues of a different character to the identification of a dictated manuscript? With all reserve, I would suggest the following point as deserving further investigation. While identical visual errors and identical phonetic errors may be made by different scribes, the mistakes due to lack of liaison between scribe and dictator are more likely to be different in each case. As a result, a dictated manuscript may be expected to contain a larger or smaller number of *singular* errors; and this is in fact the case with most of the manuscripts which have been examined above. If, then, we come across a manuscript which reveals singular errors, there may be a certain presumption in favour of its having been dictated; and if this observation can be fully tested and corroborated, it may turn out to be a touchstone of the greatest value.

2

EARLY CHRISTIAN BOOK-PRODUCTION: PAPYRI AND MANUSCRIPTS

Prehistory of the Christian Book: Papyrus and Parchment

The discoveries of the present century have completely revolutionized our ideas of the early Christian book and its ancestry. Handbooks written thirty years ago, or even less, are now largely obsolete, and it is only today that it is becoming possible to envisage the basic problems which have still to be solved. This advance in knowledge has been all the more dramatic because no early Christian writer has anything to tell us about the way in which Christian, or indeed any, books were written and circulated. Nor are pagan writers of the contemporary Graeco-Roman world much more informative: in common with the general paucity of technological literature, no treatise on ancient book-production has come down to us, and we have had to glean what knowledge we could from casual references and allusions, often incomplete or ambiguous.

Now, however, the picture is altered to the extent that finds of papyri, predominantly in Egypt, have provided us with hundreds of specimens of works of literature produced during the period in which Christian literature was born: and, still more recently, the astonishing discoveries in the deserts of Palestine have revealed numerous examples of the types of books and writing materials with which the earliest members of the Church would have been familiar and which they would have used themselves in daily life.

Three distinct types of writing material, papyrus, parchment, and wooden tablets, contributed, though in very different ways, to the formation of the Christian book, and all were in common use in Palestine and most of the Near East during the first century A.D. The first which we shall consider is papyrus. This legendary material, once used so widely throughout the whole of the ancient world that Pliny describes it as co-existent with civilization, has, after its virtual eclipse during the middle ages, once more become familiar through the tens of thousands of examples which have come to light in Egypt, mainly during the last hundred years. With the aid of these specimens, and numerous modern experiments, we can now form a much better picture of the method of its manufacture than we could from the *locus classicus* in Pliny's *Natural History*, in which he attempts to describe the process in language which is neither as clear nor as precise as could be wished.

The papyrus plant, *Cyperus Papyrus* L., is a species of reed which once grew in the greatest profusion in Egypt, particularly in the marshes of the Nile Delta, and also in other parts of the Near East, including Palestine, where it is still to be found in the neighbourhood of Lake Huleh. Today, ironically, it has completely died out in Egypt, and can only be seen there either in the Cairo Botanical Gardens or, immortalized in stone, in the papyrus columns beloved of the Egyptian architect. The plant grows with its roots submerged in water, from which the jointless stem, triangular in section, rises to a height of 10–15 feet, ending in a tuft of flowers. For the manufacture of papyrus the plant was cut down and the stem was divided into sections, the length of which determined the height of the papyrus roll which was to be made. From these sections the outer rind was stripped off, and the soft pith, while still fresh, cut lengthwise into thin strips. These strips were laid side by side, slightly overlapping, on a hard surface, and a second layer was laid over them, the strips running at right angles to those in the first layer. The two layers were then consolidated by hammering and pressing, and then dried. The sheet thus formed was then trimmed, and the surface smoothed with pumice and burnished with rounded polishers of shell or ivory. Finally, a number of sheets were pasted together with flour paste to make long lengths which were then rolled up for storage or transport.

Newly made papyrus was white in colour, or nearly so, although it yellowed with age, like paper. Dio Chrysostom (A.D. 30–117) records how booksellers artificially 'aged' papyrus books by plunging them in wheat, to yellow them and give them an appearance of antiquity. Specimens now to be seen in museums vary in colour from a very pale yellow or beige to a deep brown or purplish-black, the last-named being characteristic of papyri which have been affected by damp and partially carbonized. On the whole, the thinnest and finest papyri are the earliest, one New Kingdom specimen measured by Professor Černý being only 0.1 mm. in thickness; by contrast, some papyri of the Byzantine period are almost as thick and stiff as card.

The individual sheets of papyrus varied greatly, both in height and width, the broadest sheets being considered the hall-mark of the finest quality. According to Kenyon, an average size of sheet during the Graeco-Roman period would be 25 cm. high and 19 cm broad, the former figure representing the height of the roll as finally made up. The joins of the sheets were so skilfully made as to be almost invisible, and certainly scribes paid little attention to them, carrying the writing across the junctions without any apparent difficulty.

Papyrus was always rolled up in such a way that the horizontal fibres were on the inside and thus not subjected to strain, while the vertical fibres, which naturally had more 'give', were on the outside. The side with the fibres running horizontally was the one intended to receive the writing, and as such was more carefully smoothed and finished. It is customary to describe this side as the 'recto' and the side with vertical fibres as the 'verso' in order to distinguish them. It is an axiom of papyrology that scribes always used the recto of the papyrus first, and the verso was only written on, if at all, after the recto had been used. It is very rare for the same work to be continued from the recto to the verso; much more frequently the verso of a discarded roll was employed, as a cheaper form of writing material than new papyrus, for the reception of a different work altogether, and lengthy tax-registers or accounts, which would be discarded after a fairly limited period, formed a prolific source of this second-class writing material. The famous roll of Aristotle's *Constitution of Athens*, for example, is written on the back of a roll containing agricultural accounts.

Details of the development of Greek writing must be sought in the manuals of palaeography, but some idea of the general appearance of a typical Greek book at the beginning of the Christian era may conveniently be given here, if only because it differed so greatly from the book of today. The text consisted of a succession of columns of writing, the lines of writing within the columns being parallel to the length of the roll. To those familiar with the exquisite regularity of the finest medieval manuscripts most papyri present a relatively unsophisticated appearance. The Graeco-Roman scribe wrote entirely by eye, without the aid of any ruled lines either to guide the writing or to mark the borders of the columns, which in fact often slope markedly from left to right. Nor was much trouble taken to 'justify' the lines (to bring them to a regular margin on the right), though a filling-mark (>) was sometimes used for this purpose. Although it is sometimes stated that scribes used the horizontal fibres of the papyrus as guides to keeping their lines of writing straight, this is not borne out by the papyri, which often show the scribe writing at a slight angle to the fibres. The truth is that the fibres of the papyrus tend to mask defects in straightness and regularity, whereas a smooth and fiberless material like vellum highlights such imperfections.

In the columns of writing the text ran on continuously, without any division of words and few, if any, accents or breathings and little or no punctuation. Any kind of aids to the reader such as capital letters, italics, divisions of text, cross-headings, title-pages, lists of contents, indexes, footnotes, illustrations, bibliographies, etc., were entirely unknown. In addition to these (to us) shortcomings, the physical difficulty of reading from a roll has often been emphasized. The reader needed both hands for the purpose, the right to hold the roll, the left to hold the initially unrolled portion, and to roll it up as the reading proceeded. Cumbersome though this sounds, long practice probably made it an automatic process; certainly, as we shall see, ancient readers in general were in no hurry to adopt what seems to us the infinitely more convenient codex type of book. Finally, when the reader came to the end of the roll, he had to re-roll it in the reverse direction in order to make it ready for the next reader; as ancient authors never make mention of this essential 'chore' one suspects that it was left to servants or slaves.

Few subjects are more obscure than the methods of ancient book-production. We do indeed hear of booksellers, and it is clear that production on a commercial scale existed; for example, Cicero's friend Atticus was an active publisher and kept a large staff of slaves to produce copies of books. And apart from individual publishers, the great libraries such as that at Alexandria also functioned as centres of book-production. But of practical procedure we know nothing for certain. It has been confidently asserted, and just as energetically denied, that an 'edition' was produced by means of one person dictating the 'copy' to a roomful of slaves writing simultaneously; but clearly dictation would give no advantage in the case of single orders. Possibly both dictation and visual copying were employed according to the needs and circumstances of the case. How the scribe carried out his task is again a matter for conjecture: there is virtually no evidence for the use of chairs, tables or desks, and it would appear that the scribe sat on a stool or even on the ground and rested the section of the roll on which he was writing on his knee, holding the remainder of the roll with his free hand.

The date of the invention of papyrus is unknown, but its use in Egypt can be traced back to the fourth millennium B.C., and it retained its predominant position in that country until long after the Arab conquest in 640–5. Although the decline and eventual extinction of the papyrus industry in Egypt is generally ascribed to the rivalry of paper, which finally replaced it in the tenth or eleventh century A.D., it is in fact difficult to establish whether the dying out of the papyrus plant was the result, or the cause, of the disappearance of the material. During the whole of this immense period of time almost no change can be detected in the method of manufacture, except a very gradual decline in quality.

From Egypt papyrus was exported, from a very early date and certainly centuries before the Christian era, to many parts of the ancient world, and the only reason why so few papyri have been found outside Egypt is that apart from a few exceptions, such as the Dead Sea caves, it is in Egypt alone (and then only in certain parts of the country) that the soil and the climatic conditions are dry enough to enable it to survive. A few papyri written in neighbouring countries have been discovered in Egypt and give us valuable information about writing habits in their countries of origin, but for the most part inferences have to be drawn from, for example, linguistic evidence, representations on monuments, impressions on clay sealings, and the like. In Assyria papyrus was certainly in use as early as the eighth century B.C., since the word used by the Assyrians to denote papyrus has been found in texts of that date. This papyrus was no doubt imported from Egypt, although some centuries later (perhaps under the Seleucids) the papyrus plant was introduced into Mesopotamia and papyrus was presumably manufactured there. Papyrus must have been equally well known in Syria and Palestine, and in fact the Murabbaʿat cave has produced a Hebrew papyrus, written in Phoenician script, which has been ascribed on palaeographical grounds to the seventh century B.C.—the oldest Semitic papyrus in existence. In later centuries the conquests of Alexander and the subsequent incorporation of Palestine in the empire of the Ptolemies, who ruled it from 304 to 200 B.C., must have greatly fostered the use of papyrus imported from Egypt, and indeed a number of Greek papyri written in Palestine in the middle of the third century B.C. have come to light in Egypt.

Before we leave the subject of papyrus, two major misconceptions, often reflected in older handbooks (and in some of more recent date), must be cleared away. The first is the supposition that papyrus was relatively expensive, and that its everyday use was restricted accordingly. In fact, the prices which have come down to us suggest that a roll of papyrus was by no means an expensive commodity; and in any case the lavish manner in which papyrus was often used, with wide margins and large unwritten areas, shows that the cost of the material was never a limiting factor. Furthermore, although, as has been pointed out, inscribed rolls or sheets of papyrus could easily be re-used either by washing off the original writing or by writing on the verso, this expedient was employed in only a minority of cases, and many discarded rolls which could have been used in this way were thrown away on the rubbish-heaps of Oxyrhynchus and elsewhere. The truth is that the consumption of papyrus in the ancient world was on a scale which almost passes belief. The celebrated Egyptian story of the travels of Wen-Amon (*c.* 1090 B.C.) represents him as carrying 500 blank rolls of papyrus 'of the finest quality' to Phoenicia to barter for wood. From a papyrus account of 258/7 B.C. we learn that one section of the accounting staff of Apollonius, the Finance Minister of Ptolemy II, received and used 434 rolls of papyrus in 33 days; this, moreover, was merely part of the travelling staff which accompanied Apollonius on his tours of the provinces, and not the permanent Treasury staff at Alexandria, the requirements of which must have been infinitely greater.

Another misconception which it is equally necessary to dissipate is the idea that papyrus is a particularly fragile material, of very limited durability. It is true that papyri which have survived to the present day, after centuries of desiccation, although they may be handled with reasonable care, can be crushed to powder between the fingers. But all the evidence indicates that in its original state papyrus was at least as durable as the best hand-made paper, if not more so. This proposition could be supported by numerous examples, of which only a few can be quoted here. Pliny, for instance, speaks of having seen autograph letters of the Gracchi, which must have been some 200 years old, while Galen mentions having handled rolls 300 years old without suggesting that they were in any way fragile or that they were indeed anything out of the common. The famous 'find' of manuscripts of Aristotle at Scepsis, where they had been hidden in a cellar to save them from the attentions of the Attalid kings of Pergamum, was followed by their transport to Athens, where they were seized by Sulla and carried off as spoils of war to Rome, subsequently forming the basis of the edition of Aristotle's works by the philosopher Andronicus of Rhodes in the middle of the first century B.C.: thus, despite their vicissitudes, the manuscripts, which must have been on papyrus, remained usable for over 250 years. Finally must be mentioned the specimens, rare it is true, of papyri which have survived in Western Europe, the most ancient of which are documents from Ravenna written in the fifth century A.D.; although these papyri have been continuously above ground since the time of their creation, they have survived to modern times without any of the benefits of present-day conservation techniques. But the most striking example of all of the durability of papyrus is of a different kind: this is the fact that the Qumran leather scroll of Samuel (4QSam[a]), when beginning to deteriorate, was strengthened on the back with a strip of papyrus, which has helped to preserve it. Yet we are continually informed that parchment and vellum are greatly superior to papyrus in durability!

The myth of the fragility of papyrus can thus be discarded once and for all, and, as we shall see, other grounds must be sought for its gradual replacement by parchment and vellum as the principal, and eventually the sole, material for book-production.

At this point a few words may be said about pens and inks. The pen used by the ancient Egyptians was a slender rush, *Juncus maritimus*, the end of which was cut at an angle and then chewed in the mouth, producing something like a very fine brush. With this simple implement the Egyptians produced miracles of craftsmanship both in their hieroglyphic writing and their vignette illustrations. The Greeks, on the other hand, invariably used, at least as early as the third century B.C., a reed with a much thicker stem, *Phragmites aegyptiaca*, the end of which was cut to a point, forming a nib, which was then slit as in modern pens. The Romans used the same reed-pen, which has remained in use in the East down to the present time. Metal pens with split nibs have also been found on Roman sites, perhaps as substitutes in areas where suitable reeds were not available.

The most ancient form of ink is undoubtedly that employed by the ancient Egyptians from time immemorial, made from carbon, obtained as lamp-black or soot, mixed with thin gum to hold it in suspension and provide adhesion. The Egyptians used this in the form of solid cakes which were ground up and mixed with water just like the present-day Indian or Chinese ink. Owing to its totally inert composition this ink is not subject to fading and, as the oldest

Egyptian papyri prove, is virtually everlasting. A later invention is the metallic-based ink, usually made from an infusion of oak-galls mixed with green vitriol (iron sulphate). This ink undergoes chemical changes which can, in course of time, liberate minute quantities of sulphuric acid which may eat right through the writing material. It has sometimes been suggested that metallic inks were introduced specifically for writing on parchment, the greasy surface of which gives poor adhesion for carbon inks, but this explanation is not borne out by the evidence. Traces of metallic ink have, for instance, been found on the Lachish ostraca of the sixth century B.C., whereas the ink of the Dead Sea scrolls is mainly, if not entirely, carbon. The Talmud prescribed the use of carbon ink for writing the books of the Torah, and this practice has been followed for writing the Torah down to the present day, although metallic ink came into general use among Jews of the middle ages. Practically all Greek papyri use carbon ink, but from the fourth century A.D., and perhaps earlier, Greek parchment manuscripts used metallic ink: notable examples of the use of metallic ink are the *Codex Sinaiticus* and the *Codex Alexandrinus*; the latter has sustained serious damage as a result of the ink eating through the parchment. The general canon enunciated by Driver (*Semitic Writing*, 1954, p. 89) that carbon ink was used for parchment and metallic ink for papyrus, however true it may be for Semitic manuscripts, is almost exactly the reverse of the practice of the Greek scribes.

The second basic type of writing material to be considered is the group formed by leather, parchment and vellum. These must be taken together, since they merely represent different methods of utilizing the skins of the smaller quadrupeds, mainly sheep, goats and calves, for writing material. The terms parchment and vellum are virtually indistinguishable, since though by derivation vellum means a preparation from skins of calves, it is now customarily used as a generic term, irrespective of the source of the material. Parchment (Latin *pergamena*) owes its name to the kings of Pergamum in Asia Minor, one of whom, the bibliophile Eumenes II (197–158 B.C.), is credited with having invented it during a temporary shortage of papyrus. Pliny quotes this story on the authority of Cicero's contemporary Varro, but our confidence is somewhat shaken when he follows it up with another quotation from Varro, to the effect that papyrus was invented after the founding of Alexandria by Alexander the Great! However this may be, parchment is a convenient term since it is not linked with any particular animal, and it will accordingly be used in the succeeding paragraphs.

The difficulty of differentiating between leather prepared for writing and parchment is illustrated by describing the normal process of manufacture, which has changed little over the centuries. After flaying, the epidermis, with the hair or wool, is removed from the outer side of the pelt, and the flesh from the inner, after soaking in a bath of lime. This is followed, in the case of leather, by tanning; but for parchment the skin, after liming, is washed, placed in a stretching frame, and allowed to dry. It is then shaved on both sides with a heavy iron knife to the required thickness, smoothed and whitened with pumice and chalk, and finally trimmed. The fineness of the resulting product depends upon the extent to which the reduction by shaving is pursued. The skin or dermis consists of three layers, the outermost being known from its granular appearance as the grain layer, the next one below, containing the roots of the hair follicles, as the papillary layer, and the innermost layer, next to the flesh, as the reticular layer or corium. In the finest quality parchment the two outer layers are completely removed, leaving only the reticulan layer. Today, skins are split into layers by machinery, but in antiquity the reduction had to be effected by laborious scraping. Possibly it was this final reduction to the reticular layer which constituted the innovation of Eumenes.

Parchment has two sides, known as the 'hair side' and the 'flesh side'. The hair side, which was originally towards the outside of the skin, is clearly distinguishable in the coarser types of parchment by its yellow colour, rougher surface, and clearly visible remains of the hair roots. By contrast the flesh side, the original inner side, is whiter and smoother. In the case of documents, therefore, where only one side of the parchment has to be used, it is usual to write on the flesh side because of its better appearance, much as the writer on papyrus used the recto. Despite the superiority of the flesh side, it is usually the hair side, with its rougher and more absorbent surface, which holds the ink better than the smooth and shiny flesh side, from which ink tends to flake off. Often, when the leaves of an ancient manuscript are turned over, revealing alternate openings of flesh side and hair side, there is a surprising difference of legibility in favour of the hair side.

Despite the predominance of papyrus, leather rolls for written records were occasionally used in ancient Egypt, the earliest example known being of the sixth Dynasty, though most date from the New Kingdom. In the Persian empire leather was certainly in use in the fifth century B.C., since the Greek historian Ctesias speaks of the 'royal skins' on which the acts of the Achaemenid kings were chronicled. Actual examples of Persian parchments have survived through the discovery in Egypt, in the early 1930s, of a leather bag containing some twenty letters, of which thirteen were complete or nearly so. All were written on parchment, in Aramaic, and were addressed to an official of the Persian administration in Egypt; though undated, they can be assigned to the later years of the fifth century B.C. A number of the letters emanated from ʾAršam, the Persian satrap of Egypt, and what gives them an especial interest is that ʾAršam was not in Egypt at the time, the letters being written in Babylon or Susa. We know from the Aramaic papyri which have been found in Egypt that ʾAršam used papyrus whilst in that country, and we may perhaps infer that the Persians when in their homeland had a definite preference for parchment, since they could, of course, have perfectly well imported papyrus from Egypt had they wished to do so. This preference for parchment continued into the Parthian period, from which have survived three documents found at Avromān in Persian Kurdistan: these comprise one bilingual, in Greek and Middle Iranian, dated 88 B.C., one wholly in Greek, dated 22–21 B.C., and one wholly in Iranian, dated 12–11 B.C. Similarly at Doura on the Euphrates parchment is the normal material for documents in both Greek and Aramaic until the Romans captured the town in about A.D. 165; thereafter papyrus becomes the commonest material, and is used exclusively by the Roman military authorities until the town was captured and destroyed by the Persians in 256.

The foregoing documents are of non-literary character, and are written on separate pieces of parchment, often roughly prepared. They show no trace of ruled lines or margins, nor of any special preparation of the material for writing, and thus give us little or no idea of the probable appearance of contemporary literary works.

In any case, these discoveries have now been completely eclipsed by the astonishing finds in the Dead Sea caves and elsewhere in the Judaean desert. These sites have now produced fragments, some extensive but for the most part very small, of nearly 800 manuscripts, said to range in date (with a few exceptions) from the end of the third century B.C. to the second century A.D. All, where ascertainable, are in the form of rolls, and the great majority are on skin or parchment, though a small proportion, which fluctuates considerably from cave to cave, are on papyrus. The main body of texts are in Hebrew or Aramaic, in various scripts, but there are a few in Greek, both on parchment and papyrus. It is only quite recently that specimens of the scrolls have been subjected to technological examination, with interesting results. The methods of manufacturing the skins and preparing them for writing have been found to correspond, in remarkable degree, with the directions incorporated in medieval rabbinic literature. The skins were not steeped in lime, indeed lime was not used at all; instead, the skins were cured with salt and then treated with flour and other vegetable substances to remove the hair, clean the substance, and loosen the fibre structure. Three kinds of skin were distinguished, the first being whole-hide leather, while the other two were formed by splitting the skin into an inner and outer layer. After the processes of salting and flouring already described, all three types of skin are stated to be 'tanned', but in fact this 'tanning' amounts to no more than brushing over the surface, on both sides, with a gall-wood dressing which coloured it a dark yellow-brown. The object of this dressing was said to be to improve the surface for writing and to make erasures and alterations difficult, thus protecting the integrity of the text.

The rabbinic rules also prescribed which religious writings were to be written on each of the three different kinds of parchment, and also which side of the skin was to be used for writing in each case. Thus, the whole-hide skin was reserved for the Torah which must be written on the hair side. Of the split skins, the outer skin was be inscribed on the flesh side, and the inner skin, which was presumably, as we have seen, the finest material, was to be inscribed on the hair side. Horizontal lines to guide the writing, which hung from the lines, and vertical lines to mark off the margins, were ruled with a dry point, a practice which scribes of the third century A.D. regarded as an essential feature of a manuscript, and of which they traced the origin back to Adam, which at any rate shows that it was no recent innovation. This ruling is in sharp contrast to the practice of scribes on papyrus, who, as already stated, needed no such aids. To form rolls, the separate skins were sewn together; whereas medieval rabbinic regulations prescribe the use of sinews for this purpose, the sewing in the Dead Sea scrolls appears to be of vegetable origin. Although these joins were made very neatly, they are inevitably much more prominent than those in papyrus rolls, and scribes consequently avoided writing across them.

The stage is now set for considering the beginnings of the Christian book. If we consider the everyday world in which the earliest Christians lived, we might have expected that they would adopt as the vehicle for their literature either the parchment scroll of contemporary Judaism, or the papyrus roll universal throughout the Gentile world, or both. But in fact they did neither of these things: in this, as in other matters, the men who 'turned the world upside down' had different ideas.

The Origin of the Codex

Today the codex form of book, that is, the book with separate leaves secured down one side, and with writing on both sides of the leaf, is virtually universal, and was so throughout the middle ages. The story of its ultimate origins is a long one, and the stages by which it gained this ascendancy are complex. There can be no possible doubt that the form of the codex derives from the multi-leaved writing tablets used by both the Greeks and the Romans. The classic form of Graeco-Roman writing tablets consisted of two or more (the largest number known is ten) thin rectangular wooden boards, held together down one side by means of strings passing through holes pierced near the inner edge. The inner surfaces of the boards were slightly hollowed out, and the cavities filled with a thin layer of black wax. On this wax, writing was traced with a metal stylus. This device formed an ideal vehicle for rough notes and memoranda, as alterations or deletions could be effected with the greatest ease by reversing the stylus and using its flattened end to smooth the wax and enable the correction to be written; or the whole surface could be smoothed, thus obliterating the writing, and enabling the tablet to be used again and again.

Although most of our knowledge comes from Greece and Rome, waxed tablets were certainly used in other parts of the Near East, as is shown not merely by representations of them, in Neo-Hittite reliefs, but by the actual example of the magnificent ivory tablets, still bearing traces of their original green wax, recently found at Nimrud and dating from the eighth century B.C. Their distribution was thus extensive; but the Romans seem to have had a special predilection for them, employing them for permanent records such as wills and registrations of birth. And before the middle of the first century B.C. the Romans took what proved to be a momentous step: for the bank of wooden leaves, which they called a *codex* (from *caudex*, a log of wood), they substituted a bundle of sheets of parchment, sewn or tied together, which served much the same purpose and possessed decided advantages in lightness, portability and general convenience. The principle of indefinite reusability was preserved, since although the writing now had to be in ink, the carbon ink then in use could easily be washed or scraped off as required. These rough parchment notebooks, which the Romans called *membranae*, must have spread rapidly to the Near East, since it is virtually certain that it is notebooks of this type to which Paul refers in II Tim. 4:13, when he asks Timothy to bring with him, not only the cloak left behind at Troas, but 'the books, especially the *membranae*', his use of the Latin term confirming the theory that the parchment notebook was of Roman invention. (It is worth noting that the New English Bible at this passage has been sufficiently influenced by the results of the latest research to translate it 'the books, above all my notebooks'.)

From the rough parchment codex used for ephemera it may seem only a short step to the employment of a codex, whether of parchment or papyrus, for the permanent reception of literary works. But this step was slow in coming, and for centuries yet the public remained mesmerized by the papyrus roll to which it had for so long been accustomed. The first indications of the next step are to be found in certain poems of Martial written between A.D. 84 and 86. The poems in question are a series of distichs meant to accompany gifts exchanged by well-to-do Romans at the Saturnalia. The gifts include writing tablets, of ivory or costly woods, or, in one case, of parchment, this last providing us with another example of the parchment notebook. But the innovation consists in five couplets intended to accompany copies of famous books (Homer, *Iliad* and *Odyssey*; Virgil; Cicero; Livy; and Ovid, *Metamorphoses*), all of which are described as being written on parchment, and, in at least three cases and probably in all, in the form of codices.

Nearly all the distichs emphasize the compendiousness of the parchment codex (in tacit but obvious contrast to the papyrus roll), and the Cicero is specifically recommended for taking to read on a journey. Both these sentiments are echoed in another poem of Martial, advertising a revised edition of his own poems, in which he urges those who wish to possess his poems, and in particular to read them on a journey, to buy a copy of the new edition written in parchment codices, which takes up so little space that it can be held in one hand instead of needing a whole bookcase; and he concludes by giving the name and address of the bookseller from whom they can be obtained. Here then we have, for the first time on record, an instance of not merely a single copy, but an entire edition of a literary work being published in parchment codices.

Despite the efforts of Martial and his publisher, the venture does not seem to have been a success, and it is a long time before we hear again of parchment codices on any large scale. But the invention was not wholly forgotten, for we have a minute fragment of a page of a parchment codex containing a Latin historical work, which has been dated both on palaeographical and philological grounds to *c.* A.D. 100. We have also two single leaves from Greek parchment codices, one containing the *De falsa legatione* of Demosthenes, the other the lost *Cretans* of Euripides, which have been variously dated on the evidence of the script to the second century, to *c.* A.D. 100, or even to the late first century A.D.

The fact that, despite its obvious advantages, the parchment codex failed to secure a foothold indicates that the reading public of the Graeco-Roman world was conservative in its outlook (it is noticeable that Martial never commends his innovation as a *novelty*), and that, whatever possible advantages the parchment codex might have, they were simply not interested in the new form. But some other people were.

Christianity and the Codex

One possible reason why the parchment codex failed to catch on is that in the public mind parchment was associated with rough, untidy drafts or notes, whereas papyrus was traditionally the 'right' material for books. However this may be, a very short time after Martial's experiment someone conceived the idea of making a codex, not of parchment, but of papyrus. Where and by whom the idea was first tried out we do not know; but we do now know that the new form is directly connected with the earliest days of Christianity, and that the inventor may actually have been a Christian.

Realization of this fact has been slow in coming. Possibly the earliest hint of it is to be found in the article 'Writing' which Kenyon contributed to Hastings's *Dictionary of the Bible* as far back as 1902, and which includes the observation: 'There are signs, however, that it [the codex form] was early taken into use among the Christians for their private copies of the Scriptures. The evidence at present available is too scanty to justify dogmatism, but it is certainly the case that several of the earliest examples of the codex form contain Christian writings, and that the majority of the third century containing Christian writings are in the codex form.' A few years later and, it would seem, independently, C. R. Gregory in his *Canon and Text of the New Testament* (1907), pp. 322–3, put forward as 'a mere theory, a hypothesis', the suggestion that the change from the roll to the codex form, which he assigned to about the year A.D. 300, was motivated by the Christians: 'The theory touches the person or persons who made the change, who invented leaf-books. I am ready to believe that leaf-books are due to a Christian; that a Christian was the first one who felt the need of a change, and who effected the change.' And he goes on to suggest that the reason for the change was the need of the Christians to be able to refer quickly to different passages of Scripture when engaged in theological debates.

In assigning the change from roll to codex to about the year 300 Gregory added, 'a new papyrus may to-morrow show that the change came earlier'. This prophecy was fulfilled by later discoveries, above all by the finding, in about 1930, of the Chester Beatty biblical papyri. This group of eleven early Christian manuscripts, all on papyrus and all in codex form, and ranging in date from the early second century to the fourth, justified their editor, Kenyon, in observing: 'Not only do they confirm the belief that the Christian community was addicted to the codex rather than to the roll, but they carry back the use of the codex to an earlier date than there has hitherto been any good ground to assign to it.' Finally the whole question was investigated in depth in the magisterial monograph by C. H. Roberts, 'The Codex', in 1954. While it is true that the statistics quoted by Roberts need to be recalculated to take account of discoveries since 1952, these have not materially altered the general picture.

As Roberts shows, the most effective way of approaching the problem is to classify all extant fragments as coming either from rolls or codices, and to tabulate the results chronologically. Taking first pagan literature, Roberts gives the percentage of codices to rolls among fragments which have been dated second century as 2.31 per cent; among those dated second-third century, 2.9 per cent; among those dated third century, 16.8 per cent; among those dated third-fourth century, 48.14 per cent; and among those dated fourth century, 73.95 per cent. Thus, in the case of pagan literature, the codex barely existed before A.D. 200, and did not achieve a sizeable proportion until after A.D. 250.

When, however, we turn to Christian literature, the position is entirely different. If we take as a whole all the Christian biblical fragments which have been found in Egypt and which were written up to the end of the fourth century or not long thereafter, we find that, on the figures given by Roberts, these total in, sixty-two coming from the Old Testament and forty-nine from the New. Of these 111, ninety-nine are from codices and only twelve are from rolls. If, however, we examine the evidence more closely, we find the proportion of codices to be even higher than would at first sight appear. First, five of the twelve rolls are on the backs of rolls already written on the recto, that is, the scribe had no option but to adopt the form of the earlier writing, and their witness is therefore irrelevant. Secondly, of the remaining seven rolls, three are probably Jewish, and another three possibly so. Thus, out of over a hundred Christian texts, only one—a roll of the Psalms—is an unequivocal example of the roll form.

When we shift the emphasis to the earliest surviving examples of Christian papyri, the contrast with pagan literature is, if anything, even more sharply drawn. There are now at least eleven Christian biblical papyri which can be assigned to the second century, and at least another three or four which can be placed on the borderline between the second and third centuries. Of these fourteen or fifteen specimens, every one is a codex. The proportion of codices to rolls is thus 100 per cent, whereas for pagan literature during the same period the proportion is only 2.5 per cent. Despite the fact that the overall number of Christian texts is so much smaller than the pagan, the discrepancy remains overwhelming, and has been so consistently reinforced by further discoveries that it cannot possibly be the result of chance; and we must now seek the cause.

In the past, all sorts of reasons have been put forward to explain the Christian preference for the codex. Thus, it has been claimed that papyrus codices were cheaper than rolls because both sides of the material were used, and that most of the earlier Christians came from the poorer classes, to whom the economy would be a strong motive. Against this it should be pointed out that while it is true that none of the early Christian codices have the appearance of *éditions de luxe*, they equally reveal no attempt to make the most of the available space; and in any case, the supposed dearness of papyrus has already been shown to be mythical. Another argument is that codices were more convenient for peripatetic missionaries to carry about with them. As Roberts points out, this is an application to the Christians of the argument put forward (unsuccessfully) by Martial in urging the parchment codex upon his readers. In fact, a papyrus roll, when tightly rolled, as it customarily was, to a small diameter could contain a surprising amount of material: thus, a papyrus 6 m. in length could easily be rolled up into a cylinder no more than 5 or 6 cm in diameter, which could be comfortably held between the thumb and forefinger. If anything, a roll could probably be more conveniently carried in a fold of the garment than a codex with its projecting and vulnerable edges. Lastly there is the argument put forward, as we have seen, by C. R. Gregory, to the effect that the codex form was more convenient for quick reference in theological controversy. This is a pure hypothesis, and it is at least doubtful whether it could be justified on practical grounds. Without any system of chapter or verse divisions, finding one's way about the text would be no easier in a codex than in a roll, indeed a roll, in which the eye could survey perhaps four or more columns of writing at a glance, might well be the superior. Nor were Christians the only controversialists of the ancient world.

Roberts accordingly rejected all these would-be explanations, and sought, rightly, for a deeper and more compelling reason behind the Christian addiction to the codex. The solution he proposed was ingenious, and has found a wide measure of support.

In the first place, it must be remembered that the surviving examples of Christian codices are common provincial productions, and can in no circumstances be regarded as probable trend-setters. The origin of the Christian codex must therefore be sought in a period considerably earlier than the earliest surviving examples: as Roberts has pointed out, 'so universal is the use of the codex by Christians in the second century that the beginnings of this process must be taken back well into the first century'. This conclusion has lately been reinforced by the publication of a fragment from a papyrus codex of Genesis in Yale University Library (P. Yale 1) which the editor assigns to the late first century, 'perhaps between A.D. 80 and 100', thus making it the earliest Christian papyrus in existence. If this judgement is accepted, the origin of the Christian codex must be placed not later than A.D. 70. This condition is fulfilled by the solution propounded by Roberts, to which we now turn.

Roberts begins by arguing that Mark, when he came to write down his Gospel in Rome in or shortly after A.D. 70, would have employed the rough parchment notebook which, as we have seen, was in common use in Rome for notes and literary drafts. Roberts further suggests that the traditional association of Mark with the church of Alexandria reflects a real link between the Alexandrian church and the West, and that Mark's Gospel was the first authoritative Christian writing to reach Egypt. He further assumes that it was Mark's original autograph manuscript, in the parchment notebook, which so reached Egypt, and argues that it would have been regarded with such veneration by the Alexandrian Christians that copies taken from it would have been made in the same codex form, but utilizing the universal writing material of Egypt, papyrus. The papyrus codex, once established and backed by the authority of Alexandria in bibliographical matters, would have rapidly spread to other Christian writings both inside and outside Egypt.

As will be seen, this explanation involves acceptance of, not one, but a whole chain of hypotheses, all unproved and, in all probability, unprovable; and apart from this, there are several points about which doubts can be expressed. For instance, many other literary works must have started life as drafts in parchment notebooks and been subsequently transferred onto papyrus rolls, and it is not clear why the Alexandrian Christians should have felt the need to adopt any different procedure in multiplying copies of Mark's Gospel. Even if we accept Roberts's theory of the extraordinary reverence attaching to Mark's original manuscript in the parchment notebook, we have still to explain why, if format was of such vital importance as to compel adoption of the codex form, it did not equally compel adoption of parchment as the writing material. For the moment, at any rate, Roberts's theory cannot be regarded as more than a working hypothesis.

Whatever the explanation of its origin may be, the fact remains that the papyrus codex *was* invented, and that within a very short space of time it won acceptance as the only possible format for the Christian Scriptures. Such radical innovations are usually the work of individuals rather than committees—or churches—and we may perhaps imagine the invention as originating with some leading figure in the early Church, who, whatever the ultimate source of his inspiration, succeeded both in devising a distinctive format for Christian manuscripts of the Scriptures, differentiated equally from the parchment roll of Judaism and the papyrus roll of the pagan world, and in imposing its use throughout the Church. Here the reader may reasonably ask whether there is any other evidence pointing to the existence of such a dominating genius at work in the field of the earliest Christian literature. The answer is, surprisingly, yes.

Hand in hand with the papyrus codex goes a palaeographical peculiarity which, right from the earliest period, enables one to distinguish, almost at a glance, manuscripts of Christian literature from all others—the so-called *nomina sacra*. This term denotes certain stereotyped abbreviations, or rather compendia, for a limited number of words of divine significance or association, such as the Greek equivalents of 'God', 'Lord', 'Father', 'Jesus', 'Christ', 'Son', 'Man' (included through the influence of the term 'Son of Man'), 'Cross', 'Spirit', and a few others. These compendia are marked off from the surrounding text by a horizontal line above the letters, and one of them, $\overline{\text{IHS}}$ or $\overline{\text{IHC}}$ for 'Jesus', has survived to the present day. These compendia are found in virtually all Christian manuscripts, although some are so fragmentary that they provide no opportunity for the use of *nomina sacra*.

Why the early Christians should have taken this surprising step remains a mystery. Possibly it was a deliberate attempt to differentiate the Christian Scriptures from other literary forms, to mark them out as sacred books by investing them with a species of *cachet*. However this may be, the significant fact is that the introduction of the *nomina sacra* seems to parallel very closely the adoption of the papyrus codex; and it is remarkable that those developments should have taken place at almost the same time as the great outburst of critical activity among Jewish scholars which led to the standardization of the text of the Hebrew bible. It is no less remarkable that they seem to indicate a degree of organization, of conscious planning, and uniformity of practice among the Christian communities which we have hitherto had little reason to suspect, and which throws a new light on the early history of the Church.

Before we leave the papyrus codex, some technical points may be adverted to. The most primitive type of codex was that formed by piling the sheets of papyrus one on top of the other and doubling them over in the middle, thus making a single huge quire. The resultant bundle was held together by means of threads passing through holes stabbed right through the codex, not in the fold but some way inwards from it. If no precautions were taken, this produced a very awkwardly shaped volume, since the leaves near the centre of the book projected beyond those at the beginning and end, with consequent exposure to wear and damage; this defect could be overcome by cutting the sheets narrower and narrower as the centre of the book was approached, and examples of this are found, but it must have been a cumbersome process. Another defect of the single-quire codex was that the scribe had to calculate pretty exactly the number of leaves he required, since under- or overestimating would result in blank pages at beginning or end, with consequent waste of material. It is not therefore surprising that the alternative was tried of forming the codex of a number of quires, as in the modern book. In some cases, as in the Chester Beatty codex of Gospels and Acts, quires of only two leaves—a single sheet folded in half—were used, but larger quires, of six, eight, ten or twelve leaves, also occur at different times. These various arrangements overlapped for long periods, and no steady development can be traced. Eventually the single-quire codex faded out, and the multi-quire form, usually with quires of eight leaves, achieved universal acceptance; but by this time the papyrus codex itself had been superseded by the parchment codex.

In most papyrus codices the sheets were cut from papyrus already made up into rolls, with the result that the joins in the material are visible in the pages. An exception is the group of Coptic Manichean codices, which despite their present lamentable state were originally *éditions de luxe*, made up of individual sheets of papyrus of fine quality and specially prepared for writing on both sides. The question of sides, the so-called 'recto' and 'verso' of the papyrus, is important because it may be possible, from the order in which the sides follow each other, to infer the original contents of an entire codex from a few small fragments. In making up a single-quire codex, the natural method is to pile up the sheets one on top of the other with the 'recto' uppermost in each case. When a codex so made up is opened, one of the two leaves exposed to view will show the fibres running horizontally, the other vertically. This incongruity was clearly felt, since the practice arose of arranging the sheets with 'recto' facing upwards and downwards alternately, so that the opened book would show either horizontal fibres or vertical fibres on both of the facing pages. Similar variations are possible in the case of multi-quire codices.

Finally, just as the papyrus roll could be protected by being enclosed in a parchment sheath or *capsa*, so the papyrus codex needed protection from external wear and tear. No early papyrus codex in Greek has preserved any trace of a binding; but the great find of Coptic Gnostic papyrus codices made at Nag Hammadi in Upper Egypt in 1947 has provided us with no fewer than eleven leather bindings, all more or less intact, which enable us to form some idea of the external appearance of the earliest Christian books. These bindings, which are presumably of the same period as the manuscripts they contain, and thus range in date from the end of the third century to the beginning of the fifth, are in fact more like satchels or envelopes than bindings as we know them today. Many have triangular or rectangular flaps which cover the fore-edge of the manuscript, and to which long leather laces were attached, intended to be wound two or three times round the closed book. Within these covers the papyrus codices were attached with leather thongs.

Supremacy of the Parchment Codex

The change to the parchment codex now to be described is a complex one, since it affected Christian and non-Christian literature alike, and in the case of the latter involved not only the change of form from roll to codex but also the change of material from papyrus to parchment, whereas in the case of Christian literature the change was a straightforward one from the papyrus codex to the parchment codex. Moreover, all these changes were gradual processes and overlapped for considerable periods, with the result that, for instance, in non-Christian literature of the fourth century A.D. we find the papyrus roll, the papyrus codex, and the parchment codex all competing for popularity.

The complete dominance achieved by the papyrus codex in the field of early Christian literature, and its long survival, prove that it was a perfectly adequate form of book, and in the course of the second century it was apparently beginning to influence certain forms of non-Christian literature: there are eleven fragments of non-Christian papyrus codices which are assigned to the second century, though they are still enormously outnumbered by the fragments of papyrus rolls. During the third century there is a marked change in the situation. About one-sixth of the non-Christian texts are now in codex form, and of these some half-dozen are parchment codices. From the same century comes the earliest example of a New Testament parchment codex. But the real watershed is the year 300. The celebrated Edict of Diocletian (301), imposing a freeze on prices and wages, specified maximum rates of pay for scribes writing in parchment codices; this shows, better than any assemblage of fragments, how common the parchment codex was becoming. Then, in 332, we have the letter of Constantine the Great to Eusebius, bishop of Caesarea, ordering him to supply fifty vellum bibles for use in the new churches which he was building in Constantinople. These volumes were specifically ordered to be 'written on prepared vellum, easy to read and conveniently portable, by professional scribes with an exact understanding of their craft', and the letter makes it clear that no expense was to be spared. It is plain that by this date the parchment codex had come to be regarded as the supreme form of the Christian book, and superior to the papyrus codex, at least for such official and ceremonial purposes.

The triumph of the parchment codex is signalized not only by the literary evidence quoted above, but by the actual survival of two magnificent Greek bibles written at precisely this period—the *Codex Sinaiticus* and the *Codex Vaticanus*. It has even been suggested that these two great bibles are survivors from the consignment ordered by Constantine, and though this cannot be proved, and is in fact on the whole improbable, it is certainly true that they represent accurately the type of book which Constantine had in mind.[1] And although they are the only two parchment codices of the Bible to have come down to us from this period in a reasonably complete state, they are not isolated specimens. Indeed, in the latest list of manuscripts of the Greek New Testament there are at least sixteen fragments of other parchment codices written in the fourth century. From the fourth century also comes the most ancient manuscript of the Old Latin version of the New Testament, the *Codex Vercellensis*, in parch-

[1] But see chapter B7.

ment codex form, while in the field of pagan literature we have monumental parchment codices such as the *Codex Palatinus* of Virgil or the famous palimpsest of the *De Republica* of Cicero, a manuscript which resembles the *Codex Sinaiticus* in its combination of external magnificence and astonishing scribal lapses.

Nevertheless it must not be inferred that the supremacy of the parchment codex involved the disappearance of the papyrus codex. On the contrary, it displayed a remarkable vitality. In Egypt it remained in common use down to the sixth or seventh century, and even later. In the case of Greek classical literature it even seems to have staged a revival in the fifth century, the proportion of papyrus codices to vellum codices being almost twice as great then as in the fourth century. In the West, remnants of eight Latin papyrus codices, written in France or Italy, have survived all the hazards of the middle ages down to the present day. These codices, all containing Christian or legal texts, show that here also the papyrus codex long resisted the competition of parchment. It is true that Roberts quotes a letter written to Ruricius, bishop of Limoges, in the first half of the fifth century, in which the remark occurs 'a papyrus book is less capable [i.e. than a parchment one] of resisting damage, since, as you know, it deteriorates through age'. But this may be countered by the fact that when Cassiodorus, writing to the monks of Vivarium in Southern Italy about 550, says he is leaving them a manuscript of the Pauline epistles for them to work on and purify the text on the lines laid down by him, he specifically mentions that the manuscript was a papyrus codex.

As in the case of the change from roll to codex, all sorts of reasons have been put forward to explain the change from papyrus to parchment. For instance, it has been stated that parchment was cheaper than papyrus. But we have no information about the relative prices of parchment and papyrus at any period. Again, it has been suggested that papyrus was basically unsuitable for a codex, because it was difficult to fold, or cracked when folded. This is simply untrue, as is shown by the examples of papyrus codices which have survived more or less intact; by experiments with modern papyrus; and by the existence of a large number of private letters and other documents which for transmission have been folded up into extremely small shapes, and unfolded by the recipients without damage. This is, in fact, part of the more general claim that parchment was tougher, longer-lasting, and more resistant to damage than papyrus—a claim largely based upon the supposed fragility of papyrus, which has already been shown to be illusory. Some writers have even suggested that parchment was preferred to papyrus because it offered scope for manuscript illumination; yet Egyptian scribes for thousands of years had produced papyri illustrated with coloured drawings, and coloured illustrations do occur, though rarely, in Greek papyri.

Another possible explanation is the following. The sole source of papyrus, then as always, was Egypt, whereas parchment could be produced anywhere. The continued use of papyrus, in competition with parchment, thus depended upon uninterrupted commerce with Egypt. If the fall of the Western Empire caused increasing dislocations of such trade, parchment would naturally obtain the preference. This explanation does not, however, explain the replacement of papyrus by parchment within Egypt itself.

It will be seen, therefore, that it is very difficult to find practical reasons for the supersession of the papyrus codex by the parchment codex. One is almost driven to conclude that it is a mistake to search for a purely practical explanation, and that the need for a change of writing material may reflect some deeper, psychological cause, associated with the great changes which came over the ancient world in the fourth century. Possibly papyrus was seen, to an increasing extent, as a symbol of the old order which was passing away; if so, its survival into the sixth and seventh centuries for manuscripts, and much longer than that for documents, must be ascribed to sheer conservatism. Here, for the time being, the question must be left without any clear solution.

It now remains to give some account of the technical make-up and external appearance of the parchment codex, and for this purpose it may be convenient to take a single example of a manuscript which has been the subject of intensive study and analysis—the *Codex Sinaiticus*. This manuscript, now one of the greatest treasures of the British Museum, consists of parchment from both sheepskin and goatskin. The parchment is finely prepared and thin in relation to the size of the book. Originally the double sheets must have measured about 40 × 70 cm., so that when doubled over they formed pages 40 × 35 cm. The makers of parchment codices had learnt from the papyrus codex the disadvantages of the single-quire codex, so that all parchment codices, so far as is known, are in multi-quire format. The *Codex Sinaiticus* consists, with a few exceptions, of quires of eight leaves, a figure which remained the most popular make-up throughout the middle ages. In the quire, the sheets of parchment were arranged so that (*a*) flesh side faced flesh side and hair side hair side throughout the quire, and (*b*) flesh side was on the outsides of the quire. This arrangement became stereotyped in later Greek (though not Latin) manuscripts. The pile of sheets was then folded over to form the quire, and two vertical rows of small holes were pricked right through the eight leaves, near the fore-edge, to act as guides for the ruling lines. These lines were ruled with a hard point, always on the flesh side, so that they appear as raised lines on the hair side. The lines to guide the writing were ruled right across the double leaf, and then vertical lines were added to mark the margins of the columns of writing. Each page contained four narrow columns of writing, except in the poetical books of the Old Testament, which were ruled for two broad columns to the page. At a normal opening, therefore, eight narrow columns are presented to the reader's view, and it has often been claimed that this arrangement is derived from the succession of columns in a papyrus roll. The suggestion is, however, groundless, since in the first place the *Codex Sinaiticus* is exceptional in having as many as four columns to the page, most codices, whether papyrus or parchment, having only one or two, and secondly, narrow columns of the proportions found in the *Codex Sinaiticus* are by no means characteristic of papyrus rolls. After ruling, the writing area on each page was rubbed down with an abrasive to enable the ink to take a secure hold.

The quires were numbered to keep them in the correct order when the book was bound, but at this point our knowledge comes to an end, since neither the *Sinaiticus* nor any of the other great parchment codices have preserved any traces of their bindings. When Constantine wrote to Eusebius of Caesarea, as mentioned above, ordering bibles for the churches in Constantinople, Eusebius tells us that the manucripts were supplied in 'expensively worked containers' though it is uncertain whether this means bindings of the satchel- or envelope-type found on the Gnostic codices from Nag Hammadi, which could easily be given a more luxurious appearance by decorating the leather, or some kind of decorated book-boxes.

The *Codex Sinaiticus* is a fitting point at which to end this survey, since it represents in fully developed form the type of book which was to dominate Christianity for the next thousand years. Changes of scale indeed took place, from the huge bibles of the Romanesque period to the astonishing small bibles of the thirteenth century, with parchment pared thin as India paper and almost literally microscopic script; but the basic method of construction remained unaltered. Nor did manuscript illumination, with its panoply of decorated initials, borders and miniatures, affect the make-up of the books so embellished. Towards the end of the period, it is true, paper had begun to supplant parchment; but this change was far from complete when the final revolution took place—the invention of printing—and the manuscript book, which had moulded the minds of men for upwards of five thousand years, vanished for ever from the scene of everyday life.

Bibliography

The subjects studied in this chapter fall within the scope of what has in recent years come to be known as the science of codicology, i.e. the study of the material aspect of manuscripts as distinct from palaeography, which is concerned with the history and development of scripts. Indeed, 'The codicology of the early Christian book' might have been a more accurate, if somewhat pedantic, title.

Most of the best-known handbooks on palaeography and manuscripts, such as Sir Edward Maunde Thompson, *Introduction to Greek and Latin palaeography* (1912), or R. Devreesse, *Introduction à l'étude des manuscrits grecs* (1954), include sections on writing materials and die physical make-up of manuscripts, but generally speaking are not sufficiently up-to-date to take account of the striking additions to knowledge made in recent years. In English, the only comprehensive work dealing with the subjects of this chapter is Sir F. G. Kenyon, *Books and Readers in Ancient Greece and Rome* (2nd ed. 1951), which covers both classical and early Christian literature. Clear, accurate and eminently readable, it is nevertheless beginning to wear something of an old-fashioned look. An equally valuable survey, in German, is Wilhelm Schubart, *Das Buch bei den Griechen und Römern* (and ed. 1921); though containing much acute observation of permanent value, this is even more in need of modern revision, since the so-called '3rd edition' of the book, published in 1962, is merely a reproduction of the 2nd edition, shorn of its invaluable footnotes and references.

Probably the best all-round account at present available is to be found in that work of composite authorship, the *Geschichte der Textüberlieferung der antiken und mittelalterlichen Literatur* (Zürich, 1961). The two initial sections of volume 1, *Antikes und mittelalterliches Buch- und Schriftwesen*, by H. Hunger, and *Überlieferungsgeschichte der Bibel*, by O. Stegmüller, cover between them all the topics discussed in this chapter. The information is accurate, authoritative, and up-to-date, with references and bibliographies. A later section, *Überlieferungsgeschichte der lateinischen Literatur*, by K. Büchner, contains a valuable discussion on the transference of literature from roll to codex. A masterly summary of the present state of knowledge is provided by E. G. Turner in the opening chapter (Chapter 1: 'Writing Materials and Books') of his *Greek Papyri: an Introduction*, (Oxford, 1968).

As regards writing materials, J. Černý's inaugural lecture, *Paper and Books in Ancient Egypt* (1952), contains much useful information about papyrus and its use, which though related to the Pharaonic period is in many respects relevant to later ages. For the Graeco-Roman period N. Lewis, *L'industrie du papyrus dans l'Égypte gréco-romaine* (1934), remains indispensable. It is unfortunate that there are no similar works dealing with parchment, and indeed few of the writers who have attempted to deal with this subject have possessed the necessary scientific and technological qualifications. The first section of R. J. Forbes, *Studies in Ancient Technology*, vol. V (2nd ed. 1966), is concerned with leather in antiquity, and this includes a brief discussion of parchment (pp. 63–6), with valuable bibliography. Lastly, no comprehensive codicological study has yet been made of the Dead Sea scrolls, and none can be effectively undertaken until conservation and study of the material have reached a much more advanced stage.

For the codex, as stated in the text, the monograph of C. H. Roberts, 'The Codex', *Proceedings of the British Academy*, XL (1954), 169–204, is of fundamental importance. The arguments drawn by Roberts from the writings of Roman jurists have been carried further by F. Wieacker, *Textstufen klassischer Juristen, Abhandlungen der Akademie der Wissenschaften in Göttingen*, Phil.-hist. Kl., 3. Folge, Nr. 45 (1960), especially in his section 4, 'Rolle und Codex, Papyrus und Pergament', which deals comprehensively with the transition from papyrus roll to parchment codex, and its effects on literature; but this work must be used with caution, since it contains a number of careless misstatements. For the Yale papyrus mentioned on p. 71 see now the authoritative article by C. H. Roberts, 'P. Yale 1 and the early Christian Book' in *Essays in honor of C. Bradford Wells*, New Haven, 1966, pp. 25–8.

Lastly, for details of the bindings of the Nag Hammadi Gnostic codices (p. 74) see Jean Doresse, 'Les reliures des manuscrits copies découverts à Khenobaskion', *Revue d'Égyptologie*, XIII (1961), pp. 27–49.

3

TWO NOTES ON PAPYRUS

1. Was Re-rolling a Papyrus Roll an Irksome and Time-consuming Task?

Professor Montevecchi, who has herself made so many valuable contributions to our knowledge of antiquity, has recently pointed out[1] how little we really know of the ancient world, apart perhaps from Egypt, where papyrology has provided an additional dimension to our traditional sources. Nowhere is this lack of direct evidence more acute than in the fields of bibliography and codicology, and although it is tempting to supply the gaps in our knowledge with what seem to us reasonable conjectures and reconstructions, we must never overlook the risks involved. I make no apology for quoting once again the words of the late Professor Zucker on this very point: "Ich möchte überhaupt grundsätzlich bemerken, dass wir im Buchwesen in weit grösserem Ausmass, als man vielfach anzunehmen scheint, auf die Erwägung von Möglichkeiten angewiesen sind. Das Material ist gefährlich ungleichmässig, in mancher Hinsicht überaus reich, in mancher überaus dürftig. Vor allen muss man davor warnen, Lücken unserer Kenntnis auf Grund gewisser allgemeiner Vorstellungen auszufüllen, die uns selbstverständlich erscheinen".[2]

Many writers on the use of manuscripts in the ancient world have emphasised the difficulty experienced by the reader of a papyrus roll when he came to the end of the roll and was faced by the necessity to re-roll it for the benefit of the next reader; and it has often been claimed that this re-rolling was a troublesome and lengthy process. This view is perhaps best put by Schubart: "War der Leser am Ende angelangt, so hielt er sie als geschlossene Rolle in der linken Hand, wobei nun das Ende sich aussen, der Anfang sich innen befand. Das war freilich ein entschiedener Nachteil dieser Buchform, denn um die Rolle wieder für das nächste Mal benutzbar zu machen,

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[1] "Aevum", L (1976), pp. 83–4.

[2] "Gnomon", VIII (1932), p. 384. I had previously quoted this in "Proceedings of the British Academy", XLII (1956), p. 186 (= chapter A1).

musste der Lesende sie von neuem so rollen, dass der Schluss nach innen kam. Es mag ihm manchmal langweilig geworden sein, und wie wir wohl ein Buch aufgeschlagen liegenlassen, so mochte er auch die gelesene und verkehrt gewickelte Rolle, wie sie war, in den Bücherbehälter stecken".[3]

So far as I know, this suggestion has never been put to a practical test, and my own experience may therefore be of some interest. I took a roll of wallpaper, 52 cm. in height and about 10 m. in length, and cut it in half, thus producing two rolls, each 26 cm. in height and 10 m. in length and thus of about the same dimensions as a typical papyrus roll, though perhaps rather longer than the average. Taking one roll in my right hand, I pulled out part of it with my left hand and proceeded to roll this up, imitating the action of a reader in the ancient world. When the roll had been completely unrolled it lay, rolled up, in my left hand. I was now ready to begin the process of re-rolling.

According to Schubart,[4] "Wie die gelesene Rolle wieder im richtigen Sinne wickeln wollte, drückte das Ende unters Kinn, wobei sie natürlich herunterfiel, und rollte sie so zusammen". But what could be the object of holding the end of the roll under one's chin and letting the rolled-up portion fall down to the floor? This could be achieved much more easily by simply holding the free end in one's hand. Maunde Thompson is more explicit but equally impractical: "By the time the reader had read the entire roll, it had become reversed, the beginning being now in the centre and the end being outside; therefore, before putting it away, it must be rolled back into its proper form, a process which the idle man would shirk and the methodical reader would accomplish by holding the revolving material under his chin while his two hands were employed in winding up the roll. Hence Martial, i, 66 refers to 'virginis . . . chartae, quae trita duro non inhorruit mento', and again, x. 93 he has: 'Sic nova nec mento sordida charta iuvat".[5] The idea of holding the rolled-up roll under one's chin is in fact absurd, since even if one succeeds in gripping it between one's chin and chest, it cannot be unrolled without releasing it. In short, I do not think that either Schubart or Maunde Thompson can have tried to put their suggestions into practice; in

[3] W. Schubart, *Das Buch bei den Griechen und Römern*, 1962³, p. 98.
[4] Schubart, ibid.
[5] *An Introduction to Greek and Latin Palaeography*, Oxford 1912, pp. 49–50.

any case, it is by no means clear to me that Martial is referring at all to the process of re-rolling a roll.

I began my own experiments by resting the rolled-up portion on my lap, pulling out the free end and rolling it up with both hands while remaining seated. This proved quite practicable, and with my rolls took about 2 minutes, but involved a considerable amount of work, since the hands were kept moving during the whole period. I next tried holding the free end and letting the rolled-up portion fall to the floor. This proved to be a more successful method, since the portion of the roll between my hands and the rest of the roll on the floor gave a degree of tension which resulted in a neater and more tightly-rolled roll. It was also quicker, taking only some 70–80 seconds, but again the amount of work involved was considerable.

Thirdly, I laid the roll on a flat surface and pulled out a length of about 1 m. with my right hand. I soon found that the natural elasticity of the paper, and the fact that, like papyrus, it had been rolled up during manufacture and constantly kept so, caused it to roll up almost by itself, and only a touch of the hand was required as stretch after stretch of paper was pulled out and rolled up. No effort was required, as the secret is to make the paper itself do the work. After a little practice I found the process could be completed in about 45 seconds. The wallpaper I used was fairly thick (about 0.4 mm.) and I also experimented with a thinner paper, about 0.2 mm. thick, with much the same result. If the roll proves to be too loosely rolled, it takes only a few seconds to pull out the core of the roll and revolve it, thus tightening it.

Re-rolling the roll by simply reversing the procedure followed in reading, i.e. by holding the rolled-up portion in my right hand, pulling out the free end and rolling it up until the entire roll lay, rolled up, in my left hand, without using any form of support, proved to be much more awkward and laborious, taking about 3 minutes. I also experimented with a roll the end of which was glued to a stick with a wooden knob at each end, to represent the *umbilicus* and *cornua* known from allusions in Latin literature. I found this of very little assistance, either in 'reading' the roll or re-rolling it; perhaps this may explain why so few traces of *umbilici* have been found in papyri from Egypt. The knobs were certainly useful in tightening a roll that had been to loosely rolled, and no doubt helped to protect the edges of the roll to some extent.

I conclude therefore that re-rolling a papyrus roll presented no great difficulty to a reader, especially in an age when there was no demand for labour-saving devices. And the necessity for re-rolling is unlikely to have been an important factor in the long drawn-out battle between the roll and the codex. Those who stress the advantage of the codex form in this regard must in any case explain the existence of hundreds of thousands of rolls which have survived from the Middle Ages and even later; but this is another and larger question, too long to be debated here.

2. Last Words on the Question: Was an Adhesive Used in the Manufacture of Papyrus?

In his *Papyrus in Classical Antiquity*, pp. 47–49, with exhaustive bibliography, Professor Naphtali Lewis has disposed once and (one may hope) for all of the suggestion that the two layers of which papyrus is composed were held together by an artificially applied adhesive. We now know, of course, through modern scientific analysis that the juice of the papyrus reed itself contains a gummy substance so that the addition of an adhesive is in fact unnecessary.

What, then, is one to make of a letter of the 4th century Father St. Nilus[6] in which he speaks of writing-paper as composed of the papyrus-reed and paste (ἐκ παπύρου καὶ κόλλης χάρτης κατασκευασθείς)? It is certain that the Saint was not thinking of the paste used to join the individual sheets of a papyrus roll, since this could not be described as an essential ingredient of χάρτης, and in any case the context makes it clear that he had in mind, not a papyrus roll but a single document.[7] We must also note that the casual way in which the statement is made implies that it was common knowledge and thus familiar to the Saint's correspondent.

The explanation is, I would suggest, very simple, St. Nilus, despite his name, seems never to have visited Egypt and certainly can have

[6] Quoted by R. Devreesse, *Introduction à l'Étude des Munuscrits grecs*, 1954, p. 7, n. 5. I am not here concerned with the question of the authenticity of the letters in the Corpus, on which there is a considerable literature, cf. A. D. E. Cameron, *The Authenticity of the Letters of St. Nilus of Ancyra*, "Greek, Roman and Byzantine Studies", XVII (1976), pp. 181–96, and "Byzantinische Zeitschrift", LXXI (1978), pp. 10–11 and references there given.

[7] ἐκ παπύρου καὶ κόλλης χάρτης κατασκευασθεὶς χάρτης ψιλὸς καλεῖται, ἐπὰν δὲ ὑπογραφὴν δέξηται βασιλέως, δῆλον ὡς σάκρα ὀνομάζεται. (Migne, *Patrologia Graeca*, 79, col. 104); this statement is used as an allegory of Transubstantiation.

had no practical experience of papyrus manufacture. On the other hand he and his public would have known, as every papyrologist knows to-day, that the two layers of which papyrus consist adhere to each other with remarkable tenacity.[8] And since everyone knew that paste was used to fasten the individual sheets together it would have been a natural assumption that paste was used between the two layers of the material. In other words, what St. Nilus is repeating is a *popular fallacy*.[9] Anyone who has studied popular fallacies knows how tenaciously they are held and how difficult to eradicate, and the statement echoed by St. Nilus, which appeared to offer a rational explanation of an observed fact, is likely to have achieved an enduring popularity.

In the light of the foregoing we may look once again at the famous sentence in Pliny's description of papyrus manufacture, *turbidus liquor vim glutinis praebet*, 'the muddy water (of the Nile) provides the effect of paste'. Unlike St. Nilus, Pliny's informants would have been perfectly aware that no adhesive was used in the manufacture of papyrus, but they were faced by the same difficulty of explaining why the two layers adhered so firmly. Whether Pliny's informants, or the Egyptians from whom they ultimately derived their knowledge, actually believed that the water of the Nile possessed this magical adhesive property, or whether they concocted this explanation merely to satisfy inquisitive strangers, we cannot tell. But at any rate Pliny accepted the statement.[10] And it follows that the various textual emendations which have been propounded[11] are now seen to be wholly unnecessary: the text is perfectly sound.

[8] Schubart, op. cit., pp. 11–12.

[9] The best-known work in English is perhaps A. S. E. Ackermann, *Popular Fallacies*, 4th ed., London 1950. Philip Ward, *A Dictionary of Common Fallacies*, Cambridge 1978, has a useful bibliography on pp. 289–94.

[10] Lewis, op. cit., p. 49.

[11] Lewis, op. cit., p. 48, n. 16.

4

THE LENGTH OF THE STANDARD PAPYRUS ROLL AND THE COST-ADVANTAGE OF THE CODEX

Naphtali Lewis in his invaluable *Papyrus in Classical Antiquity* (Oxford 1974) in commenting on Pliny's well-known statement that in the manufacture of papyrus there were never more than twenty sheets (of papyrus) in a roll (*nunquam plures scapo quam vicenae*) remarks that there is an "a priori consideration that an industry which had standardized the sizes of the individual sheets of papyrus would also standardize the length (or lengths) of the rolls" (p. 55). There is in fact direct evidence from both the Pharaonic and Arab periods[1] pointing to 20 as the normal number of sheets to a roll, while from the Ptolemaic period there are references which indicate that there was a standard and well-recognised length for a roll. For instance, in P.Cair.Zen.59317 (250 B.C.) a correspondent asks for 4 rolls of papyrus (χάρται) for recording certain statistics, while in P.Lond.vii.2066, also from the Zenon archive, the writer states that 4 rolls will be sufficient for a transcript of a register of loans. Finally, the account of papyrus rolls given out in the secretariat of Apollonios the Dioiketes (P.Col.Zen.i.4) proves the existence of a standard roll, since otherwise any form of accounting would have been impossible.

It is the purpose of this note to suggest that during the Graeco-Roman period this standard length of roll was indeed the roll of 20 sheets mentioned by Pliny. It is true that we have references to rolls of 50 and even 70[2] sheets, but the fact that they are so described shows that these were not rolls of standard length. We also find references to rolls consisting of a varying number of τόμοι, viz. χάρτης τρίτομος, τετράτομος, ἑπτάτομος, ὀκτάτομος.[3] If this unit, the τόμος, could be identified with the standard length of 20 sheets, we would get the following schema:

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[1] Quoted by Lewis, ibid.

[2] 50 sheets: P.Cair.Zen.59054. 70 sheets: P.Oxy.ined.ap. E. G. Turner, The Typology of the early Codex, p. 54, n. 4.

[3] ἑπτάτομος in P.Lond.Inv.2110; references to the others are given by Lewis, op. cit., p. 77, n. 9.

Name	Number of τόμοι	Number of sheets
τόμος or χάρτης without qualification	1	20
χάρτης πεντηκοντάκολλος	–	50
χάρτης τρίτομος	3	60
χάρτης ἑβδομηκοντάκολλος	–	70
χάρτης τετράτομος	4	80
χάρτης ἑπτάτομος	7	140
χάρτης ὀκτάτομος	8	160

It is perhaps worth noting that the rolls of 50 and 70 sheets are are both rolls consisting of sheets that are not exact multiples of 20.

We can attempt to calculate the actual length of these rolls by taking the statement of E. G. Turner[4] that the commonest interval between kolleseis (and therefore the commonest breadth of the individual sheets) is 18 or 16 cm.[5] On this basis the standard roll of 20 sheets would have a length of 320–360 cm., an average of 340 cm., and the longer rolls in proportion, so that the longest (160 sheets) would measure 2720 cm.—much too long to be handled in one piece, but quite convenient if used as a stock of material for the writing of single documents or short rolls. We can attempt to test this hypothesis with the aid of P.London Inv.2110, a fragmentary account, dating from the first half of the 3rd century A.D.,[6] of the receipts of a professional scriptorium. The full text is published by K. Ohly, *Stichometrische Untersuchungen* (61 *Beiheft zum Zentralblatt für Bibliothekswesen*, 1928) pp. 88–90, 126–129). Two entries concern us here, and as printed by Ohly they read (col. i, 6–10):

Πτολ]εμαίου τ̣[. . . .]αδ̣ος καὶ Διονυσίου —
τοῦ Δι]ογένους τῶν Διδύμου προγραφῶν
] . . το Ἑρμογένους <μα
ὑπὲρ γράπτρ]ω̣ν τῶν αὐτῶν βιβλίων στίχων
Μ ς]χ ὡς τῶν $\overset{\alpha}{\mathrm{M}}$ <κη <μζ

[4] *The Typology of the Early Codex*, p. 48.

[5] This gives an average width of 17 cm. It is perhaps worth noting that the sheets of the *amphitheatrica* grade of papyrus, which Pliny apparently regards as that in everyday use, since he characterises it as *plebeia*, measured 9 Roman inches in width = 16.65 cm.

[6] Bell, in publishing extracts from the text in *Aegyptus*, ii, 1921, pp. 281–288 dated the papyrus to the end of the 2nd century, and this date is still sometimes quoted (cf. E.G. Turner, *Greek Papyri*, p. 87; *Greek Manuscripts of the Ancient World*, p. 1), but he subsequently revised this to the first half of the 3rd century A.D., cf. Ohly, op. cit., p. 88, n. 4.

I may add that the restorations in ll. 9 and 10 can be taken as certain.

As Ohly has observed (p. 127) the words τῶν αὐτῶν βιβλίων in l. 9 imply that the preceding item, costing 41 dr., must be something other than the cost of writing the texts in question, and this other expense can only be the cost of the necessary papyrus. How much papyrus would in fact be required to contain a text or texts running to 16,600 στίχοι? Obviously this would vary a great deal according to the style and size of the writing, but we can perhaps make a very rough approximation. If we take a random example, P.Oxy.xviii.2181, a roll of the Phaedo of Plato written in what is described as a compressed hand, we find that this contains about 60 στίχοι (of 16 syllables) to the column. The column is 10 cm. broad, and allowing 2 cm. for the intercolumniations means that 12 cm. of papyrus is needed for 60 στίχοι. The 16,600 στίχοι of P.London Inv.2110 would then require 3320 cm. of papyrus, costing 41 dr. On this basis a standard roll of 340 cm. would cost very nearly 4.2 dr. But since the writing is compressed this may not be a typical case. A more normal one might be P.Oxy.ix.1182, a roll of Demosthenes. Here the column contains only about 10 στίχοι to a column 4 cm. wide with intercolumniations of 2 cm. On this basis the 16,600 στίχοι would require 9960 cm. of papyrus and if this cost 41 dr., the standard roll of 340 cm. would cost 1.4 dr. = 1 dr. 2 1/2 ob. nearly. Obviously these calculations contain many imponderables, but at any rate the resultant figures are not noticeably out of scale with the prices of what are presumably standard rolls quoted by Lewis, op. cit. p. 132.

We may now turn back to Pliny's statement that the (standard) roll never contained more than 20 sheets. This statement has been criticised, Lewis[7] remarking that *never* "may be too strong a word", while E. G. Turner[8] bluntly says that Pliny was wrong. I do not think this is a necessary conclusion. In my view it is perfectly conceivable that papyrus left the factories in rolls of 20 sheets, and that the middlemen or retailers through whose hands it passed before reaching the customers pasted rolls or portions of rolls together to make up whatever sizes were in demand. If it is objected that the purchasers themselves could have made up rolls in this way, this might not have been convenient, and we should in any case remember that the

[7] Op. cit., p. 54.
[8] *Greek Papyri*, p. 4.

making of neat joins between sheets of payrus, as described in detail by Turner,[9] clearly required skill and care. On balance, therefore, I am inclined to accept Pliny's statement as strictly correct.[10]

Now that we have obtained a very rough idea of the cost of papyrus, we can look again at a much-debated subject, the alleged advantage of the codex over the roll. It has been often asserted that one practical advantage which the codex possessed was that it was cheaper to produce, since it made use of both sides of the writing material. The saving would not, of course, be exactly 50% since the successive columns of writing in a codex are separated from each other by two margins, which together would be much wider than the single intercolumniation of the roll. We can test this by taking as an actual example the Chester Beatty codex of the Pauline Epistles (P.Beatty II). This is a single-quire codex, which means that the leaves diminish in size from the ends of the book towards the middle, and the width of the written area diminishes proportionately. The codex originally consisted of 52 bifolia = 104 leaves = 208 pages, and although both beginning and end are lost it can be calculated that the outermost leaves had a width of about 17.2 cm., while the narrowest leaves (in the middle) are about 13 cm., giving an average of 15.1 cm. The total length of the 104 leaves, placed side by side, would therefore have been 15.1 × 104 = 1570 cm. How much papyrus would have been required to write the same manuscript in the form of a roll? The width of the columns of writing vary, for the reason given above, from an estimated 13.5 cm. down to 9.5 cm., an average of 11.5 cm. If we assume that the manuscript was completely filled with text, there would have been 208 columns of writing, and at an average of 11.5 cm. these would have had a total width of 208 × 11.5 = 2392 cm. To this we must add the intercolumniations, and if we estimate these at 2 cm., we must add 207 × 2 cm. = 414 cm., making a total of 2392 + 414 = 2806 cm. This amounts to a saving, by using the codex format, of 1236 cm., or about 44% of the amount needed for the roll.

Of course the total expense of producing the manuscript would

[9] *The Typology of the Early Codex*, p. 47.

[10] Professor Lewis, in a letter, has pointed out that Pliny's "never" can be defended on the grounds that what he is describing in this context is the normal, or standard, factory production; it is only later that he turns to speaking of "defects" or divergences from the standard.

not be reduced by 44%, since the cost of writing would remain the same whichever format was chosen. The stichometrical totals appended to some of the Epistles (in a cursive hand, but apparently little, if at all, later than the text) indicate that it was a commercially produced copy,[11] and it is therefore legitimate to use the figures for the cost of writing in the contemporary P.London Inv.2110 referred to above. This gives two figures, presumably for two qualities of writing, viz. 28 dr. for 10,000 στίχοι, and 13 dr. for 6,300 στίχοι. We may take, as an average figure, 24 dr. for 10,000 στίχοι. How many στίχοι were there in the Beatty manuscript? It is true that some of the Epistles have stichometry appended, but the figures are far from reliable. In general, such figures are often exaggerations, corrupt, or mere approximations. It thus seems better to adopt the figures given by Rendel Harris from actual calculation,[12] using Westcott and Hort's text and making allowances for nomina sacra. For the Epistles in the Beatty codex (including 2 Thessalonians, which presumably followed 1 Thessalonians) Rendel Harris's figures give a total of 4422 στίχοι. To this we must make some addition, since there was one blank leaf at the beginning of the codex,[13] and 5 leaves after the presumed end of 2 Thessalonians. The leaves at beginning and end of the codex probably contained about 46 στίχοι apiece, and the 6 leaves mentioned would have thus contained 276 στίχοι, making a total of 4422 + 276 = 4698. At 24 dr. for 10,000 στίχοι the cost of writing would have been 11 dr. 2 ob. nearly. How much would the papyrus have cost, for the two formats of roll and codex? If we take as an average figure 2 dr. for a standard roll of 340 cm., the cost of the 1570 cm. needed for the codex would have been 9 dr. 1 1/2 ob. The 2806 cm. needed for the roll would have been 16 dr. 3 ob. The relative costs of the two formats would thus have been:

[11] Cf. E. G. Turner, *Greek Papyri*, p. 95: "If they (i.e. stichometrical totals) are present in a text, we may be sure that the copy was professionally made and paid for".

[12] J. Rendel Harris, *Stichometry*, Cambridge 1893, p. 40; the figures are based on a στίχος of 16 syllables.

[13] According to Kenyon, *The Chester Beatty Biblical Papyri*, Fasc. iii, p. vi, only the first page of the codex was left blank. But in fact the first surviving leaf (f. 8) must have begun at θάνατος in Romans v. 17. From the beginning of the Epistle to this point is about 279 στίχοι, and since in this part of the codex there are about 46 στίχοι to the leaf, this amount of text would have occupied exactly 6 leaves, with the result that both sides of the first leaf must have been blank.

	As roll	As codex
Cost of writing	11 dr. 2 ob.	11 dr. 2 ob.
Cost of papyrus	16 dr. 3 ob.	9 dr. 1 1/2 ob.
Total	27 dr. 5 ob.	20 dr. 3 1/2 ob.

This represents a saving of 26% of the cost of the roll format by changing to the codex. Of course if the price of a standard roll was more than 2 dr. the saving would be proportionately greater.

No conclusion will be drawn here. The sole purpose of this note is to suggest a positive evaluation of one of the advantages which has been claimed for the codex in its long-drawn out duel with the roll.

5

ROLL VERSUS CODEX—A NEW APPROACH?

In *Scritti in Onore di Orsolina Montevecchi*, Bologna 1978, pp. 373–376, I published a note entitled "Was re-rolling a papyrus roll an irksome and time-consuming task?",[1] in which I described experiments with rolls cut from rolls of wall-paper, on the basis of which I concluded that re-rolling a roll was much easier and quicker than had been supposed, and that the secret lay in letting the roll do the work of rolling through its natural tendency to roll up. I assumed that a roll of papyrus, having been rolled up at the time of manufacture and kept constantly rolled up except when opened for the purposes of writing and reading, would have possessed the same tendency to roll up, but of course I had no means of proving it. Now the proof has come to light in a surprising way. Among the great find of papyri at Dishna, not far from the better-known Nag Hammadi, were a number of papyrus rolls. The owner of one of these rolls tried to unroll it, but found that the papyrus began to break. He thereupon immersed the roll in warm water, after which he found that he could unroll it without damage either to the roll or the writing. He left it unrolled, and five minutes later *the roll had rolled itself up*. It is surely remarkable that 1500 years or so after its manufacture a papyrus roll should still retain its capacity to roll itself up and thus completely confirm the results of my experiments.

Now that what has so often been claimed as one of the signal disadvantages of the roll has been eliminated, we may perhaps look again at the question of why it took so long for the codex to replace the roll. I myself have long thought it possible that the roll might have possessed some psychological advantage in that reading a roll is a continuous process, unbroken by the necessity for page-turning, which cuts the reader off from all that has gone before and gives only limited access, in the form of the facing page, to what is to come. To put this to a practical test I chose a fairly abstruse article of which I happened to have photocopies, viz. Walter F. Snyder,

[1] Chapter A3 above.

'When was the Alexandrian Calendar established?', *American Journal of Philology*, lxiv, 1943, pp. 385–389, and pasted the pages down on to a roll of stout paper, forming a roll exactly 2 m. long. The result was remarkable. Not only did I find the argumentation easier to follow, but there were several practical advantages. For instance, the article is illustrated with four Charts, each occupying most of a page. Chart I, in the original, comes on a right-hand page while discussion of it cames on the following page, which meant that Chart and discussion could not be viewed simultaneously. Chart II also begins on a right-hand page, while discussion of it follows on the next two pages, from which the Chart is of course invisible, Discussion of Chart III begins two full pages before the Chart itself appears. With my 'roll', on the other hand, it was quite easy to unroll it so that one could view four or five pages at once, so that, e.g., Charts I and II and the discussion of them could all be seen simultaneously. I found this extremely helpful.

Of course I would not claim that my single experiment is sufficient to prove my point. But I would suggest that this is an aspect of the contest between roll and codex which deserves consideration. One has only to think of such examples as the Parthenon frieze or the Column of Trajan to realise that the advantages of a panoramic or narrative presentation were fully appreciated in antiquity, and may have influenced the literate classes whose views determined the form of ancient books.

6

IRENAEUS AND THE FOUR-GOSPEL CANON

Every study of the Canon of the Four Gospels begins, and rightly begins, with the famous passage in which Irenaeus, writing about the year 185, seeks to defend the Canon by finding a mystical significance in the number four.[1] The Gospels, he says, cannot be either more or less than four in number, since there are four quarters of the earth and four principal winds, and since the Church is spread over the world, it needs four columns for its support. He then produces his celebrated identification of the Four Evangelists with the four "Living Creatures" of the Apocalypse.[2] For the present purpose it is necessary to give the wording of the whole passage in the original Greek:[3]

Ἐξ ὧν φανερὸν ὅτι ὁ τῶν ἁπάντων Τεχνίτης Λόγος, ὁ καθήμενος ἐπὶ τῶν Χερουβὶμ καὶ συνέχων τὰ πάντα, φανερωθεὶς τοῖς ἀνθρώποις ἔδωκεν ἡμῖν τετράμορφον τὸ εὐαγγέλιον, ἑνὶ δὲ Πνεύματι συνεχόμενον. Καθὼς καὶ ὁ Δαυὶδ αἰτούμενος αὐτοῦ τὴν παρουσίαν φησίν· «Ὁ καθήμενος ἐπὶ τῶν Χερουβίμ, ἐμφάνηθι» Καὶ γὰρ τὰ Χερουβὶμ τετραπρόσωπα καὶ τὰ πρόσωπα αὐτῶν εἰκόνες τῆς πραγματείας τοῦ Υἱοῦ τοῦ Θεοῦ. «Τὸ μὲν γὰρ πρῶτον ζῷον», φησίν, «ὅμοιον λέοντι», τὸ ἔμπρακτον αὐτοῦ καὶ ἡγεμονικὸν καὶ βασιλικὸν χαρακτηρίζον· «τὸ δὲ δεύτερον ὅμοιον μόσχῳ», τὴν ἱερουργικὴν καὶ ἱερατικὴν τάξιν ἐμφαῖνον· «τὸ δὲ τρίτον ἔχον πρόσωπον ὡς ἀνθρώπου», τὴν κατὰ ἄνθρωπον αὐτοῦ παρουσίαν φανερῶς διαγράφον· «τὸ δὲ τέταρτον ὅμοιον ἀετῷ πετομένῳ», τὴν τοῦ Πνεύματος ἐπὶ τὴν ἐκκλησίαν ἐφιπταμένου δόσιν σαφηνίζον. Καὶ τὰ εὐαγγέλια οὖν τούτοις σύμφωνα, ἐν οἷς ἐγκαθέζεται ὁ Χριστὸς Ἰησοῦς. Τὸ μὲν γὰρ κατὰ Ἰωάννην τὴν ἀπὸ τοῦ Πατρὸς ἡγεμονικὴν αὐτοῦ καὶ πρακτικὴν καὶ ἔνδοξον γενεὰν διηγεῖται, λέγον· «Ἐν ἀρχῇ ἦν ὁ Λόγος, καὶ ὁ Λόγος ἦν πρὸς τὸν Θεόν, καὶ Θεὸς ἦν ὁ Λόγος», καί· «Πάντα δι' αὐτοῦ ἐγένετο καὶ χωρὶς αὐτοῦ ἐγένετο οὐδὲ ἕν.» Διὰ τοῦτο καὶ πάσης παρρησίας πλῆρες τὸ εὐαγγέλιον τοῦτο · τοιοῦτο γὰρ

[1] On Irenaeus and the Canon see the full discussion in Hans von Campenhausen, *The Formation of the Christian Bible*, London, 1972, pp. 176–201.

[2] Apocalypse 4: 6–8.

[3] Taken from Irénée, *Centre les Hérésies*. Livre 3. Texte et traduction. Ed. Adelin Rousseau et Louis Doutreleau. Paris, 1974, pp. 161–169.

τὸ πρόσωπον αὐτοῦ. Τὸ δὲ κατὰ Λουκᾶν, ἅτε ἱερατικοῦ χαρακτῆρος ὑπάρχον, ἀπὸ Ζαχαρίου τοῦ ἱερέως θυμιῶντος τῷ Θεῷ ἤρξατο · ἤδη γὰρ ὁ σιτευτὸς ἡτοιμάζετο μόσχος, ὑπὲρ τῆς ἀνευρέσεως τοῦ νεωτέρου παιδὸς μέλλων θύεσθαι. Ματθαῖος δὲ τὴν κατὰ ἄνθρωπον αὐτοῦ γέννησιν ἐξηγεῖται, «Βίβλος» λέγων «γενέσεως Ἰησοῦ Χριστοῦ, υἱοῦ Δαυίδ, υἱοῦ Ἀβραάμ», καὶ πάλιν · «Τοῦ δὲ Χριστοῦ ἡ γέννησις οὕτως ἦν.» Ἀνθρωπόμορφον οὖν τὸ εὐαγγέλιον τοῦτο· διὰ τοῦτο καὶ καθ' ὅλον τὸ εὐαγγέλιον ταπεινοφρονῶν καὶ πραῢς ἄνθρωπος διατετήρηται. Μάρκος δὲ ἀπὸ τοῦ προφητικοῦ Πνεύματος, τοῦ ἐξ ὕψους ἐπιόντος τοῖς ἀνθρώποις, τὴν ἀρχὴν ἐποιήσατο, «Ἀρχὴ» λέγων «τοῦ εὐαγγελίου, ὡς γέγραπται ἐν Ἠσαΐᾳ τῷ προφήτῃ», τὴν πτερωτικὴν εἰκόνα τοῦ εὐαγγελίου δεικνύων · διὰ τοῦτο δὲ καὶ σύντομον καὶ παρατρέχουσαν τὴν καταγγελίαν πεποίηται · προφητικὸς γὰρ ὁ χαρακτὴρ οὗτος.

When the passage is examined in detail, some very curious anomalies emerge which demand explanation:

1. It will be seen that Irenaeus clearly identifies the "Living Creatures" of the Apocalypse as Cherubim, although they are not so described in the Apocalypse, where the word "Cherubim" does not in fact occur.
2. The image of the Deity as "seated upon the Cherubim", though commonplace in the Old Testament, is quite inappropriate here, since the Apocalypse states that the "Living Creatures" were stationed round the throne of the Deity. They do not support anything, and indeed could not have done so, since they are described as prostrating themselves in adoration at the appearance of the Lamb.[4]
3. The description of the Cherubim as "four-faced" is puzzling, since although the Four "Living Creatures" had, collectively, four faces, this is not a natural description: in fact only in one case, that of the Creature with a Man's face,[5] is the face specially mentioned.
4. Although as will have been seen, Irenaeus quotes the descriptions of the Living Creatures from the Apocalypse, and uses the word φησίν to emphasise that he is quoting verbally, *he nowhere mentions either the Apocalypse or its author*, with the result that φησίν is left in the air without a subject, either actual or implied.[6]

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[4] Apocalypse 5: 8, *cf.* 7: 11.
[5] Apocalypse 4: 7.
[6] This posed a problem for the authors of the French translation which accom-

5. But it is the identification of the Evangelists with the Four "Living Creatures" which provides the greatest surprise. Since in the Apocalypse they are not only described but also numbered 1st, 2nd, 3rd and 4th, we might expect the identification with the Evangelists to follow some recognised order. Instead, Irenaeus gives us:[7]

   1st "Living Creature." Lion. John.
   2nd "Living Creature." Ox. Luke.
   3rd "Living Creature." Man's Face. Matthew.
   4th "Living Creature." Flying Eagle. Mark.

This order, John, Luke, Matthew, Mark is unique[8] and does not agree with any used by Irenaeus himself. In describing the origins of the Gospels he lists them in what became the canonical order of Matthew, Mark, Luke, John,[9] but elsewhere, according to von Campenhausen, he always uses the order Matthew, Luke, Mark, John.[10]

It is well known that the Four "Living Creatures" of the Apocalypse derive from the vision of the four "Living Creatures" in the first chapter of the Book of Ezekiel,[11] and if we turn to Ezekiel it is remarkable how everything falls into place. Firstly, although the "Living Creatures" are not called Cherubim in the first chapter, they are so described when they reappear in Chapter 10, and verse 20 of that Chapter specifically refers this description back to the vision in Chapter 1.

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panies the text. They could not translate φησίν as 'dit-il' because there is no clue to the identity of the 'il'. They consequently rendered it as 'est-il dit' which of course is *not* a translation of φησίν!

[7] The same identification of the "Living Creatures" with the Evangelists is found in Ambrose, who mentions the Apocalypse but has rearranged them in the "Western" order of the Gospels, *cf.* T. Zahn, *Forschungen zur Gesch. d. neutest. Kanons*, II, Erlangen, 1883, p. 259 n. 7, *cf.* p. 266.

[8] T. Zahn, *Gesch. d. neutest. Kanons*, Erlangen-Leipzig, 1890. 2. Band, pp. 364–375, 1013–1015, "Die Ordnung der Evangelien", does not quote any example of this order. Von Campenhausen, *op. cit.* p. 195 n. 243 explains it by saying that the order had to conform to that of the epochs of salvation-history as listed by Irenaeus; but this is only just one of the minor illustrations which follow *after* the identification of the "Living Creatures" and as this identification is the centrepiece of Irenaeus's exposition it is unlikely that it would be governed by an order which had not yet been mentioned. Zahn (*op. cit.* p. 365) explains it by suggesting that Irenaeus had to find an appropriate Evangelist for each "Living Creature" without regard to any order of the Gospels.

[9] Adv. Haer. III. I. 1.

[10] *Op. cit.*, p. 195 n. 243 *cf.* Zahn, *op. cit.*, p. 365. The only example which Zahn can quote for this order is the mysterious 4th cent. Ambrosiaster (*op. cit.*, p. 368).

[11] Ezekiel 1: 1–21.

Once the reference to Cherubim is explained, we can go further and consider the reference to the Deity as "seated upon the Cherubim." Here again Ezekiel furnishes the explanation, for above the Cherubim[12] is said to be a crystal firmament upon which was placed the throne of the Deity, to which the Cherubim gave both support and movement.

We now turn to the description of the Cherubim as given by Ezekiel, and we find that here *each* of the Cherubim possessed four faces,[13] so that the term "four-faced" applied to them by Irenaeus is entirely correct. On the other hand, the order in which the faces are listed differs from that in the Apocalypse,[14] *viz*.:

1st face. Man.
2nd face. Lion.
3rd face. Ox.
4th face. Eagle.

If we now take this order of the faces, and place against them the Evangelists identified with them by Irenaeus, we get the following schema:

1st face. Man. Matthew.
2nd face. Lion. John.
3rd face. Ox. Luke.
4th face. Eagle. Mark.

This is at once recognisable as the well-known so-called "Western Order" of the Gospels,[15] which despite its name is in fact attested in both East and West. It seems to have been a primitive order, and it was not finally replaced by the present canonical order until the time of Jerome.[16] Most significantly, it seems to have been the order followed in the earliest surviving manuscript of the Four Gospels, the 3rd century Chester Beatty papyrus codex of the Gospels and Acts.[17]

The foregoing investigation indicates, without a shadow of doubt, that the celebrated exposition of Irenaeus, or at any rate that part

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[12] Ezekiel 1: 22; 10: 1.

[13] Ezekiel 1: 6.

[14] Ezekiel 1: 10. In Ezekiel 10: 14 the four faces are described differently, *viz*. 1st face. Cherub; 2nd face. Man; 3rd face. Lion; 4th face. Eagle, the Ox disappearing. But the whole of this verse is omitted in the Septuagint.

[15] Zahn, *op. cit.*, pp. 370–1. To the examples there given must be added the 4th–5th cent. Freer gospels found in Egypt.

[16] Zahn, *op. cit.*, pp. 367–8.

[17] *Cf.* T. C. Skeat and B. C. McGing, "Notes on Chester Beatty Biblical Papyrus I (Gospels and Acts)". See chapter B4 below.

of it which related to the "Living Creatures" of the Apocalypse, was taken by him from an earlier source[18] which, starting from the vision of Ezekiel, went on to discuss the Apocalypse, with verbal quotations, and perhaps offering some explanation for the differing order there. Irenaeus, one must conclude, took the quotations from this source and never looked at the Apocalypse himself. He even copied the word φησίν from his source, not realising that he himself had not mentioned the Apocalypse. All the inconsistencies and contradictions in his account are thus explained. This defence of the Four-Gospel Canon must have originated at a date early enough to be used as a source by Irenaeus—say, perhaps, not later that 170 or thereabouts.

But there is more than the defence of the Canon involved. As Zahn pointed out a century ago,[19] any question of the order of the Gospels only makes sense when all four have been brought together in a single volume, which must be a codex, since no roll, however economically written, could contain all four Gospels. The source used by Irenaeus must therefore have possessed such a codex, and the Four-Gospel codex can now be traced back to about the year 170. It follows that the Section in *The Birth of the Codex* dealing with the Four Gospel Canon[20] was perhaps too dismissive of the idea of a Four-Gospel codex in the second century (though admitting that it was technically possible). Of course the aim of both Irenaeus and, no doubt, his source was to demonstrate the spiritual unity of the Four Gospels, without regard to bibliographical considerations. But the practical aspect likewise demands recognition. For how could a random assemblage of four separate codices of the Gospels, differing perhaps in size, appearance, style of writing and so on, be regarded

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[18] But *cf.* von Campenhausen, *op. cit.*, p. 199: ". . . there is no reason to search for earlier models. As a 'New Testament' theologian Irenaeus was compelled to break his own trail; and this highly original typology of the Four Gospels is so closely bound up with his fundamental concerns, his polemic, and especially his exegesis of the 'beginnings' of the Gospels, that for this very reason it is extremely difficult to believe in an earlier provenance." In the note to this passage (n. 259) he mentions that according to Zahn the source was "ein uns unbekannter Exeget oder Homilet des zweiten Jahrhunderts", but Zahn gives no evidence or justification for this.

It is remarkable that despite the intensive research into the sources of Irenaeus, this particular section of the *Adversus haereses* has been universally accepted as his original work, *cf.* von Campenhausen, *op cit.*, p. 189 n. 205, who points out that even the most radical searcher for "sources" in Irenaeus, F. Loofs, accepts this.

[19] Von Campenhausen, *op. cit.*, p. 173.

[20] C. H. Roberts and T. C. Skeat, *The Birth of the Codex*, London, 1983, Section 11, 'The Christian Codex and the Canon of Scripture' (pp. 62–66).

as having both the unity and (which is just as important) the exclusivity which Irenaeus and, presumably, his source were at such pains to establish? Conversely, how better could these qualities be demonstrated than by combining all four in a single codex? In short, I would now go so far as to suggest that the Four-Gospel Canon and the Four-Gospel codex are inextricably linked, and that each presupposes the other.[21]

[21] This is in fact the original view of P. L. Hedley, which he subsequently withdrew, *cf.* von Campenhausen, *op. cit.*, p. 173 n. 126.

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THE ORIGIN OF THE CHRISTIAN CODEX

In *The Birth of the Codex*, published in 1983, my co-author, the late C. H. Roberts, and I put forward, in a very tentative manner, two alternative hypotheses to explain the extraordinary predilection of the early Christians for the codex form of book as opposed to the roll.[1] Neither of these theories has found acceptance, and the purpose of the present article is to approach the problem from a different standpoint.

First, the facts, and here I shall restrict myself entirely to the Gospels, since it is in them, as will be shown, that the solution to the problem is to be sought. When Colin Roberts published his magisterial monograph *The Codex* in 1954 there were 22 known papyrus fragments of the Gospels, ranging in date from the 2nd century to the 6th or 7th. Every one of these was from a codex. Since then 20 more Gospel fragments have come to light, and again every one is from a codex.

This is an astonishing statistic, if we reflect that among non-Christian papyri the roll form predominated for centuries, and it was not until about 300 A.D. that the codex achieved parity of representation with the roll, and another two or three centuries passed before the roll disappeared altogether as a vehicle for literature.

Hitherto, all the advantages claimed for the codex as opposed to the roll have been matters of degree—the codex is *more* comprehensive, *more* convenient in use, *more* suited for ready reference, *more* economical (because both sides of the writing material were used), and so on. But in the case of the Gospels, representation of the codex is not a matter of degree—it is total, 100%, and the motive for adopting it must have been infinitely more powerful than anything hitherto considered. What we need to do, in fact, is to look for something which the codex could easily do, but which the roll could not, in any circumstances, do. And if the question is posed in this way, we do not have to look very far, for a codex could contain the texts of all four Gospels. No roll could do this.

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[1] *The Birth of the Codex*, Section 10, pp. 54–61.

To illustrate the last statement we can take the Chester Beatty papyrus codex of the four Gospels and Acts, written about the middle of the 3rd century. The pages are numbered, and we know that the Gospels occupied pp. 1–167, since Acts began on p. 168.[2] Each page contained a column of writing about 16 cm. in breadth and, originally, about 19 cm. in height. If we imagine these columns set out on a roll, with space of, say, 2 cm. between them, the lengths of the individual Gospels will be as follows:

Matthew (49 pp.)	49 × 18 = 882 cm.
Mark (32 pp.)	32 × 18 = 576 cm.
Luke (48 pp.)	48 × 18 = 864 cm.
John (38 pp.)	38 × 18 = 684 cm.
	Total = 3006 cm.

It can be stated, without the possibility of contradiction, that a roll 3006 cm. = 30 m. in length would be completely unhandleable, as anyone sufficiently interested to make the experiment can find out for themselves. The maximum length of a roll is generally taken to be about 10 m. On the other hand, as the Beatty papyrus shows, a codex could contain not only all four Gospels, but Acts as well.

But, it will be asked, is there any evidence for a codex of all four Gospels as early as the 2nd century? This was discussed in *The Birth of the Codex*, pp. 65–66, in which the conclusion was reached that, although it would have been technically feasible, "a second-century codex of all four Gospels seems unlikely". Since then the position has altered somewhat. Of course the Chester Beatty codex itself must have ancestors reaching back towards, if not into, the second century. And I have published an article[3] in which I argued that not only was Irenaeus, writing about 185 A.D., familiar with a four-Gospel codex, but that he used a source which had the four Gospels in the so-called "Western" order of Matthew, John, Luke, Mark, which implies that all four were in a codex. Furthermore, it seems to me quite possible that the Bodmer codex of Luke and John, P 75, is in fact the second half of a four-Gospel codex, since it consisted, when complete, of a single-quire codex of 72 leaves. A single-quire codex of double this size, 144 leaves (288 pages), would

[2] For these and the following statistics see my article, *A Codicological Analysis of the Chester Beatty Papyrus Codex of Gospels and Acts* (P 45). See chapter B5 below.

[3] "Irenaeus and the Four-Gospel Canon", *Novum Testamentum*, xxxiv. 2 (1992), pp. 194–199 (= chapter A6 above).

have been almost impossible to handle,[4] owing to the bulk of the central fold (100 leaves, or a little more, seems to have been about the maximum for a single-quire codex; the Chester Beatty codex IX, of Ezekiel, Daniel and Esther, which originally ran to 118 leaves, was probably about the limit). If then P 75 was originally a four-Gospel codex, it must have consisted of two single-quire codices sewn together, the first containing Matthew and Mark, the second Luke and John. P 75 was dated by its editors to between 175 and 225 A.D. and most later estimates place it early in the third century. This, of course, must also have had ancestors.

Nevertheless it is certainly true that most of the earliest Gospel fragments come, or appear to come, from single-Gospel codices. This might appear at first sight to invalidate the hypothesis put forward above, that the Christian use of the codex originated in the four-Gospel codex. I would suggest, however, that, paradoxical though it may seem, these single-Gospel codices are in fact evidence for the existence of the four-Gospel codex.

Let us imagine a second-century Christian confronted with a choice between a codex and a roll, each containing the same single Gospel. For an example of the codex we can take the earliest known Christian papyrus, the Rylands fragment of the Gospel of John, thought to have been written about 125. This must have originally been a codex of about 55 leaves (110 pages) measuring about 21 × 18 cm.[5] and perhaps about 3 cm. thick if we allow for some form of binding. We can compare this with a roll of the same Gospel using the figures deduced from the Chester Beatty papyrus above, the length of papyrus for John being 6.84 metres. Schubart has pointed out that a roll 18 cm. in height and 6 m. in length, only slightly shorter, could be rolled into a cylinder 5–6 cm. in breadth, which could easily be held in the hand.[6] Faced with such a choice, the modern reader would, of course, immediately choose the codex, because that is the only

[4] On the awkwardness of handling large single-quire codices cf. W. Schubart, *Das Buch*[3], p. 114.

[5] C. H. Roberts, *An Unpublished Fragment of the Fourth Gospel in the John Rylands Library*, Manchester, 1935, p. 21, calculated that the original codex consisted of 66 leaves (132 pages) measuring 21 × 20 cm. My own estimates, given in the text, are somewhat smaller.

[6] It was a mistake (of mine) to say in *The Birth of the Codex*, p. 47, that a roll 18 cm. high and 6 m. long could be rolled into a cylinder 3–4 cm. in diameter. The correct figure should have been 5–6 cm. in diameter. It was also incorrect to say that such a roll could 'easily' have accommodated any of the Gospels. The lengths quoted in the text, derived from the Chester Beatty codex, are more realistic.

form of book known to him. But to our second-century Christian the choice would not be so simple. Having been born into and lived in a society dominated by the roll, and having himself used rolls for many purposes, he might well have hesitated.

Of course it has often been claimed that the codex was cheaper than the roll, since both sides of the papyrus were utilised. However, the cost of writing would have remained the same, and this would have been the greater part of the expense. Some rough calculations which I have made[7] indicate that the net saving by using the codex might be in the order of 25%, against which would have to be set the cost of sewing the leaves together and applying some form of cover or binding, so that the net savings, if any, would be minimal.[8] On the other hand we have the sheer simplicity of the roll, which was ready for use as soon as the ink had dried on the last column of writing.

Again, it is often claimed that reading a codex is easier than reading a roll. Here there is much misconception. It is said, for example, that in reading a roll the right hand unrolls the roll while the left hand rolls it up. Both statements are incorrect. What actually happens is that the right hand merely *supports* the bulk of the roll while the left pulls out a stretch for reading. When this had been read, the left hand does not roll it up—it rolls itself up, the left hand merely preventing it from rolling up too far.[9] The left hand then pulls out another stretch of papyrus, and the reading proceeds. With practice these operations would have become as automatic as turning the leaves of a codex.

There is a further important difference between reading a roll and reading a codex, which I have called the panoramic aspect.[10] In reading a roll the reader's eyes travel continuously over the text without interruption, like the smooth sequence of the frames of a cinematograph film, melting into each other, in contradistinction to

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[7] "The Length of the Standard Papyrus Roll and the Cost-advantage of the Codex" *ZPE*, 45, 1982, pp. 169–76.

[8] The early single-Gospel codices are very lavish in their use of papyrus because of their format, viz. a large number of comparatively small pages, which means that much papyrus is wasted in the margins. The conclusion in *The Birth of the Codex*, p. 47, that "the argument from economy would seem to be negligible" is amply confirmed.

[9] For the capacity of the papyrus to roll itself up cf. my note "Two Notes on Papyrus. 1. Was Re-rolling a Papyrus Roll an Irksome and Time-consuming Task?" in *Scritti in onore di Orsolina Montevecchi*, 1981, pp. 373–6, and the following note.

[10] "Roll *versus* Codex: a New Approach?", *ZPE*, 84, 1990, pp. 297–8.

the blinkered vision of the codex reader, to whom the text appears in a series of disjointed snapshots. I have quoted elsewhere the case of a technical article containing diagrams and explanatory text, in which the explanatory text often appeared overleaf from the diagram, so that one was left with the choice of looking at the diagrams without the explanatory text, or looking at the explanatory text without the diagrams. Had this been a roll, of course, there would have been no difficulty as both could be seen simultaneously.

After this long digression we can at last return to our 2nd century Christian, and we know that, faced with the choice, he decided upon the codex, not the roll, for his manuscript of John. In view of what has been said, it must have been a very powerful reason which induced him to abandon the practice of a lifetime and choose the codex. And I suggest that the reason must have been the fact that the four-Gospel codex was already in existence and had thus set the standard for manuscripts of individual Gospels.

We are now at the heart of the matter. We must assume, in the absence of any evidence to the contrary, that the Gospels originally circulated in the usual way, on papyrus rolls. What can have induced the Church so suddenly, and totally, to abandon rolls, and substitute not just codices but a single codex containing all four Gospels?

It is my belief that the key to the whole situation was the publication of John (*circ.* 100 A.D.), which may well have caused a crisis in the Church. It certainly must have been received with very mixed feelings. Coming as it did with such apparently impeccable credentials, as the work of the "beloved disciple", it could neither be rejected nor ignored. But if it was accepted, a whole host of problems arose.

First of all, there was the sheer multiplicity of the Gospels. As Harnack reminded us nearly a century ago,[11] we are so accustomed to the Four Gospels that it is difficult for us to appreciate what an extraordinary phenomenon this is—four different narratives, all purporting to record the life and teaching of Christ, but differing widely among themselves in approach and presentation. The danger was obvious. We know that later on the multiplicity of Gospels was a source of derision among unbelievers, but by 100 A.D. the alarm bells must already have been ringing. Questions must have been asked:

[11] A. von Harnack, *Geschichte der altchristlichen Litteratur bis Eusebius*: 2. Theil. *Die Chronologie* . . ., 1897, p. 681.

when would the production of Gospels come to an end? Could anything be done to prevent the production of further Gospels? Should anything be done? What authority did the existing Gospels in fact possess? Moreover, since it was unlikely, in spite of the reference at the end of John to the "many other things" still said to be unrecorded, and the hyperbole of the final verse, that any authentic further evidence concerning the life and mission of Jesus could be recovered, the probability was that any new Gospels would be either romantic or sensational inventions designed to interest or amuse rather than instruct, or else intended to promote beliefs which the Church had rejected. Though the earliest evidence comes from a slightly later date, the threat of gnosticism must already have been looming on the horizon. Obviously the production of such writings could not be prevented. Was there then any way in which the existing four Gospels could be safeguarded from either addition or subtraction?

A variety of different courses was open to the Church, and must have been considered. Would it be possible to collect together all the authentic or seemingly authentic information concerning the life and mission of Jesus and on this basis construct one completely new and authoritative Gospel? After all, this was what Luke had tried to do, and failed. It must, however, have very soon become evident that such a scheme was impracticable. Years of work would be needed, and the situation was one of urgency. What tests could be applied to determine authenticity? Furthermore, the popularity of the existing Gospels was such that it would be difficult for such a new work to replace them, and in any case the special message which each of the Evangelists had tried to convey would be lost.

Alternatively, would it be possible to exploit this very popularity of the existing Gospels by welding them into a continuous whole—the Diatessaron solution? It must have been obvious that such a proceeding was fraught with all sorts of difficulties, but that it was feasible is proved by the fact that it was actually achieved in the Diatessaron of Tatian. But although this proved popular in Syriac-speaking churches, it never gained acceptance in Greek- and Latin-speaking areas, where the existing four Gospels were so firmly entrenched that they could withstand the challenge of any competitor.

Since, then, any radical solution such as those considered above seemed to be impossible, the Church must have been forced to consider any physical means by which the four Gospels could be brought together and at the same time additions to their number could be discouraged. At this stage the proposal must have been made to include all four in a codex, the new form of book recently developed in Rome. Experiment would have shown that this was perfectly feasible, and although the earliest Roman codices seem to have been on parchment, it would be natural to replace this with the universal writing material, papyrus. Whether, in fact, the parchment codex or the papyrus codex was the earlier is still uncertain,[12] but at any rate so far as Egypt was concerned the choice of papyrus would have been automatic.

How the decision was reached we have no means of knowing. Clearly there must have been correspondence between the major churches, and perhaps conferences.[13] And once the Four-Gospel codex had been decided upon, every means must have been taken to spread the news throughout the Church. But merely publicising the decision would not have been enough. After all, if a codex could hold four Gospels, it could just as well hold three, or five. The choice of four had somehow to be justified, and we can trace in the pages of Irenaeus some of the ways in which this was done. Commentators usually appear to take the view that the arguments put forward by him are his own, but it seems to me much more likely that they reflect reasons adduced when the original decision was taken, since this was when they would have been most needed, whereas by the time of Irenaeus the battle had already been won. Some of the reasons can hardly be described as convincing—the four regions of the world, the four principal winds, the claim that since the Church was spread out over the whole world it needed four pillars for its support,[14] that 'since the creations of God are well-proportioned and harmonious, the same must apply to the form of the Gospel',[15] and the fact that they were used shows how desperate was the need for support by any and every possible means. Something more definitely theological was clearly needed, and the parallel of the Four Covenants goes some way towards this, but what was wanted above all was an appeal to Scripture, *i.e.* the Old Testament. The centrepiece of Irenaeus's

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[12] Cf. *The Birth of the Codex*, p. 29.
[13] See below, Appendix A.
[14] Adv. haer. III, 11, 8.
[15] Adv. haer. III, 11, 9.

exposition is his famous identification of the Evangelists with the four 'Living Creatures' of the Apocalypse, but as I have shown[16] this is based on an earlier comparison with the faces of the four Cherubim in the vision in the first chapter of Ezekiel, and this may well have been one of the principal arguments used when the decision to adopt the codex was made. It would in fact have been a much better illustration than the Apocalypse since in Ezekiel *each* of the Cherubim has four faces, which thus form an indissoluble unity.[17]

It might have been expected that when the decision was taken, the order in which the Gospels appeared in the codex would have also been decided, but this does not appear to have been the case. No doubt it was felt that so long as the codex contained all four, their order was unimportant, and any local preferences could be tolerated. Of course it was not to be expected that every Christian would possess, or even have access to, such a codex, but provided a sufficient number was available in major churches this should be adequate to ensure both the survival of the Four and the exclusion of others, and there could thus be no objection to the circulation of individual Gospels, which would naturally tend to be in the same codex format.

Such was, I believe, the solution reached. It was ingenious, daring, and totally successful, since no other Gospel gained entry into the Canon and none was lost. How great was the danger is shown by the vehemence with which Irenaeus (and perhaps also, as I have suggested, his source) defended the Four-Gospel Canon. Why no record of the decision has survived may perhaps be due to its instant and total success, so that memory of it would have soon faded. That Rome played a leading part is suggested by the decision to use the codex, a Roman invention, and involvement of Rome is perhaps confirmed by the inclusion of Mark, which at the time had seemed to be heading for oblivion: thanks to the codex, it survived—and bequeathed to us the Synoptic Problem.

Of course other Gospels still circulated freely, and continued to be read and quoted. But inevitably the selection of the Four and their physical unity in the Codex gave them, right from the start, an authority and prestige which no competitor could hope to rival. The

[16] Cf. note 3 above.

[17] For the whole of this symbolism cf. H. von Campenhausen, *The Formation of the Christian Bible*, 1972, pp. 197–200.

Four-Gospel Canon and the Four-Gospel Codex are thus inseparable.

Finally, as has been emphasised thoughout, much of what has been here proposed is inevitably based on conjecture and unless, for instance, fragments of a four-Gospel codex should come to light which could be securely dated to the earlier part of the 2nd century, is likely to remain so;[18] and it is on this basis that the present article is laid before the reader.[19]

[18] See chapter B6 below.

[19] While I alone am responsible for the views expressed in this article, I wish to record my gratitude to Professor Bruce Metzger and Professor Martin Hengel for encouraging me to put them forward in this form.

8

WAS PAPYRUS REGARDED AS "CHEAP" OR "EXPENSIVE" IN THE ANCIENT WORLD?

1. Introduction

The question of whether papyrus was regarded as a "cheap" or "expensive" commodity in the ancient world has been much debated. A detailed summary of the question, with references, is given by Professor Naphtali Lewis in his *Papyrus in Classical Antiquity*, 1974, pp. 129–134, his final conclusion being that in social milieux more elevated than that of a prosperous Egyptian villager the purchase of papyrus is not likely to have been regarded as an expenditure of any consequence, but to have fallen, rather, into a category comparable to that of our 'incidentals', or 'petty cash'.

Despite this authoritative pronouncement, claims that papyrus was 'expensive' continue to appear, and it has therefore seemed desirable that the whole question should be reviewed on a more extended basis.

In the first place, we must recognise that the question asked is purely a modern one. No ancient writer, no source of any kind specifically indicates that papyrus was thought of as either 'cheap' or 'expensive'. On the contrary, it appears that whatever it cost was regarded as a fact of life which had to be accepted. It follows that whatever conclusion we come to is only our own conclusion and does not necessarily reflect any considerations which may have been felt in the ancient world.

Since, then, there is no direct answer to the question, we can only hope to form an opinion by considering attitudes. For example, does the way in which papyrus was used suggest any attempt to use it as economically as possible? Was any attempt made to identify and promote alternative and cheaper materials for writing? When opportunities arose for making economies, e.g. by the invention of the codex, how soon and how enthusiastically were they welcomed?

There are no simple answers to such questions. For example, a desire to economise in the use of papyrus may be balanced by a wish not to appear miserly or penurious. Nor must the power of tradition or custom be ignored. Finally, we must always keep in mind the fact that 99,9% of our evidence comes from Egypt and is not necessarily applicable to the rest of the ancient world.

I myself have no doubt that the persistence of the claim that papyrus was 'expensive' is due primarily to the influence of Wilhelm Schubart's well-known manual *Das Buch bei den Griechen und Römern* (3rd edition, 1961) in which he propagated this view and sought to support it by detailed arguments. The passages in question must therefore be quoted in full. The first is on p. 21:

«Wenn man aber beobachtet, wie sparsam selbst in Ägypten das Material ausgenutzt wurde, wie die Rückseite in zahllosen Fällen herhalten musste, wie sogar die Schrift getilgt wurde, um Platz für einen neuen Inhalt zu gewinnen, so vermag man nicht an eine unbeschränkte Erzeugung zu glauben. In Oberägypten ist vielfach für kleinere Aufzeichnungen, namentlich für Steuerquittungen, die Tonscherbe an die Stelle des Papyrusblattes getreten; es muss also an Papyrus gefehlt haben». On p. 23 much the same arguments are repeated all over again: «Wer sich aber an die Verwendung der Ostraka, der Tonscherben, erinnert, wer in manchem Brief gelesen hat, dass der Schreiber den Empfänger um Zusendung eines Papyrusblattes bittet, damit er antworten könne, wer von Martial gelernt hat, wie wertvoll ein leeres Blatt im kaiserlichen Rom war, wird geneigt sein, den Papyrus als ein ziemlich teures Material zu betrachten, das naturgemäss ausserhalb Ägyptens noch teurer war. Weshalb hätte man sonst so häufig beide Seiten der Rolle beschrieben, ja sogar die Schrift abgewaschen, um das Blatt wieder zu benutzen? . . . Wenn im ersten Jahrhundert der Kaiserzeit neben der Papyrusrolle der Pergamentkodex als bescheidenere Buchform zur Geltung kommt, spricht auch dies für die Kostspieligkeit des Papyrus».

These are all serious arguments, and must be considered in detail. However, before we do this, it must be pointed out that the whole of Schubart's position is disastrously weakened by a fundamental flaw, namely his belief that χάρτης meant, not a roll, but a single *sheet* of papyrus, and that papyrus was consequently bought and sold in single sheets. This is central to Schubart's position and must therefore be considered in full. It is true that on p. 21, in quoting an entry in the building accounts of the Erechtheum which he translates as «Es wurden 2 Papyrusblätter gekauft, auf die wir die Abschriften geschrieben haben» he ends by saying «Ob hier einzelne Blätter oder ganze Rollen gemeint sind, können wir nicht entscheiden», thus appearing to leave the matter open, but when he returns to the subject on p. 22 his views become more definite: «Als am Ende des

5. Jahrhunderts v. Chr. in Athen die erwähnte Baurechnung für den Erechtheustempel aufgestellt wurde, bezahlte man für zwei Papyrusblätter 2 Drachmen und 4 Obolen; jedes Blatt entsprach dem Inhalt einer Holztafel, wird also jedenfalls keine Rolle von vielen Metern gewesen sein».

The falsity of this argument is authoritatively demonstrated by Lewis (op. cit. p. 73): "In the extant inscriptions recording expenditure for the construction of the Erechtheum at Athens mention is made of wooden tablets (σανίδες), on which temporary, presumably day-to-day records were kept. In addition, the ninth prytany of 407 B.C. records the disbursement of 2 drachmas 4 obols for the purchase of two χάρται 'on which we inscribed the copies'. As these copies were—it is generally agreed—the final records of the accounts transcribed for the archives from four or more *sanides*, and as each such *sanis* con be calculated to have afforded space for writing totalling some 3000 letters or more, it would clearly have been impossible to transcribe the contents of four such *sanides* (*a fortiori* as such preliminary records are likely to have been written in abbreviated form) on to two χάρται if these were single sheets of papyrus. Again the reasonable deduction is that these χάρται were rolls of papyrus".

To this I would only add that, so far as Egypt is concerned, the *coup de grâce* to the theory that χάρτης meant a sheet of papyrus was delivered in 1933 by Professor A. E. R. Boak, who in his edition of the records of the grapheion at Tebtunis (P. Mich. ii) says on pp. 99–100: "If χάρτης means a single sheet, the number of leaves purchased would be entirely inadequate for the needs of the grapheion . . . If it meant a roll of twenty leaves, the amount would be more proportionate to the demands of the office".

All this is conclusive, and it is now universally agreed that χάρτης means a roll of papyrus, as does, of course, *charta* in Latin. But although the main foundation of Schubart's argument has now been removed, the detailed reasons with which he sought to support it remain, and deserve consideration.

2. Ostraca

Pieces of broken pottery offered a source of writing material which cost nothing and was universally available in unlimited quantities.

Their employment, therefore, does no more than illustrate the obvious fact that a writing material which costs *nothing* must always be cheaper than a writing material which costs *something*. There is accordingly nothing surprising in the fact that they were used for a variety of purposes, and their use tells us nothing about the cost of papyrus. Their massive use for tax receipts, for example, in Upper Egypt is readily understandable if, as has been asserted,[1] tax-payers were required to provide the writing material for their receipts. In any case, there are still questions to which we do not know the answers. For instance, since ostraca were so widely used in Upper Egypt, why are they found elsewhere in much smaller quantities? Why are tax-receipts for which ostraca would certainly have been used in Upper Egypt, found elsewhere, e.g. in the Fayum, written on papyrus? And why were such supremely ephemeral documents as, say, customs receipts written on papyrus? Certainly no explanation is needed for the use of ostraca in such places as the mines and quarries of the Eastern Desert, or in the guard-posts on the roads leading to them, since here papyrus would usually be unobtainable.

Thus, whether papyrus was considered to be "cheap" or expensive, ostraca will always be cheaper, and thus contribute nothing to the solution of the question.

3. Martial

«Wer von Martial gelernt hat, wie wertvoll ein leeres Blatt im kaiserlichen Rom war, wird geneigt sein, den Papyrus als ein ziemlich teures Material zu betrachten» is in all three editions of *Das Buch*, and for me the most baffling of all Schubart's assertions. I have searched in vain through Martial for anything to substantiate it, and although, as is shown below, I think I may have found an explanation, I feel that I can only put it forward as a suggestion.

Certainly the prices of Martial's own poems do not seem especially exorbitant. The highest is five denarii for a *de luxe* edition in a purple *paenula* (V. cxvii): "It's not worth it" says Lupercus, and Martial agrees. But of course Martial is being sarcastic, for it is Lupercus who had tried to borrow from Martial a copy of his poems

[1] N. Lewis, *Life in Egypt under Roman Rule*, 1983, pp. 166–7.

to save himself the cost of buying it. Elsewhere we have four sesterces for a manuscript of the *Xenia* (Book XIII), on which Martial remarks that the bookseller could sell it for two and still make a profit. Ironically, it is in Martial that we find what I think is the only passage in ancient literature where papyrus in specifically described as *cheap*. The plagiarist of his works, he says (I. lxvi) who wishes to be acclaimed as a poet must understand that it needs more than the fee for a professional copyist and a cheap roll of papyrus:

> Erras, meorum fur avare librorum
> Fieri poetam posse qui putas tanti
> Scriptura quanti constat et tomus vilis.
> Non sex paratur aut decem «sophos» nummis.

The only explanation I can offer of Schubart's statement is as follows. Book XIV, the *Apophoreta*, consists of pairs of distichs intended to accompany gifts, one for an "expensive" gift and one for an "inexpensive" one. Two of these concern gifts of blank papyrus rolls entitled respectively «Chartae Maiores» and «Chartae Epistulares».[2] The text of the former is as follows:

> Chartae Maiores.
> Non est munera quod putes pusilla
> Cum donat vacuas poeta chartas.

The intention is obviously humorous, and the clue is given by the word *poeta*: the recipient of a package of blank rolls from a poet thinks it a fine present because he has got something useful and will not have to waste his time reading through long stretches of indifferent verse. Schubart has not seen the joke. For him, charta means a single *sheet* of papyrus, and thus the description of a number of "sheets" as «munera non pusilla» proves that papyrus was "expensive"!

In any case, if it is claimed that the former of the two distichs is proof that papyrus was "expensive", the latter, which is for the "cheap" present, can equally be held to prove the opposite. In other words, the two distichs cancel each other out, and cannot therefore contribute anything to the solution of the question.

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[2] According to Pliny, when the size of the largest grade was increased to 1 roman foot and named the *Claudia*, the *Augusta* was demoted to second place, but remained the favourite for letter-writing. It seems probable, therefore, that the "Chartae Maiores" and "Chartae Epistulares" of Martial are the Claudia and Augusta grades respectively.

Certainly the general picture which emerges from the poems of Martial is of a world in which papyrus is freely available and freely used, without any special regard for its cost.

4. Palimpsests

«Wenn man aber beobachtet . . . wie sogar die Schrift getilgt wurde, um Platz für einen neuen Inhalt zu gewinnen, so vermag man nicht an eine unbeschränkte Erzeugung zu glauben» says Schubart. And we have, of course, all seen, very occasionally, papyri showing smears of ink where attempts have been made to wash writing off the surface. But how easy was this, and how successful? In the course of an article, «La Falsification des Actes dans l'Antiquité» in *Mélanges . . . Jules Nicole*, 1905, pp. 111–134, Henri Erman thus describes his experiments in removing ink from two scraps of papyrus: «Sur les deux également l'encre s'enlevait avec une facilité étonnante et sans trace perceptible à l'oeil nu. Et cela non seulement à l'eau chaude, mais simplement du bout du doigt mouillé ou encore en grattant avec l'ongle dans le sens des fibres» (p. 119).

That writing on papyrus could be washed off with a sponge was well known in the ancient world, and could be illustrated by a number of quotations, beginning with Aeschylus, *Agamemnon* 1329, βολαῖς ὑγρώσσων σπόγγος ὤλεσεν γραφήν.

I may add that in the accounts of the *grapheion* at Tebtunis the purchase of a sponge for 1 obol is mentioned (P. Mich. ii 123 verso 30), doubtless, as the editor remarks, for washing out writing where a mistake has been made.

Since washing off writing was feasible, it was possible to treat whole rolls of papyrus in this way, thus producing palimpsests, as they were called. Some of the literary evidence for these is discussed in the book by Colin Roberts and myself, *The Birth of the Codex*, pp. 16–18. And if this was so easy, one is inclined to ask why a whole 're-cycling' industry did not spring up for re-using papyrus in this way? The answer, I think, is that it was not easy to wash writing off so completely that no traces were left behind, and palimpsests therefore were readily identifiable as such and were looked down upon as inferior material, fit only for such things as drafts or scribbling paper.

The difficulty in ensuring 100% removal of the writing is well illustrated by Ammianus Marcellinus in his account of the downfall of the usurper Silvanus (XV. 5. 12). Silvanus was *magister peditum* in Gaul under Constantius, and enemies of his tried to ruin him by obtaining some letters of his, washing off everything except his name, and inserting treasonable material. The letters were then shown to Constantius, and a furious dispute broke out in the Consistory about the authenticity of the letters. Silvanus, fearing the worst, proclaimed himself emperor at Cologne, and was assassinated soon after. Too late, the forgery was discovered by one of the judges, who «*contemplans diligentius scripta apicumque pristinorum quandam umbram repperiens, animadvertit ut factum est, priore textu interpolata longe alia quam dictaverat Silvanus*».

This episode is particularly instructive, since we may reasonably conclude that the plotters would have taken all possible measures to ensure complete removal of the original writing, but even so some traces remained. This may help to explain why palimpsesting was not more widely practised. Washing off the writing from the recto of a 10 m. roll must have involved a considerable expenditure of time and effort, and the result was only a second-class product. In other words, it was uneconomic.[3]

In any case, if one wished to re-use a roll already written on the recto, there was a very much simpler method available, *viz*, to turn the roll over and write on the other side. And opisthograph rolls will form the subject of the next section.

5. Opisthograph rolls

Rolls of papyrus in which the recto is covered with writing presented the most obvious possibility of re-use by simply writing on the verso. How prevalent was this practice, and what conclusions does it suggest for the costliness or otherwise of papyrus?

Ideally, we should need to consider all extant examples of rolls, irrespective of the nature of the contents on either side, but this is impracticable since it would involve a search through all known editions of papyri. What I propose to do, therefore, is to take the limited case of papyri which contain either on the recto or the verso,

[3] Palimpsesting was widely practised in Pharaonic Egypt, with one exception: manuscripts of the Book of the Dead were never so treated. Cf. Ricardo A. Caminos, "*Some Comments on the Re-use of Papyrus*" in *Papyrus: Structure and Usage* (British Museum Occasional Paper 60, 1986), pp. 43–53.

or both, texts of known authors, *viz.* the field covered by Pack[2] nos. 1–1566, and here through the great kindness of Professor Paul Mertens I am able to quote the latest available figures from his work on the forthcoming new edition of Pack, to be called MP[3]. I shall here use his terminology for the different classes of material, in which R° and V° stand for recto (*i.e.* with horizontal fibres) and verso (*i.e.* with vertical fibres), litt and doc stand for literary and documentary respectively, while blanc is self-explanatory. On this basis, the total number of literary papyri *not* used on the verso, i.e. R° litt/V° blanc, is 1537. Against this there are 85 R° litt/V° doc and 65 R° litt/V° litt, a total of 150 re-used. Thus, out of a total of 1537 + 85 + 65 = 1687, 1537, or 91%, were *not* re-used.

It will be seen that I have omitted one further category, viz. R° doc/V° litt, which number 164 from Pack and 170 MP[3] additions, a total of 334. The reason for excluding them is that, statistically, they ought to be compared with documentary rolls *not* re-used for any purpose *i.e.* R° doc/V° blanc, and this again could only be ascertained by searching through all known editions of papyri; possibly we should also have to calculate the number of R° doc/V° doc which would be equally difficult. Nevertheless, even if the R° doc/V° litt were to be added, the overall picture would not be very greatly altered: the new re-used total would be 150 + 334 = 484, while the new grand total would be 1687 + 334 = 2021. The number *not* re-used remains as before, and the proportion *not* re-used is reduced from 91% to 76%—still a large majority.

Furthermore, a high proportion of all literary papyri come from Oxyrhynchus, and virtually all papyri from Oxyrhynchus come from rubbish heaps. This means that a roll written only on the recto must have been thrown away as refuse, and cannot therefore have had any commercial value. That a certain number of such rolls were re-used does not invalidate this statement, but merely illustrates the principle, already stated above in regard to ostraca, that a writing material which costs nothing will always be cheaper than one which costs something.

Conditions in Rome do not appear to have been greatly different, to judge from what Martial tells us. There, the re-use of rolls written on the recto seems to be regarded with contempt:

> Scribit in aversa Picens epigrammata charta
> Et dolet averse quod facit illa deo. (viii. 62)

that is, Picens writes his poems on the back of a roll, and the god turns his back on him (*i.e.* his work is unsuccessful). What was this roll? It cannot have been intended for commercial sale, for in that case it would have had to be written by a professional scribe on new papyrus. Is it then his rough draft? But surely no one could condemn a writer for using scrap paper for this purpose. It seems to me that what is meant is Picens' fair copy intended to be given to a professional copyist for reproduction: if a poet had so little faith in his work as to use scrap paper for this purpose, he deserved to be ridiculed.

To-day, unsuccessful books are pulped. In Martial's day they were put to commercial use, as he tells us repeatedly. For instance, when he says that it is essential for a work to obtain the approval of the famous critic Apollinaris, and details the dreadful fate which awaits them, in kitchens, shops and schoolrooms, should they fail to do so:

> Si vis auribus Atticis probari,
> exhortor moneoque te, libelle,
> ut docto placeas Apollinari,
> nil exactius eruditiusque
> sed nec candidius benigniusque.
> Si te pectore, si tenebit ore
> nec rhonchos metues maligniorum
> nec scombris tunicas dabis molestas.
> Si damnaverit, ad salariorum
> curras scrinia protinus licebit,
> inversa pueris arande charta. (iv, 86)

The last line is important, since it shows that Martial had in mind rolls written on the recto only, but which even so had no commercial value. Similarly he tells Populus that it is not enough for a poet to be called «disertus»:

> Quam multi tineas pascunt blattasque diserti
> Et redimunt soli carmina docta coci. (vi. 61.7–8)

That is, only cooks will buy them, for culinary purposes. These again would be rolls written on the recto only.

In conclusion, we must consider the case of the greatest re-users of papyrus known to papyrologists, namely the management staff of the Appianus estate in the Fayum in the middle of the 3rd century A.D., for which we now have the detailed study of Dr. Dominic Rathbone, *Economic Rationalism and Rural Society in Third Century A.D. Egypt: The Heroninus Archive and the Appianus Estate*, 1991. As he has shown (pp. 9–14), the staff of the estate used anything and everything which they lay their hands on. Literary rolls (how they obtained them we do not know) provided an excellent source of material; but the greatest source of supply was provided by obsolete (and some not so obsolete) documents from the state and municipal archives at Arsinoe. Finally, they even cannibalized their own archives. All this does *not*, of course, prove that papyrus was "expensive", but that such a procedure, if pursued (and *only* if pursued) as a settled policy by all members of the staff (including Aurelius Appianus himself) would be bound to produce worthwhile savings. But to the ordinary citizen such a programme was not available; and the Appianus archive thus provides a perfect example of the proverb *exceptio probat regulam*.

As a kind of footnote to this section I would like to refer to a passage in Sir Eric Turner's *Greek Papyri*, 1968, pp. 5–6, in which, after stating that "there were times in the ancient world when papyrus was relatively inexpensive" he goes on to say: "nevertheless, even upper-class individuals would often emulate 'paper-sparing Pope' and write on the back of a sheet or roll already used for other purposes". I have long been puzzled to know who these 'upper-class individuals' were. Were they some prominent Oxyrhynchus family? But the reference to Pope suggested to me that Turner might have had in mind a passage in C. C. Edgar's introduction to his volume of the Zenon papyri in Michigan (P. Mich. Zen.), p. 25, where, after saying that the papyri can have been of little use to him in later life, he remarks "occasionally we find that he utilized an empty verso for an agricultural account or for a register of letters, but he is not to be regarded as a miserly collector of writing material". He then quotes the well-known quatrain on Pope:

> Send these to paper-sparing Pope[4]
> And when he sits to write,
> No letter with an envelope
> Will give him more delight.

This passage does not, however, agree very well with Turner's remarks. Whether Zenon could be described as an 'upper-class individual' is at least debatable, and in any case he certainly did not 'often' reuse papyrus. On the whole, therefore, it seems to me more likely

[4] Alexander Pope (1688–1744). The original manuscripts of his translations of Homer, written on envelopes and the backs of letters, are in the British Library, numbered Additional MSS. 4807–4809.

that the personage whom Turner had in mind was none other than Aurelius Appianus. As a man of equestrian rank, a former exegetes, a senator of Alexandria, an owner of extensive estates in the Fayum and, probably, elsewhere, and referred to as εὐσχήμων, he was certainly an upper-class individual; and Rathbone has noted that of his thirteen published letters, three were known to have been written on wholly blank pieces of papyrus and six on re-used pieces (*op. cit.*, p. 11). That this is the most likely source of Turner's statement is confirmed by his subsequent article on the re-use of papyrus on the Appianus estates, "Writing materials for businessmen", *BASP*, 15, 1978, pp. 163–9. Of course Appianus could perfectly well have afforded to buy new papyrus. But having decided upon a policy of reusing scrap materials wherever possible, and imposed it on his staff, he may well have felt that he himself ought to set an example.

6. Requests for papyrus

«Wer in manchem Brief gelesen hat, dass der Schreiber den Empfänger um Zusendung eines Papyrusblattes bitter, damit er antworten könne . . . wird geneigt sein, den Papyrus als ein ziemlich teures Material zu betrachten» says Schubart. It is certainly true that letters exist in which papyrus is asked for (BGU 828, 28 is a good example) or, alternatively, that papyrus is being sent.[5] But what conclusions can be drawn? The fact is that private letters are full of notices of goods of all sorts being sent or requested, in the vast majority of cases without any mention of payment being asked or expected. Of course the circumstances would be perfectly well known to the correspondents, and so did not need to be spelled out, but in our total ignorance of the situation we are not entitled to speculate, as Schubart does, that such a writer was too poor to buy the "expensive" papyrus, (the cost of which would thus presumably fall on the addressee). As Lewis says (*op. cit.*, p. 91 n. 8) requests for papyrus may simply mean that the writer is far from a source of supply, as was certainly the case in one of the ostraca from the Wadi Fawâkhir in the Eastern Desert (SB 9017/15), in which the writer asks his correspondent to

[5] "Das tat man ofters, weil Papyrus teuer war" (W. Schubart, *Griechische Papyri: Urkunder und Briefe vom 4. Jh. v. Chr. bis ins 8 Jh. n. Chr.*, 1927, p. 65).

bring with him when he comes χάρτην ἐπιστολικὸν ὀβολῶν η´ since papyrus would obviously be unobtainable in this desolate outpost. In the reverse case, where a writer states that papyrus is being sent, this may be merely to encourage the addressee to reply, for instance in P. Flor. 367, where the writer is trying desperately to extract an answer from his hard-hearted brother. Similarly in P. Mich. viii 481, where Terentianus sends papyrus to his sister Tasoucharion, asking her to write περὶ τῆς ὑγίας ὑμῶν she certainly cannot have been short of money, in view of the supplies which she is being asked to send.[6] To send some blank papyrus with a letter was thus no more significant than a stamped and addressed envelope would be to-day.[7]

Finally, "in *manchem* Brief . . ." Are such letters really so common? As an experiment, I looked through all the published volumes of the Oxyrhynchus Papyri, checking the indexes under χάρτης, since this word must inevitably occur in such a context. The result was that no such mentions of requests or sendings emerged out of the 460 private letters included in the volumes. All in all, therefore, even if Schubart was correct, I do not think this could be a significant factor in the debate.

7. The Price of Papyrus

At first sight, the most obvious means of deciding whether papyrus was regarded as "cheap" or "expensive" in the ancient world might appear to be to determine what papyrus actually cost. But here there are almost insuperable difficulties. Although purchases of papyrus are sometimes recorded in accounts, we are never told how much papyrus is being bought, or which of the various sizes and qualities listed by Pliny is involved. In addition, the prices themselves show a wide range and puzzling fluctuations which make it very difficult to form even a general impression.[8]

In these circumstances what I propose to do here is to take two groups of prices which seem to offer some prospect of reaching

[6] *Viz.* two *keramia* "of the largest size" of ἔλαιον ῥαφανινόν, and 1 artab of olyra.

[7] A rough estimate of what papyrus for the writing of a single letter might cost is given in §7.

[8] For a complete list of prices of papyrus during the first three centuries A.D. see H. J. Drexhage, *Preise, Mieten/Pachten, Kosten und Löhne im römischen Ägypten bis zum Regierungsantritt Diokletians*, 1991, pp. 384–7.

conclusions. The former is the accounts of the Appianus estate in the mid-third century A.D., for which we now have the study of Dominic Rathbone mentioned above. The draft monthly accounts of the estate regularly include an item τιμὴ χάρτου εἰς τὸν λόγον (δρ. δ), and Rathbone has shown that these entries relate to the fair copies of the accounts which had to be rendered to the headquarters of the administration at Arsinoe. These fair copies were not in fact executed monthly, but were sent in all together at the end of the year, and the monthly charge was therefore a provision against future expenditure, since at the end of the year the papyrus would need to be bought.

Can we say how much papyrus would be needed for the fair copy of a typical monthly account? For this purpose we can take the draft monthly account for Payni, 253 A.D., which has been reconstructed from fragments and published by Rosario Pintaudi and Dominic Rathbone in *Analecta Papyrologica*, i, 1989, pp. 79 ff., with facsimile. This account is written in a small cursive hand with numerous abbreviations (in fact the autograph of Heroninus, the local manager at Theadelphia), on a roll 190 cm. in length, to which must be added an account of hay on the verso, 100 cm. long, making a total of 290 cm. I do not think there can be any doubt that the fair copy of this, written in a legible hand with expansion of the abbreviations, would have necessitated a roll of the standard length of 340 cm., for which we now have a price of 4 dr.

The second group which I propose to examine is in the accounts of the grapheion at Tebtunis (*circ.* 45–49 A.D.), published in P. Mich ii. This is especially valuable as the largest surviving collection of prices of papyrus, which are conveniently listed on p. 98 of the edition. In the great majority of cases the price of a roll is given as 4 dr. (or two for 8 dr.), and the few variations from this are mostly insignificant, *viz.* 4 dr. 1 ob. (once), 23 ob. (twice) and 20 ob. (twice); but there are two startling discrepancies of 2 dr. which are discussed below.

Since, as we have seen, the regular price for a standard roll in the mid-third century was 4 dr., it is disconcerting to find apparently the same figure two centuries earlier, since there can be no doubt that during this interval there was a general rise in prices of about 100%. For instance, the daily wage of a labourer in the 1st cent. A.D. very rarely exceeded 1 dr., whereas on the Appianus estate the average was 2 dr. In the Tebtunis accounts the price of an artaba of wheat was about 8 dr., but on the Appianus estate the average was 16 dr. 4 ob. Wine prices are notoriously difficult to compare because of the varying capacity of the keramion, but we may note that the highest price at Tebtunis was 4 dr., compared with the Appianus estate average of 12 dr. 2 ob. It is thus impossible to believe that the price of papyrus remained unaltered during this period, and I do not think there can be any doubt that the rolls normally purchased by the Tebtunis grapheion must have double the normal length. As I pointed out in *ZPE* 45, 1982, 169–175, (= chapter A4, above) although papyrus left the factories in standard rolls of 20 sheets, the dealers through whom it reached the customers could, and did, make up rolls of any length desired by pasting together rolls or sections of rolls, and such oversize rolls were designated either by the number of τόμοι they contained, e.g. τρίτομος, or the total number of sheets, e.g. πεντηχοντάχολλος. I conclude therefore that the rolls normally used at Tebtunis consisted of two standard rolls pasted together, and would have been classified as δίτομοι, although this term is not yet attested. That they are not so described in the accounts in due to the fact they were the normal size in use and therefore familiar to everyone. When this is accepted, the two cases of 2 dr. fall into place, as they are simply rolls of normal 20-sheet size.

We have now two reasonably certain prices for rolls of papyrus of standard size, *viz.* 2 dr. in the mid-1st cent. A.D., and 4 dr. two centuries later. But how much writing material did such a standard roll contain? In my article quoted above, I argued that the length of the standard roll of twenty sheets would be approximately 320–360 cm in length, an average of 340 cm.; and this figure of 340 cm. now seems to be widely accepted, although it is, of course, only a rough approximation.

If we accept this figure of 340 cm. as the length of the standard roll, what about the other dimension which determined the area of writing surface provided, *viz.* the distance between the upper and lower edges of the roll, which all papyrologists have hitherto called the "height"? Here I must declare my support for the brilliant suggestion of Professor Bülow-Jacobsen that this dimension is what Pliny calls the *latitudo*, hitherto interpreted as the distance between *kollemata*.[9] Once seen, this is obvious, since *kollemata* were totally

[9] *ZPE* 60, 1985, pp. 273–4.

ignored by users of papyrus, who wrote straight across them, whereas the "height" is of vital importance since, as just mentioned, this, plus the length, determines the surface area of the roll.

Pliny quotes the *latitudo* of the various grades and qualities of papyrus concluding with the statement that Claudius increased that of the largest size to 1 Roman foot (= 29.5 cm.), *auxit latitudinem pedali mensura*, which he says was preferred to all others (*praelata omnibus*); und when we look at the rolls used in the grapheion at Tebtunis during the reign of Claudius we find that P. Mich 121 is stated to be 29.3 cm. broad, while P. Mich. 123 is described as 28 cm. broad but "having the upper and lower edges worn down and badly frayed" (as is confirmed by the plate), so that its original breadth must have been much closer to 29.5 cm. I would therefore identify these two rolls as speciments of the *Claudia* grade, which accordingly contained about 340 × 29.5 or round about 10,000 cm^2 of writing surface.[10]

This seems about as near as we can get to estimating the actual cost of papyrus in money terms; and this general impression would seem to confirm the verdict of Professor Lewis quoted at the outset of this paper.

We can now return to §6 and the writer who, on Schubart's thesis, could not afford to buy papyrus to write a letter. If we imagine a papyrus roll of standard length (340 cm.), costing 2 dr., cut into 60 portions, each would measure about 28 × 22 cm., ample for a average letter, and would cost one-fifth of an obol—surely not an excessive expense!

8. The Challenge of the Codex

For those who believe that papyrus was "expensive" the invention and development of the codex is of crucial importance, since it offered the possibility of reducing the amount of papyrus used in the manufacture of a book by almost 50%. We might therefore have

[10] The smaller sizes, according to Pliny, were measured in Roman *digiti* (1 digitus = 1/16 of a Roman foot). Although I do not recollect ever having seen this clearly stated, it seems to me evident that the measurements are given in *digiti* because they were manufactured in *digiti*—in other words, the Romans had reorganised the industry and had naturally imposed their own scale of measurements. Since the widest grade was standardised at 1 Roman foot, it would obviously have been sensible to fix the smaller sizes in terms of *digiti*.

expected that the new format would be speedily and enthusiastically adopted. We might also have expected that it would be welcomed by professional scribes and bookdealers, since it offered the chance of reducing their costs and therefore encouraging sales. But the result was far otherwise, for it took some 200 years from the time of its introduction for the codex to achieve equality with the roll, and another 200 or more before finally replacing it.

The forces militating against adoption of the codex must have been numerous and powerful. But it there any way in which the degree of preference in favour of the codex (and therefore also the strength of the opposition to it) can be measured? It seems to me that one way might be to consider the proportion of the page of a codex which is occupied by the column of writing, since one obvious way of exploiting the advantage of the codex would be to reduce margins as far as possible, thereby saving on the cost of papyrus. This is in fact an area of study suggested by Turner in his *Typology*, p. 25, where he says: "The relationship between the size of the written area (the β measurement) and complete page (the α measurement) deserves investigation".

If we examine Turner's "Consolidated List of Codices Consulted" (List 16) in *Typology*, we find that among the 411 numbers covering the general field of Greek literature there are only fourteen cases of papyrus codices where the dimensions of both page and written area are precisely known. Let us however take these 14 and see where they lead us. In the following list the number is followed by the title or other description of the work, the date, and the proportion of the page occupied by the text:

45.	Demosthenes. 5(?)th cent.	54.6%
91.	Hesiod. 5th cent.	50.6%
104a	Homer. 5th or 6th cent.	63%
106	Homer. 3rd cent.	78.9%
148	Homer. 5th–6th cent.	41.5%
210	Isocrates. 5th or 6th cent.	49.4%
225.	Menander. 4th cent.	72.3%
227.	Menander. 4th–5th cent.	53%
243.	Philo. 3rd cent.	50.4%
244.	Philo. 3rd cent.	61.3%
266.	Theocritus. 5th–6th cent.	72.4%
341.	Astronomy. 3rd cent.	74.4%
347.	Life of Alcibiades. 5th cent.	50.1%
409.	Aristotelian physics. 6th–7th cent.	56%

As will be seen, there is a very wide range in the percentages, some extremely high, others surprisingly low, without any discernible pattern. All one can say is that the cost of papyrus does not seem to have played any part in choosing the format of the manuscript.

A different line of approach is through consideration of Christian codices of the Gospels, which have been far more intensively studied from the codicological point of view, and statistics are readily available in the repertoria of Kurt Aland and Joseph van Haelst. Here I propose to consider the total amount of papyrus, calculated in square centimetres (cm^2) used in the construction of the codices.

For these calculations I have used the methods employed in my reconstruction of the Chester Beatty codex of the Gospels and Acts (P 45), published in the journal *Hermathena* [see B5 below]. I must emphasise that except in the cases of P 66 and P 75 the figures are based on small fragments and must be regarded as no more than very rough approximations.

As controls, it is convenient to use the Chester Beatty codex, since the number of pages occupied by each Gospel, and therefore the amount of papyrus used, is precisely known. I have therefore placed the P 45 figure at the head of each column.

Matthew		Mark		Luke		John	
P 45	*12,250*	*P 45*	*8,000*	*P 45*	*12,000*	*P 45*	*9,500*
P 1	13,364	P 88	16,715	P 4	11,088	P 5	14,451
P 19	22,425			P 75	14,703	P 28	13,455
P 21	23,437			P 82	20,295	P 52	21,087
P 37	11,447					P 66	17,943
P 86	17,417					P 69	16,500
P 87	17,638					P 75	9,633
						P 95	13,730

The foregoing figures demonstrate, beyond any doubt, that despite all that has been claimed for the economy of the codex, when a manuscript came to be written, no account whatever was paid to the amount of papyrus consumed. Indeed, some of the Gospel codices are constructed in such a way that the text could have been written on a roll using the same amount of papyrus. This applies even to the earliest specimens. P 52 is the celebrated Rylands fragment of John, still generally regarded as the earliest surviving Christian manuscript, yet it used more than twice as much papyrus as P 45.[11] In fact, it is another very early Christian manuscript, Chester Beatty MS. VI, of Numbers and Deuteronomy, which I think must hold the record for its lavish use of papyrus, the text occupying only 30% of the total page area, leaving 70% blank.[12]

Thus, wherever papyrus may have been regarded as "expensive", it was certainly *not* in the field of book-production.

9. Conclusion

We have seen that the opinion of Wilhelm Schubart, that papyrus was relatively expensive ('ziemlich teuer') was based primarily on his belief, now universally discredited, that χάρτης denoted, not a roll, but a single sheet of papyrus. The additional arguments with which he attempted to support this thesis have now been considered individually and have found to be either false or greatly exaggerated. This might seem decisive. But there is another factor, deeply rooted in human nature. For some, anything which appears to question established facts seems to have a fatal fascination, or, as Sir Frederic Kenyon once happily put it, "the attractiveness of the improbable".[13] And if anything at all is certain, it is that the present article will not be the last word upon the subject.

Finally, I wish to express my gratitude to Dr. Revel Coles for his unfailing help and encouragement.

[11] The figure given here is based on Turner's estimate of the original page size and my own estimate of the original number of pages. In the *editio princeps* of 1935 Roberts estimated that the manuscript would have consisted of 66 leaves measuring 21 × 20 cm., which would have produced the enormous total of 27,720 cm^2, almost three times the P 45 figure.

[12] In estimating the original dimensions of the codex, a warning must be given, *viz.* that in the volume of plates issued in 1958 the reproductions of the larger fragments are slightly reduced in size to accommodate them within the page. One page, in original size but with upper and lower margins cut off, appears in the General Introduction. My figures are based on Kenyon's estimate of the original page size, 13 ins. × 7 1/2 ins. = 33.02 × 19.05 cm. = 629 cm^2. The two columns of writing measure about 19,5 × 5,3 cm. each, a total area of 206,7 cm^2.

[13] Introduction to *The Codex Alexandrinus in Reduced Photographic Facsimile*, Old Testament, Part 1, 1915, note 1. The comment related to the suggestion that the codex came from Mount Athos.

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B

NEW TESTAMENT MANUSCRIPTS

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1

A) FOUR YEARS' WORK ON THE CODEX SINAITICUS: SIGNIFICANT DISCOVERIES IN RECONDITIONED MS.

Two years of almost complete silence have followed the first excitement of the purchase and exhibition in London of the Codex Sinaiticus. Mr. Skeat describes below how the task of binding and reconditioning the manuscript has been performed, and sets forth some of the striking results which have attended research, by himself and a colleague, Mr. Milne, into textual obscurities by the light of the ultra-violet lamp.

Just over four years have passed since, on Dec. 27, 1933, the Codex Sinaiticus was first placed on public exhibition in the British Museum and at once became the centre of a pilgrimage unequalled in the history of that institution.

The unexpectedness of its purchase from the Russian Government, the evergreen story of Tischendorf's romantic discovery, and, above all, the traditional interest of this country in the Bible, stirred national curiosity to a high pitch; for weeks long queues of sightseers filed past the showcase in which the manuscript lay.

The Binder's Triumph

Once the purchase had been completed by the public subscription of £50,000 (a figure since increased to nearly £65,000) the most urgent task facing the Museum authorities was the provision of a new binding.

On arrival at the Museum the Codex was in exactly the same state as when Tischendorf first saw it on that memorable evening of Feb. 4, 1859—a bundle of loose leaves and quires, held together mainly by glue liberally applied to the back by some particularly inexpert binder of the Middle Ages. After months of careful preparation and experiment by Mr. Douglas Cockerell, who had been called in for the purpose, the method and style of binding were agreed upon, and the work was carried out in the summer and autumn of 1935.

No pains were spared to obtain the best and most durable materials. Vellum in many different thicknesses and shades to match variations in the manuscript itself was specially prepared, and samples of sewing-thread, hempen cord, linen, paper, and even glue, were examined and compared.

Many days were then spent in patching and repairing innumerable small slits and tears, after which the badly cockled leaves were pulled out flat by an ingenious device of Mr. Cockerell's own construction.

Tischendorf Tested

Each leaf was placed in a humid atmosphere, and when thoroughly limp was transferred to a wooden frame having clips, attached to weighted strings, ranged round the edges. These clips were fastened to the edges of the vellum until the pull was equal in every direction, and the sheet dried as flat as a drum-head. With vellum 1,600 years old, and often only two or three thousandths of an inch thick, the risk of its parting under the strain might have seemed a grave one; but Mr. Cockerell's good opinion of its strength was confirmed, and no mishap took place.

The actual sewing and binding brought Mr. Cockerell's task to its close, and for many generations to come the two stately volumes, with their white morocco backs and stout boards of English oak, are likely to remain a witness to his skill and patience.

Now that the manuscript could be easily handled, the time had come for a new investigation of its contents. In spite of Tischendorf's well-established reputation as a palæographer, there had hitherto been no real opportunity to test the validity of his statements. St. Petersburg was remote, life there difficult and expensive, and except for Professor Kirsopp Lake, who made two journeys thither, in 1908 and 1911, to photograph the whole manuscript for his facsimile edition, few scholars had set eyes upon it during its 70 years' sojourn in the Imperial Library. Now, however, each of Tischendorf's 15,000 critical notes has been compared afresh with the original; and he emerges from the test with flying colours. Considering the extraordinary difficulties he had to overcome, and the high pressure at which he worked, his great *editio princeps* of 1863, with its full-size facsimile in specially-cut type, is a marvel of precise and painstaking scholarship.

Nevertheless, in the course of our work a number of discoveries have been made. In some cases mechanical aids unknown to Tischendorf such as the ultra-violet lamp, have literally shed new light. One example is the last verse of St. John's Gospel (John 21:25),

> And there are also many other things which Jesus did, the which if they should be written every one, I suppose that even the world itself could not contain the books that should be written.

The eminent French critic, Professor Vaganay, in a recent study of this very passage, tentatively concludes on grounds of vocabulary, style and contents that it is no part of the original Gospel, but a makeshift ending put together when the real ending got accidentally transferred to the preceding chapter (John 20:30–31).

New "Final Verse"

Now Tischendorf has noted that in the Sinaiticus there is something peculiar about this final verse; the ink, the shapes of the letters, and the whole appearance of the writing of this verse looks slightly different compared with the rest of the page. And he inferred that it had been omitted by the original scribe, and supplied at a later stage by one of the other scribes working on the manuscript.

Responsible scholars have since questioned or even contradicted Tischendorf's conclusions. Now, however, the question has at last been settled; for ultra-violet light brings up traces of half-effaced writing which show that the scribe actually did stop at the last verse but one, and finished off the book in the usual way by adding a *coronis* or tail-piece, and the title, "Gospel according to John" (which, as in all early manuscripts, is regularly placed at the end, not at the beginning). Subsequently, however, he changed his mind, washed the vellum clean, and inserted the final verse, rewriting the tail-piece and title lower down the page.

The Sinaiticus in its "first state" still remains the only authority for omitting the verse, but it is an authority of prime importance, and its evidence, as now for the first time revealed, will play an important part in future discussions of the problem.

Work of Three Hands

Mention of the tail-pieces brings us to a fresh point which investigation of the manuscript has made clear.

In this instance it is useful as proving that one and the same scribe omitted, and eventually included, the final verse. For the various scribes employed on the manuscript were so highly skilled that they could write practically identical hands, and previous scholars have been hard put to it to distinguish them. Here the tail-piece provides a new and welcome clue. It is clear that when the scribe had finished the actual text he considered himself "off duty" and at liberty to indulge his individual fancies; hence in the tail-piece each scribe develops his own favourite design or stock of basically similar designs, which in fact amount to his sign-manual. This enables us to prove that the Codex is the work of three different scribes, and not, as Tischendorf had supposed, four—the section he assigned to his "Scribe C" being in reality partly by Scribe D and partly by Scribe A.

But why, it may be asked, was it necessary to employ a number of scribes to write a single manuscript?

Before answering this we must clear our minds of all ideas of Christian monasteries where the copying of manuscripts was part of the routine work. In the main it is highly probable that Christian book-production was still on a purely commercial basis, and the well-known letter in which the Emperor Constantine orders Eusebius, Bishop of Caesarea, to supply 50 vellum Bibles for the new churches of Constantinople shows that these great volumes were the result of something like mass production.

Ill-paid Scribes

Naturally, a customer ordering a Bible would not wish to wait a year or so while a single scribe copied out the entire book; and consequently the bookseller as he received each order, would turn several scribes to work on different parts of the same volume.

These scribes, we can be sure, were no pious Christians engaged on a labour of love, but merely skilled and industrious artisans, miserably paid according to our standards, and probably of little education. But for all that, it is their handiwork and the way in which they accomplished it that has given scope for fresh discoveries, and the conclusions we have been able to draw reach out beyond the Codex itself over the whole field of ancient literature.

Strange to say, there are few phases of ancient economic life about which we know so little as book-production. Hitherto, for instance, scholars have not even been able to agree whether books were copied by eye from another manuscript lying in front of the writer, or whether a reader dictated to one or more scribes. Here the Codex speaks with no uncertain voice, for differences in the standards of spelling of the three scribes are so marked as to leave no doubt that dictation was the method employed.

How such vagaries of spelling could be caused by dictation needs to be explained. By the fourth century the pronunciation of Greek had largely approximated to that of the modern language, many of the ancient vowels having changed their quality so as to become mutually indistinguishable, while important consonantal changes added to the confusion.

It follows that dictation opened the flood-gates to all kinds of corruptions, unless the scribe had had a thorough grounding in spelling. In the case of the Codex, Scribe D evidently possessed this important advantage, for his spelling is practically beyond reproach. Not so Scribe A, whose representation of vowels is often at fault. But it is when we come to Scribe B that all records are broken; in fact, the real difficulty is to understand why he could ever have been chosen for the work. Not only is he all at sea with vowels and consonants alike, but his writings are disfigured by gratuitous blunders of the crudest kind.

There are a number of signs which suggest that the Codex was never finished, but was laid aside, perhaps as unsaleable; if this was really the case the unsatisfactory nature of Scribe B's work must have been one of the prime causes of the decision.

B) STRIKING RESULTS OF EXPERTS' DETECTIVE WORK

Prolonged study of the text of the Codex Sinaiticus in the British Museum, after the leaves were reconditioned and bound, has enabled Mr. Skeat and his colleague, Mr. H. J. M. Milne, to bring to light a number of important discoveries. Some were described in an article yesterday, when it was shown that the manuscript was compiled by scribes working from dictation.

In the following article interesting evidence is quoted to show where

> it was compiled and how it was corrected. The very faults and errors of the original text are regarded as conferring on it a special value which no facsimile of the manuscript could possess.

The proof that dictation was employed in the compilation of the Codex is of vital importance, for it affects our whole attitude to the book.

A manuscript faultlessly copied by eye, letter for letter, is by comparison a dead thing, telling us little or nothing of the personality or circumstances of the writer; but a dictated manuscript is a very different proposition, for the vastly increased opportunities for error, however regrettable they may seem in themselves, do give us a chance to learn something of scribe and reader alike.

The Hesitant Dictator

Most people know only too well what absurd mistakes can arise through dictation—how a chance remark or interjection becomes incorporated in the text, a hard or unusual word supplanted by a commoner one of roughly the same sound, a phrase written twice over, the order of words altered, and so on.

An excellent example of this in the Codex itself is to be found at I Maccabees 5:20; here the scribe, instead of writing the figure "8,000," produced a seemingly nonsensical jumble of letters and numerals which can, however be correctly translated as "either 6 or 3,000"; and there is not the least doubt that the reader, unable to decipher the number in the manuscript before him, called out "either 6 or 3,000", which the scribe innocently wrote down verbatim!

Another such case is one to which Dr. Rendel Harris first called attention in a brilliant essay over 50 years ago, when it was sought to identify the Codex with Caesarea.

In Matthew 13:54, where the scribe should have written *eis ten patrida* (= to his own country), he substituted *eis ten antipatrida* (= to Antipatris).

When the Scribe Nods

Now Antipatris was a town of some size 30 miles south of Caesarea, and to quote Dr. Harris's own words: "As it seemed impossible to me that this should be an assimilation to a passage in the Acts [Acts 23:31] where Antipatris is mentioned, I referred it to the aberration of a scribe's brain as he sat writing in the neighbouring city of Caesarea. It is to my mind much the same as if a printed text of Shakespeare should put into Mark Anthony's speech the line—

> I come to Banbury Caesar, not to praise him.

Such a text would probably be the work of Oxford printers."

But this instance does not stand alone; for in Acts 7:5, in the sentence "Philip went down to Samaria," the scribe actually writes "to Caesarea." At first sight this may not seem significant, for Caesarea is mentioned often enough in Acts; but the *first* mention of it does not occur till 8:40—after the verse in question. This is accordingly striking confirmation of Dr. Harris's theory.

Moreover, following the same line of thought, there is even a third scrap of evidence pointing to Palestine; in I Maccabees 14:5, we find the word *Hippos* substituted for *Joppa*, and though it might be argued that no Palestinian scribe would bungle the name of Joppa, the fact remains that there actually was a town named *Hippos* in Galilee. And those who have studied the Codex will be cautious in setting any limits to the type of mistake which the scribes could *not* make.

A very strong case can therefore be made out for Caesarea as the provenance of the Codex, especially as from later notes inserted in some of the Old Testament books we can infer with practical certainty that it was at Caesarea in the fifth or sixth century.

Link Between Two Codices

The importance of all this detective work derives largely from the fact that the Codex Sinaiticus and the Codex Vaticanus are so indissolubly linked that they must both have been written in the same neighbourhood, if not in the same writing establishment. Thus to fix the Sinaiticus at Caesarea would settle the origin of the two most important manuscripts of the Bible.

Finally, something remains to be said of the various corrections in the Sinaiticus, for their great number alone puts the manuscript in a class by itself, and the study of them, intricate though it certainly is, bids fair to be the most fruitful field of research in the future.

The later corrections made in the fifth and sixth centuries simply attempt to adapt the Codex to the type of text then fashionable, tending towards the "Received Text" which in the end superseded all others. But besides these are a large number of corrections put in at a very early date, almost as soon as the Codex was written.

These are especially abundant in the Gospels, where hundreds of them are to be found. Many are merely corrections of transcriptional blunders, and represent the revision undertaken in every respectable writing establishment to ensure that the manuscript was a faithful reproduction of the one from which it was copied. But there is a large residuum of a very different kind, where the corrector introduces some change of wording or makes some addition or deletion in the text.

Source of Corrections

For example, the original scribe omitted the explanatory sentence in John 4:9, "for the Jews have no dealings with the Samaritans" with some of the "Western" family of manuscripts; it is, however, supplied by one of these early correctors. On the other hand, the verses of describing Christ's agony in Gethsemane (Luke 22:43–44) and the Word from the Cross, "Father, forgive them, for they know not what they do," omitted by the Codex Vaticanus, are both included in the Sinaiticus by the original scribe, but are marked for deletion by an early corrector.

It is clearly very desirable to identify the authorship of these corrections and the source from which they come. Former scholars have erected an elaborate system postulating no fewer than eight different correctors—so elaborate, in fact, as to be its own reductio ad absurdum. On the contrary, there are good reasons for believing that two persons and two only were concerned, and that they were none other than our old friends Scribe A and Scribe D, who seem to have both revised the Gospels independently.

Moreover, we can even make a shrewd guess at the source of the corrections. In some instances we have been able to prove that they actually existed in the very manuscript from which the Codex was copied, and in the absence of contrary evidence we may reasonably assume it for the remainder. This "Codex behind the Codex" we can picture as a kind of master-copy, swarming with corrections and various readings which had accumulated during years of research and comparison of different manuscripts.

The importance of the correct understanding of these matters is obvious when it is remembered that the Sinaiticus and Vaticanus are the twin pillars of the so-called "Neutral" text, on which the English Revised Version is based and which, it is still generally agreed, represents the purest version of the Gospels.

The discoveries of the last 50 years, while they have not shaken the prevailing faith in the Neutral text, have placed it in an altogether new light. To its original champions, Westcott and Hort, its excellence seemed evidence of a primitive integrity, uncontaminated by later textual changes and corruptions. To-day this is no longer tenable, and we have to regard it as the product of a deliberate editorial revision.

Virtues in the Defects

But it is not necessarily the worse for that; the general reasons which in the first instance commended the Neutral text to Westcott and Hort still retain their cogency, and in accepting it as a revision we must recognise in it the hand of a master-editor whose fearless and objective treatment of the text mark him out from all other scholars of the early Church (not even excluding Origen, who was a very learned man but a very bad textual critic). It is not impossible that the Sinaiticus, or rather the manuscript from which it was copied, gives us an actual glimpse into the workshop in which the Neutral text took shape, the work of the early correctors reflecting the actual labours of the editor, excising, adding or altering to bring his text into conformity with his idea of the primitive form of the Scriptures.

Much emphasis has been laid here on the defects of the Sinaiticus, and it is certainly true that, for all its fine looks, as a book it is exceptionally faulty. But it would be wrong to conclude that these errors detract in any way from its value as a witness to the Bible text, for they are almost without exception due to pure carelessness or ignorance, and hence can be easily discounted. Indeed, so far from lowering the value of the Codex, these apparent defects are its peculiar asset, for, as we have seen, they reveal to us precious and intimate details about the writing of these great manuscripts which could never be learned from other and more correctly written copies.

Where Facsimile Fails

The foregoing will, perhaps, have served to dispel any idea that existence of a complete facsimile makes the original worthless.

Nothing could be farther from the truth. Even where the manuscript is in perfect preservation, inspection of the facsimile can never give the same absolute certainty as a glance at the original, and where the ink has faded, the vellum become rubbed or dirty, or the evidence obscured by over-writing or erasure, the best reproduction in the world is useless. In every instance the final appeal must be to the manuscript and the possession of such a treasure, and the preservation of so venerable a relic for future generations, is something of which the country may feel proud.

2

THE PROVENANCE OF THE CODEX ALEXANDRINUS

In *JTS* xi (1909–10), pp. 603–6, Professor F. C. Burkitt challenged the generally held view that the Codex Alexandrinus came from Alexandria. Discussing the well-known Arabic note signed 'Athanasius the humble', and attaching the manuscript to the Patriarchal Library,[1] he objected to the ruling identification of the writer with Athanasius II,[2] Patriarch of Alexandria from 1276 to 1316, on the grounds that (*a*) a Patriarch would not fail to style himself as such, and (*b*) that palaeography was not able to determine the date of the note. He then adduced the reported statement by Matthaeus Muttis, a deacon of Cyril Lucar, to the effect that the manuscript had been found on Mount Athos; and he suggested that, if this report be accepted, the manuscript might have been taken to Egypt by Cyril in 1616, when he returned to that country after a sojourn in Constantinople, and that all the Arabic writing in the manuscript could have been inserted between that date and 1621, when Cyril was elected Oecumenical Patriarch. On this supposition 'Athanasius the humble' might, he argued, have been 'some person of Cyril's staff who had charge of his library'. Burkitt's hypothesis is of more than mere bibliographical interest, since he went on to suggest that, if the manuscript was found on Athos, it probably came originally from Constantinople, and represents a Constantinopolitan text. 'All this', Burkitt concluded, 'is quite inconclusive'; but this has not prevented his theory from being quoted favourably in such a work as Mrs. Lake's *Family Π and the Codex Alexandrinus*, 1937, p. 9.

Sir Frederic Kenyon, in his introduction to the first Old Testament volume of the reduced facsimile of the Codex Alexandrinus (1915), referred to Burkitt's article, and had no difficulty in showing that

[1] The note, as translated by Burkitt, reads: 'Bound to the Patriarchal Cell in the Fortress of Alexandria. He that lets it go out shall be cursed and ruined. The humble Athanasius wrote (this).'

[2] Usually called Athanasius III in English textbooks, following Le Quien's *Oriens Christianus*, but the Athanasius II implied by that numeration was Monophysite and therefore not recognized by the Orthodox Church.

whatever his reasons, and whatever the date of the note by 'Athanasius the humble', Cyril himself firmly believed in the Egyptian origin of the manuscript, and the statement of Muttis must therefore be rejected. The matter was carried a stage farther in 1938 when, in connexion with a popular booklet on the manuscript then being prepared in the British Museum, Dr. A. S. Fulton, then Keeper of the Department of Oriental Printed Books and Manuscripts, whose reputation as an Arabic scholar needs no emphasis, re-examined the Athanasius note, and gave it as his opinion that on palaeographical grounds it could be dated thirteenth to fourteenth century and that the seventeenth century was excluded. The former identification of Athanasius with the Patriarch of that period thus again became plausible. The final proof—or what can reasonably be claimed as equivalent to proof—came in 1945, when T. D. Moschonas published a catalogue of the library of the Patriarchs of Alexandria.[3] In this he printed two notes, both in tenth-century manuscripts of St. John Chrysostom, and reading as follows:[4]

> Τὸ παρὸν βιβλίον προσεκτήθη μοι ἐν τῇ Βασιλευούσῃ τῶν πόλεων, ἀφιερώθη δὲ τῇ κατὰ Ἀλεξανδρείαν ἁγιωτάτῃ τοῦ Θεοῦ ἐκκλησίᾳ τοῦ Πατριαρχείου· ὃ καὶ ὀφείλει ὁ τὸν θρόνον διαδεξόμενος διακομίσαι καὶ ἀποδοῦναι ἐκεῖσαι ἐν οἷς καὶ ἀφιερώθη. τίθημι δὲ ἀφορισμὸν ἐπὶ τῷ ἀφαιρήσοντι τοῦτο ἢ ἀποστερήσοντι:—✠ ὁ ταπεινὸς Ἀθανάσιος Ἀλεξανδρείας (MS. 12).
>
> Τὸ παρὸν βιβλίον ἀπεχαρίσθη μοι παρὰ τοῦ κὺρ Δημητρίου τοῦ Ἰατροποῦλου ἐν Κωσταντίνου πόλει, ἀνετέθη δὲ παρ' ἐμοῦ τῇ ἁγιωτάτῃ τοῦ Θεοῦ ἐκκλησίᾳ τῇ ἐν Ἀλεξανδρείᾳ εἰς μνημόσυνον αὐτοῦ· ὀφείλει γοῦν ὁ τὸν θρόνον διαδεξάμενος ἀναλαβεῖν καὶ διασῶσαι ἐν τῷ πατριαρχείῳ ἐν οἷς καὶ ἀφιερώθη· ὅστις δὲ πειράσεται ἀφαιρῆσαι τοῦτο ἀφορισμῷ ἀλύτῳ καθυποβληθήσεται: ✠ ὁ ταπεινὸς Ἀθανάσιος Ἀρχιεπίσκοπος Ἀλεξανδρείας (MS. 34).

There can be no possible doubt that the above notes were inserted in the manuscripts by the Patriarch Athanasius II; and that both came from Constantinople is readily explained by the fact that for nearly three-quarters of his forty years' patriarchate Athanasius in fact resided in the capital, and did not finally return to Egypt until about 1308. The two notes must therefore have been written between 1308 and the year of his death, 1316.

[3] Καταλόγοι τῆς Πατριαρχικῆς βιβλιοθήκης, Τόμος Α΄, Χειρόγραφα (Alexandria, 1945).
[4] I have made some minor corrections on the basis of photographs kindly supplied by Mr. Moschonas.

Although the note in the Codex Alexandrinus is entirely in Arabic, and therefore no identity of hand with the Greek notes can be expected, the above similarity of wording leaves no doubt that this also is the work of Athanasius II. Two further consequences now follow. In the past it has generally, though illogically, been assumed that the note in the Alexandrinus indicated that the manuscript had been in Alexandria from time immemorial; but comparison with the Greek manuscripts now shows precisely the opposite, viz. that the notes were inserted because the manuscripts had *not* previously been in the Patriarchal Library. Secondly, Athanasius's long absence in Constantinople makes it highly probable that the Codex Alexandrinus, like the two Greek manuscripts, was acquired by him in the capital. Whether all, or any, of the three manuscripts were originally written, in Constantinople is, of course, another question; but if any future scholar wishes to claim a Constantinopolitan origin for the Codex Alexandrinus, it is at least open to him to do so. In short, Burkitt's conclusion may be right, though his reasons were wrong. What is now virtually certain is that the manuscript was carried from Constantinople to Alexandria between 1308 and 1316, and that it remained in Alexandria until 1621, when Cyril Lucar removed it once more to Constantinople, to present it, six years later, to the King of England.

3

THE CODEX VATICANUS IN THE FIFTEENTH CENTURY

One of the main purposes of this article is to draw attention to a most valuable paper by Father Janko Šagi, S. J., 'Problema historiae codicis B', written in Latin and published in the periodical *Divus Thomas* (1973), pp. 3–29. I take this step because *Divus Thomas* is not to be found in any major library in this country, indeed the only location given in the British Union-List of Periodicals is Blackfriars, Oxford.[1]

I shall first give a brief summary of Father Šagi's paper, although I fear this will not do justice to his detailed and well-researched study, carried out in the Pontificio Istituto Biblico.

After a brief 'Introductio' (pp. 3–4) in which he records the common opinion that the manuscript was written in Egypt,[2] he explains that, as a beginning, he proposes to investigate the history of the manuscript in the fifteenth century.

Section I (pp. 5–8). 'Testimonia directa de historia codicis B' begins by explaining how he, like Mercati before him, had searched through various manuscript sources in vain for references to the manuscript. He then rejects Batiffol's suggestion that some of the annotations in the manuscript are in a hand similar to that of certain manuscripts from Rossano, stating that neither he nor Canart could detect any decisive similarities of script. Other suggestions linking the manuscript with Southern Italy are likewise dismissed for lack of evidence. Mercati's claim to have found in the manuscript the name of the well-known scribe John Chortasmenos is rejected as based upon a

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[1] On the Blackfriars Library see Paul Morgan, *Oxford Libraries outside the Bodleian* (2nd edn. 1980), p. 17.

[2] The latest discussion I can find of the provenance of the Codex Vaticanus seems to be a paper read by the late Professor W. H. P. Hatch to the Society of Biblical Literature of which a very brief summary is printed in the *Journal of Biblical Literature*, 72 (1953), pp. xviii–xix. Hatch's conclusion is 'that Codex Vaticanus was written in Upper Egypt. This view is suggested by the position of the Epistle to the Hebrews in the archetype of the Vatican MS, and is strongly supported by certain textual and palaeographical arguments.' So far as I can discover, this paper has never been published.

misreading, while the assertion of Turyn (followed by Hunger) that Chortasmenos wrote the supplementary portions of the manuscript is also rejected as incorrect. Here Šagi adds the important statement that the writer of the supplementary portions cannot be identified with any known fifteenth-century Greek scribe; this statement is based on unpublished researches by Canart, who had utilized the Bodleian collection of microfilms of fifteenth-century scribes.

As regards the entry of the manuscript into the Vatican Library Šagi points out that it does not feature in a list of Greek manuscripts owned by Eugenius IV in 1443, which includes only two Greek items. Devreesse, however, mentions other Greek manuscripts known to have been in the possession of Eugenius IV which are not in the inventory, adding 'Il n'est pas douteux que d'autres volumes grecs soient venus enrichir la collection d'Eugène IV, mais en fournir la preuve n'est guère possible.[3] Clearly, therefore, 1443 cannot be safely taken for a *terminus post quem* for entry of the manuscript into the Library. On the other hand, there is a clear *terminus ante quem* of 1475, since the manuscript is identifiable with certainty in an inventory of that date.[4]

The suggestion that Bessarion was involved in the acquisition of the manuscript by the Vatican Library is also considered, and dismissed as being based on no firm evidence and intrinsically improbable.

Section II. 'Testimonia indirecta de historia codicis B', consists of two parts. The first, headed 'Transcriptiones ex codice B' (pp. 9–13) investigates the claim that the books of Esther, Wisdom, Judith, and Tobit in Codex Venetus Gr. 6 (122 in the enumeration of Old Testament manuscripts) were transcribed from the Codex Vaticanus. This claim is examined in detail with the aid of Ziegler's edition of Wisdom (1962) and Šagi demonstrates that nearly all the variant readings of B are also found in 122, the very small number of discrepancies probably being due to scribal error. 122 was certainly owned by Bessarion, and was probably written for him, and this suggests that Bessarion had knowledge of B, but does not prove that he ever owned it, nor that the transcription was made before B

[3] R. Devreesse, *Le Fonds grec de la Bibliothèque Vaticane des origines à Paul V* (= Studi e Testi 244) (1965), p. 8.

[4] The entry reads as follows: 649. Biblia Ex membr. in rubeo (Devreesse, op. cit., p. 73), and as this is the only complete Bible in the list of Greek MSS the identification is certain and it is not clear why Devreesse follows the identification with a query. Cf. also J. H. Ropes in F. J. Foakes Jackson and K. Lake, *The Beginnings of Christianity*, vol. iii, p. xxxi n. 1.

entered the Vatican Library. Similar investigations have not been made for Esther, Judith, and Tobit in 122, but even if it is proved that these also were transcribed from B this would not get us any further.

The second part, headed 'Suppletiones in codice B', attempts to identify the manuscripts from which the supplementary portions of B were transcribed in the fifteenth century. Minute examination of the Genesis portion (Gen. 1:1–46:28) proves that the text was taken from the Codex Chisianus R VI 38 (19 in the list of Old Testament manuscripts). Unfortunately we know nothing about the early history of 19, so this fact, though valuable in itself, does not contribute towards the history of B.

The remaining supplementary portions of B, comprising Ps. 105:27–137:6, Heb. 9:14–13:25 and the whole of the Apocalypse, are similarly examined in detail, but although the textual affinities of their sources can be established it has not been possible to identify the particular manuscripts from which the transcriptions were made.

Finally, a brief 'Conclusio' (pp. 28–9) sums up the results and points out the work which still needs to be done, in particular an exhaustive and systematic examination of B itself.[5] Such an examination, as was abundantly proved in the case of the Codex Sinaiticus, can only be carried out by the Library authorities themselves.

In the present article I propose to consider the manner in which the fifteenth-century restoration was executed, and to conjecture the circumstances in which it might have been undertaken. I should add that my remarks are based only on the published facsimiles of the manuscript, and references are to the modern pagination.

The first remarkable fact is that the restoration should have been undertaken at all. As was pointed out by N. Bees in *Rheinisches Museum*, N.F., 66, p. 638 (quoted in B. Atsalos, *La Terminologie du livre-manuscrit à l'époque byzantine*, i (1971), p. 201) 'Ce qui était difficile à déchiffrer

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[5] No such study at present exists, although it was apparently planned over eighty years ago, to judge from the Preface to the New Testament volume of the photographic facsimile published in 1994: 'Curatores . . . prolegomena ampliora, in editione a. 1889–1890 de industria praetermissa, parari jusserunt, in quibus saltem potiora accurate dissererentur atque illa etiam minoris momenti, quae hactenus minus probe observata sunt. Nam huiusmodi complura occurrunt, quae legentibus difficultatem faciunt neque sine codicis inspectione explicari possunt: multo plura sane quam quae vel ipsi suspicabamur. Praefatio iusto volumine comprehensa prodibit intra proximum annum, non serius', etc. At the moment not even a modern catalogue description of the manuscript is available. For the progress of cataloguing the Codices Vaticani Graeci see the volume *Paléographie grecque et byzantine* (Paris, 1977), p. 537.

à une époque postérieure, ce n'était pas la minuscule avec ses abréviations, mais l'onciale, comme le prouvent des palimpsestes dont, souvent, le même texte a été récrit en minuscule.' It can therefore be taken as certain that the restoration was undertaken for some specific purpose; and it can hardly be doubted that the purpose was for presentation to the Pope, in whose ownership the manuscript is found.

Detailed examination of the successive steps by which the restoration was carried out brings to light an astonishing story of incompleteness, incompetence, and changes of purpose which it is very difficult to explain. From my examination of the facsimiles I think I can detect the following stages:

1. Chapter and section numbers in large numerals (sometimes replacing earlier numerations) were added, though in a very haphazard manner. There are no such numbers in the supplementary portions, indicating that they were inserted before the latter had been written. The numbering begins immediately on the first surviving page of the original part of Genesis (Section 34 on page 43), and they continue up to the end of 4 Kingdoms. The numeration is faulty in several places, viz. in Deuteronomy 84 precedes 83; in 1 Kingdoms there is no 76; in 2 Kingdoms no 14, 15, or 55; in 3 Kingdoms no 20 or 55. From this point the numbers become smaller and their insertion more perfunctory. They are absent altogether in Wisdom, Sirach, Judith, and Tobit, but reappear in Ecclesiastes. They are present in the Minor Prophets, also in Isaiah, Jeremiah, and Daniel (not Ezekiel) but many numbers are missing in Isaiah and a great many more in Jeremiah. The Gospels are left untouched, but the numbers reappear in Acts, where they replace the much earlier numeration peculiar to B. The last number is Section 5 in Hebrews (p. 1518).

Why these numbers should have been inserted is wholly obscure. If they were intended to facilitate consultation of the manuscript, why were they inserted in so half-hearted a manner, and, above all, why were the standard kephalaia not inserted in the Gospels? Possibly they were originally intended to act as a primitive form of collation, i.e. a check that the text was complete and continuous, but if so the intention was very soon forgotten.

2. The second stage was an attempt to embellish the manuscript. The severity and simplicity of the original, which to our eyes is one of its greatest virtues, were clearly not suited to fifteenth-century taste. The embellishment consisted in placing rectangular panels of

colour, red, green, or blue, surmounted by three red crosses,[6] at the beginning of each book. Apparently at the same time the first letter of the text of each book was erased and replaced by a large ornamental initial, partly coloured.[7] This latter step necessitated the additional erasure of the large *alpha* indicating section or chapter number 1 at the beginning of each book. This number then had to be rewritten on a much smaller scale in the margin above the ornamental initial.[8]

3. Apparently it was only after these operations that it was decided to supply the missing portions of the text. These were written in three columns to the page, imitating the layout of the original, but in a competent fifteenth-century hand: obviously any attempt at reproducing uncial script was beyond the capabilities of the available scribes. The writer was not very skilful in calculating the space he required for his text. There is a whole column blank at the end of the Genesis portion, a whole page blank at the end of the Psalms portion, and a whole blank leaf at the end of the Apocalypse.

The scribe was clearly instructed to leave some lines blank at the beginnings of Genesis and the Apocalypse for the insertion of titles, which he certainly did not execute himself, and also to indent certain initial lines of text to leave space for the addition of ornamental initials. To these further additions I now turn.

4. The next stage seems to have been the addition of a would-be grandiose title at the beginning of Genesis, which I illustrate (Plate 1). This title, unparalleled so far as I am aware in any other manuscript, is written in an elaborate type of script, embodying epigraphic forms, which Professor Hunger has christened 'Epigraphische Auszeichnungs-Majuskel.' The text, in ordinary Greek lettering, reads as follows:

> Βιβλίον περιέχον
> πᾶσαν τὴν παλαιαν γρα-
> φην καὶ τὴν νέαν:—
> Μωυσέως ἡ Γένεσις

The scribe was not very accurate. He omitted the latters αια in παλαιαν, and had to insert them in very small letters above the line.

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[6] There are no crosses in Joshua or the Prologue to Wisdom.

[7] The scribe omitted to erase the first letter of the text in Psalms, Amos, Tobit, and Matthew. In Sirach he carelessly erased the first two letters, and had to rewrite the second.

[8] This erasure can be clearly traced in a number of cases; it is especially clear in James, 1 John, 1 Peter, and Philippians.

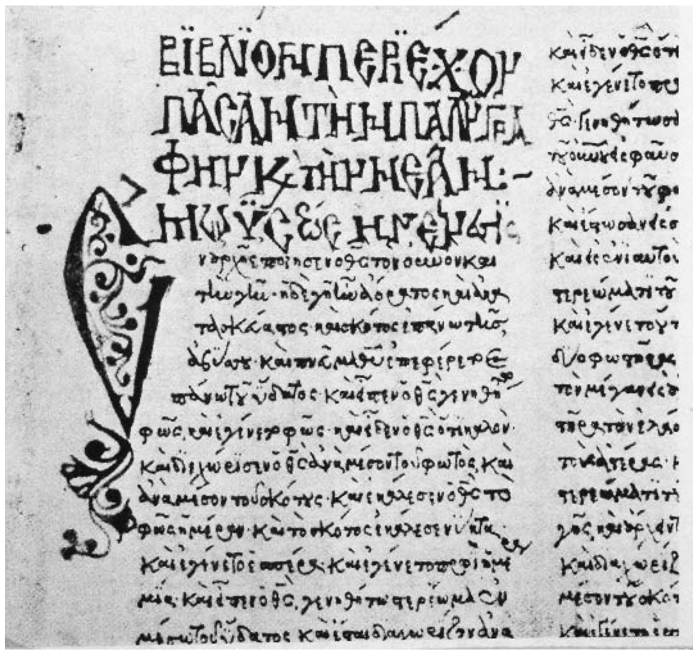

Plate 1

And although the scribe of the text had been told to leave the first six lines of the column blank, this proved insufficient, and when the writer of the title had completed the first three letters of Γένεσις, he had to compress the remaining four letters into a very small space, using a semi-cursive script. I would add that the manner in which the title impinges upon the first line of text proves beyond all doubt that the title was a later addition.

5. After the title had been written, a further addition was a large ornamental *epsilon* at the beginning of Genesis. As will be seen, the scribe of the text had been instructed to omit the first letter of the text and to indent the first five lines to a diminishing extent to provide space for the *epsilon.* However, when the writer of the initial arrived on the scene, he found that part of the space intended to have been left for him had been pre-empted by the writer of the title, and in consequence the *epsilon* had to be placed further out into the left-hand margin than had been planned.

6. However, it is in the supplementary section of Psalms that the changes of purpose and incompetence most clearly appear. As will be seen from the portion reproduced here (Plate 2) the scribe of the

Plate 2

text had been instructed to omit the first letter of the text of each Psalm, which was to be represented by the addition of a large ornamental initial. In the second and third columns of the page the first two lines of text were indented to provide space for this. No such indentation was made in the first column since there was ample space available in the outer margin. In the event, although the writing of the initial in the margin presented no problem, the space in the two inner margins was quite inadequate, with the result that the initials had to be squeezed in with a very unsatisfactory appearance.

7. The next stage seems to have been certain additions in red ink. It was realized, too late, that the Psalms in the supplementary portion had no numbers or titles, and it was decided to insert them. Possibly red ink was used to make them stand out from the text. The difficulty of the rubricator in finding space for his additions is obvious. The numerals had to be squeezed in anywhere, and the (e.g. Ψαλμὸς τῷ δᾱδ) heavily abbreviated or even written between the lines of the text.

The rubricator next turned his attention to the Apocalypse (Plate 3). Here, the scribe of the text had been instructed to leave the first four

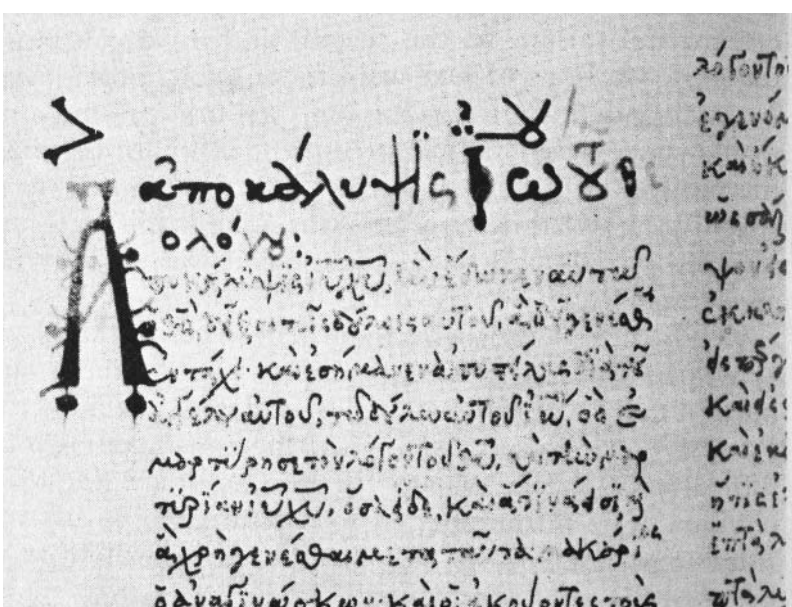

Plate 3

lines of the column blank for the insertion of the title, and also to omit the first letter of the text.[9] Curiously he did not indent the first few lines of text to allow for the addition of an ornamental initial; perhaps it was intended from the start that this should be placed wholly in the margin. The rubricator found that the space left for the title was far too large, and he made no attempt to fill it. In fact he left the first two lines of the column empty and even abbreviated the name Ἰωάννου, although there was no need for him to do so. Altogether the whole title gives the impression of an afterthought, written in haste at the last moment.

What are we to make of all this? Presumably the Vatican authorities would take the view that the operations I have described represent isolated and separate attempts, perhaps spread over several centuries, to conserve and improve the manuscript: 'Adnotaties variae, ut numeri diversarum sectionum et emendationes quae volventibus saeculis factae sunt, et praesertim rescriptio totius codicis novo atramento[10] et adornatio litterarum initialium, ostendunt codicem per plura saecula, fortasse diversis in locis, in pretio habitum esse et ad studium adhibitum.'[11] This, however, seems to me altogether too rosy a picture of the history of the manuscript, which by the fifteenth century was clearly in a very dilapidated state,[12] with the binding torn off, many leaves lost at beginning and end, and a whole quire missing from Psalms. I would further argue that the successive interlocking processes which I have described above are only intelligible if they were undertaken at more or less the same time and for the same specific purpose, namely to render the manuscript fit for presentation to the Pope.

Where was the restoration carried out? If, as has been suggested, the manuscript came to light in one of the Greek monasteries of Southern Italy, it is very unlikely, in view of the depressed state of these monasteries in the fifteenth century, that the monks themselves

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[9] That the initial *alpha* was added before the rubricator arrived is likely since it is identical in form with the initial *alpha* of Psalm 116 (see Plate 2), insertion of which preceded rubrication.

[10] I do not deal here with the re-inking of the manuscript which if correctly assigned to the ninth or tenth century must be quite unconnected with the fifteenth century restoration.

[11] C. M. Martini, *Introductio ad Novum Testamentum e Codice Vaticano Graeco 1209* (Codex B) (1968), p. xvi.

[12] It is true that the first and last surviving leaves are in a good state, but naturally the restorers would have discarded any leaves which were soiled or damaged.

would have been able to undertake the work. On the other hand, by 1475 there would have been plenty of Greek scribes in Rome or elsewhere in Italy capable of restoring the manuscript.[13] But then we have no explanation of the extraordinary sequence of events which I have detailed above.

With much hesitation I venture to put forward here a conjecture of my own regarding the circumstances in which the manuscript came into Papal possession. The one hypothesis which would seem to fit all the facts is that the manuscript was discovered, perhaps in Constantinople, shortly before the departure of the Greek delegation to the Reunion Council of Ferrara-Florence in 1438–9. It is well known that in preparation for the Council libraries in Constantinople and elsewhere were ransacked for manuscripts which would support the Orthodox position in controversies with the Latins. If we suppose that the Codex Vaticanus came to light during these searches, its finders might have conceived the idea of presenting it to the Pope. They would, of course, have had no means of estimating its age, but its ruinous condition might have led them to judge it to be of great antiquity. To the poverty-stricken Greeks, whose financial problems were a constant theme during the proceedings of the Council, the appearance of such a manuscript might have seemed a heaven-sent opportunity of securing a gift suitable for the Pope at small cost to themselves. Clearly a minimum of restoration was unavoidable, although it seems that the original plan was to present it in much the same state in which it was found; later, however, it was decided to restore the missing portions of the text, and this was carried out. If we also conjecture that time was short, this might explain the various deficiencies in the work of restoration.

I must make it clear that there is no direct evidence whatever for this supposition that the manuscript was presented to the Pope at the Council. Indeed, Father Joseph Gill, whose knowledge of the Council is unrivalled, has been kind enough to assure me that there is no record of the Greeks having made presents of any description during their stay. On the other hand, they certainly received gifts from various sources, including the Pope, and protocol would have demanded the giving of presents in return. Such an obligation is in fact specifically acknowledged in the narrative of Syropoulos, who

[13] Cf. Šagi, op. cit., p. 8: 'Supplementa in codice potius Romae quam in Italia meridionali addita essent. Monasteria enim Italiae meridionalis tunc temporis ob suum miserabilem statum vix scribam et membranas . . . suppeditare potuissent.'

records that when the Patriarch received a gift of 400 florins from the Pope, he distributed the money to his entourage, but kept some back to buy δίσκον ἕνα, ἵνα παρέχῃ τὸ ἀντίδωρον δι' αὐτοῦ.[14]

As already mentioned, it has so far been impossible to identify the scribe of the supplementary portions of the manuscript. Should he ever be identified as a scribe working in Constantinople at the relevant period, my hypothesis would be considerably strengthened. The intensive study now being devoted to Greek manuscript production at this very time may prove fruitful in this respect.[15]

Some information on its history might have been deducible from the binding of the manuscript, but unfortunately there can be little doubt that the fifteenth-century binding has long since disappeared. I can find no description of the binding in any Vatican publication, but there is an illuminating passage in C. R. Gregory, *Canon and Text of the New Testament* (1907), pp. 344–5, which it will be convenient to reproduce here. Gregory, who had seen the manuscript in 1886, writes as follows:

> There is an amusing circumstance to be mentioned touching this manuscript. On many of the leaves a sharp eye can detect the myriad lines that we see in paper and which I suppose are due to the wires upon which the paper is made. A hasty observer might declare this fine parchment to be paper. But if that sharp eye should look still more closely it would in some places find Italian words, printed backwards, it is true. At some time or other, without doubt when the manuscript was bound in the present binding and was to be pressed, paper was put in between the leaves to prevent them from printing the old Greek letters off upon each other. Under such conditions, with such a sacred and costly manuscript, it should have been a matter of course to use for this purpose clean thin paper. Instead of that the profane binder put in ordinary everyday newspapers, hence these marks.

From this I conclude that the binding which Gregory saw is not likely to have been earlier than the late seventeenth or eighteenth century. Nowadays, of course, the manuscript has no binding of any kind.[16]

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[14] V. Laurent, *Les 'Mémoires' du Grand Ecclésiarque de l'Église de Constantinople Sylvestre Syropoulos sur le Concile de Florence* (1438–9) (= Concilium Florentinum: Documenta et Scriptores, Series B, vol. ix) (Rome, 1971), p. 626.

[15] Notably O. Kresten, *Eine Sammlung von Konzilsakten aus dem Besitze des Kardinals Isidoros von Kiev* (Öst. Akad. d. Wiss., phil.-hist. Klaase, Denkschriften 123) (1976), especially Section A, Das Konzil von Ferrara-Florenz und die griechische Handschriftenproduktion um die Mitte des 15. Jahrhunderts (pp. 17–26).

[16] Cf. C. M. Martini, op. cit., p. x: 'Hoc saeculo ineunte, quo codex meliua asservaretur, quiniones soluti et folia duplicia in thecis separatis reposita sunt.'

I have left to the last what to my mind is the most puzzling feature of the restoration process—the complete absence of the Pastoral Epistles. Despite the doubts of Martini[17] it seems to me inconceivable that a manuscript of the entire Greek Bible written in the middle of the fourth century could have omitted the Pastorals.[18] But whether it did or not, why were they not included in the fifteenth-century restoration? If my hypothesis is correct, and the scribe of the supplementary portions had been desperately pressed for time, he might have deliberately omitted the Pastorals, hoping that their absence would escape notice. Concluding the volume with the Apocalypse would have been an unavoidable necessity. On the other hand, the Pastorals are so brief that they could have been transcribed in a very short time. Alternatively, the absence of the Pastorals might be simply due to inadvertence, the scribe assuming that they must have come before Hebrews, which often stands at the end of the Pauline corpus.[19] Whichever explanation is correct the fact of the omission does not say much for the diligence or perspicacity of the restorers.

I have no intention of reviving here the old suggestion which would connect the Codex Vaticanus, or the Codex Sinaiticus, or both, with the order given by Constantine to Eusebius to supply fifty bibles for use in the new churches in Constantinople.[20] Even if it could be proved that the Codex Vaticanus was in Constantinople in 1438 it would not follow that it was there 1100 years earlier, since manuscripts moved about: the Codex Alexandrinus, carried to Egypt in the early fourteenth century, is a prime example. I may, however, take this opportunity to correct, with Professor B. M. Metzger's full approval, a statement by him in his *The Text of the New Testament* (2nd edn., 1968), pp. 47–8, where he mentions a conversation with me in the course of which I suggested that the Codex Vaticanus might have been a 'reject' from the fifty manuscripts written for Eusebius. My remarks in fact related to the Codex Sinaiticus, and I have amplified the suggestion in my paper 'The Use of Dictation in Ancient Book-production', *Proceedings of the British Academy*, xlii (1956), pp. 179–208 (Codex Sinaiticus discussed on pp. 191–7) (= chapter A1, above).

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17 Ibid., p. xi: 'Utrum in codice primitivo fuerint necne, non liquet.'

18 The omission of the Books of Maccabees is of course a different matter altogether.

19 Cf. W. H. P. Hatch, *Harvard Theological Review*, 29 (1936), pp. 133–51.

20 This Skeat did later. See chapter B7 below.

So the ultimate provenance of this famous manuscript still eludes us. If, however, this article should in some small way encourage the Vatican authorities to undertake an intensive study of their greatest treasure, it will not wholly have failed of its purpose.

NOTES ON CHESTER BEATTY BIBLICAL PAPYRUS I (GOSPELS AND ACTS)

Although it has survived only in a mass of fragments, the jewel of the Chester Beatty Biblical Papyri is undoubtedly the 3rd century codex of the Four Gospels and Acts, and we are grateful to the Trustees of the Chester Beatty Library for permission to publish the new fragments here edited.

Among the hundreds of minute fragments from the codex of Numbers and Deuteronomy (Chester Beatty Biblical Papyrus VI) are several from the codex of Gospels and Acts, easily distinguishable by their characteristic small sloping script. Some are clearly of no value, for example a small slip containing the single word ειπεν, the other side being blank. Another contains the beginnings of four lines, but only the first letter of each, α[, κ[, μ[, ε[. There are, however, three rather more substantial fragments, here denominated A, B and C. The first comes from the Gospel of Matthew, and in fact joins on to the earliest published fragment of the Gospel. It does not therefore add anything to our knowledge, but is included here for the sake of completeness.

FRAGMENT A

Size 3.1 × 1.8 cms

Vertical fibres (top of page). Matthew 20:24–6

αδελφω̣ | ν̣ ·ο δ | [ε ι̅η̅
αρχο̣ν̣τ̣ε̣ς τ̣ω̣ | ν̣ ε̣θ̣ | [νων
κατεξουσιαζουσι[ν] | αυτω̣ | [ν
γε[ν] | ε̣σθ | [αι

Horizontal fibres. Matthew 21:15

The upper portion of the fragment on the horizontal fibres has flaked off, leaving remains of only two lines of text. Vertical lines indicate the edges of the fragment.

τ]α θα[υμασια
εν τω]ι ϊε̣[ρωι

The remaining fragments, B and C, are of much greater interest, since they come from the early part of the Gospel of John, which otherwise has been wholly lost, the published text beginning at John 10.7. We first print the text of the fragments, with restoration only of words imperfectly preserved on the papyrus.

FRAGMENT B
Size 2.2 × 1.2 cms
Horizontal fibres. John 4:51–2

υπ]η̣ντ̣[ησαν
επυθ]ε̣το ου[ν
ου]ν̣ οτι ε̣[χθες
]ο̣ π̄ρ̄ οτ̣[ι

Vertical fibres. John 5:21–3

ωσπ]ε̣ρ γα̣[ρ
θε]λ̣ει ζ[ωοποιει
πα]σ̣αν εδ̣[ωκεν
το]ν̣ π̄ρ̄ᾱ̣[

FRAGMENT C
Size: 2.2 × 1.4 cms
Horizontal fibres. John 4:54–5:1

]τ̣[ο]υ̣[το
·ι] ·ουδα̣[ιας
ιουδαιω]ν̣ ·κα̣ι̣[
ιεροσολυ]μο̣ι̣ς[
]ε̣ . . [

Vertical fibres. John 5:24–5

τ]ον λο[γον

]α̣ιων[ιον
]θ̣α̣να[του
ωρ]α̣ κα̣[ι

We now print a tentative reconstruction of the text surrounding the fragments. It must be emphasised that the purpose is solely to indicate the relationship of the fragments to each other, since there is no means of discovering where the lines of text began and ended in the original. Thus, although our reconstruction shows one fragment near the centre of the column and the other near the edges, these positions could just as easily be interchanged, and there are any number of intermediate positions. Whatever positions are chosen for one side, the other will of course be the mirror image of it.

Horizontal Fibres

υπ]η̣ντ̣[ησαν αυτωι και απηγγειλαν]
[λεγοντες οτι ο υ̅ς̅ σου ζηι ·επυο]ε̣το ο[υν την ωραν παρ αυτων εν]
[ηι κομψοτερον εσχεν·ειπαν ου]ν̣ οτι ε̣[χθες ωραν εβδομην αφη–]
[κεν αυτον ο πυρετος·εγνω ουν]ο̣ π̅ρ̅ οτ̣[ι εν εκεινηι τηι ωρα εν ηι ει–]
[πεν αυτωι ο ι̅η̅ ο υ̅ς̅ σου ζηι ·και επιστευσεν αυτος και η οικια αυτου]
[ολη·]τ̣[ο]υ̣[το δε παλιν δευτερον σημειον εποιησεν ο ι̅η̅ ελθων εκ]
[της ·ι] ·ουδα̣[ιας εις την γαλιλαιαν·μετα ταυτα ην εορτη των ϊου–]
[δαιω]ν̣ ·κ̣α̣ι̣[ανεβη ι̅η̅ εις ιεροσολυμα· εστιν δε εν τοις ιεροΓσο–]
[λυ]μ̣ο̣ι̣ς[επι τηι προβατικηι κολθμβηθρα
]ε̣ . .[

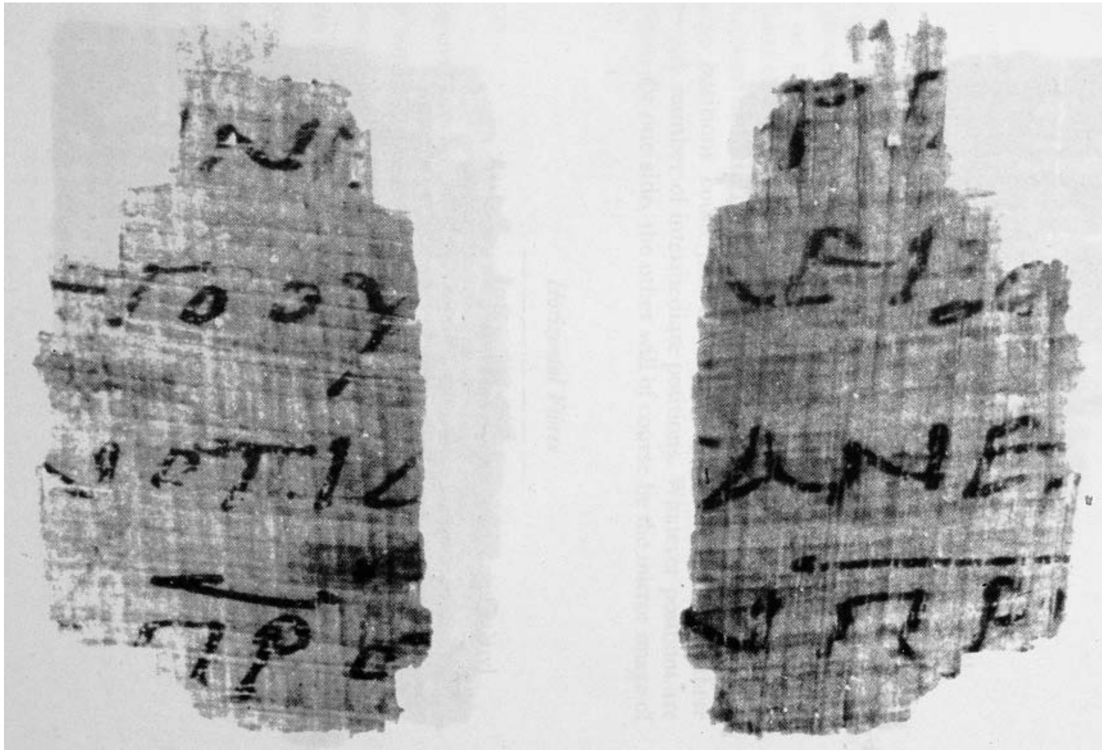
Vertical Fibres

[ινα υμεις θαυμαζητε ωσπ]ε̣ρ γα̣[ρ ο π̅ρ̅ εγειρει τους νεκρους και ζω–]
[οποιει ουτως και ο υ̅ς̅ ους θε]λ̣ει ζω[οποιει·ουδε γαρ ο π̅ρ̅ κρινει ου–]
[δενα αλλα την κρισιν πα]σαν εδ̣[ωκεν τωι υ̅ω̅ ινα παντες τι–]
[μωσι τον υ̅ν̅ καθως τιμωσι το]ν π̅ρ̅α̣̅[ο μη τιμων τον υ̅ν̅ ου τιμα]
[τον π̅ρ̅α̅ τον πεμψαντα αυτον·αμην αμην λεγω υμιν οτι ο τ]ον λο[γον]
[μου ακουων και πιστευων τωι πεμψαντι με εχει ζωην] α̣ιων[ιον]
[και εις κρισιν ουκ ερχεται αλλα μεταβεβηκεν εκ του]θ̣α̣να[του]
[εις την ζωνη·αμην αμην λεγω υμιν οτι ερχεται ωρ̣]α̣ κα̣[ι νυν]

One feature of this reconstruction may appear puzzling. On the side with horizontal fibres, if we disregard the tips of letters at the top of Fragment C, there are two complete lines of text lost between the fragments. On the vertical fibres side, however, the position is exactly the opposite, the top line of Fragment C following on directly from the bottom line of Fragment B. So far as we can see, the only possible explanation is the following. The facsimile shows that on several pages the lines of text are not strictly horizontal but slope markedly up or down. If we assume that, had the lines been perfectly horizontal, there would have been one line lost between the fragments on both sides, then if the writing on the side with horizontal fibres sloped downwards, the space would have been increased from one line to two. On the other side, however, where Fragment C comes at the end of the lines, the result would have been the opposite, and a downward slope of the writing would have reduced the space from one line to *nil*.

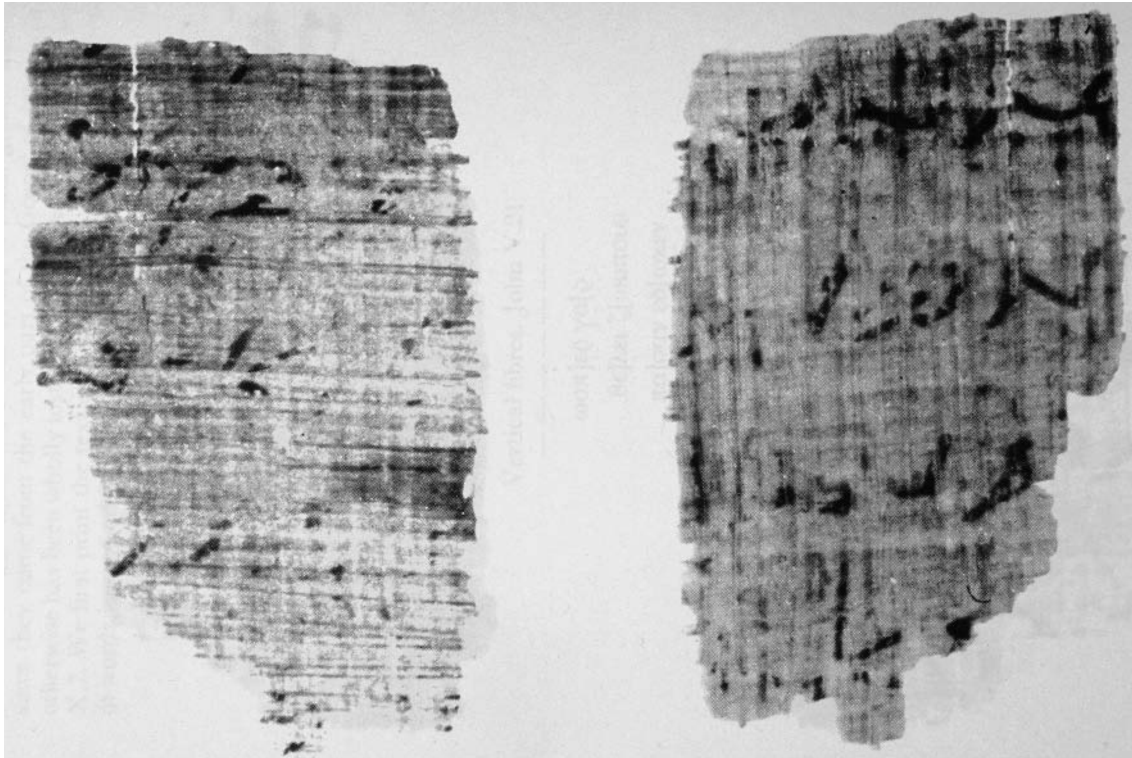
Despite their small size, the fragments are not without some textual significance, and it is their relationship to the two other early papyri of John, P^{66} and P^{75}, which is of interest. At John 4:51 the papyrus, like P^{66} (but not P^{75}), clearly had some addition after υπηντησαν αυτωι, and of the various alternatives και απηγγειλαν, the reading of P^{66}, is perhaps the most likely (in any case without the addition of αυτωι). In the same verse παις αυτου, as read by both P^{66*} and P^{75}, is clearly too long for the available space, and we have conjectured υιος σου (written ῡς̄ σου, as in P^{66c}). (On this passage see J. K. Elliott [ed.], *The Principles and Practice of New Testament Textual Criticism. Collected Essays of G. D. Kilpatrick* [Leuven 1990] 354). In verse 52 P^{75} (but not P^{66}) has εκεινην after την ωραν, but there is clearly not room for the addition here. In the next line we have printed ειπαν ου]ν as P^{66c} P^{75}, but και ειπα]ν as in other ancient authorities is equally possible (P^{66*} has ειπαν *tantum*). Most authorities follow this with αυτωι, which however is clearly omitted here. In verse 53, after ο π̄ρ̄ P^{66} (but not P^{75}) adds αυτου but this was not the case here. After this, reasons of space suggest that εν εκεινηι, with P^{66}, is more likely than the reading of P^{75}, which omits εν. In verse 54 τουτο δε, as in P^{66} and P^{75} is perhaps to be preferred to τουτο alone through reasons of space. At John 5:22 the reading of the papyrus, εδ[ωκεν is apparently an otherwise unrecorded variant for δεδωκεν.*

* A further study will consider the position of the fragments in the codex, and ways in which the contents of the codex as a whole could be evaluated (= chapter B5).



Fragment B Recto

Fragment B Verso



Fragment C Recto

Fragment C Verso

5

A CODICOLOGICAL ANALYSIS OF THE CHESTER BEATTY PAPYRUS CODEX OF GOSPELS AND ACTS (P 45)

The Chester Beatty papyrus codex of the Four Gospels and Acts (P 45 in the official list of New Testament papyri), usually considered to have been written about the middle of the 3rd century A.D., is still the earliest surviving manuscript to contain all four gospels, and as such is a unique monument of early Christian literature and a treasure of the Irish nation.[1] The contents and structure of the manuscript are well described by Kenyon,[2] and all that has been attempted in the present publication is to verify and build upon his conclusions.

The fortunate survival of two successive bifolia in the Gospel of Luke enabled Kenyon to conclude that the manuscript was made up of quires of two leaves (four pages) only, formed by folding a single sheet of papyrus in two, in such a way that the side of the papyrus on which the fibres are horizontal formed the two inside pages, while the other side, where the fibres are vertical, formed the outsides. The succession of fibres in the quire may thus be designated VHHV and this sequence is a vital factor in the reconstruction of the manuscript.[3] The other clue on which Kenyon based his reconstruction was the preservation of two page-numbers, 193 and 199. Both are on the second page of a leaf, from which Kenyon deduced

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[1] For bibliography see K. Aland, *Repertorium der griechischen christlichen Papyri*, Berlin & New York, 1976, Nr. 0104 = NT 45 (with exhaustive bibliography). J. van Haelst, *Catalogue des Papyrus Littéraires juifs et chrétiens*, Paris, 1976, no. 371, pp. 136–7. Günther Zuntz, "Reconstruction of one leaf of the Chester Beatty Papyrus of the Gospels and Acts (P 45)", in *Chronique d'Egypte*, 26, 1951, pp. 191–211, is valuable not only for the reconstruction but also as containing the only detailed description of the script and the scribal habits of the writer.

[2] *The Chester Beatty Biblical Papyri*, Fasciculus I: General Introduction, 1933, pp. 6, 12–13; *The Chester Beatty Biblical Papyri*. Fasciculus II. The Gospels and Acts: Text, 1933; *The Chester Beatty Biblical Papyri*. Fasciculus II. The Gospels and Acts: Plates, 1934.

[3] The terms recto and verso are ambiguous, since when employed by papyrologists (and so used by Kenyon) they mean respectively the side on which the fibres are horizontal and that on which they are vertical, whereas in describing manuscripts recto means the right-hand facing page when the book is opened, and verso the reverse of the page. We have therefore avoided the terms as far as possible.

that p. 1 of the codex must have been the *second* page of the first quire, the first (outside) page having been left blank and unnumbered. The first page of the second quire of the codex would therefore have been numbered p. 4, and the first page of all subsequent quires would have borne numbers which are multiples of 4.

In calculating the space occupied by the different Gospels Kenyon based his reconstruction on his observation that an average page of the codex contained text equivalent to about 36 lines of the edition of the Greek New Testament by Alexander Souter.[4] The text of this edition is that underlying, or presumed to underlie, the English Revised Version of the New Testament published in 1881, to which Souter added a brief but valuable apparatus. Obviously as a means of comparison such a method suffers certain disadvantages: in the printed edition there are, of course, spaces between words and at the beginnings and ends of paragraphs, *nomina sacra* are printed in full, and the text is not necessarily that of the papyrus. Nevertheless, as will be seen, Kenyon's system works remarkably well in practice. No doubt the spaces to which we have alluded tend to be spread out evenly throughout the text so that the overall result is little affected.

Our first task was to check Kenyon's estimate that an average page of the codex contained approximately 36 lines of Souter's text, and for this purpose we made as many measurements as possible. No page is complete, but the probable content can be measured by taking equivalent positions on both sides of the fragment, and also equivalent positions in the case of facing pages. This basis produced a total of 30 measurements, with an average of 35.92 Souter lines to the page in the Gospels (Acts will be dealt with separately, for reasons which will appear later). There is, however considerable variation from page to page, and three of the measurements are decidedly anomalous, giving figures of only just over 30 Souter lines.[5] If these anomalies are excluded the average number of Souter lines per page rises to 36.5.

As a further check we have employed stichometry, marking up the text of Souter into *stichoi* of 15 syllables and making allowances

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[4] *Novum Testamentum Graece*, Oxford, Clarendon Press, 1910. A revised edition appeared in 1947, and there have been several reprints. We have used the original 1910 edition employed by Kenyon.

[5] There is no obvious reason for these discrepancies. The script is normal, the lines are of about usual length (average 44 letters) and though spaced a little more widely, not enough to produce a significant difference.

for the *nomina sacra* regularly used in the codex. This is the method recommended by Rendel Harris as producing results nearest to the average of those recorded in New Testament manuscripts.[6] Rendel Harris used the text of Westcott and Hort, but the figures he obtained for the Gospels are remarkably similar to those recorded using Souter's text, as may be seen from the following table:

	Our measurements	Rendel Harris
Matthew	2,549.5	2,557
Mark	1,594.0	1,617
Luke	2,711.5	2,720
John	2,021.0	2,029
Total	8,876.0	8,923

It should be mentioned at this point that all these measurements *include* the Long Ending of Mark (16:9–20) but *exclude* the *Pericope de Adultera* (John 7:53–8:11).

We can now compare these figures with the number of Souter lines for each Gospel:

Matthew	1,780.7
Mark	1,076.6
Luke	1,811.8
John	1,350.3
Total	6,019.4

It is worth noting that the *ratio* of Souter lines to *stichoi* is remarkably constant. In the 30 measurements mentioned above it varies only between 1.41 and 1.58, with an average of 1.486, but this includes two extreme figures of 1.41 and 1.58. If these are excluded, the range is only 1.44–1.54.

It is now clear that both Souter pages and *stichoi* provide alternative reliable bases of measurement, with which we can consider the individual Gospels. Before doing so we may give Kenyon's estimate of the space they occupied, based on his calculations of Souter lines:

[6] *Stichometry*, Cambridge University Press, 1893, p. 51.

Matthew	49 1/3	pages
Mark	30	pages
Luke	50 2/3	pages
John	38	pages
Acts	50	pages
Total	218	pages

We can now proceed to an investigation of these figures.

Matthew

Only two fragmentary leaves of Matthew have survived, one (f. 1) containing parts of 20:24–21:19, the other (f. 2, *plus* the Vienna fragments) parts of 25:41–26:39. Since in both cases the top line of the page is partly preserved, they must be separated by a number of complete pages. Moreover, since in the first fragment vertical fibres precede horizontal, while in the second horizontal fibres precede vertical, the number of pages separating the two points 20:24 and 25:41 must represent a number which is a multiple of 2. In fact, this amount of text occupies 364.25 Souter lines, and there can be no doubt that this represents 10 pages with an average of 36.4 Souter lines each, since the nearest alternatives of 8 and 12 pages both produce quite unacceptable averages.

We can check this result with stichometry. The amount of text between the two points 20:24 and 25:41 amounts to 526 *stichoi*, with average of 52.6 *stichoi* to the page. This gives a ratio of Souter lines to *stichoi* of 1:1.44, which is within the range quoted above. Here again the alternatives of 8 and 12 pages produce unacceptable results.

We can now turn to the preceding portion of the Gospel. Matthew 1:1–20:24 occupies 1144.2 Souter lines. Since in the first fragment vertical fibres precede horizontal, this must have been the first leaf of a quire, and must therefore have been preceded by a number of complete quires. But Kenyon has shown that the first page of the codex was left blank and unnumbered. The number of Souter lines quoted above, 1144.2, must therefore represent a number of pages which is a multiple of 4, minus 1, e.g., 23, 27, 31, 35, 39. There can thus be no doubt that the number of pages is in fact 31, giving an average of 36.9 Souter lines to the page, the nearest alternatives, 27 and 35, giving averages of 39.45 and 32.69 respectively, both of which are unacceptable.

Here again we can check with stichometry. The same passage, 1:1–20:24 occupies 1641.5 *stichoi*, which for 31 pages gives an average of 52.95 *stichoi* to the page, the ratio of Souter lines to *stichoi* being 1.43. Here also the alternatives of 27 and 35 pages produce unacceptable results.

We can now recreate the part of the codex up to Matthew 20:24 by means of the following diagram which shows the alternation of sides between vertical and horizontal fibres and the beginnings and ends of quires:

Quire		1				2				3				4		
Page	—	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Fibres	V	H	H	V	V	H	H	V	V	H	H	V	V	H	H	V
Quire		5				6				7				8		
Page	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31
Fibres	V	H	H	V	V	H	H	V	V	H	H	V	V	H	H	V

We can now attempt to estimate the space occupied by the concluding portion of the Gospel. From 25:41 to the end occupies 262.65 Souter lines. Up to 25:41 the Gospel has occupied 41 pages, *viz*. the 31 from 1:1 to 20:24 plus the 10 pages from 20:24 to 25:41. This gives a combined average of 36.8 Souter lines to the page. 262.65 Souter lines would therefore have occupied 7.14 pages, i.e., seven complete pages and part of an eighth. The entire Gospel would thus have occupied a total of 48 complete pages, and would have come to an end about five or six lines down page 49. This is about one page less than Kenyon's estimate of 49 1/3 pages.

Stichometry confirms these findings. From 25:41 to the end of the Gospel occupies 379.5 *stichoi*. At an average of 52.95 to the page, as found earlier, this gives 7.17 pages, almost identical with the figure obtained from Souter lines. The total number of *stichoi* for the Gospel, 2549.5, when divided by the number of pages, 48.14, gives an average per page of 52.96.

We can now complete the diagram given earlier as follows:

Quire		9				10				11				12			13	
Page	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49
Fibres	V	H	H	V	V	H	H	V	V	H	H	V	V	H	H	V	V	H

If we assume that each Gospel began at the top of a page, the next Gospel would have begun on page 50. But which Gospel was it?

The Order of the Gospels

Kenyon (introduction, p. viii) states that "with regard to the order of the books, the only evidence lies in the fact that Mark and Acts were closely associated in the papyrus as brought to England. This makes it probable that Mark stood last among the Gospels, as in the Freer MS. at Washington (W), where the order of the books is Matthew, John, Luke, Mark, the so-called Western order, which is found in the Codex Bezae and several MSS. of the Old Latin version".

Several factors confirm Kenyon's conjecture. In the first place there is the appearance of the fragments themselves, those of Mark showing a distinct resemblance to those of Acts. Secondly, a point not mentioned by Kenyon, is the fact that the slanting strokes added by a later hand (Kenyon, p. ix) appear in *all* the fragments of Mark and in *all* the fragments of Acts, but nowhere else in the codex. It is obviously simpler to assume that the writer began in Mark and continued uninterruptedly into Acts rather than marking up Mark and leaving the other Gospels untouched before resuming his work in Acts. But it is now possible to provide positive proof that, at least, Mark did *not* follow immediately after Matthew.

The proof resides in the fact that first surviving leaf of Mark (f. 3) shows horizontal fibres preceding vertical fibres. It must therefore have been the *second* leaf of a quire. This leaf probably began in the latter part of Mark 4:32. From the beginning of the gospel to this point is approximately 220 Souter lines. This must be a complete number of pages, implying 6 pages at an average of 36.6 Souter lines to the page. Stichometry gives a similar result, since Mark 1:l–4:32 occupies about 325 *stichoi*, which for six pages gives an average of 54 *stichoi* to the page.

The next step is to continue the diagram used in the consideration of Matthew. This is as follows:

Quire		13				14			15	
Page	48	49	50	51	52	53	54	55	56	57
Fibres	V	H	H	V	V	H	H	V	V	H
	Matthew ends		Next Gospel begins							

If therefore Mark followed directly after Matthew, the first page would have been page 50 of the codex, and the first surviving fragment would have begun six pages further on, at page 56. But the first surviving fragment of Mark shows horizontal fibres preceding vertical fibres. It cannot therefore have stood at this position in the codex, and accordingly whichever Gospel followed Matthew, it was *not* Mark. Nor can it have been Luke, since as will be shown below Luke began on the first page of a quire. Since both Mark and Luke are excluded, we have no option but to accept Kenyon's suggestion that the Gospels were in the "Western" order, and that the Gospel which followed Matthew was consequently that of John.

John

As stated above, all calculations regarding John assume that the codex omitted the *Pericope de Adultera* (John 7:53–8:11).[7] They necessarily also omit Chapter v, verse 4, since Souter omits this from his text, relegating it to his apparatus.

Passing over for the moment the two minute fragments from the early part of John recently published, the surviving text begins at John 10:7. This is at the top of a page (f. 16^{v}), and if we assume that the Gospel began at the top of a page, the text preceding this point must represent a complete number of pages. John, 1:1–10:6 comprises 650.4 Souter lines, and there can be little doubt that this

[7] The Pericope runs to 16.4 Souter lines—nearly half a page—and if it had been included the Gospel would have had to commence on the page in which Matthew ended, beginning immediately below the colophon to that Gospel, which at the least is highly unlikely.

implies 18 pages at an average of 36.13 lines to the page. Before we consider possible alternatives, we may extend the diagram in the previous section as follows:

Quire	13		14				15				16				17				18				19
Page	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72
Fibres	H	V	V	H	H	V	V	H	H	V	V	H	H	V	V	H	H	V	V	H	H	V	V

As will be seen, if John 1:1–10:6 occupied 18 pages, these will be pages 50–67 of the codex, and the first surviving page of John would be p. 68, and this does indeed have vertical fibres followed by horizontal fibres as in the diagram. The diagram also shows that the nearest alternatives to p. 68 are p. 64 and p. 72, both of which produce quite unacceptable averages of 46.45 and 32.52 Souter lines respectively.

Stichometry gives a similar result. John 1:1–10:6 comprises 974.8 *stichoi*, and division by 18 gives a normal average of 54.15 to the page. Now we know that the missing portion at the beginning comprised 18 pages, the contents of each can be estimated with a fair degree of accuracy.

Of the newly published fragments,[8] Fragment B contains part of John 4:51–52 on one side and part of 5:21–22 on the other. Fragment C contains part of John 4:54–5:2 on one side, and of 5:24–25 on the other. Both fragments must therefore have come from near the foot of the fourth leaf of the Gospel (pp. 56–57 of the codex), which would have contained something like John 4:35–5:5 on one side and John 5:5–5:28 on the other. There is however a problem, since, as the diagram shows, p. 56 of the codex should show vertical fibres and p. 57 horizontal, whereas the fibres on both fragments, which are clearly visible and cannot be mistaken, are the exact opposite of this.

To move the fragments to a location where the fibres are in the desired order is quite impracticable, since both of the nearest alternatives giving the correct sequence of fibres, viz. pp. 54–55 and 58–59, involve massive dislocations in the distribution of text throughout these 18 pages.

[8] T. C. Skeat and B. C. McGing, "Notes on Chester Beatty Biblical Papyrus I (Gospels and Acts)", *Hermathena*, cl, 1991, pp. 21–25 (= chapter B4).

How is this anomaly to be explained? If we imagine the scribe working with a pile of sheets before him, cut to size and folded in two, it is possible that one had been folded incorrectly, *viz.* with the horizontal fibres on the outside, and the scribe, familiar with writing on both sides, might not have noticed, or not have noticed until it was too late, that the order of fibres was incorrect.[9] That he had no intention of altering the system deliberately is shown by the fact that the normal arrangement is found both before and after the leaf from which the John fragments come, and especially in the two surviving bifolia in Luke which formed the basis of Kenyon's reconstruction. That the scribe did eventually change his system will be shown when we come to consider Acts.

The two largely complete leaves of John, Kenyon's f. 16 and f. 17, are both defective at the foot, about seven lines being lost from f. 16 and nine or ten from f. 17. It can be calculated that f. 17 would have ended at about John 12:6. If so, the two leaves (four pages) contained about 141.5 Souter lines, or 216 *stichoi*, giving normal averages of 35.37 Souter lines or 54.15 *stichoi* per page respectively.

We can now sum up for the entire Gospel, by building on the figures we have already established with certainty, *viz.* 18 pages lost at the beginning of the Gospel, plus four pages represented by the extant leaves = 22 pages. We have estimated that the extant leaves ended at about John 12:6, and from this point to the end of the Gospel occupies 556.75 Souter lines. If we now divide this by 35, 36 and 37 we get the following result:

$$556.75 \div 35 = 15.9$$
$$556.7 \div 36 = 15.46$$
$$556.7 \div 37 = 15.04$$

It will be seen that all three figures are over 15 but less than 16, indicating that the Gospel occupied 15 complete pages and came to an end in the sixteenth, to be followed by the colophon. Adding these 16 pages to the 22, we get a total for the Gospel of 38 pages—exactly Kenyon's estimate.

Stichometry gives a similar result. From John 12:6 to the end is 832 *stichoi*. If we divide this by the average mentioned above for John 1:1–10:6, viz. 54.15, we get a figure of 15.36, i.e. 15 full pages

[9] For similar mistakes in the matching of sides cf. E. G. Turner, *The Typology of the Early Codex*, 1977, pp. 67–8.

overflowing into a sixteenth. We can now complete the diagram given earlier as follows for the conclusion of the Gospel:

Quire	19				20				21				22			
Page	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87
Fibres	V	H	H	V	V	H	H	V	V	H	H	V	V	H	H	V

Luke therefore began at page 88.

Luke

Luke is by far the best preserved of the Gospels, with substantial portions of six leaves (twelve pages) containing nearly 300 complete or virtually complete lines of text—about one-sixth of the Gospel. We have seen that John ended on page 87 and Luke therefore began on page 88. This, as a multiple of 4, must be the first page of a quire, in which vertical fibres precede horizontal.

The first extant fragment of Luke (Kenyon's f. 9^v) begins in Luke 6:31, but comparison with the other fragments suggests that perhaps at least five lines have been lost from the top, in which case the text would have begun at about Luke 6:27. From the beginning of the Gospel up to this point is about 436.8 Souter lines, suggesting 12 pages with an average of 36.4. Stichometry gives a similar result. From the beginning of the Gospel up to the same point is 645 *stichoi*, and 12 pages gives an acceptable average of 53.75 to the page. The fragment f. 9^v would thus be the thirteenth page of the Gospel, and page 100 of the codex. This also would have been the first page of a quire, in which vertical fibres precede horizontal, and the fragment shows that this is indeed the case. All this confirms the view that John ended at page 87 of the codex.

The next surviving fragment is Kenyon's f. 10^r. The top line is partially preserved, and indicates that the text began in Luke 9:25. From the beginning of the Gospel up to this point is 708.9 Souter lines or 1056 *stichoi*, which suggests 18 pages with an average of 39.38 Souter lines or 58–7 *stichoi* to the page. Both these figures are above the average, but as will appear later, the scribe is beginning to increase noticeably the amount of text on the page. In any case, since the only alternatives are four pages before or after this point, and both give totally impossible averages, Kenyon's f. 10 would thus have been pp. 106–107 of the codex.

This and the following five leaves (ff. 11–15) are all consecutive, and thus involve no further calculations so far as their position is concerned. They do however, provide an opportunity to calculate how much text the scribe is getting on the page, which, as has been said, seems to be increasing. The final page (f. 15^{r}) probably began at about Luke 14:15, and from our last fixed point, at Luke 9:25, to here is exactly 11 pages. These 11 pages contain 419.85 Souter lines or 616 *stichoi*, giving averages of 38.17 Souter lines and 56 *stichoi* respectively, again noticeably higher than in the earlier part of the codex.

We can now extend the diagram to cover this part of Luke as follows:

Quire	23				24				25				26			
Page	88	89	90	91	92	93	94	95	96	97	98	99	100	101	102	103
Fibres	V	H	H	V	V	H	H	V	V	H	H	V	V	H	H	V

Quire	27				28				29				30	
Page	104	105	106	107	108	109	110	111	112	113	114	115	116	117
Fibres	V	H	H	V	V	H	H	V	V	H	H	V	V	H

From the top of the last surviving page of Luke (page 117 of the codex) to the end of the gospel occupies 693.17 Souter lines or 1053.8 *stichoi*. Since the whole of this section is lost it is not easy to calculate the number of pages which it would have occupied. As we have seen, at this point the scribe was writing with over 38 Souter lines to the page. If we divide the figure of 693.17 just quoted by 37.5, 38 and 38.5 we get the following figures:—

$693.17 \div 37.5 = 18.48$
$693.17 \div 38.0 = 18.24$
$693.17 \div 38.5 = 18.0$

Similarly, 1053.8 *stichoi*, divided by 56, the figure found above, gives 18.8 pages. It seems likely, therefore, that the concluding portion occupied 18 full pages and ended some way down a nineteenth. On this basis the diagram above can be continued as follows:

Quire	30				31				32			
Page	116	117	118	119	120	121	122	123	124	125	126	127
Fibres	V	H	H	V	V	H	H	V	V	H	H	V
Quire	33				34							
Page	128	129	130	131	132	133	134	135				
Fibres	V	H	H	V	V	H	H	V				

The next Gospel would thus have begun at the top of page 136 of the codex. This, being a multiple of four, would have been the first page of a quire, and would have shown vertical fibres succeeded by horizontal. This exactly suits Mark, since as we have already shown, the first extant fragment of Mark shows horizontal fibres preceding vertical, and must therefore be the second leaf of a quire, and since calculation shows that this was preceded by exactly six pages, the Gospel must have begun at the top of the first page of a quire, in this case page 136 of the codex. Our estimate of the number of pages occupied by the conclusion of Luke is thus confirmed.

The two pairs of conjugate leaves in Luke, still fortunately attached to each other, which gives us such vital information about the construction of the codex (f. 11 + 12, 13 + 14), form quires 28 and 29, and would have been paginated 108–11, 112–115.

Mark

We have seen that Luke ended at p. 135, the last page of a quire. The next Gospel, Mark, would therefore have begun at p. 136, at the beginning of the next quire. We have already seen, in discussing which Gospel followed Matthew, that the first extant fragment of Mark was preceded by six complete pages. These would have been pp. 136–141 of the codex. The first extant fragment of Mark (f. 3) would thus have formed pp. 142–143 of the codex. This is immediately followed by another fragment of similar size (f. 4) which in turn is succeeded by three much larger fragments which constitute the only substantial portion of the Gospel to have survived (ff. 5–7). These five fragments, ff. 3–7, are consecutive, so no calculations are necessary to determine their position in the codex, in which they would have formed pp. 142–151.

The rest of the Gospel is lost, except for three very small fragments of a leaf (f. 8) containing portions of verses from Mark 11 and 12. A reconstruction of the leaf is offered by Kenyon in the Addendum on pp. 51–2 of the text volume. An attempt can be made to determine its position in the codex. Since all the Marcan fragments (like those of Acts, which follows) come from the upper part of the page, we may perhaps conjecture that f. 8 would have begun at about Mark 11:24. From the top of f. 7^{v} to here is 174 Souter lines or 258 *stichoi*. How many pages does this represent? Since in F. 8 vertical fibres precede horizontal, it must have come from the first leaf of a quire, and, as the following diagram shows, must be separated from p. 151 by a number of pages which is a multiple of 4, plus 1, *e.g.*, 5, 9, 13, etc.

Quire	35				36				37				38			
Page	136	137	138	139	140	141	142	143	144	145	146	147	148	149	150	151
Fibres	V	H	H	V	V	H	H	V	V	H	H	V	V	H	H	V

There can be no possible doubt that the number of Souter lines quoted above, 174 must represent five pages, since the next alternative, nine, is totally unacceptable. These 174 Souter lines give an average for five pages of 34.8, considerably lower than the average for the earlier part of the Gospel. The number of *stichoi* gives an average of 51.6, which also is lower than usual. As will soon appear, there can be no doubt that the scribe is beginning to reduce drastically the amount of text on the page.

This brings us to the fragments (f. 9) themselves. Taking equivalent points of both sides of the fragments gives a page content of only 30.3 Souter lines or 43.9 *stichoi*. The fragments are so small and isolated that it is not possible to conjecture what caused this change; but the facts are not open to doubt.

Turning to the diagram and allowing for the five pages mentioned above it can be seen that the fragments must come from pp. 156 and 157 of the codex. From the top of p. 157, presumed to be at Mark 12:10, to the end of the Gospel, including 16:9–20, occupies 326.57 Souter lines or 484 *stichoi*. If we take the above, very low averages for Souter lines and *stichoi* to the page, 30.3 and 43.9 respectively, these give results of 10.77 and 11.0 pages. These figures might appear to suggest that the concluding portion of the Gospel occupied eleven full pages, coming to an end, with the colophon, at the top of the twelfth. Here, however, we must anticipate. As will be shown later, there is no possible doubt that Acts began at the top of page 168 of the codex, and, as the diagram below shows, eleven pages is the absolute maximum available. With only a very modest increase in the extremely low figure of 30.3 Souter lines to the page, the conclusion of the Gospel, including 16:9–20 and the colophon, could have been accommodated in these eleven pages. Of course without 16:9–20 it could have been fitted in without any difficulty whatsoever, but as these verses amount to only 18.35 Souter lines or 27.13 *stichoi*, this is too small an amount to decide the question one way or the other; and the codex cannot therefore be claimed to support either the inclusion or the exclusion of the controversial verses.

Quire	39				40				41				42			
Page	152	153	154	155	156	157	158	159	160	161	162	163	164	165	166	167
Fibres	V	H	H	V	V	H	H	V	V	H	H	V	V	H	H	V

Acts

Acts is represented by a consecutive series of thirteen fragments (26 pages), and in every case the top line of the page is preserved, thus greatly simplifying calculation. In addition, two of the original page-numbers, 193 and 199, have survived, and these form the basis both of Kenyon's reconstruction of the codex and of that given here.

The first fragment (f. 18) begins in Acts 4:27, and the two preserved page-numbers make it absolutely certain that this was p. 174 of the codex. From the beginning of Acts to 4:27 occupies 204.45 Souter lines or 326 *stichoi*, and if we divide these figures by 5, 6 and 7 we get the following results:

$$\text{204.45 Souter lines} \begin{cases} \div 5 = 40.9 \\ \div 6 = 34.1 \\ \div 7 = 29.2 \end{cases}$$

$$\text{326 } \textit{stichoi} \begin{cases} \div 5 = 65.2 \\ \div 6 = 54.3 \\ \div 7 = 46.6 \end{cases}$$

These figures leave no possible doubt that the number of pages of Acts preceding the first extant fragment was six. We can now represent these pages in diagrammatic form:

Quire		43				44		
Page	168	169	170	171	172	173	174	175
Fibres	V	H	H	V	V	H	H	V

Here, however, there is a major problem. Since the first extant fragment is unquestionably pp. 174–175, it should show horizontal fibres preceding vertical fibres. But in fact it shows the exact opposite, *viz.* vertical fibres preceding horizontal. Moreover, examination of the remaining fragments of Acts reveals that in every case the sequence of sides is the opposite of what would be expected. Of this fact there can be no possible doubt, since it is guaranteed by the surviving page-numbers.

How is this to be explained? Why did the scribe alter the system which he had maintained for so long? No answer can be given, but one factor may be tentatively put forward. We have seen that the scribe, apparently in error, used a quire folded the wrong way round in the early part of John. That this was unintentional is suggested by the fact that he soon after returned to his normal procedure. The result of folding a sheet the wrong way round would be to cause a mis-match of sides (vertical fibres facing horizontal) at the beginning of such a quire, and a second mis-match (horizontal fibres facing vertical) when normal practice was resumed. If the scribe had made the same mistake again, causing an initial mis-match of sides, he might perhaps have decided to continue on the same basis and thus avoid a second mis-match.

Where was the change made? Clearly it must have been at the beginning of a quire. The last fragment of Mark came from the first leaf of a quire, and that quire must have been normal. The change therefore must have occurred at one or other of pages 160, 164, 168 or 172. It *may* thus have happened at the beginning of Acts, but this is only one of the four possibilities.

However this may be, the sequence of pages and their contents are now established up to the last surviving page, numbered 199.

The question remains to decide how many more pages would have been needed to complete Acts. Kenyon estimated that "just over 18 pages" would have been needed to complete the book (and the codex), and this we can now proceed to check.

From the top of p. 199 to the end of Acts occupies, according to our count, 733.65 Souter lines (Kenyon's 634 is obviously a slip of the pen). In order to determine how many pages this would have required, it is necessary to examine the page content in Souter lines of the extant fragments, since, as will be seen, this differs considerably from what is found in the earlier part of the codex. Taking measurements from the top of each page to the top of the next produces the following 25 results: 36.4, 35.15, 35.85, 32.66, 37.83, 37.02, 36.73, 34.22, 33.83, 34.46, 32.7, 34.23, 33.4, 37.17, 35.64, 33.2, 34.71, 35.31, 33.19, 29.7, 28.87, 31.57, 31.75, 32.6, 31.16. As will be seen there is a gradual, though by no means steady, reduction in page content. The average of the last seven measurements is only 31.26, and if we divide 733.65 by this the result is 23.4. On this basis the end of Acts, followed by the colophon, would have come on the twenty-fourth page from the top of p. 199. This would have been p. 222 of the codex, and the third page of a quire, the fourth and final page being left blank. This quire would have been quire 56 of the codex, and if the scribe maintained his altered practice of folding, the order of sides would have been HVVH.

Acts would thus have occupied a total of 55 pages, against Kenyon's estimate of 50; and we can now, at long last, give figures for the content of the entire codex, as follows:

Matthew	49 pages (1–49)
John	38 pages (50–87)
Luke	48 pages (88–135)
Mark	32 pages (136–167)
Acts	55 pages (168–222)

The codex would thus have consisted of 56 quires of 4 pages = 224 pages, one page at the beginning and one at the end being blank and unnumbered.

When complete the codex would have formed a substantial volume, about 25 cm. in height and 20 cm. in width,[10] and a thickness of perhaps 5–6 cm. apart from any binding. An attempt can

[10] It would thus fall into Turner's Group 4 (*op. cit.* above n. 9, p. 16).

be made to determine the probable cost on the basis of an almost contemporary papyrus.[11] This is P. Lond. Inv. 2110, of the early 3rd century, which provides two different scales of pay of professional scribes, *viz.* 28 dr. for 10,000 *stichoi* and 13 dr. for 6,300 *stichoi* (i.e. 20.6 dr. for 10,000 *stichoi*). Presumably the difference in rates reflects a difference in the style of writing, the higher being for a hand with some pretensions to calligraphy. The Beatty codex, though competently written and obviously the work of a trained scribe, can hardly be described as calligraphic, and the lower rate would thus be the more appropriate.

If, then, we adopt the lower of the two rates we get the following result. The Gospels and Acts comprise a total of about 11,540 *stichoi*, and at the lower rate mentioned would have cost 23.8 drachmae to write. To this must be added the cost of the papyrus. Each quire was formed from a sheet of papyrus about 30 cm. in width, folded in half to form a quire of 2 leaves (4 pages). As the codex contained 56 quires the total length of papyrus required would have been 56 × 30 cm. = 1,680 cm. If we accept the figure of 4 dr. for a standard roll of papyrus of length 340 cm.,[12] the cost of the papyrus would have been just under 20 drachmae, making a total of about 43–44 drachmae. Any binding would have been an extra.

There are, of course, many uncertainties in these calculations but they may be sufficient to give some idea of the expense incurred by the Christian community (if such it was) which commissioned the project.

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[11] See Kurt Ohly, *Stichometrische Untersuchungen*, 1928, pp. 88–90, 126–129; cf. T. C. Skeat, "The length of the standard papyrus roll and the cost-advantage of the codex", *Zeitschrift für Papyrologie und Epigraphik*, 45, 1982, 169–176 (= chapter A4).

[12] This is the figure for a standard roll of papyrus in the almost contemporary Heroninus papyri, cf. Dominic Rathbone, *Economic Rationalism and Rural Society in Third-century Egypt: The Heroninus Archive and the Appianus Estate*, 1991, p. 11.

THE OLDEST MANUSCRIPT OF THE FOUR GOSPELS?[1]

Since the summer of 1994 I have been studying the Gospel fragments known as P4, P64 and P67, in order to determine whether they are all the work of the same scribe, and if so the nature of the manuscript of which they formed part. The fragments have been published as follows:

P4: the definitive edition is by Jean Merell in *RB* 47 (1938) 5–22 and Planches I–VII.
P64: first edited by C. H. Roberts in *HTR* 46 (1953) 233–7 and plate; re-edited, with revised edition of P67, in R. Roca-Puig, *Un papiro griego del Evangelio de San Mateo, 2a edic., con una Note de Colin Roberts* (Barcelona, 1962) with plate.[2]

Full details of the papyri can also be found in two bibliographies, viz. K. Aland, *Repertorium der griechischen christlichen Papyri. Band I. Biblische Papyri: Altes Testament, Neues Testament, Varia, Apokryphen* (Berlin & New York: De Gruyter, 1976), under the numbers NT 4 and NT 64; J. van Haelst, *Catalogue des Papyrus littéraires juifs et chrétiens* (Paris: Publications de la Sorbonne, 1976), under nos. 336 and 403.

It might be useful at the outset to state what these three groups of fragments contain. P4 consists of portions of four leaves from the early part of Luke. The original contents of each can be seen in the table on p. 173. All are so fragmentary that it is not practicable to list precisely the surviving portions of text. P64 comprises three very small fragments of a leaf containing verses from Matt 26. Their exact contents can be seen in the reconstructions on pp. 169–71, in which brackets denote the portions of text preserved. P67 consists of two fragments, Folio A and Folio B. The former, which is very small,

[1] The answer to the query at the end of the title will be given in Section 10, 'The Date of the Fragments', below.

[2] For earlier editions, before it was realised that the fragments were from the same codex as P64, cf. van Haelst, *Catalogue*, no. 336 (I).

contains a few letters from Matt 3:9 on one side and from Matt 3:25 on the other. Folio B, one side of which is reproduced in the Plate on p. 168, contains part of Matt 5:20–2 on one side and 5:25–8 on the other.

My investigation was prompted by the remark of C. H. Roberts in the publication of his Schweich lectures, *Manuscript, Society and Belief in Early Christian Egypt* (Oxford, 1979) 13: 'There can in my opinion be no doubt that all these fragments come from the same codex which was re-used as packing for the binding of the late third century codex of Philo (= H. 695)' (where H. stands for the catalogue of van Haelst). There is a further reference to the subject on pp. 22–3: 'It was remarked above that among the second-century Christian texts were a few whose style of writing did not tally with that of the majority . . . One, no. 14 in the list, certainly comes from the later part of the century and on palaeographical grounds the other two, nos. 8 and 10, may be ascribed to the same period. These are incontrovertibly literary in style. In the first, no. 8 [viz. P^4 + P^{64} + P^{67}] the text is divided into sections on a system also found in the Bodmer codex of Luke and John that recurs in some of the great fourth-century codices and was clearly not personal to this scribe. Once again we find in a manuscript of this early period a characteristic that appears to be not specifically Egyptian but of wider application. In its handsome script as well as in its organization—there are three different positions for punctuation as well as omission and quotation signs—it is a thoroughgoing literary production.'

My original, perhaps over-ambitious, plan was to produce a complete new edition of all the fragments, with original-size facsimiles and full palaeographical details. This, however, would obviously be a lengthy undertaking, and in view of the recent surge of interest in the fragments, which was quite unexpected when I began my researches, it seems preferable to publish a provisional account of my observations and conclusions.

1. The Script of the Fragments

Although the editions of these fragments have been available for many years, so far as I am aware no study in depth of their script has been undertaken. The script is in fact a very early stage of that known as 'Biblical Uncial' because it is most familiar to us through the great Biblical codices of the fourth and fifth centuries. The comprehensive study of Professor Guglielmo Cavallo[3] presents a clear picture of its origins and development, and it remains to be seen how far the present fragments conform to its standards.

Biblical Uncial is basically bilinear, i.e. with certain exceptions the letters are bounded by the space between two notional horizontal lines. Vertical strokes are thick, horizontal ones relatively thin, while sloping strokes come between these extremes. Rectangular letters maintain strictly right-angled shapes, and circular letters true circles—never oval. There are no ligatures, and even where letters just touch each other, this does not affect their formation. No ornamentation at the ends of strokes is permitted. As will be seen when we come to consider individual letters, the script of the present fragments does not conform entirely to these criteria, although it appears to be moving towards them.

The script of the fragments is certainly basically bilinear, and this is emphasised by the rectangular letters, *eta*, *mu*, *nu* and *pi*. *Zeta*, *xi* and *chi* might also be included in this group, since their extremities form a rectangle which is always bilinear. Typical features of individual letters in this group are as follows:

Eta. The cross-bar is always placed high, just above the centre line, and is often slightly tilted downwards, always from left to right.

Mu. The central member is always angular, never curved; it is placed very low down, the angle always touching the lower notional line.

Nu. There is very little variation in shape or size.

Pi. There is very little variation. The horizontal never overlaps the vertical members.

Zeta and *xi* are written with a single continuous stroke of the pen. The top and bottom sections are sometimes slightly bowed, towards a mid-point between the two notional lines. The cursive form of *xi* is never used.

The circular letters, *epsilon, theta, omicron* and *sigma* show a degree of variation in shape and size which would not have been tolerated in the classic Biblical uncial of the 4th century. The full-size, bilinear forms of *epsilon* and *sigma* often have a rather straight back, producing

[3] G. Cavallo, *Ricerche sulla Maiuscola Biblica* (Studi e Testi di Papirologia, 2; Firenze: Le Monnier, 1967).

a shape which is oval rather than circular. The smaller forms of these letters are centrally placed, mid-way between the two notional lines. As noted below, these smaller forms are especially frequent in conjunction with *tau* and *upsilon*. Truly bilinear *omicrons* do occur, but are rare, the smaller size predominating and occupying a central position. The cross-bar of *epsilon* often slopes slightly downwards, anticipating a fashion which was to become standard in, e.g., Sinaiticus. *Theta* is always strictly circular, with little variation in size. The cross-bar never projects beyond the circle.

This variation in size of the circular letters has an important effect on the general appearance of the script. As H. J. M. Milne and I wrote of Scribe D of Sinaiticus: 'In general we may say that he stands out as the most individual of the scribes . . . Not only are the letters as a whole smaller and more delicate, but their relative proportions are different. In A and B the rounded letters are of practically the same height and width as the square letters, so that the writing appears to be enclosed between two parallel lines. In D, on the other hand, the rounded letters are noticeably smaller, and this gives a slightly undulating effect.' (*Scribes and Correctors of the Codex Sinaiticus* [British Museum, 1938] 23.) These words might almost have been written about the present fragments.

The triangular letters, *alpha*, *delta* and *lamda* are nearly always bilinear and show little variation in general formation. The basic shape is an equilateral triangle, and *delta* nearly always conforms to this. *Alpha* and *lamda*, however, are often laterally compressed, the angle at the apex sometimes being no more than 30°. In all these letters the right-hand sloping member often projects beyond the left-hand member at the top. Combinations of triangular letters, in words such as αλλα, are often placed very close together, though not actually ligatured.

Gamma and *iota* are both bilinear. The horizontal of *gamma* varies considerably in length, and often ends in a small thickening or blob.

The rather small circular top of *rho* is always on the top line. The upright, as in classical uncial, always reaches down below the lower line.

The curved portion of *phi* is oval, not circular, and is centrally placed. The upright is very tall, projecting beyond both upper and lower lines. The upright of *psi* is similar; the cross-bar is virtually a horizontal line, only very slightly turned up at the ends, so that the general effect is that of a Latin cross.

Kappa has a flamboyant appearance, the upper half of the angular portion often projecting well beyond the lower, and ending in a small thickening or blob. The frequency of this striking form in all the fragments might alone have led to the realisation that they are all the work of one scribe.

Tau and *upsilon* may be considered together. In both the thick upright always extends below the lower line, while the curved top of *upsilon* sometimes rises slightly above the upper one. The cross-bar of *tau* is always on the upper line, but there is great variety both in its overall length and in the distance by which it extends on either side of the upright. The cross-bar is sometimes slightly tilted, the maximum degree of tilt I have noted being about 5°. In the case of both letters, round letters preceding or following them always have the small shape and can thus take advantage of the space under the arms of the letters. Triangular letters do the same.

Omega is the one letter which can be definitely classified as suspended. There are certainly a few cases in which it can be regarded as bilinear, but in the great majority it has a shallower form and is suspended from the upper line. Apart from this there is very little variation in shape.

Beta occurs in two forms. One is a commonplace type in which the curved portion is drawn in a single undulating line. It is not bilinear, but reaches well below the lower line. The other form is remarkable, since the curved portion appears to consist of two semicircles, separately drawn and with a small space between them. Three, or possibly four examples can be seen in the word σαββατω, which occurs twice in Fragment D recto, col. 1, while in col. 2 of the same page there are three examples of the commonplace form. The unusual form may also occur in one of the Barcelona fragments, P^{67}, in Folio B verso, 1.4.

Other scribal features

Ligatures, rigorously eschewed in classical Biblical uncial, occur only between *tau* and *upsilon*, and when these letters occur together the cross-bar of the *tau* and the top of the *upsilon* are written continuously without lifting the pen. In collocations of triangular letters, the letters are often very close to each other, as noted above, but these are not true ligatures.

Diëresis. This, in the form of two rather heavy dots, appears sporadically over initial *iota* and *upsilon*.

Final nu. Final *nu* after a vowel at the end of a line is often represented by a horizontal stroke placed above the vowel. This stroke is not centrally placed, but begins over the centre of the vowel and is extended beyond it into the margin.

An *apostrophe* occurs once after the name δαυειδ at Luke 1:69, but there are no apostrophes in the genealogy or elsewhere. Nor is there any sign of the apostrophe between doubled consonants or gutturals which is common in papyri from the late 2nd century onwards and is found in, e.g., P^{75}.

Numbers are written as numerals, surmounted by a horizontal stroke and preceded and followed by a short space containing a large, centrally placed dot.

Reduction of letters at the end of the line. The scribe made no attempt to justify the lines, i.e. to make them of equal width, which is characteristic of the great uncials, and involved a progressive diminution in the size of the letters as the scribe approached the end of what would otherwise have been an over-long line. In the present papyri I can find no real example of this; on the contrary, in lines of unusual length, they are often written out boldly into the margin without any attempt at economy.

Filling-marks are not used at line-ends.

Nomina Sacra. The only examples are $\overline{\theta\varsigma}$, $\overline{\theta\upsilon}$; $\overline{\iota\varsigma}$, $\overline{\iota\upsilon}$; $\overline{\kappa\varsigma}$, $\overline{\kappa\upsilon}$; $\overline{\chi\varsigma}$; $\overline{\pi\nu\alpha}$, $\overline{\pi\nu o\varsigma}$, $\overline{\pi\nu\iota}$. Notable absentees are those for υιος (even when applied to Jesus) and πατηρ ($\overline{\pi\rho\varsigma}$ restored in a lacuna in P^{67} is an error). Joshua in the Genealogy appears as ιησους written out in full. All the later additions to the list are of course also absent.

Punctuation and Text-Division. This is an important subject and cannot be fully investigated here. All that can be done is to chronicle the facts. To take the single stop, Roberts as quoted above states that there are three positions, which Aland's *Repertorium* describes as 'hoch'-, 'tief'- and 'mittel-Punkte'. I have, however, been unable to find any such distinctions. As regards the low point, Merell appears to print only two instances of this, viz. at Luke 3:16 and Luke 3:23, but in neither case is the stop actually on the lower line. Many of the stops printed by Merell as high could in fact be better described as in a median position. This lack of differentiation is most clearly shown in the Genealogy, where each name is followed by a point, which clearly ought to be the same in each case. In fact, although some could certainly be called high points, others are much nearer to a median position. The same situation is also found in the list of Apostles with which P^{4} comes to an end, and in which the names are separated by stops. I conclude therefore that the scribe recognised only the single point as a means of punctuation, and although he often placed it fairly high up, he took no pains to set it in any precise position.

The colon (:) is a very different matter since it signifies a major text division. It is always combined with *ekthesis*, the projection into the left margin of the initial letter or letters of the next complete line, surmounted by a paragraphus. The amount of the projection varies between one and two letter-spaces, and as it has been claimed that there is a difference in practice between P^4 on the one hand and P^{64+67} on the other, I have carefully examined all the instances. In P^4 I found a total of 18 examples. Some of these are in lacunae, but can be inferred by the unusual length of the line, and the colon in the preceding line. The amount of the projection varies. In two instances it is of two letters, i.e. the *third* letter of the text is aligned with the left-hand edge of the column. But in the rest of the cases the projection is less, i.e. the third letter is further towards the right. Very approximately, one can say where the amount of projection can be estimated there are, as just mentioned, two cases of projection by 2 letters, 8 of about 1.75 letters, 2 of about 1.5, and 1 only just over one letter. In P^{64} there is 1 of one letter. In P^{67} there is one of one letter, but in a correction, and one in a lacuna which was almost certainly of two letters. It is clear therefore that the scribe made no attempt to achieve any particular amount of projection within these limits.

Roberts stated that the above system of text-division was also found in the Bodmer codex of Luke and John, P^{75}, but since the beginning of Luke is lost in that manuscript, there is only one instance where the two can be compared, viz. at Luke 6:12, where both agree in making a division. The next point of division in P^4, at Luke 6:14 σιμωνα ον και ωνομασεν, is not distinguished in P^{75}.[4]

[4] This is not strictly a text-division but is intended to draw attention to the list of the Apostles. Merell does not print ll.20–5 of the column, which should be restored as follows:

 κ[αι εκλεξαμενος]
 [απ αυτων ·ι̅β̅·ους]
 [και αποστολους]
 [ωνομασεν : σιμωνα]
ο[ν̄ και ωνομασεν]
 [πετρον και ανδρεαν]

The omicron of ο[ν in l. 24, projecting into the margin, is clearly visible in the plate here published.

There are, of course, many parallels to the text-divisions in P^4 in Vaticanus and Sinaiticus, and in the Ammonian sections, but as all these reflect natural breaks in the narrative it is not easy to say whether there was one single widely-diffused system. The important point here is that the practice of organised text-division is now carried back well into the second century.

In addition to text-division, Roberts appears to say that P^4 contained 'omission and quotation signs', but I can find no sign of either. Omission signs could in any case only occur where the scribe has made a mistake, and the scribe is scrupulously accurate both in the text and orthography, except that ει is sometimes used for long *iota*, as in P^{75}. Otherwise itacism is totally absent. The only error I have detected is at Luke 5:36, where he accidentally wrote παλαι/ου instead of και/νου. Realising the error, he deleted παλαι and wrote και in its place, adding a ν before the ου in the next line. Merell does not note this. That the scribe himself is the corrector is proved by the distinctive shape of *kappa*.

There is no trace of any pagination.[5]

2. The Identity of the Script of P^4, P^{64} and P^{67}

The identity of P^{64} and P^{67} was recognised in 1963, when Dr Roca-Puig re-published P^{67} together with a note by C. H. Roberts, giving a revised text of P^{64}. But the connection of these fragments with P^4 has taken much longer to establish, and even now cannot be described as generally accepted. It was first mooted in 1966 by Kurt Aland, in an article 'Neue neutestamentliche Papyri' in *NTS* 12 (1966) 193–5; but while admitting the great similarity between the papyri, he did not accept their identity, as is shown by the fact that both then and ever since he assigned them different dates, and this differentiation has persisted down to the present day, P^{64} and P^{67} being dated 'um 200', while P^4 is dated 'III' (so in both N–A^{27} and UBS^4). Other authorities have drawn even wider distinctions, as will be seen when the whole question of date is discussed below.

I myself am convinced that the script is the same in all the fragments. They agree even in the minutest details, so far as these can

[5] On the absence of pagination cf. E. G. Turner, *The Typology of the Early Codex* (University of Pennsylvania Press, 1977) 74–5.

be checked, for P^{64} and P^{67} are so small that they cannot be expected to show examples of the complete range of features observable in P^4.

In these circumstances one is bound to ask why it has taken so long for the identity of the script in all the fragments to be recognised. I believe there are several reasons for this. In the first place, the appalling damage suffered by the P^4 fragments, resulting in innumerable lacunae and passages where the writing has disappeared or been obliterated or obscured by set-offs[6] and other extraneous marks, appears at first sight to set them apart from P^{64} and P^{67} which are in relatively good condition. In this connection I may mention an explanation I once offered of the fact that whereas vellum manuscripts are invariably ruled for writing, papyrus manuscripts never are. The reason, I suggested, is that in the case of papyri the textured surface tends to mask any irregularities whereas the smooth surface of vellum not only reveals but accentuates them. In much the same way the damage suffered by P^4 tends to mask any small irregularities in writing, while the far better condition of P^{64} and P^{67} highlights them. I must admit that I myself, when I first looked at the plates of P^4 in the *Revue Biblique*, felt that the script was lighter, more delicate and more regular than that of P^{64} and P^{67}, and it was only after I had obtained a set of full-size photographs of P^4 that I realised that the script was in fact identical.

Another, and perhaps equally valid reason is that, although not stated in the publication, the plates in the *Revue Biblique* illustrating the two best-preserved leaves, Fragments B and D, are both shown reduced in size, those of Fragment B (Planches III and IV) being approximately 90% original size, while those of Fragment D (Planches VI and VII) are only about 80% original size. Conversely, the plates illustrating P^{67} are *larger* than the original—about 1.25 times actual size. Dr Roca-Puig, op. cit. p. 31, says that the fragments are reproduced 'en el mismo tamaño del original', but this is incorrect. As every papyrologist knows, this makes comparison very difficult, if not impossible.

In addition, the fact that in the *Revue Biblique* the papyri have, idiosyncratically, been photographed against a black background

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[6] Set-offs can be seen in various places where the leaves have been stuck together, e.g. in P^4, Fragment B, col. 3, where portions of two lines of writing can be seen in the top left-hand corner, reading ΠΕΝΑΥ and ΝΟΝΤΗΝΧ, in reverse script. These are impressions from the first two lines of Fragment D, col. 4, which read: ειπεν αυτω εκτει/νον την χειρα.

provides a further obstacle to identification. I have recently seen a Japanese book on New Testament papyri which included illustrations of P^{4}, and at first I assumed that these were based on new photographs since they appeared against a white background. But minute examination revealed what had happened: the plates in the *Revue Biblique* had been re-photographed and the black background painted out. This had been done with great skill, and it was only after the most minute examination that I was able to establish the fact. I mention this because the effect of seeing the papyri against a white background was startling: one appeared to be looking at the papyri in what was, quite literally, a different light.

Finally, the skill with which the entire page is set out, and the care taken to maintain equal spacing of the lines, are far more apparent in the whole pages of P^{4} than in the small fragments P^{64} and P^{67}, where even the slightest irregularities are accentuated.

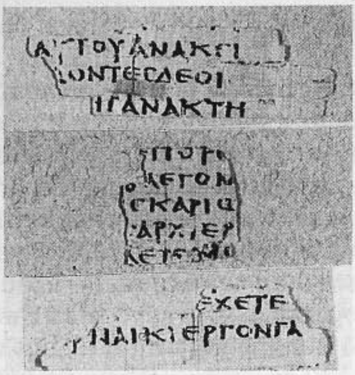
This lengthy discussion has been necessary because otherwise it would be difficult to explain why it has taken over thirty years for the identity of the fragments to be recognised, while even now it is subject to question. To enable readers to make up their own minds, the plate reproduced on p. 168 shows examples of all three sets of fragments.

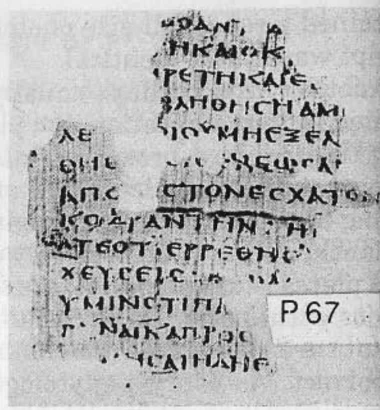
3. Reconstruction of the Oxford Leaf (P^{64})

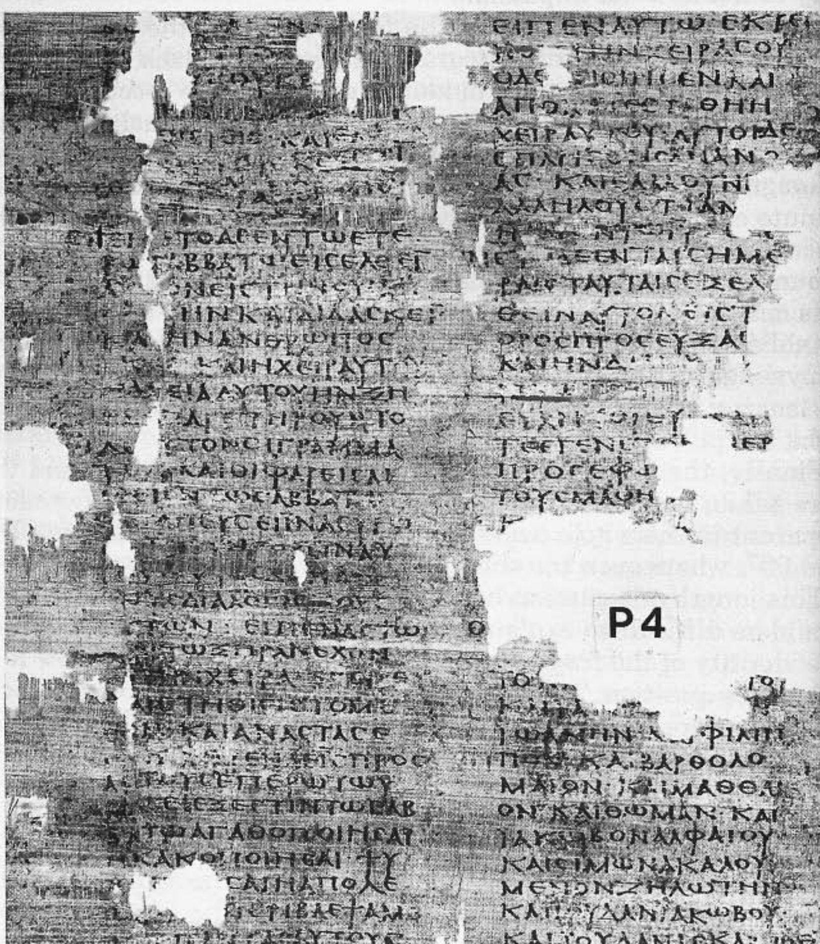
P^{64} consists of three small fragments, all from the same leaf of the manuscript. As Roberts correctly observed in the *editio princeps*, the manuscript must have been written with two columns to the page. In dealing with such a manuscript I have decided to number the columns as 1, 2, 3 and 4, i.e. the order in which the text follows, and thus avoiding any mention of recto or verso, terms which cause hopeless confusion when used in connection with a papyrus codex, since to papyrologists they inevitably suggest the sides with horizontal and vertical fibres respectively, which is irrelevant and confusing the the case of a codex. Accordingly, col. 4 is on the back of col. 1, and col. 3 is on the back of col. 2.

By a most fortunate chance, one of the Oxford fragments (fragment B) contains the last line of col. 1 on one side, and consequently the last line of col. 4 on the other. As a result, the amount of text between these two points is the exact contents of cols. 2 + 3 + 4,

64







and this makes it possible to attempt a reconstruction of these columns with very little possibility of error by dividing the text equally between them (see pp. 12–13). Col. 1, on the other hand, is admittedly more speculative. Its restoration is based on the following calculations. The two fragments A and B provide 6 lines of text (for although incomplete, they can be restored with certainty), and these comprise 95 letters, an average of 15.83. The lost text between these fragments comprises 79 letters, obviously five lines with an average of 15.8, and these added to the six extant lines give us the last eleven lines of the column. The average of these 11 lines is 15.9 letters, and it is on this basis that the upper part of the column has been restored. The total number of letters in this restored column is 570 (with an average of 15.83 per line), which may be compared with the figures for col. 2 (567), 3 (563) and 4 (567).

In cols. 2 and 3 the only possible change is that which could be effected by moving Fragment C up or down the column. To take col. 2, if Fragment C is moved up by one line, the average number of letters in the preceding 16 lines is increased to 16.9, while at the same time the average number of letters *after* Fragment C in col. 3 is reduced to 14.76. Neither of these figures is probable, and of course any larger movement of the fragment would result in even greater anomalies.

Of course, the actual division of text between lines cannot be guaranteed; but there can be little doubt of the overall correctness of the restoration, as anyone who tries to make a major alteration will soon find out. In this connection I may mention that in my original draft I had included μαθητων after $\overline{\iota\beta}$ at Matt 26:20. This has good authority (including Sinaiticus), but it is omitted by Vaticanus, and is consequently omitted in N–A^{27} and UBS^{4}.

	COL. I		COL. II	
		Matt 26		Matt 26
1	μαθηταις αυτου οι	2	καλον ηργασατο εις	11
2	δατε οτι μετα $\bar{\beta}$ ημε		εμε παντοτε γαρ τους	
3	ρας το πασχα γινεται		πτωχους εχετε μεθ ε	
4	και ο υιος του ανθρω		αυτων εμε δε ου παν	
5	που παραδιδοται εις		τοτε εχετε βαλουσα	12
6	το σταυρωθηναι τοτε	3	γαρ αυτη το μυρον του	
7	συνηχθησαν οι αρχι		το επι του σωματος	
8	ερεις και οι πρεσβυ		μου προς το ενταφι	

Table (*cont.*)

	COL. I		COL. II	
9	τεροι του λαου εις τη̅		ασαι με εποιησεν α	13
10	αυλην του αρχιερεως		μην λεγω υμιν οπου εα	
11	του λεγομενου και		κηρυχθη το ευαγγε	
12	αφα και συνεβουλευ	4	λιον τουτο εν ολω τω	
13	σαντο ινα τον ι̅ν̅ δο		κοσμω λαληθησεται	
14	λω κρατησωσιν και		και ο εποιησεν αυτη	
15	αποκτεινωσιν ελε	5	εις μνημοσυνον αυ	
16	γον δε μη εν τη εορτη		της τοτε] πορε[υθεις α̅	14
17	ινα μη θορυβος γενη		των ·ι]β· λεγομ[ενος ιου	
18	ται εν τω λαω του δε	6	δας ι]σκαριω[της προς	
19	ι̅υ̅ γενομενου εν βη		τους] αρχιερ[εις ειπε̅	15
20	θανια εν οικια σιμω		τι θε]λετε μο[ι δουναι	
21	νος του λεπρου προ	7	καγω υμιν παραδωσω	
22	σηλθεν αυτω γυνη ε		αυτον οι δε εστησαν	
23	χουσα αλαβαστρον		αυτω ·λ̅· αργυρια και	16
24	μυρου βαρυτιμου και		απο τοτε εζητει ευ	
25	κατεχεεν επι της κε		καιριαν ινα αυτον	
26	φαλης] αυτου ανακει[		παραδω τη δε ·α̅· των α	17
27	μενου ι]δοντες δε οι[	8	ζυμων προσηλθον οι	
28	μαθηται] ηγανακτη[		μαθηται τω ι̅υ̅ λεγον	
29	σαν λεγοντες εις τι		τες που θελεις ετοι	
30	η απωλεια αυτη εδυ	9	μασωμεν σοι φαγειν	
31	νατο γαρ τουτο πρα		το πασχα ο δε ειπεν	18
32	θηναι πολλου και δο		υπαγετε εις την πο	
33	θηναι πτωχοις γνους	10	λιν προς τον δεινα	
34	δε ο ι̅ς̅ ειπεν αυ]τοις[		και ειπατε αυτω ο	
35	τι κοπους παρ]εχετε[		διδασκαλος λεγει	
36	τη γυ]ναικι εργον γαρ[		ο καιρος μου εγγυς	
	Total no. of letters: 570		Total no. of letters: 567	

	COL. III		COL. IV	
		Matt 26		Matt 26
1	εστιν προς σε ποιω		και ευλογησας εκλα	
2	το πασχα μετα των μα		σεν και δους τοις μα	
3	θητων μου και εποι	19	θηταις ειπεν λαβε	
4	ησαν οι μαθηται ως		τε φαγετε τουτο εστι̅	
5	συνεταξεν αυτοις		το σωμα μου και λαβων	27
6	ο ι̅ς̅ και ητοιμασαν		ποτηριον και ευχα	
7	το πασχα οψιας δε γε	20	ριστησας εδωκεν αυ	
8	νομενης ανεκειτο		τοις λεγων πιετε εξ	

Table (*cont.*)

	COL. III		COL. IV	
9	μετα των ·ι̅β̅· μαθητων		αυτου παντες τουτο	28
10	και εσθιοντων αυτω̅	21	γαρ εστιν το αιμα μου	
11	ειπεν αμην λεγω υ		της διαθηκης το πε	
12	μιν οτι ·α̅· εξ υμων πα		ρι πολλων εκχυννο	
13	ραδωσει με και λυ	22	μενον εις αφεσιν α	
14	πουμενοι σφοδρα ηρ		μαρτιων λεγω δε υμι̅	29
15	ξαντο λεγειν ·α̅· εκα		ου μη πιω απ αρτι εκ	
16	στος αυ]τω μ[ητι εγω		τουτου του γενημα	
17	ειμι κ̅ε̅] ο δ[ε αποκρι	23	τος της αμπελου εως	
18	θεις ει]πεν ο [εμβαψας		της ημερας εκεινης	
19	μετ εμ]ου τη[ν χειρα		οταν αυτο πινω μεθ υ	
20	εν τω τ]ρυβ[λιω ουτος		μων καινον εν τη βασι	
21	με παραδωσει ο μεν	24	λεια του πατρος μου	
22	υιος του ανθρωπου		και υμνησαντες εξηλ	30
23	υπαγει καθως γεγρα		θον εις το ορος των	
24	πται περι αυτου ου		ελαιων τοτε λεγει	31
25	αι δε τω ανθρωπω ε		]αυτοις ο ι̅ς̅ παν[τες	
26	κεινω δι ου ο υιος του		]σκανδαλισθη[σεσθε	
27	ανθρωπου παραδιδο		]εν εμοι εν τ[η νυκτι	
28	ται καλον ην αυτω ει		]ταυτη γεγ[ραπται	
29	ουκ εγεννηθη ο ανθρω		γαρ παταξω τον ποι	
30	πος εκεινος αποκρι	25	μενα και διασκορπι	
31	θεις δε ιουδας ο πα		σθησονται τα προβα	
32	ραδιδους αυτον ει		τα της ποιμνης μετα	32
33	πεν μητι εγω ειμι ραβ		δε το εγερθηναι με	
34	βι λεγει αυτω συ ει		προαξω υμας εις την	
35	πας εσθιοντων δε αυ	26	]γαλειλαιαν α[ποκρι	33
36	των λαβων ο ι̅ς̅ αρτον		]θεις δε ο πετρος ε[ιπε̅	
	Total no. of letters: 563		Total no. of letters: 567	

Total no. of letters (4 columns): 2267

I have followed the practice of the scribe in writing numbers as numerals. At Matt 26:14 I have printed α̅ των ι̅β̅, but I have some doubts about this since I believe that scribes, though perfectly happy to use numerals, disliked aggregations of them: cf. P^{75} at Luke 12:52 διαμεμερισμενοι γ̅ επι δυσιν και β̅ επι τρισιν, or Sinaiticus at Matt 12:40 τρις ημερας και γ̅ νυκτας.

Finally, this reconstruction is not a mere exercise, but has two definite objectives: firstly, to show that the fragments can be fitted into the general framework as evidenced by P^{4}, and secondly to provide a firm basis for assessing the text content of the leaf and its individual columns, and thus giving statistics which can be used when the structure of the whole manuscript is considered. For instance, it seems certain that all four columns contained approximately the same number of letters, in marked contrast to what we shall find when we come to examine P^4 below.

4. The End of Matthew

With the aid of the reconstructed leaf of Matthew we can now attempt to calculate where the end of the Gospel would have come. Using the text of N–A^{27} and making allowances for the *nomina sacra* used by the scribe and for numbers written as numerals, I calculate that the remaining portion of the Gospel after the end of the reconstructed leaf contains about 10,115 letters. Since the reconstructed leaf contains 2,267 letters, the remainder of the Gospel would have occupied 10115/2267 = 4.46 leaves, i.e. 4 complete leaves, with the Gospel ending just before the foot of col. 2 of the fifth leaf—probably about 3 or 4 lines from the foot, leaving enough space for the colophon. The next Gospel would then have begun overleaf, viz. at the head of col. 3. Was this Gospel Luke?

5. The Beginning of Luke

When we turn to Luke we are dealing with substantial portions of pages instead of tiny fragments, and calculations can thus be more reliable. The following reconstruction assumes that each leaf contained approximately the same amount of text, measured in numbers of letters; and it will be seen that on this basis the extant fragments come from the 3rd, 6th, 8th and 10th leaves of the Gospel, i.e. none of them are consecutive. The reconstruction also shows that the Gospel must have begun at the beginning of a leaf, i.e. at the top of col. 1, and this is incompatible with the ending of Matthew, which shows that the next Gospel must have begun at the top of col. 3, overleaf from the end of Matthew. For Luke to have followed after Matthew, we should have to assume that the scribe left cols. 3 and 4 blank, and why would he have done so? Coupled with this is the great improbability that Luke would have followed immediately after Matthew, and we come to the conclusion that another

Gospel or Gospels must have intervened at this point; and since a codex containing three gospels is unthinkable, the only possible conclusion is that the manuscript *originally contained all four Gospels*.

Leaf	*Lost or Extant*	*Approximate text contents*	*Number of letters in the column*				
			Col. 1	Col. 2	Col. 3	Col. 4	TOTAL
1	Lost	Luke 1.1–1.25	–	–	–	–	2101
2	Lost	1.26–1.57	–	–	–	–	2101
3	Extant (Frag. A)	1.58–2.8 ησαν	518	553	538	500	2109
4	Lost	2.8 εν τη—2.36 φυλης	–	–	–	–	2188
5	Lost	2.36 ασηρ—3.8 τεκνα	–	–	–	–	2193
6	Extant (Frag. B)	3.8 τω αβρααμ—4.2 αυτων	513	557	545	511	2126
7	Lost	4.2 επεινασεν—4.29 οφρυος	–	–	–	–	2169
8	Extant (Frag. C)	4.29 του ορους—5.9 περιεσχεν	–	–	–	–	2118
9	Lost	5.9 αυτον—5.30 εσθιετε και	–	–	–	–	2116
10	Extant	5.30 πινετε—6.16 ισχαριωθ	537	574	551	507	2169

As will be seen, there is a considerable variation in the number of letters in the column. This will be discussed below, in connection with the general organisation of the manuscript. Only an overall figure has been given for Fragment C because so little remains that it would be hazardous to attempt to estimate the number of letters in the individual columns.

If another Gospel came between Matthew and Luke, which Gospel was it? This question will be considered in the next section, in which the probable contents of the entire manuscript will be considered.

6. The Contents of the Manuscript

It might appear hopeless to attempt to reconstruct the manuscript on the basis of the few scattered fragments which have survived. However, certain conclusions can be drawn. It is, of course, impossible to reconstruct the leaves from which the two Barcelona fragments come, since we have no means of deciding their position in the page.

Both clearly come from the outer half of a leaf, viz. col. 2 on one side and col. 3 on the other, and by taking equivalent points on both sides of the fragments we can estimate the number of letters in one column. In the earlier fragment, Folio A, the number of letters so calculated is 618, while in Folio B it is 572. In P^{64} the average of the four columns in the reconstruction given above is 567. In the Lucan fragments, P^{4}, the average contents of a column can be calculated from the statistics in the preceding table. It will be convenient to show all these figures in tabular form:

P^{64}	Folio A	618 letters
	Folio B	572 letters
P^{64}	(average)	567 letters
P^{4}	Fragment A (average)	537 letters
	Fragment B (average)	531 letters
	Fragment D (average)	542 letters

As will be seen, there is more or less a steady decline in the amount of text in the column, and of this there can be only one possible solution: the manuscript must have been a single-quire codex.

The single-quire codex,[7] the most primitive form of codex, is made by laying down a number of sheets, one on top of the other, and then folding over the pile at the centre. Where a large number of sheets is concerned, the bulk of the fold causes the sheets near the centre of the fold to protrude beyond those at the ends, producing a wedge-like shape. For practical purposes this foredge must be cut square, with the result that the leaves near the centre are now narrower than those at the ends, so that the width of the page steadily diminishes towards the centre of the book, after which it increases again.[8] This is a well-known feature of the single-quire codex, and may be illustrated by the Chester Beatty codex of the Pauline Epistles (P^{46}), in which the width of the page diminishes from about 15.5 cm to 13 cm in the centre, the column of writing narrowing correspondingly from 12.5 cm to 8.5 cm at the central fold, thereafter increasing again.

If, then, the manuscript with which we are concerned was a single-quire codex, this would account for the more or less steady diminution in the average amount of text in the column. The figures also

[7] On the single-quire codex cf. E. G. Turner, *Typology*, 55–60.
[8] E. G. Turner, *Typology*, 58.

provide a further reason for concluding that Luke did *not* follow immediately after Matthew, for since, as shown above, only some 4.5 pages after the end of P^{64} would have been needed to complete Matthew, it is difficult to see how such great variations both in total page content and in individual column content as are found in P^4 could have arisen in so short an interval.

We must, however, be careful in using such arguments, since there is an obvious discrepancy between the figures for the column content of the two Barcelona fragments, P^{67}, viz. 628 and 572 respectively. Folio A ends at Matt 3:15 δικαιοσυνην, while Folio B begins at Matt 5:20 εαν μη. The intervening text comprises about 3,800 letters. Since both fragments come from the outer half of a leaf, i.e. with col. 2 one side and col. 3 on the other, the number of complete columns between them must be a number in the series 2, 6, 10, 14 . . . Of these, 6 is clearly the only possibility, made up of col. 4 of Folio A + 1 complete leaf (= 4 columns) + col. 1 of Folio B.

How could such a dramatic change in the number of letters to the column have occurred when the fragments are separated by only a single leaf? The only suggestion which I can make is as follows. The writer of a single-quire codex has only a predetermined number of leaves into which his text must be fitted. Consequently he is constantly under pressure to avoid under-running or, still worse, overrunning his target.[9] Possibly, therefore, the scribe of our manuscript may have started off determined to avoid such embarrassments by keeping well ahead of his target, relaxing his efforts slightly when he realised that he was well on course to complete his task in the space allowed.

Since P^{67} Folio A comes from so near the beginning of the Gospel, it should be possible to reconstruct this section of the manuscript. From Matt 1:1 to Matt 3:9, where Folio A begins, comprises about 5020 letters. If we assume that Folio A came at the foot of a column, the preceding part of the column (lines 1–31) would have contained about 550 letters. Deducting this from 5020 leaves 4470 letters, which must represent a complete number of columns. Division of 4470 by 6, 7, and 8 produces 745, 638.5 and 558.7 respectively, of which 638.5 must obviously be the right answer, i.e. there were 7

[9] See E. G. Turner, *Typology*, 73–4.

complete columns preceding Folio A. This number must be made up of col. 1 of the leaf containing Folio A, plus 6 columns. With 4 columns to the leaf, this must be one complete leaf, plus columns 3 and 4 of the preceding leaf, i.e. the Gospel began on the inner side of the first leaf of the codex, just as in the Chester Beatty codex of Gospels and Acts, P^{45}. This presupposes a column with an average content of 638 letters, which is higher, but not markedly so, than the figure of 618 deduced from Folio A itself.

The foregoing calculations are based on the assumption that Folio A came from the foot of a column. If it is placed higher up, the average number of letters in the preceding columns increases up to a maximum of 717, which is highly unlikely.

Has the first leaf survived? There is an irregularly-shaped fragment (extreme dimensions 15.5 × 9.5 cm.) containing the inscription:

ΕΥΑΓΓΕΛΙΟΝ
ΚΑΤΑΜΑΘ’ΘΑΙΟΝ

in a hand which is certainly later than that of the manuscript and may be ascribed to the 3rd century, since the hook-shaped mark between the two *thetas* does not become common until about 200 A.D. This is on the side with horizontal fibres. On the other side are some faint traces of writing which are probably set-offs, i.e. impressions of writing transferred from another piece of papyrus to which it had been stuck. The first line seems to contain something like οθου, which certainly does not suggest the beginning of Matthew; moreover, the *theta* has the central bar projecting beyond the circular part on both sides—something which the scribe of P^4 never does. If this side did originally contain the commencement of Matthew, it would certainly be suitable since, as stated above, Matthew appears to have begun on the inside (cols. 3 and 4) of a leaf. But we should then have to assume that the whole of the original writing had been effaced, leaving no trace, which is very unlikely. The probability is therefore that the fragment comes from a fly-leaf at the beginning of the manuscript.[10]

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[10] K. Aland, *Studien zur Überlieferung des neuen Testaments und seines Textes* (Berlin: de Gruyter, 1967) 108, mentions ‘v° mit bisher unidentifizierten Buchstabenspuren in 2 Spalten’ but I myself have been unable to detect any traces of writing in two columns.

Finally, if, as suggested above, the manuscript contained all four Gospels, can we form some estimate of its size? In *The Birth of the Codex*,[11] p. 66, the figure of 144 leaves (= 288 pages) was suggested, but this was based on the remains of Luke, where, as we have seen, the pages were probably at their narrowest. If we take account of the pages before and after this point, which would have held a gradually increasing amount of text, a smaller figure of, perhaps, 120–130 leaves is more probable. Even so, it must have been very near to the limit for a single-quire codex.

If the Gospels in the present codex were in the canonical order of Matthew, Mark, Luke and John, the mid-point of the codex would have come somewhere about Luke 2. If they were in the so-called 'Western' order of Matthew, John, Luke, Mark, the mid-point would have come earlier, about John 20. In the fragmentary state of the manuscript it is not possible to choose between the two, though there is perhaps a slight bias in favour of the latter hypothesis.

7. The Structure of the Manuscript

The overall size of the page is uncertain, since none of the original edges appear to have survived. All that can be said is that the page must have measured at least 17 cm. in height by 13.3 cm. in breadth. Margins were probably generous, since the surviving lower margin of Fragment D of P^4 measures 2.5 cm.

Within the page the writing was set out with the greatest care and precision in two columns[12] of 36 lines each, spaced out with extreme regularity, as will be shown below. When the volume was opened, the blocks of writing on the two facing pages would have matched each other completely. This cannot be demonstrated directly, since, as noted above, none of the surviving leaves are consecutive. It can, however, be shown by tracings,[13] which prove that not only the blocks of text, but the individual lines on the two sides of the

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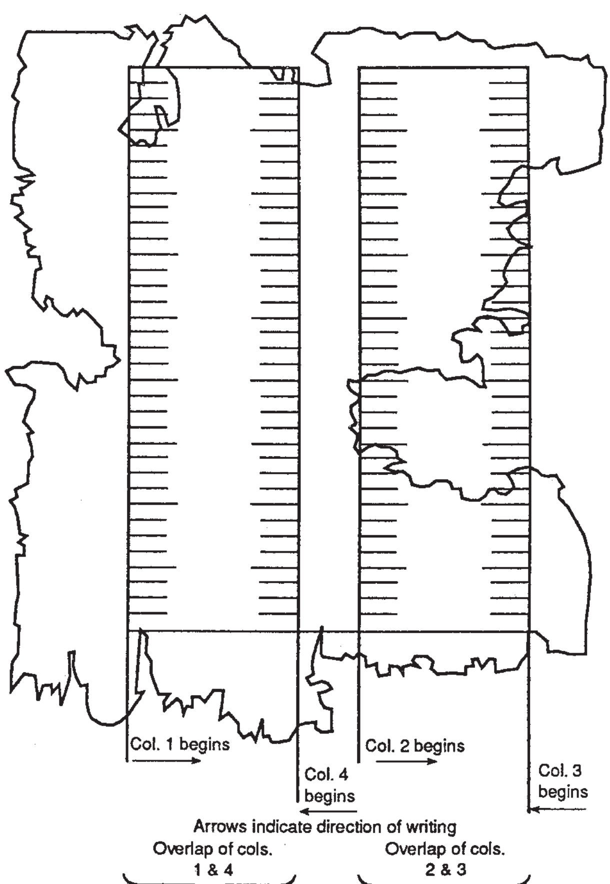
[11] C. H. Roberts and T. C. Skeat, *The Birth of the Codex* (Oxford, 1983).

[12] Lists of codices written with two columns to the page are given by E. G. Turner, *Apology*, 36.

[13] Ordinary tracing paper is useless for this purpose, as it is too opaque. Shops dealing in drawing-office requisites can supply sheets of clear plastic which can be written on with a special pen.

same leaf match each other precisely. Tracings also show that not only do the two sides of the same leaf match, but all the sides on all the fragments exhibit the same pattern, which is shown in the diagram reproduced overleaf.[14]

Of course, since the writing on the two sides of a leaf runs in opposite directions, and since in any case the lines are not of uniform length, they do not correspond completely. What *is* constant, however, is the extent to which the lines on the two sides overlap. This is represented by the distance between the beginning of col. 1 on one side and col. 4 of the other, and also between the beginning of col. 2



[14] It must be stressed that the lines in the diagram do *not* represent lines actually drawn on the papyrus, but serve merely to indicate the position of the lines of writing.

one side and col. 3 on the other. This distance is constant, measuring almost exactly 4 cm, and this provides us with a valuable statistic, since Folio B of P[67] also shows an overlap of about 4 cm. In P[64] there are no complete lines, but by taking those lines where restoration is certain, it can be calculated that the overlap there also was approximately 4 cm. This provides a further argument for concluding that all these fragments come from the same manuscript.

In P[4] the height of the column of writing, where it can be measured, varies slightly, from 13.2–13.4 cm. It must originally, of course, have been a fixed figure, and the variations must be due to the damage and distortion which the fragments have suffered. With 36 lines to the column, the amount of space allotted to each line is approximately 3.7 mm. How does this compare with the evidence of the other fragments? In P[67] the larger fragment, Folio B, contains 13 lines measuring about 5 cm., an average of 3.84 mm., which is probably as near as can be expected in view of the obvious damage and distortion visible. In P[64] the fragments are really too small to provide measurements, but Fragment C contains 5 lines measuring about 1.9 cm, an average of 3.8 mm. Calculations from these figures would give a total column height of 13.8 cm. in the case of P[67] and 13.7 for P[64]. Once again these calculations support the view that all these fragments come from the same manuscript.

One special feature of the *mise en page* must be mentioned, although I have no explanation to give, namely the variation in the text content of the columns of writing in P[4]. As is clear from the table printed above, there is a marked difference between the number of letters in the columns near the binding, cols. 1 and 4, and those near the foredge, cols. 2 and 3. This differentiation is found in all three fragments of P[4], A, B, and D, and cannot therefore be fortuitous. It is true that these results are based on letter-counts, which may be misleading, though the similarity of results in all cases inspires confidence. Can they be checked by another method? With this in view I have taken the length of all those lines in Fragment D which are sufficiently well preserved to be measurable, and then averaged the results for each column. The results are as follows:

Col. 1	Col. 2	Col. 3	Col. 4
4.08 cm.	4.39 cm.	4.20 cm.	3.87 cm.

As will be seen, these figures match very closely the results of letter-counts: in particular, col. 2, with the highest letter-count, has the longest average line, while col. 4, with the lowest count, has the shortest.

Why these differentiations were made, and how they were carried out, are questions I must leave to others.

It is obvious that such elaborate ruling-patterns could not have been set out freehand, but must have involved some mechanical means. Since to lay out each page separately would have been very time-consuming, I think it likely that a species of template must have been employed to make some sort of marks on the papyrus. What these marks were is another mystery. In some early papyri, such as P. Bodmer II and P. Bodmer XIII, small holes were punched through the papyrus to form guides, but I can find no trace of these in P^4, nor is there any sign of the ink dots which occur, though with the greatest rarity, in literary rolls.[15] It seems, therefore, that the columns and lines must have been marked out with some substance such as lead which could be subsequently washed off or erased.[16]

8. The Text

A detailed analysis of the text of the fragments is outside the scope of this article, but it may be useful to quote some basic facts. P^{64} and P^{67} are too small to provide any significant information, but in P^4 we have a substantial body of text and here the IGNTP edition of Luke provides a wealth of information. I should explain that throughout this section I have omitted the Genealogy (Luke 3:23–38) because it involves so many irrelevant variations of names and spelling. With this reservation I find that in P^4 there are altogether 107 disagreements with the Textus Receptus. These passages show the following number of agreements and disagreements with some of the principal manuscripts:

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[15] Cf. W. A. Johnson, 'Column Lay-out in Oxyrhynchus Literary Papyri', *ZPE* 96 (1993) 211–15.

[16] For such markings on Greek papyri cf. E. G. Turner, *Greek Manuscripts of the Ancient World*², pp. 4–5. For the use of guide-lines in Demotic papyri of the Roman period see the extensive study by W. J. Tait, 'Guide-lines and Borders in Demotic Papyri', in *Papyrus: Structure and Usage* (British Museum Occasional Paper 60, 1986) 63–9. In the present codex it is very unlikely that the lines would have been ruled right across the column, since subsequent erasure, after the text had been written, would have been very difficult and time-consuming. Probably, therefore, as suggested in the text, only the beginnings of the lines were marked out.

	Agreements	*Disagreements*
With א	67	40
With B	84	23
With A	13	94
With D	41	66
With L	65	42
With W	62	45
With Θ	22	85

There are also 19 agreements (and no disagreements) with P[75], though 9 of these are conjectural, i.e. the readings are uncertain because of damage to the lettering, or are based on considerations of space. In 79 of the 107 passages P[75] is defective. The most striking agreement with P[75] is in the omission of the nonsensical δευτεροπρωτω at Luke 6:1. Direct collation with the Codex Vaticanus gives the following result:

Luke 1:64 ανεωχθη δε το στομα αυτου παραχρημα B
η[νεωχθη δε] παραχρη[μα το στο]μα αυτου P[4]
1:65 διελαλειτο B
ελαλειτο P[4]
1:67 επροεφητευσεν B* i.e. with double augment (and some MS support). IGNTP does not record this.
επροφητευσε P[4]
1:68 κ̅ς̅ ο θ̅ς̅ B
ο θ̅ς̅ P4
1:76 ενωπιον κ̅υ̅ B
ενωπιον του κ̅υ̅ P[4]
3:9 καρπον καλον B
καρπον P[4]
3:16 ιωανης B
ιωαννης P[4]
3:21 απαντα B
παντα P[4]
3:22 σωματικω ειδει B
π̅ν̅ι [*sic*] ειδει P[4]
4:2 υπο N
απο P[4]
5:3 Here Merell prints δε καθισας instead of καθισας δε, but the reading is very doubtful. The papyrus is in very bad condition here, and needs to be re-examined on the original.

	5:3	εκ του πλοιον εδιδασκεν B
		εδιδασκεν εκ του πλοιου P^{4}
	5:4	εις αγραν B
		πρ]ος αγραν P^{4}
	5:4	χαλασατε B
		?του χαλ]ασαι P^{4}
	5:31	αυτους B
		αυτον P^{4}
	5:37	ρηξει B
		ρηγνυσι P^{4}
Luke	5:38	αλλ οινον B
		αλλα οινον P^{4}
	6:5	του σαββατου B
		και του σαββατου P^{4} (the και is required by the demands of space)
	6:6	εν ετερω B
		εν τω ετερω P^{4}
		δεξια B
		δεξια αυτου P^{4}
	6:11	διελαλουν B
		ελαλουν P^{4}
	6:15	ιακωβον αλφαιου B
		και ιακωβον αλφαιου P^{4}
	6:15	τον καλουμενον B
		καλουμενον P^{4}

It follows from the above that, until a very few years ago, the text of the fragments would have been described as Alexandrian, or perhaps Proto-Alexandrian, but now that the whole theory of localised text-forms has been virtually abandoned, the most that can be said, if any label is to be attached, is to describe the text as ‘Alexandrian’ in inverted commas. Under the scheme of the ‘Kategorien’ propounded by the Alands, P^{4} undoubtedly falls under Kategorie I, which includes P^{75} and B. It may perhaps be noted here that in only two of the above passages where P^{4} differs from B does it do so with any ‘Western’ support (at 1.68 and 3.9).

9. Provenance

The immediate provenance of P^4 is well known. A papyrus codex, containing two works of Philo, in a leather binding which had been reinforced by leaves from a Gospel manuscript glued together, was found at Coptos in Upper Egypt in 1889 (the date 1880 given by Merell is a misprint for 1889), concealed in a recess in a wall, encrusted with mortar and efflorescence of salt crystals. It was purchased, in Luxor, by the French scholar, Vincent Scheil,[17] who published the Philo treatises in the *Mémoires publiés par les membres de la Mission archéologique française au Caire*, tome 9, fasc. 2, 1893. In this work he included some of the Gospel fragments. He then presented the manuscript, together with the Gospel fragments, to the Bibliothèque nationale, where it bears the number MS. Suppl. grec 1120.[18] Scheil told Merell that he had purchased the manuscript in 1891, but it must have been in his possession at least a year earlier, since in the preface to his edition, which is dated 20 November 1891, he mentions an earlier draft made 'il y a un an' (i.e. in 1890); he must therefore have acquired the manuscript very soon after its reputed discovery.

Since the manuscript, though found at Coptos, was purchased in Luxor, there can be little doubt that it was bought from a dealer in antiquities there; and although there is no reason to doubt the truth of the story, it must be taken, like all dealers' stories—in this case quite literally—*cum grano salis*.

The Gospel fragments were finally separated and mounted by that master of restoration, Hugo Ibscher, apparently in about 1913.

How P^{64} and P^{67} came to be separated from the rest of the find we have no means of knowing. P^{64} was bought in Luxor by the Rev. C. B. Huleatt, minister at the English church there, and presented by him to Magdalen College, Oxford, in 1901. Of P^{67} Dr. Roca-Puig merely says: 'No consta el lugar de origen.' (op. cit., p. 35).

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[17] (Jean) Vincent Scheil, O.P., was born 10 June 1858. He studied Egyptology in Paris under Maspero and joined the Mission Archéologique in Cairo in 1883. He subsequently made a career as an Assyriologist. He died in Paris 21 Sept. 1940, cf. *Who was Who in Egyptology* (ed. W. R. Dawson and E. P. Uphill; 2nd ed.; London: Egypt Exploration Society, 1972) 263.

[18] Merell gives the location of the fragments in the Bibliothèque nationale as 'n° Gr. 1120, suppl. 2', van Haelst simply as 'Suppl. gr. 1120'. 'Suppl. grec 1120', however, is the press-mark of the Philo codex. According to C. P. Thiede in *Tyndale Bulletin*, 46 (1995) 55, the fragments are now kept in 'Box 5', not 'Box 2', under the number Suppl. grec. 1120.

In his little book *Buried Books in Antiquity* (London: The Library Association, 1963) 12–14, Colin Roberts suggests that the Philo manuscript might have been written at Caesarea (in which case the Gospel fragments are likely to be of Palestinian origin) because its text, like that of the medieval manuscripts of Philo, derives from an archetype once in the library at Caesarea. On the other hand, as Roberts admits, it might equally well have been a copy made in Egypt from a Caesarean exemplar; and certainly there must have been Philo manuscripts in Alexandria which provided the source of the papyri of Philo which have been found at Oxyrhynchus. In any case, the overwhelming probability must be that a manuscript found in Egypt was written in that country. That a manuscript of such high quality could have been written in Coptos is very unlikely. Instead, we have to think of some major Christian centre, perhaps even Alexandria itself (in the scriptorium of Pantaenus?) as the most likely place where the codex was produced.

10. The Date of the Fragments

The dating of these fragments is a complex process because of the variety of opinions which have been expressed and the number of factors which have to be taken into consideration.

In the first place the date of the Philo codex from which the P^4 fragments were extracted must be considered. Its original editor, Vincent Scheil, suggested the sixth century, but this is obviously far too late, and as early as 1899 Kenyon, in his *Palaeography of Greek Papyri*, 145, dated it '? 3rd century'. The same date was assigned by Hunt, who in his publication of some Oxyrhynchus fragments of Philo (P. Oxy. 9.1173) assigned the codex without qualification to the third century. Since then a third century date has been generally accepted. Eric Turner, for instance, in his *Typology of the Early Codex* (1977) dates it 3rd century in the 'Consolidated List of codices consulted', where it appears on p. 113 as no. 244 with the date 'iii (A. S. Hunt, E.G.T.)'. It is also dated 3rd century on p. xii (a full-page reproduction of a page of the codex), and on pp. 21, 36, 67, 87, 92 and 98. On p. 66 it is dated 'iii?', but the query is perhaps an oversight.

I may mention at this point that in van Haelst's catalogue, where the Philo codex appears as no. 695, among the dates assigned to it is 'iv (Merell)', but in fact Merell offered no opinion about the date of the codex.

Colin Roberts, in his booklet, *Buried Books in Antiquity* (1963) 12–13, likewise accepted the 3rd century date, but in his *Manuscript, Society and Belief in Early Christian Egypt* (1977), the Philo codex is dated 'later third century' on p. 8, and again 'late 3rd century' on p. 13. But the distinction is perhaps not of great importance.

How does all this bear on the date of the present fragments? Obviously they must be earlier than the date of the Philo codex. Obviously, too, one must allow some time for such an *édition de luxe* of the four Gospels to become so dilapidated that it was torn up and used as waste paper; and if a third-century date for the fragments is to be considered at all, it must surely fall within the very early years of that century. So much, then for the *terminus ante quem*.

We now turn to the fragments themselves, beginning with Merell's edition of P^4, and his statement on their date is so remarkable that it must be quoted in full: 'Les autorités telles que MM. F. Kenyon, l'éditeur des papyrus Chester-Beatty, P. Collart, professeur de papyrologie à la Sorbonne et A. Dain, professeur de paléographie grecque à la Sorbonne, après l'examen des photographies, font remonter notre manuscrit au IV^e siècle' (p. 7). The names of Kenyon, Collart and Dain certainly constitute a formidable trio, but their verdict is surprising. In the first place, one may wonder why, since Collart and Dain were both at the Sorbonne, they did not examine the actual fragments instead of relying on photographs. But the real puzzle is how they could have assigned a fourth-century date to fragments taken from the binding of a third-century manuscript, which must be later than the fragments.

In considering this, we must begin by turning back to Kenyon's *The Palaeography of Greek Papyri* (1899), where, as stated above, a date in the 3rd century was suggested for the Philo codex. But on p. 132 of the same work we find, in a list of Biblical papyri, the entry:

> Luke v. 30–vi. 4, in book form (attached to MS. of Philo, *vid. infr.*). Fourth century. Paris, Bibl. Nat. Scheil, *Mém. de la Mission Arch. Française au Caire*, tom. 9 (1893), with facs.

In other words, although Kenyon had suggested a 3rd century date for the Philo codex, he dated the Gospel fragments taken from its binding 4th century. How is this contradiction to be explained?

That in 1899 P^4 should have been dated 4th century occasions no surprise. At that time it was believed that the codex, and especially the Christian codex, did not become common until the 4th century, and this would have appeared to be reinforced by the script, with its affinity to Biblical uncial, then exemplified only in the great codices of the 4th and 5th centuries. But the contradiction between the two dates, viz. 3rd century for the Philo and 4th century for P4 still remains. It will be seen that Kenyon does not actually state that P4 came from the binding of the Philo, but uses the curious phrase 'attached to' it. Perhaps he was so convinced of the 4th century date of P4 that he believed that its connection with the Philo might be adventitious (although Scheil's statement is quite specific: the Gospel fragments were stuck together 'pour remplir la capacité de la couverture').

All this, however, was in 1899. But by 1938, at any rate, the whole picture had changed. Two examples of 'Biblical uncial' (P. Oxy 661 and P. Ryl. 16) had been published which could be approximately dated, and showed that the beginnings of the script could be traced back to the second century and one would have expected Kenyon and Collart, at least, to have been aware of these. Moreover, Kenyon himself was engaged on editing the Chester Beatty codices, and had concluded that in the case of Christian literature, 'the codex form was in use in the second century, and even probably in the earlier part of it' (*The Chester Beatty Biblical Papyri* [Fasc. I, General Introduction, 1933] 12).

However this may be, this flawed verdict seems to have had an enduring effect on the dating of the P4 fragments. In Turner's *Typology*, for example, P4 is sometimes dated 'iii' or 'iii/iv' although the latter date at least is incompatible with Turner's own 3rd century date for the Philo codex. In the 'Consolidated List' where P4 appears on p. 145, the date is given as 'iii or iii/iv', and the same appears on p. 36, in the 'List of codices written in two columns'. But on p. 3, and again on p. 22, P4 is merely dated 'iii'. On p. 92 P4 appears in the list of codices assigned to the 3rd century, and *not* in the list of those assigned to the 3rd–4th century.

Had Turner ever seen the P4 fragments? The entry in the Consolidated List just quoted is starred, which means that he had seen 'either the original or a good photograph'. Could the deplorable plates in Merell's edition be described as 'good photographs'? Certainly Roberts was under the impression that Turner had *not* seen the fragments, for I have found a letter from him to me, dated 5 Sept. 1977, in which he says: 'I think there can be no doubt that P4, P64 and

P^{67} all belong to the same codex. I have sent Eric (Turner) a photograph of P^{4} to compare with P^{64} but have not yet heard his reaction.' That is all I can find.

Certainly my own impression is that Turner had *not* seen the P^{4} fragments, and that he had somehow overlooked the fact that they come from the binding of the Philo codex which he himself had dated 3rd century.

From this rather confused picture it is a relief to turn to P^{64} + P^{67}, which right from the start have been dated either to the late second century or something very like it, such as circ. 200. 'Late second century' was Roberts's own, conclusion in the *editio princeps* of P^{64}, where he described the hand as 'an early predecessor of Biblical uncial' and quoted for comparison P. Oxy. 843 (Plato *Symposium*, circ. 200), P. Oxy. 1620 (Thucydides, late 2nd–early 3rd century), and P. Oxy. 1819 (Homer, 2nd century). All these dates are those of the editor. On this basis Roberts dated P^{64} to the late second century, stating that this conclusion has been agreed by Bell, Turner, and myself.

In his note appended to the republication of P^{67}, after it had been realised that it was part of the same manuscript as P^{64}, Roberts again characterised the script as 'a precursor of the style commonly known as Biblical uncial', and for comparison referred to P. Oxy. 661 (Callimachus, latter half of 2nd century), P. Berol. 7499 (Homer, 3rd century), illustrated in Schubart's *Griechische Paläographie*, 136, and the Dura fragment of Appian which he himself had included in his *Greek Literary Hands, 350 B.C.–400 A.D.* and which must be earlier than 256 A.D. He also referred to P. Oxy. 1179 (Apollonius Rhodius, late 2nd–early 3rd century) and P. Oxy 405 (Irenaeus, circ. 200), and repeated his conclusion that the fragments were written in the late second century.

This verdict was endorsed by Eric Turner in his *Typology of the Early Codex*, p. 99, where he mentions the codex from which P^{64} and P^{67} come 'which I believe is of the second century'. On p. 25 the codex 'may be as early as ii'. In the 'Consolidated List' (p. 149), date of $P^{64/67}$ is given simply as 'ii', and so also on p. 36 in the list of papyrus codices written in two columns. Curiously, however, it is not included in the list on p. 89 of 'codices of c. ii', but only in the section 'other scholars add' on p. 90. According to the statement on p. 89, this section covers 'those codices which other palaeographers whose judgment I respect have assigned to c. ii or ii/iii, but which I think belong to a later date'. Finally, although the entry in the consolidated list is obelized, which means it has not been used in Tables 1 and 2, P^{64} and P^{67} appears in Table 1 on p. 22 with the dating 'ii'.

All these verdicts are valuable,[19] but the time has now come to determine the position of our fragments in the series of examples of Biblical uncial constituted by Professor Guglielmo Cavallo in his masterly survey mentioned above, *Ricerche sulla Maiuscola Biblica*, which comprises a volume of text and an album of plates.

If we look through the plates, there are two which immediately attract attention because of their close resemblance to the script of our fragments. These are: Plate 12 *a* (P. Vindob. G. 29768) and Plate 15 *a* (P. Vindob. G. 29784). The latter indeed provides so close a resemblance that the hands might almost be described as identical, and of this Cavallo writes: 'Sempre alla fine del II secolo sono da assegnare il P. Lit. Lond. 78 (frammento di tragedia) e, forse un po' più tardo, il P. Vindob. G. 29784 (frammento mitologico).' This is followed by an analysis of the letter-forms of both fragments. Then, after noting that Gerstinger had dated G. 29784 to the third century Cavallo proceeds: 'ma il confronto con il P. Lit. Lond. 78

[19] Thirty years ago, Kurt Aland invited Colin Roberts and myself to express opinions on the dating of the registered New Testament papyri, then numbering 76, on the basis of photographs which he provided. These opinions, which Aland then published in *Studien*, 103–6, were, of course, our general impressions, since there could be no question of undertaking detailed research in individual cases. Our agreed dates were, for P^4, '3rd cent.', and for $P^{64/67}$, 'um 200', the possibility of their coming from the same manuscript being left open. I cannot now recall why we gave for $P^{64/67}$ a slightly later date than that assigned by Roberts in his edition of P^{64} and his note on P^{67}. Possibly the obvious similarity of script of both of these with that of P^4 may have influenced us towards keeping the dates fairly close together. Incidentally, our date for P^{75} was 3rd cent.

I must add at this point that the entry regarding P^4 in Aland's *Repertorium*, where P^4 appears as NT 4 on pp. 219–20, is misleading because a number of the references given relate to the Philo codex and not to P^4. Thus, support for a 4th century date for P^4 includes 'Grenfell/Hunt, p. 16', where the reference must be to vol. IX of the Oxyrhynchus Papyri, in which some fragments of Philo are published, the editor remarking (p. 16) that Scheil's date of 6th century for the Coptos codex is impossible, and should be 3rd century. Similarly, the reference 'Kenyon, p. 145' is to Kenyon's *The Palaeography of Greek Papyri*, on p. 145 of which he mentions the Coptos codex, suggesting a date of '? 3rd cent.' The references 'Cohn, p. XLII' and 'Wilcken, p. XLII' both refer to Vol. I of the Cohn-Wendland edition of Philo in which the Coptos codex is discussed, with Wilcken's comment that there is no reason to date it so late as the 6th century. None of the foregoing make any mention of P^4. Conversely, the entry makes no mention of the fact that Kenyon, Collart and Dain had unanimously agreed on a 4th century date for P^4.

e l'analisi paleografica . . . mi sembrano probanti per una retrodatazione alla fine del II' (pp. 35–6).

The other fragment mentioned above, P. Vindob. G. 29768, despite a superficial resemblance to our fragments, does show some differences, notably because the letters are more widely spaced, and this affects the whole appearance of the hand. More importantly, the reduction in size of letters at the end of a line (cf. col. i, 11.4–7) marks the beginning of a practice which was brought to a fine art in the great codices of the fourth and fifth centuries, but of which there is no sign in our fragments.

To sum up: on the basis of the foregoing, the only real difference of opinion regarding the dating of P^{4} and P^{64} + P^{67} is whether they are to be described as 'late 2nd century or '*circa* 200'. The difference between these two assessments is very small, and if I say that I would prefer to keep Roberts's 'late second century' it is mainly because I feel that '*circa* 200' gives an unwarranted air of precision. To say '2nd–3rd century' would be definitely misleading, since, as pointed out above, if a third-century date is to be considered at all, it must be confined to the very early years of that period.

In any case, it is clear that the codex has a very good claim to be regarded as the oldest known codex of the four Gospels, and to that extent the answer to the question asked in the title to this article must be: 'Yes.' The only possible rival, as it seems to me, is P^{75}, if, as I have suggested, this is the second half of a four-Gospel codex, made up of two single-quire codices—in fact, a two-quire codex (or were there two separate volumes?). The editors dated P^{75} to 175–225, but Eric Turner preferred a later period, cf. *Typology*, 95: 'my own dating, reached on the basis of morphological analysis, of P. Bodmer XIV/XV = P^{75} to c. A.D. 225–275 rather than to a period fifty years earlier'. Similarly, P^{75} is dated 'iii' on pp. 20, 24, 59, 93, 150. Turner's view seems to have been generally accepted. A detailed comparison of P^{4} and P^{75} is quite outside the scope of the present article, and I would only mention the much less developed system of *nomina sacra* in P^{4} (cf. Section 2 above) as a factor possibly worthy of consideration (P^{75} has a wide range—ανθρωπος, θεος, ιερουσαλημ, κυριος, πατηρ, πνευμα, υιος, χριστος, and the so-called 'staurogram' in the case of σταυρος and σταυρουν).

11. The Significance of the Codex

In *ZPE* 102 (1994) 263–8,[20] I advanced the theory that the reason why the Christians, perhaps about 100 A.D., soon after the publication of the Gospel of John, decided to adopt the codex was that only a codex could contain all four Gospels. I suggested that the motivation for this decision was the desire to ensure the survival of the four best-known and most widely accepted Gospels, and at the same time to prevent the accretion of further Gospels which could not be expected to contain authentic information but might rather seek to propagate doctrines which the Church had rejected. Of course adoption of the codex itself could not limit the number of Gospels which could be thus brought together, and the decision must therefore have been reinforced by a massive propaganda campaign of which we hear echoes in the writings of Irenaeus, where he explains why there cannot be more or fewer than four Gospels, culminating in the famous passage in which he equates them with the four 'Living Creatures' stationed round the throne of the Deity in the Apocalypse. I have discussed this passage in *NT* 34 (1992) 194–9 (= chapter A6), where I attempted to show that the arguments used by Irenaeus are not his own, but are derived from an earlier source, which I now think must go back to the time when the codex was formally adopted by the Church.

I concluded my *ZPE* article by saying that unless fragments of a four-Gospel codex should come to light which could be securely dated to the earlier part of the second century, my proposals must remain conjectural. Although this is still the position, we now have proof of a four-Gospel codex the ancestors of which must go back well into the second century.

Looking at these fragments from a different angle, I believe they may be of significance for the history of the codex. The care and exactitude with which the text is laid out have, so far as I can trace, no parallel among papyrus codices, let alone papyrus rolls. And if we have to search for parallels, we have to turn to vellum codices, which are almost invariably ruled for writing. This differentiation arises from the difference in the writing material. While the white, smooth and featureless surface of vellum tends to accentuate any

[20] Here chapter A7.

irregularities of writing, the textured surface of papyrus tends to have the opposite effect of rendering them less conspicuous. I believe, therefore, that even the first vellum codices of which we hear, those described by Martial, are likely to have been so ruled.[21] And if the papyrus codex is a development from the vellum codex, it would have been natural to attempt to apply the same procedure to papyrus. But here there is a difficulty. The ruling of vellum manuscripts is easily carried out, first by piercing a vertical series of pin-holes at regular intervals, and then ruling across them with a hard point which causes an indentation in the vellum and appears on the other side as a slightly raised line, thus ensuring automatically that the two sides match each other exactly. With papyrus, however, as experiments with modern papyrus will demonstrate, an indented line will not show up, because it cannot be easily distinguished from the fibres of the papyrus.

In the case of the present codex, as I have explained above, the problem was presumably solved by marking out the beginnings of the lines with a substance such as lead which could be erased after the writing had been completed. As the scribe had only these marks at the beginning of the line to guide him, there was a risk that the line of writing might tend to deviate from the strictly horizontal, and this actually happened occasionally in the case of P^4, but because, as I have mentioned, papyrus tends to conceal such irregularities, and because the overall structure of the page was maintained, they are not noticeable.

It was, however, soon realised that it was possible with papyrus to write an entire column free-hand and still produce an acceptable result. Of course in a codex the scribe has the four edges of the page to act as general guides, and all he had to do was to ensure

[21] Some examples may be quoted here. The well-known vellum fragment of Demosthenes, *de falsa legatione* (Pack[2] 293), usually dated 2nd century, has every other line ruled. The Berlin leaf of Euripides, *Cretans*, also dated 2nd century (P. Berol. 13217 = BKT V ii 73) was certainly ruled. Both sides are reproduced in BKT, and as the vellum has become partially translucent it can be seen that the lines on the two sides match each other completely. As regards Christian manuscripts, no vellum manuscript of the Gospels, with one exception, is dated earlier than the 4th century. The exception is 0171, dated circ. 300. This comprises two fragments, one from Matthew (in Berlin) and one from Luke (in Florence), and tracings from the latter show that the lines on the two sides match exactly; the manuscript must therefore have been ruled, as no doubt are the later examples.

that the block of writing was roughly rectangular and that the blocks on two facing pages matched. This was not difficult. Having written a left-hand page, he had to begin the right-hand page at about the same distance from the top of the page, and, as he approached the foot of the column, to adjust the spacing of the lines so that the column ended at about the same level as the left-hand column. Thus the actual number of lines in the two facing columns could, and often did, vary from each other.

We can, perhaps, detect an interim stage in this process in the Chester Beatty codex of Numbers and Deuteronomy (Chester Beatty codex VI). This is written with two columns to the page, and tracings show that while the blocks of writing on the two sides of the same page match each other, the individual lines do not, the number of lines in the column varying from 31 to 38. Another such example is P^{66}.

If the foregoing analysis is correct, the papyrus codex is likely to be a development from the vellum codex, a question which Roberts and I left open in *The Birth of the Codex*, 29.

Lastly, the script. Colin Roberts used the term 'reformed documentary' as a general description of the script of the earliest Christian papyri, but, as Roberts himself recognised, this label certainly cannot be attached to the script of the present fragments, which exhibit a style of writing already monumental in character, owing nothing to the writing of documents, and which was destined to be brought to perfection in the magnificent codices of the fourth century.

12. Conclusion

In conclusion I wish gratefully to acknowlege the help and encouragement I have received from Professor Graham Stanton, Dr J. K. Elliott and Dr Revel Coles. Professor Alain Blanchard, Director of the Paris Institut de Papyrologie, gave invaluable assistance in obtaining photographs of P^{4}. For permission to reproduce here specimens of P^{4}, P^{64} and P^{67} I am grateful to Mme Marie Odile Germain, Conservateur en chef of the Département des Manuscrits of the Bibliothèque nationale; to the President and Fellows of Magdalen College, Oxford; and to Dr Roca-Puig of Barcelona. Finally, thanks are due to Mrs. S. Colman for expertly typing an exceptionally complex manuscript.

7

THE CODEX SINAITICUS, THE CODEX VATICANUS AND CONSTANTINE

I. Where was Sinaiticus Written?

I was moved to undertake this study by noticing the following statements in a recent publication: 'Wir z. B. überhaupt nicht wissen, wo und von welchen Vorlagen die berühmtesten und frühen Majuskelm Aleph und B abgeschrieben sind' and then, a few pages further on: 'Niemand kann zum Beispiel sagen, wo die Handschriften B und Aleph entstanden bzw. woher sie ihren Text bekamen.'[1] In spite of these wholly negative verdicts I feel that it may be worth-while to consider once more what evidence we have for the origin of these two manuscripts and their subsequent vicissitudes.[2] I shall begin with Sinaiticus.

In the volume *Scribes and Correctors of the Codex Sinaiticus* (hereafter abbreviated to *Scribes and Correctors*) (British Museum, 1938, pp. 65–9) Herbert Milne and I put forward an argument which, we said, 'appears almost incontrovertible' for the belief that Sinaiticus was written in Caesarea. This was the reading of the manuscript at Matt. 13:54, where, instead of εἰς τὴν πατρίδα, the manuscript has the unique reading εἰς τὴν 'Αντιπατρίδα.[3] Now Antipatris was a town

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[1] Barbara Aland, 'Neutestamentliche Textforschung, eine philologische, historische und theologische Aufgabe', in *Bilanz und Perspektiven gegenwärtiger Auslegung des Neuen Testaments: Symposion zum 65 Geburtstag von Georg Strecker*, herausgegeben von Friedrich Wilhelm Horn (Berlin/New York: Walter de Gruyter, 1995), pp. 10, 15.

[2] Although the opinions here expressed are entirely my own, it is with deep gratitude that I record the help and advice I have received throughout from Dr Nigel Wilson of Lincoln College, Oxford, who has read the whole in its various stages of gestation. Monsignor Paul Canart, Vice-Prefect of the Vatican Library, has been most helpful in enabling me to obtain photographs of the Codex Vaticanus and in sending me information about the new facsimile of the manuscript now in preparation. I also owe a special debt of gratitude to those eminent scholars who have been kind enough to read the completed work and support its publication: Professor Bruce M. Metzger, Professor Graham Stanton, Professor J. Keith Elliott, Professor Henry Chadwick, and Professor Dr Barbara Aland. I must also mention the contribution of Mrs Suzanne Colman, who in putting the whole work on disk has nobly coped with a complex and multilingual text.

[3] The page containing the passage is illustrated, in about 35% actual size, in *The Cambridge History of the Bible*, vol. 1 (1970), Plate 25, where the reading in question can be clearly seen in column 2, line 30.

about 45 km south of Caesarea, and Rendel Harris,[4] who first realized the significance of the reading, remarked:

> As it seemed to me impossible that this should be an assimilation to a passage in the Acts (Acts xxiii. 31) where Antipatris is mentioned, I referred it to the aberration of a scribe's brain, as he sat writing in the neighbouring city of Caesarea. It is to my mind much the same as if a printed text of Shakespeare should put into Mark Antony's speech the line:
>
> I come to Banbury Caesar, not to praise him.[5]
>
> Such a text would probably be the work of Oxford printers.

To this I would add, for those unfamiliar with the topography of Oxford, that Banbury Road is one of the principal thoroughfares of the city, while Banbury itself is distant about 35 km. to the north.

Let us consider how the error might have occurred. I shall here assume that, as Milne and I came to believe, Sinaiticus was written from dictation.[6] The immediately preceding text is ὅτε ἐτέλεσεν ὁ Ἰησοῦς τὰς παραβολὰς ταύτας μετῆρεν ἐκεῖθεν καὶ ἐλθὼν εἰς. At this point the scribe must have felt certain that a place-name would follow, and when he heard something like τὴν πατρίδα his brain struggled to convert this into the name of a locality. Suddenly he thought of Antipatris, and Ἀντιπατρίδα went down in the manuscript. The scribe himself corrected the error, possibly at the time of writing, when the succeeding text must have shown that he had made a mistake.

The foregoing argument, if it stood alone, would be impressive. But it does not stand alone, for at Acts 8:5, as Milne and I pointed out

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[4] J. Rendel Harris, *Stichometry* (Cambridge, 1893), p. 75.

[5] 'I come to bury Caesar, not to praise him' is the second line of Mark Antony's funeral oration in Shakespeare's *Julius Caesar*.

[6] I do not want to discuss here the difficult question of dictation. Milne and I, without any preconceived notions, became convinced that Sinaiticus must have been written from dictation, cf. *Scribes and Correctors*, pp. 51–9. In 1949 Professor Alphonse Dain published his little book *Les Manuscrits* in which he firmly rejected the possibility that dictation had been used in the copying of manuscripts. I subsequently attempted to review the whole question in my paper, 'The Use of Dictation in Ancient Book-Production', *Proceedings of the British Academy* xlii (1956), pp. 179–208. I sent an offprint of this to Dain, who in his next edition of *Les Manuscrits*, published in 1964, modified his total rejection of dictation: 'T. C. Skeat . . . me prend à parti courtoisement. *Adhuc sub judice lis est*'. For further discussion see Klaus Junack, 'Abschreibpraktiken und Schreibergewohnheiten in ihrer Auswirkung auf die Textüberlieferung', in *New Testament Textual Criticism: its Significance for Exegesis; Essays in honour of B. M. Metzger* (Oxford, 1981), pp. 277–95, and P. Petitmengin and Bernard Flusin, 'Le Livre Antique et la Dictée', *Memorial André-Jean Festugière: Antiquité païeene et chrétienne* (Genève, 1984), pp. 247–62.

for the first time, Sinaiticus substitutes Καισαρίας for the correct Σαμαρίας. Although Caesarea is frequently mentioned in Acts the first occurrence is at Acts 8:40 and cannot therefore have affected the reading at 8:5. Once again, therefore, the error would be understandable if Sinaiticus was being written in Caesarea. Furthermore, it is obvious that neither of these errors could have occurred if the manuscript was being visually copied from the exemplar, and this supports our theory that the manuscript was being written from dictation.[7]

A bizarre attempt to rationalize the reading ʼΑντιπατρίδα is offered by J. H. Ropes in *The Beginning of Christianity* (ed. F. J. Foakes Jackson and Kirsopp Lake, 1. iii, 'The Text of Acts' (1926), p. xlvii, note 1), which must be quoted in full:

> Harris's often-quoted geographical argument from the reading ʼΑντιπατρίδα for πατρίδα in Matt. xiii. 54, which he thinks shows that the scribe lived somewhere in the region of Antipatris (actually Rendel Harris had said in Caesarea!) has enlivened criticism but cannot be accepted. The motive for the reading, as Hilgenfeld suggested (*Zeitschr. f. miss. Theol.*, vol. vii, 1864, p. 80) is plain. The scribe, in order to avoid calling Nazareth the 'native place' of Jesus, coined a word (or else used a very rare one) to mean 'foster-native-place'. Cf. ἀντίπολις, 'rival city'; ἀντίμαντις, 'rival prophet'; ἀνθύπατος, 'pro-consul', etc. ἀντίπατρος itself seems to mean 'foster-father' or 'one like a father'. As Kenyon points out (*Handbook to the Textual Criticism of the N.T.*, 2nd ed., p. 83), 'The fact that Aleph was collated with the MS. of Pamphilus so late as the sixth century seems to show that it was not originally written at Caesarea; otherwise it would surely have been collated earlier with so excellent an authority.' Indeed, if written at Caesarea, Aleph ought to show the text of Pamphilus. To the reasons for Caesarea given by Lake, *The Text of the New Testament*, Oxford, 1900, pp. 14 ff., was later added the point that the Eusebian canons might have been inserted in Caesarea, but not one of the arguments holds, nor do all of them together constitute a cumulative body of even slight probabilities. For Lake's statement of his change of view in favour of Egypt see his Introduction to the facsimile of Codex Sinaiticus, pp. x–xv.

[7] Caesarea is mentioned 15 times in Acts, and in 13 of these Scribe A spells the name as here, Καισαρία. Twice, however (Acts x. 24, xxv. 6) the spelling is Κεσαρία. This might at first sight seem to invalidate the claim that he was writing in Caesarea, since we might have expected that a scribe writing in Caesarea would know and use the correct spelling. In the case of Scribe A, however, as was noted in *Scribes and Correctors* (p. 54), 'confusion of ε and αι can occur anywhere' (they were, of course, by this time pronounced identically, as in Modern Greek), and it really looks as though he regarded them as allowable alternatives. Cf. Matt. 22:21, where he writes: λέγουσι Κέσαρος. τότε λέγει αὐτοῖς ἀπόδοτε οὖν τὰ Κέσαρος Καίσαρι κτλ.

The absurdity of this proposed explanation hardly needs demonstration. In the first place, Ἀντιπατρίς in the sense of 'foster-native-place' is not attested, its only occurrence, in the latest edition of Liddell and Scott, being given as 'name of kind of silver vessel' found in a Delian inscription of the third century B.C. (perhaps derived from its maker or designer); and it is hardly necessary to point out that Jesus himself regarded Nazareth as his πατρίς, and said so, as did the local population (Matt. 13:55–7).

Despite this, since Kirsopp Lake was one of the general editors of *The Beginnings of Christianity* it must be assumed that he accepted this explanation.

As will have been seen, Ropes alludes to a 'change of view' on the part of Kirsopp Lake. This is certainly true, and the change can be observed in the successive editions of his little book *The Text of the New Testament*, first published in 1900. Before we do so, however, there is one feature common to all editions of the book which must be mentioned. The opening chapter of the book, entitled 'The object and method of textual criticism' contains some observations on transcriptional errors such as dittography and haplography, and then proceeds as follows:

> It is very important to collect the examples of this kind of mistake, not simply because their detection is a first step towards the purifying of the text, but because *they are an important clue to the history of the manuscript in which they occur* [my italics]. The more senseless the mistake, the more important it sometimes is, *e.g.* in Matt. xiii. 54 Cod. Sinaiticus reads εἰς τὴν Ἀντιπατρίδα, for εἰς τὴν πατρίδα, where Dr Rendel Harris has pointed out that this is a clue to the birthplace of the MS, just as we might imagine an Oxford scribe of Shakespeare writing:
>
> 'I come to "Banbury" Caesar for "bury" Caesar,'
>
> and mistakes in spelling, especially if repeated, often give a hint as to the pronunciation, and so nationality, of the scribe.

It will be seen that by omitting any mention of either Caesarea or Antipatris the whole argument put forward by Rendel Harris is rendered completely unintelligible, and it is difficult to see what inferences Kirsopp Lake's readers were expected to draw. It is especially puzzling because the error is, precisely, an example of an error giving a 'clue to the history' of a manuscript!

Despite this, the passage just quoted remains unaltered in all editions of *The Text of the New Testament* down to the last (sixth) edition, published in 1928.

For Kirsopp Lake's earlier view of the provenance of Sinaiticus and Vaticanus we can take the third edition, published in 1906. Here, the scales are equally held between Caesarea and Egypt (p. 14):

> It is now necessary to ask what is the birthplace of Aleph B? This is a question which has to be answered for both together, not because they have an extraordinary similarity of text, although that is a marked phenomenon, but because of certain facts which show that they were originally both together at the same spot. This spot is Caesarea. *Almost all critics now accept this conclusion* [my italics], though Drs Westcott and Hort in their Greek text were inclined to think that some peculiarities of spelling in proper names point rather to the West.
> The case for Caesarea is this:
> (1) The colophon of Esther in Aleph, which seems to show that in the seventh century at least Aleph was at Caesarea, and was compared with a MS written in that place by Pamphilus.
> (2) The curious reading in Aleph in Matt. xiii. 54, Ἀντιπατρίδα, which Dr Rendel Harris describes as the mistake of a local writer [here again, as will be seen, Kirsopp Lake, no doubt unintentionally, renders the argument of Rendel Harris unintelligible by omitting any mention of Antipatris or its relation to Caesarea].
> (3) The identity of hands in part of Aleph and B (if Tischendorf be right, and his view is generally allowed to be extremely probable).
> (4) A curious chapter-division in Acts, which can be traced through Euthalius to Pamphilus and Caesarea.
> More must be said on this point in Section IV, in connection with systems of chapter-divisions.
> Thus it will be seen that there is evidence to connect Aleph with Pamphilus and Caesarea, Aleph with B, and Aleph B and Euthalius with Pamphilus and Caesarea.
> Can we say more? Some critics think we can, and connect both Aleph and B with a definitive edition, which is mentioned in Eusebius' life of Constantine. Eusebius says that he sent to Constantine's new city fifty σωμάτια ἐν διφθέραις . . . ἐν πολυτελῶς ἠσκημένοις τεύχεσιν τρισσὰ καὶ τετρασσά. No one knows quite what is meant by this last phrase. Rival views are: (1) Bound up in quires of three and four sheets; (2) written in three and four columns; (3) in cases of three or four. Those who accept the second explanation point to the fact that Aleph and B are in four and three columns, and that Aleph has the Eusebian canons, by the first or a contemporary hand. They therefore regard the two great uncials, so closely connected with each other and with Eusebius' home, as part of Eusebius' present to Constantine [the whole question of Constantine's order will be discussed in Section III below].

The only argument against Caesarea is as follows (p. 15): 'On the other hand Rahlfs has pointed out that the order of the books in B corresponds to the Canon of Scripture given by Athanasius in the

"Festal Letter" of 367 A.D., and thinks that this points to Alexandria rather than Caesarea (see also p. 53)' [this suggestion also will be discussed below, chapter II]. Finally, on p. 53, Lake concludes: 'It is, however, quite possible that the archetype of both MSS (i.e., Aleph and B) came from Alexandria to Caesarea—a theory which perhaps does justice to both arguments.'

In the summer of 1908 Kirsopp Lake travelled to St Petersburg to photograph the New Testament of the Codex Sinaiticus for the facsimile which was published in 1911. A further journey was made in the summer of 1913 to photograph the Old Testament, and the facsimile of this, including the Codex Friderico-Augustanus in Leipzig, appeared in 1922. Both volumes contain Introductions, virtually identical, in which the provenance and history of the manuscript are discussed at length. These introductions constitute the fullest and most detailed study of the provenance and history of the manuscript. They have exercised, and still exercise, a decisive influence on critical opinion, and are quoted in all the textbooks.

Certainly there has been a great change in Kirsopp Lake's attitude to these questions. No longer is there any mention of Rendel Harris or the reading 'Αντιπατρίδα, which is simply ignored. Instead, all attention is now concentrated on Egypt as the provenance of the manuscript, for which a number of new reasons are adduced. At the same time, however, it must be made clear that Kirsopp Lake was not dogmatic, saying: 'As will be shewn, an Egyptian provenance is actually the most probable . . . but it is not certain, and there have been competent scholars who have been inclined to think that Caesarea not only was the resting-place of the MS in the 6th century, but also has the best claim to be regarded as its original home.' And he admits that the various arguments which he intends to produce 'are likely to seem rather unsatisfactory to those acquainted with the splendid results reached by Latin scholars in fixing the date and provenance of their manuscripts'.

The arguments begin with the shapes of three letters, 'the so-called Coptic Mu, the curious shaped Omega with a long central line, and an occasional use of the cursive Xi. The Coptic Mu is common in papyri; it is called Coptic because it happens to be the form which passed over into the Coptic alphabet; but there is no evidence to show that it was rare outside of Egypt.'[8] To this I would add that,

[8] As Cavallo says (*Ricerche sulla Maiuscola Biblica*, p. 56) 'La forma detta "copta"

for example, in the Codex Alexandrinus which, as I have shown,[9] was apparently brought to Egypt in the fourteenth century and was therefore probably *not* written there, two examples of the 'Coptic' Mu can be seen in the colophon of Proverbs reproduced in *Scribes and Correctors*, plate 31. As regards the 'long omega' which Professor Cavallo has more expressively named the 'omega ancorato' since it does indeed resemble a small anchor, Milne and I discussed this at length in *Scribes and Correctors* (pp. 24–7), where we gave a long list of manuscripts, not all of Egyptian origin, in which this form is found. One of the most prolific users is in fact the Codex Alexandrinus, then believed to be of Egyptian origin, but now, as stated above, more probably to be regarded as non-Egyptian. As regards the cursive *xi*, a very cursive form is regularly used in the Chester Beatty codex of Numbers and Deuteronomy, which has been dated second century but is probably rather later, and this might seem to favour the idea that this was a specially Egyptian form. It is, however, apparently the standard form in the Ambrosian Hexateuch, A. 147 inf., (fifth century) since no fewer than five examples appear in the single page illustrated in Cavallo's *Ricerche sulla maiuscola Biblica*, plate 56. This manuscript was brought from Macedonia in the sixteenth century and has no known connections with Egypt. The same manuscript also makes occasional use of the 'long omega' discussed above. None of Kirsopp Lake's arguments are therefore valid.

Kirsopp Lake then turns to two spellings in Sinaiticus which, he claims, support an Egyptian origin. The first of these is the spelling κράβακτος for κράββατος or κράβαττος, which is found in Sinaiticus in ten out of the eleven places where the word occurs, and Kirsopp Lake adduces specimens of the same spelling in papyri. This, so far as it goes, is certainly an argument in favour of an Egyptian origin, though it must be remembered that we have no comparable documents from any other region.

The second spelling is that of Ἰσδραηλίτης for Ἰσραηλίτης, which is found in Sinaiticus in eight cases out of nine where it occurs, and which is found elsewhere only in Vaticanus, in the Old Latin, and in some papyri and other monuments from Egypt. Milne and I

del M infatti rappresenta nient' altro che la calligrafizzazione di una forma corsiva certo molta comune in tutti i terratori ellenistico-romani e difficilmente caratteristica del solo Egitto.'

[9] 'The Provenance of the Codex Alexandrinus', *JTS*, NS, 6 (1955), pp. 233–5 (= B2 above).

discussed this in *Scribes and Correctors* (pp. 66–7), where we quoted two inscriptions from Asia Minor in which the similar form Ἰστραηλίτης is found, and concluded that the argument was therefore not decisive.

After this, Kirsopp Lake raises the question whether Sinaiticus and Vaticanus have a scribe in common, as Tischendorf had alleged. This matter was analysed in detail in *Scribes and Correctors*, Appendix I, 'Scribes of the Codex Vaticanus' (pp. 87–90), our conclusion being that although absolute identity could not be proved, 'the identity of the scribal tradition stands beyond dispute.' The whole question will be considered further in Section 2 below, when the provenance of Vaticanus is discussed.

Kirsopp Lake adduces arguments linking the text of Sinaiticus with Egypt, including a statement by Harnack that the text of Sinaiticus resembled that found in the Coptic (Bohairic) Gnostic work, the Pistis Sophia, 'wie ein Zwillingsbruder nahe'. Kirsopp Lake does not follow this up, but since the claim has been made, it may perhaps be useful to state the facts.

The Pistis Sophia quotes a total of 287 verses from the Psalter, some of them repeatedly, and my own soundings[10] produce the following results.

As is well known, Sinaiticus was very carefully corrected, in about the sixth century, in Caesarea, by the corrector known as C^a^, and where the original reading of the manuscript (S*) is left unaltered, it can be assumed that C^a^ had the same text. On this basis, wherever Swete's text shows a variant, Pistis Sophia agrees with both S* and C^a^ 19 times; with S* but not C^a^ 16 times; with C^a^ but not S* 31 times; and, finally, with neither S* nor C^a^ 32 times. The claim of Harnack is thus seen to be at best inconclusive; indeed if S* is the 'twin' of the Pistis Sophia text, this designation might equally well be applied to the text used for collation by the Caesarean corrector C^a^, and therefore does not provide any evidence for the Egyptian provenance of Sinaiticus.

Kirsopp Lake then turns to an article by Rahlfs in which he had claimed an Egyptian origin for Vaticanus, Kirsopp Lake is, of course, using this as a means of determining the provenance of Sinaiticus, since he has admitted that both MSS are products of the same scrip-

[10] For this purpose I used the English translation by Violet Macdermot, which is based on the classic edition of Carl Schmidt (*Pistis Sophia: Text edited by Carl Schmidt, Translation by Violet Macdermot*, Nag Hammadi Studies, vol. 9, Leiden: Brill, 1978).

torium. This, again, will be considered below in the chapter discussing the provenance of Vaticanus, (Section II), where it rightly belongs.

Kirsopp Lake then refers to the presence in Sinaiticus of the Eusebian apparatus which, he admits, is 'the one serious argument which really seems to direct us to Caesarea for the provenance of the Codex Sinaiticus. It possesses the Eusebian canons, and the earlier the date assigned to the MS, the more probable, it may be thought, is it that a MS containing these canons should come from Caesarea. There is certainly weight in this contention; we should expect the Eusebian apparatus in Caesarea in the fourth century; it would be rather surprising to find that it had been adopted so soon in Alexandria.' This would indeed have been surprising, since one can hardly believe that the system of Eusebius would have been so speedily embraced by his archenemy Athanasius.

However, Kirsopp Lake still tries to find a way out, with the following extraordinary statement:

> Nevertheless the obvious force of this argument must be discounted by the fact that in considering the probability of one locality over another with reference to the early use of the Eusebian apparatus, the important point is really not the place in which Eusebius wrote, but the place in which Carpianus, to whom it was sent, received it. Now, our ignorance as to Carpianus is complete. He may have been a Caesarean, or he may have been an Alexandrian, or Byzantine; we know nothing about him, and therefore when we are discussing the provenance of the Codex Sinaiticus we have really not much more right to use the Eusebian canons as an argument in favour of Caesarea than we have to use the Ammonian sections, which are traditionally ascribed to an Alexandrian scholar, as evidence for an Egyptian origin.[11]

This totally ignores the fact that the 'Letter' of Eusebius to Carpianus, explaining his system, is a purely literary device for introducing his work to the public, just as Luke addresses an unknown Theophilus at the beginning of his Gospel and Acts. The question of who Carpianus was, where he lived, or even whether he existed, is totally irrelevant. What matters is that the system was devised by Eusebius in Caesarea and was disseminated from there. It is thus, as Kirsopp Lake admits, a powerful argument in favour of a Caesarean provenance of Sinaiticus—how powerful, as Kirsopp Lake also says, depending

[11] Elsewhere Kirsopp Lake uses the *absence* of the Eusebian Canon system from Vaticanus as an argument *against* a Caesarean provenance—something which looks very like 'trying to have it both ways'!

on the date assigned to the MS, for the earlier it is, the more likely it is to have been written in Caesarea.

Kirsopp Lake also mentions the bibles, πυκτία τῶν θείων γραφῶν, which Athanasius says the Emperor Constans asked him to supply. These were presumably written in Alexandria, and if so this must have been during the brief interval between his return to Alexandria after his first exile, on 23 Nov. 337, and 16 April 339, when he fled the city and left Egypt for his long second exile.[12] During the years 337–40 the headquarters of Constans were at Naissus, the modern Nish,[13] where one can hardly imagine that there would have been a great demand for a large number of complete Greek bibles. In any case, Sinaiticus is excluded from consideration in this context for the simple reason that, as will be shown below, it was never completed but remained in the scriptorium at Caesarea until long after the time of Constans. Whether Vaticanus could have been one of these MSS will be considered in Section II below.

One final point which Kirsopp Lake makes against a Caesarean origin of Sinaiticus may be mentioned here. Opposite Matt. 2:15, ἐξ Αἰγύπτου ἐκάλεσα τὸν υἱόν μου, Scribe A has written, in minute characters, ἐν Ἀριθμοῖς, thus identifying the source of the quotation as Num. 24:8. On this Kirsopp Lake writes:

> Professor Burkitt has pointed out to me that this reference is probably to Num. xxiv. 8 (ὁ) θεὸς ὡδήγησεν αὐτὸν ἐξ Αἰγύπτου. Now according to cod. 86 the Hexapla at Hosea xi. I had the note τούτῳ ἐχρήσατο ὁ Ματθαῖος ὡς οὕτως ἔχοντος δηλονότι τοῦ Ἑβραικοῦ, ὡς καὶ ὁ Ἀκύλας ἡρμήνευσε. One may say with some certainty that the Hexapla is here implicitly criticising and correcting the exegetical tradition preserved in the Codex Sinaiticus. The question therefore arises whether this tradition is likely to have been preserved in a MS made in the scriptorium of the library of which the Hexapla was the most treasured possession? The answer is obvious, and, so far as it goes, this point is distinctly against the theory that the Codex is a Caesarean MS.

In view of the general absence of direct Hexaplaric influence in both Sinaiticus and Vaticanus I must leave to readers to decide how much weight is to be attached to this point, after which Kirsopp Lake reaches his peroration:

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[12] For these dates see T. D. Barnes, *Athanasius and Constantine* (1993), pp. 36, 46.

[13] So T. D. Barnes, *The New Empire of Diocletian and Constantine* (1982), p. 86; but in the same author's *Athanasius and Constantius* (1993), p. 224, Naissus is preceded by a query.

> Even, however, if the connection between Athanasius and the Codex Sinaiticus [he probably meant the Codex Vaticanus, see below, Section II] be given up as a guess which is too uncertain to render its consideration desirable, it remains true that all the arguments from history, criticism, palaeography, and orthography combine to give to the view that the codex (i.e. Sinaiticus) is an Egyptian manuscript of the fourth century a probability which cannot be approached by any other theory. It would be too much to call it certain; but short of this it may fairly be regarded as the hypothesis which ought to be used as the general base of any discussions as to the critical value of the Codex Sinaiticus.

Since Kirsopp Lake, no study in depth of the provenance of Sinaiticus and Vaticanus has been attempted. The massive work of M. J. Lagrange, *Introduction à l'étude du Nouveau Testament: Deuxième partie: Critique textuelle: II. La Critique rationnelle* (Paris, 1935), includes discussions of both Vaticanus and Sinaiticus, but says surprisingly little about the arguments put forward by Kirsopp Lake. Vaticanus is very summarily dealt with (p. 84): 'il est sûrement d'origine égyptienne, et date du IV^{e} siècle. Rien n'empêche d'y voir un des exemplaires que Saint Athanase envoya à l'empereur Constant vers 340.' Sinaiticus is discussed at greater length. Thus, after noting that Kirsopp Lake had concluded that the manuscript was written in Egypt, he refers to the 'C' correctors, who certainly operated in Caesarea, and proceeds (p. 91):

> Or il n'y a aucune raison solide de penser que le manuscrit n'a pas été copié à Césarée. Cela n'empêcherait nullement qu'il représentât la tradition égyptienne. Saint-Jérôme nous apprend qu'Euzoius, au temps de Théodose, fit renouveler sur parchemin la bibliothèque d'Origène et de Pamphile, déjà détériorée, ce qui signifie, dit M. Lake, qu'il fit copier des papyrus sur du parchemin. Le manuscrit Sinaiticus peut avoir été un de ces manuscrits, s'il n'avait pas déjà été écrit par les soins d'Eusèbe, pour obéir à l'empereur Constantin vers 331. Quoi qu'il en soit, nous avons essayé de prouver et ses origines égyptiennes et sa composition à Césarée. Le premier point résulte de l'existence en Égypte d'un papyrus plus semblable à Sinaiticus qu'à Vaticanus, et qui date du III^{e} siècle. Le second point est mis en évidence par les noms topographiques du Sinaiticus, qui sent ceux de la tradition palestinienne d'Eusèbe. Nous croyons done que le manuscrit Sinaitique est né à Césarée, qu'il y est demeuré longtemps, jusqu'au moment où il a été transporté au Sinai, on ne sait à quelle époque. Eusèbe lui-même aura pu le munir de ses canons et faire remplacer des feuillets défectueux; mais pour ne pas laisser attendre l'empereur, il n'a pas fait transcrire ces sections sur les feuillets remplacés. Le manuscrit a bien l'aspect d'une copie administrative, très beau en apparence, négligé pour le

> fond, fait en série. Il n'en a pas moins une grande valeur, surtout quand il coincide avec B, son cousin plutôt que son frère, et certainement pas son père.[14]

The one positive contribution, or what might have been a positive contribution, by Lagrange is to draw attention to certain readings in Sinaiticus which, he claimed, reflected Palestinian traditions. Milne and I discussed these in *Scribes and Correctors* (pp. 68–9), on the basis of an earlier article by Lagrange, and showed that their value was impaired by the fact that he had failed to distinguish between original readings of the MS and those of various correctors. The only exception is at Luke 24:13, where Sinaiticus gives the distance of Emmaus from Jerusalem as 160 stades as against 60 of Vaticanus and most authorities, and 160 is said to be the reading of Origen.

One remarkable feature of Lagrange's work is that although insisting that Sinaiticus was written in Caesarea, he makes no mention of Rendel Harris and the reading Ἀντιπατρίδα, which would have provided invaluable confirmation of his position. It really looks as if Kirsopp Lake, by omitting all reference to Rendel Harris in the introductions to the facsimiles, had in effect caused his observations to slip into oblivion.

In 1938 *Scribes and Correctors* was published. The principal aim of this work was to record the results of the palaeographical researches which Milne and I had carried out on the manuscript, but we did include two brief sections on the date and provenance of the manuscript, in the latter of which we quoted the two readings mentioned at the outset of this study, viz. Ἀντιπατρίδα at Matt. 13:54 and Καισαρίας at Acts 8:5, which, we claimed, provided 'almost incontrovertible' proof that the MS was written in Caesarea. It now remains to consider how our claim has been received.

In 1954 Robert Devreesse published his invaluable *Introduction à l'étude des manuscrits grecs*,[15] and from the Prefect of the Vatican Library some valuable insights might have been expected. But the result is disappointing. He does indeed mention *Scribes and Correctors* ('étude

[14] As will have been seen from the extracts here printed, Lagrange throws out all kinds of suggestions without following them up or discussing them critically, and I do not think it worth-while trying to do so here. As regards the remark, 'pour ne pas laisser attendre l'empereur', presumably intended to be humorous, this assumes that Sinaiticus was one of the 50 bibles ordered by Constantine, something which will be discussed in chapter III below.

[15] Robert Devreesse, *Introduction à l'étude des manuscrits grecs, Paris* (1954).

minutieuse de la paléographie du manuscrit') but all he has to say about the provenance of Sinaiticus is; 'il semble égyptien d'origine' (p. 153, cf. also p. 125, 'quant au Sinaiticus, il semble egyptien d'origine'). On Vaticanus he has more to say, even if it is only derivative: 'Sur le texte (leçons expressives, caractères particuliers, corrections, harmonisations, additions et omissions, influences reçues), voir Lagrange pp. 83–90—sur les rapports mutuels de *Aleph* B, sur leur position vis-à-vis de la recension D, sur les conditions de l'archétype égyptien d'où ils dérivent, nonobstant des milliers de petites divergences—n'étant aucun des deux la copie de l'autre, ni même la copie d'un autre exemplaire,—le P. Lagrange me parait avoir dit tout ce qu'il est possible d'observer' (p. 153, note 2).

Despite his appreciative reference to *Scribes and Correctors*, it seems certain that Devreesse had not read the book, or, at any rate, not with any degree of attention. He does not quote the number assigned to the manuscript on its acquisition by the British Museum, Additional MS 43725, although this is given in the Introduction to *Scribes and Correctors*. As a consequence, the manuscript does *not* appear in the detailed *Index des manuscrits cités* on pp. 334–44, but only in the *Index général*, where it is put under the letter S on p. 330. In any case, it is difficult to see how he could have said that the manuscript 'semble egyptien d'origine' without mentioning the reasons which Milne and I said seemed 'almost incontrovertible' for its having been written in Caesarea. Similarly, his discussion of the error in Sinaiticus whereby a section of 1 Chronicles was intruded into the text of 2 Esdras (p. 87) shows clearly that he had not looked at *Scribes and Correctors*, the first four pages of which are devoted to this very subject.

In 1967 appeared Professor Guglielmo Cavallo's magisterial study, *Ricerche sulla Maiuscola Biblica*,[16] which of course included detailed studies of both Sinaiticus and Vaticanus. The long and closely argued text is not easy to summarize, and what follows is not to be taken as reflecting all the facets of the original arguments. Vaticanus is dealt with first (pp. 52–6). Detailed assessment of the script is difficult because at some time, perhaps during the tenth-eleventh centuries, the whole of the writing, which had faded badly, was traced over, and although this was done with great skill, the style of the original was inevitably impaired. Subject to this reservation, study of the

[16] Guglielmo Cavallo, *Ricerche sulla Maiuscola Biblica*, Studi e Testi di Papirologia, 2 (Firenze, 1967).

script, and comparison with the more easily datable Sinaiticus, together with consideration of the position taken by the manuscript in the development of biblical uncial, point to a date of *c.* 350 A.D. This dating takes into account such factors as the absence of the Eusebian canons and the resemblance, both in the selection and sequence of books, to that given in the 37th Festal Letter of Athanasius, issued in 367 but probably confirming existing practice. The provenance of Vaticanus is next dealt with briefly. The letter-forms adduced by Kirsopp Lake in favour of an Egyptian origin of both Sinaiticus and Vaticanus, and already discussed (pp. 588–9 supra) are quoted, but the claim that they are specifically Egyptian is rejected. Nevertheless, Cavallo agrees that the claim of an Egyptian origin has been generally accepted: 'La tesi, attualmente prevalente, dell' origine egiziana del codice rimane fondata, di conseguenza, su altri argomenti, in particolare il tipo testuale affine a papiri egiziani, a testi bilingui greco-copti e alle versioni copte e a quello usato da Padri che operarono in Egitto, la quasi sicura relazione con Atanasio Vescovo di Alessandria e le caratteristiche della lingua. In ogni caso non è possibile andare oltre discreta probabilità, mancando indizi più sicuri.' I must add that the question of the provenance of Vaticanus will be dealt with in the next chapter of this study.

Cavallo then passes to Sinaiticus (pp. 56–63). He agrees with the conclusions in *Scribes and Correctors* as regards the number of scribes and the portions of the manuscript which they wrote (pp. 56–7). He then describes the perfection of the script, and by placing the manuscript in the line of development of biblical uncial reaches the following conclusion: 'Tali elementi paleografici portano, senz' altro, nella progressiva linea di svolgimento del canone, al IV secolo avanzato e rendono altamente probabile, per questo famoso manoscritto, una data intorno al 360 ca. o solo di qualche anno più tarda, escludendo tuttavia, come già si è detto, gli ultimi decenni del secolo.'

Cavallo notes that this conclusion is broadly in line with that in *Scribes and Correctors*, chapter VIII, pp. 60–5, viz. 'before the middle of the [fourth] century' (p. 61) and 'not likely to be much later than about A.D. 360' (p. 64). In reaching this conclusion we quoted eight papyri which seemed to be datable within a certain margin, and which, we considered, illustrated the development of biblical uncial from the late second century onwards. Some of our remarks about the dates of these pieces are criticized by Cavallo, but since in any case our final conclusions agreed so closely with his own, I do not think it would be profitable to pursue the matter. He is also critical of our very tentative suggestion that the form of numerals in the manuscript might throw light on its date, and also our observation that, if it had not been known that all three scribes were contemporary, Scribe D might have been thought half a century later than his colleagues.

Summing up, Cavallo repeats his conclusion on the question of date as 'intorno al 360 ca. o poco più tardi, in quanto una data anteriore resterebbe esclusa dalla raffinata eleganza delle forme e dalla presenza di leggeri coronamenti e una data posteriore sarebbe impossibile in quanto il canone risulta ancora pienamente sentito' (pp. 60–1).

He then turns (p. 62) to the question of the provenance of Sinaiticus, rejecting once again Kirsopp Lake's attempt to prove an Egyptian origin on the basis of the 'omega ancorato' and the 'Coptic' mu. He then proceeds: 'Il Milne e lo Skeat, che ben si rendono conto della fragilità di tali argomentazioni [i.e. the use of letter-forms], si pronunziano per Cesarea: le ragioni addotte sono degne della massima considerazione e l'ipotesi palestinese appare senz'altro probabile.' At this point the reader expects to see that the two readings which we had adduced, viz. Ἀντιπατρίδα and Καισαρίας will be quoted and fully discussed. Astonishingly, we are not even told what they are, and the reader is thus left without any means of assessing their value. Instead, Cavallo brings up the attempt of Tischendorf to identify Scribe D of Sinaiticus with Hand B of Vaticanus. Milne and I, in a special appendix to *Scribes and Correctors* (Appendix I. 'Scribes of Codex Vaticanus', pp. 87–90), had discussed in detail the hands of the manuscript, and, using a variety of criteria, had concluded that, if anything, the resemblance of Scribe D of Sinaiticus was to the *other* scribe of Vaticanus, Hand A. This verdict Cavallo appears to accept, and then applies it to determine the provenance of Sinaiticus: 'In tal caso bisognerebbe pensare ancora una volta ad una origine egiziana del Sinaitico, in quanto all' Egitto e non alla Palestina riportano molte caratteristiche del Vaticano. Ma non si può andare al di là di ipotesi più o meno plausibili' (p. 63).

Cavallo's failure even to mention what Milne and I had claimed as 'almost incontrovertible' arguments in favour of Caesarea is inexplicable, and his attempt to claim an Egyptian origin, ending with the final reservation that certainty is impossible, leaves the whole question in a most unsatisfactory state.

One of the few places (or, perhaps, the only place, for I have found no other) where the reasons propounded by Milne and myself are even so much as mentioned is in the chapter contributed by Professor J. N. Birdsall to *The Cambridge History of the Bible* (vol. 1 (1970), pp. 359–60), where we read:

> Attempts have been made to associate the great codices Sinaiticus and Vaticanus with Caesarea as their place of origin, but on rather slender data. In the first place, it would seem plausible after the palaeographical work of Milne and Skeat that the same scribe had worked upon both manuscripts, scribe A of the Vaticanus being probably identical with scribe D of the Sinaiticus. Hence any datum bearing upon the origin of the one may well be valid for the other. The Sinaiticus[17] has at Matt. 13:54 for *Patrida* (homeland) the curious variant *Antipatrida* (an unknown word), which may spring from Antipatris, a place-name of the Caesarean region: similarly it has *Kaisareias* for *Samareias* at Acts 8:40 [this should have been Acts 8:5]. Again, some of the corrections in the Sinaiticus, denominated C by the editors, were executed in the sixth century, and one of the correctors in the Old Testament laid under contribution a manuscript written by the martyr Pamphilus in prison. He was the teacher of Eusebius and an outstanding figure of Caesarean Christian learning. In the sixth century, then, the manuscript may have been at Caesarea, where such a treasured relic would be preserved with care . . . On the other side there are two weighty points which argue for the Alexandrian origin of the Vaticanus at least (and the Sinaiticus probably comes from the same scriptorium): first, that the order of books is identical with that found in Athanasius' statements about the Canon of scripture, and secondly, that a striking variant in Heb. 1:3 is known elsewhere only in a Coptic source.

The possible relationship between Vaticanus and the order of books in the Festal Letter of Athanasius of 367 will be considered in the next chapter. As regards the unique reading of Vaticanus at Hebrews 1:3, viz. φανερῶν for φέρων, said to be paralleled only in a Coptic source, I must leave it to readers to decide whether this is a 'weighty point' (or, perhaps, 'slender data'?) in favour of an Egyptian provenance.

More recently there appears to be an increasing tendency to regard the question of the provenance of both manuscripts as insoluble. For instance, G. M. Hahneman, *The Muratorian Fragment and the Development of the Canon* (Oxford, 1992), includes *Scribes and Correctors* in his bibliography, and refers to it at p. 165, n. 77 and p. 166, nn. 80, 82, but all he can say about the origin of the manuscript is 'the provenance of the Codex Sinaiticus is not certain'. Thus, although Milne and I had devoted a chapter of *Scribes and Correctors* to a discussion of the provenance of the manuscript, this is simply ignored.

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[17] Cf. note 3 above for the plate illustrating the reading in question.

A posthumous work of Günther Zuntz,[18] *Lukian von Antiochien und der Text der Evangelien* (1995), is a welcome exception to the general trend. Here, for the first time, the conclusions of Milne and myself in *Scribes and Correctors* are quoted and accepted both as regards the Caesarean origin of Sinaiticus and our rejection of Kirsopp Lake's arguments in favour of Egypt. He quotes the two crucial readings, ascribing the quotation of both to Rendel Harris (in fact the second was our contribution), and dismisses the criticisms of Ropes as inadequate.[19]

Zuntz then goes on to suggest that both Sinaiticus and Vaticanus are linked with the order given by the Emperor Constantine to Eusebius, Bishop of Caesarea, to supply 50 bibles for the new churches in Constantinople, but this is something which will have to wait to be considered in chapter III below.

I think future generations may be puzzled to understand why it has taken so long for the significance of these two readings, 'Αντιπατρίδα for πατρίδα and Καισαρίας for Σαμαρίας, to be appreciated. They are in fact first-hand direct evidence of a kind rarely available. *The scribe in, in effect, himself telling us where he is writing.* Short of a colophon saying that Sinaiticus was written in Caesarea, I do not see how this could have been more clearly expressed.

II. Where was Vaticanus Written?

Vaticanus provides no such dramatic and convincing evidence of its origin as does Sinaiticus. In fact, as will be seen, any conclusion regarding its provenance relies almost entirely upon its association with Sinaiticus. This, as mentioned above, had been realized by Kirsopp Lake, but his statement is so hedged about with qualifications that it is advisable to quote his actual words: 'It will probably not be denied that there is, in spite of all other possibilities, a probability that the Codex Sinaiticus and the Codex Vaticanus belonged to the same scriptorium' (O.T. facsimile, p. xvi). He then proceeds to consider an article by Rahlfs[20] on the provenance of Vaticanus.

[18] Günther Zuntz, *Lukian von Antiochien und der Text der Evangelien*, Abhandlungen der Heidelberger Akademie der Wissenschaften, phil.-hist. Klasse (Jahrg. 1995), 2. Abhandlung, Heidelberg, Universitätsverlag C. Winter (1995), pp. 42–5.

[19] Op. cit., p. 43, n. 127.

[20] A. Rahlfs, *Alter und Heimat der Vatikanischen Bibelhandschrift*, Nachrichten der königlichen Gesellschaft der Wissenschaften zu Göttingen, phil.-hist. Klasse (1899), pp. 72–9.

The matter is highly complex and it will be best to quote Kirsopp Lake's own summary of the argument:

> He [Rahlfs] has pointed out that the Codex Vaticanus agrees in the most remarkable way with the list of scriptural books given by Athanasius in the Paschal Letter for 367. The points of agreement against other authorities are these: (1) in the Old Testament the book of Esther is not reckoned among the books which are κανονιζόμενα, but only among those which are ἀναγινωσκόμενα; (2) in the New Testament in Codex Vaticanus Hebrews is placed between the Epistles of the captivity and the Pastoral Epistles. This agrees with the Greek and Syriac text of the Paschal Letter; but the Sahidic version, agreeing with the usual Sahidic biblical text, places it between Corinthians and Galatians. It is argued that this represents in a Sahidic text a return to an old local use in Egypt; and, curiously enough, Codex Vaticanus has a continuous numeration for the sections in the epistles which is at present dislocated in such a way as to show that it was taken from a MS which placed Hebrews after Galatians. This is not quite the same as the Sahidic, but Dr Rahlfs thinks that it is near enough to justify the view that the Codex Vaticanus is an attempt to carry out Athanasius' view as to the order of the books, and that the text of the archetype, which was being modified, belonged to the old Egyptian type represented by the Sahidic version. He therefore argues that the Codex Vaticanus comes from Alexandria and is at least as late as 367. It is, of course, plain that this is not a decisive argument: the parallel between the Sahidic text of Athanasius and that implied by the numeration in Codex Vaticanus is not quite perfect; and the textual facts in connection with Athanasius are by no means clear. Nevertheless when all these points have been discounted it will probably be agreed that there remains enough to justify the statement that as our knowledge stands at present there is a presumption in favour of Egypt as the original home of the Codex Vaticanus.

As will be seen, all this is so vague that it might have been justifiable to ignore it. Nevertheless, the alleged link between the Codex Vaticanus and the Festal Letter is so often mentioned in textbooks that it seems desirable to look directly at the evidence here.

The 39th Festal Letter of Athanasius, issued in 367, is best studied in the edition of Lefort,[21] which gives a French translation of the Sahidic with the original Greek conveniently printed alongside. From this we see that Athanasius provided detailed lists of the books of the Bible, which he divides into two classes, the κανονιζόμενα,

[21] L.-Th. Lefort, *S. Athanase. Lettres Festales et Pastorales en Copte*, Corpus Scriptorum Christianorum Orientalium, vol. 15. Scriptores Coptici Tomus 20 (Louvain, Dubecq, 1955), pp. 34–7.

which he says are the sources of faith, and the ἀναγινωσκόμενα, which have been characterized by the Fathers as suitable reading for catechumens. In the Old Testament the κανονιζόμενα are the 22 books of the Jewish canon, while the ἀναγινωσκόμενα are listed as Wisdom, Sirach, Esther, Judith and Tobit. In the New Testament the κανονιζόμενα are the usually accepted books, viz. The four gospels, Acts, 7 Catholic Epistles, 14 Pauline Epistles, including Hebrews and the Pastorals, and the Apocalypse, while the ἀναγινωσκόμενα are the *Didache* and Hermas.

It must be stressed that despite the distinction which he draws between the two classes, Athanasius regarded the ἀναγινωσκόμενα as inspired and quoted them as such (with the exception of the *Didache*).[22]

In Vaticanus the κανονιζόμενα of the Old Testament follow the same sequence as in Athanasius, while the ἀναγινωσκόμενα appear *en bloc* between Job and the Minor Prophets. In the New Testament in Vaticanus all the books are present in exactly the same order as in the original Greek text of the Festal Letter, for although the end of the manuscript is lost, breaking off in the middle of Hebrews, there can be no doubt that the Pastoral Epistles and the Apocalpyse would have followed. Whether either or both of the two New Testament ἀναγινωσκόμενα came after this is of course unknown.

There is, however, a discrepancy between the Greek and Sahidic versions of the Festal Letter in the order of the Pauline Epistles, since in the Sahidic Hebrews appears between 2 Corinthians and Galatians. Although Vaticanus does not support this, it does have a series of marginal numbers which, it has been claimed, gives some support to the Sahidic order. This series begins with Romans and continues regularly up to the end of Galatians, which is numbered §58. This is followed by Ephesians, which begins with §70, and the numeration continues from this point up to the end of 2 Thessalonians, which is §93. After this comes Hebrews, which begins with §59 and continues to §64, where the manuscript breaks off. Obviously, therefore, the numeration was derived from a manuscript in which the order of the Epistles was as follows:

Romans-Galatians	§§1–58
Hebrews	§§59–69
Ephesians-2 Thessalonians	§§70–93

[22] For this see J. Ruwet, 'Le Canon Alexandrin des Ecritures, Saint Athanase', *Biblica* 33 (1951), especially pp. 10–12.

Thus, in this sequence, Hebrews came between Galatians and Ephesians. This, admittedly, is not the same as the Sahidic, but is claimed to be 'near enough' (!) to justify the conclusion that the Vaticanus is of Egyptian origin.

All this is hardly convincing, but anyway let us look at these section numbers. If we do so, it immediately becomes obvious that they are *not* the work of either of the two scribes of the manuscript. There is no attempt to reproduce uncial forms, as there is in the numbers inserted in the early part of the gospels, and the hand could better be described as semi-cursive. Particularly noticeable letters are the *theta*, which is very small, oval and slightly tilted, the slightly sloping hasta of kappa and the almost microscopic *omicron*. The hand is in fact very like that which has added the section numbers in John. In any case, it is clear that these numbers were *not* added in the scriptorium but after the manuscript had left it, and thus provide no evidence of its origin.[23] Cf. also Plate 2.

It will be seen that Kirsopp Lake concentrated on the provenance of Vaticanus in order to determine that of Sinaiticus, having convinced himself that both came from the same scriptorium. What is surprising is that, having compared the evidence of the Festal Letter with Vaticanus, he did not go on to compare it with Sinaiticus. If we do so, the result is startling, Sinaiticus differing widely both in the selection of books and their order. This can best be shown in tabular form, in which Vaticanus has been included. I may add here that although in Sinaiticus much of the Old Testament is lost, its original contents can be restored with complete certainty on the basis of the original quire-numeration. It will also be seen that, apart from the order of books, Sinaiticus includes some which Athanasius does not include in *either* of his two categories, viz. 1 and 4 Maccabees in the Old Testament and the Epistle of Barnabas in the New.

In the table ἀναγινωσκόμενα are italicized, while those in neither category are placed within brackets. The ἀναγινωσκόμενα are not listed in the Festal Letter column because Athanasius does not allocate them any particular position.

[23] C. M. Martini, *Introductio ad 'Novum Testamentum e codice Vaticano graeco 1209 tertia vice phototypice expressum'*, (1968), p. xiii, regards all these numerals as subsequent additions (*circiter saeculo IV–V textui apposito*), and cf. note 10: 'in codice nostro hi numeri neque a prima manu neque a "diorthota" sunt appositi.'

Festal Letter (Greek)	*Vaticanus*	*Sinaiticus*
Octateuch	Octateuch	Octateuch
1–4 Kings	1–4 Kings	1–4 Kings
1 & 2 Chronicles	1 & 2 Chronicles	1 & 2 Chronicles
1 & 2 Esdras	1 & 2 Esdras	1 & 2 Esdras
Psalms	Psalms	*Esther*
Proverbs	Proverbs	*Tobit*
Ecclesiastes	Ecclesiastes	*Judith*
Song of Solomon	Song of Solomon	(1 & 4 Maccabees)
Job	Job	
Minor Prophets	*Wisdom*	Isaiah
Isaiah	*Sirach*	Jeremiah
Jeremiah	*Esther*	Lamentations
Baruch	*Judith*	Baruch
Lamentations	*Tobit*	Ep. of Jeremy
Ep. of Jeremy	Minor Prophets	Ezekiel
Ezekiel	Isaiah	Daniel
Daniel	Jeremiah	Minor Prophets
	Baruch	Psalms
	Lamentations	Proverbs
	Ep. of Jeremy	Ecclesiastes
	Ezekiel	Song of Solomon
	Daniel	*Wisdom*
		Sirach
		Job
Gospels	Gospels	Gospels
Acts	Acts	Romans
Catholic Epp.	Catholic Epp.	1 & 2 Corinthians
Romans	Romans	Galatians
1 & 2 Corinthians	1 & 2 Corinthians	Ephesians
Galatians	Galatians	Philippians
Ephesians	Ephesians	Colossians
Philippians	Philippians	1 & 2 Thessalonians
Colossians	Colossians	Hebrews
1 & 2 Thessalonians	1 & 2 Thessalonians	Pastoral Epp.
Hebrews	Hebrews	Acts
Pastoral Epp.	[Pastoral Epp.?]	Catholic Epp.
Revelation	[Revelation?]	Revelation
		(Barnabas)
		Hermas

At this point the reader must be reminded that the reason why Kirsopp Lake, when considering the provenance of Sinaiticus, devoted so much time to the origin of Vaticanus was because he had formed the opinion that both manuscripts were products of the same scriptorium, and that consequently determination of the origin of Vaticanus would automatically carry with it Sinaiticus. What is so extraordinary is that in doing so he failed to see that the order of the books in the two manuscripts is completely different, and consequently if the order in Vaticanus is taken to be proof of Egyptian origin, the order in Sinaiticus might be held to prove exactly the opposite, viz. that the manuscript is *not* of Egyptian origin!

How Kirsopp Lake overlooked this point is difficult to see. However, it is no longer necessary to pursue this line of enquiry, and a direct comparison of the two manuscripts offers a better prospect of certainty.

In *Scribes and Correctors*[24] Milne and I spent some time investigating Tischendorf's claim that Hand B of Vaticanus was identical with Scribe D of Sinaiticus. Our verdict was that all the similarities were between Scribe D of Sinaiticus and the other scribe of Vaticanus, Hand A, our final conclusion being that in spite of many similarities 'it would be hazardous to argue identity of the two hands (for one thing, D's use of the long-pronged omega in corrections seems an obstacle) but the identity of the scribal tradition stands beyond dispute'.

Milne and I found that the colophon designs in Sinaiticus were of vital importance in distinguishing the three scribes of that manuscript, and they are all reproduced (in colour where necessary) in the plates at the end of our book. If we now turn to Vaticanus, we find that some of the designs at the ends of books in the Octateuch are remarkably similar to those of Scribe D in Sinaiticus. I therefore illustrate (Plate I) the colophon of Deuteronomy in Vaticanus, enlarged to show all the small details, together with the colophon of Mark in Sinaiticus, which is the work of Scribe D, similarly enlarged, so that the vertical member of the colophon is the same height as that in Vaticanus. As will be seen, the two designs are *almost completely identical in every detail*, although whether they are actually the work of the same scribe is another matter. However this may be, the identity is so remarkable that I do not think there can be the least doubt that *both manuscripts are the work of the same scriptorium, and—which is just as important—were written at approximately the same time. Vaticanus therefore, like Sinaiticus, was written in Caesarea*. Neither can therefore be one of the bibles which Athanasius produced in Alexandria at the request of the Emperor Constans.

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[24] *Scribes and Correctors*, pp. 87–90.

The colophon of Mark in Sinaiticus is drawn partly in red. Other colophons in Sinaiticus drawn partly in red are those of Psalms, Proverbs, Song of Songs, Revelation and Barnabas—all by Scribe A. In Vaticanus red is used in the colophons of Malachi and Matthew (both by Hand B)—another link between the two manuscripts.

Both manuscripts, then, were written in Caesarea. The remainder of this study will be devoted to considering the circumstances in which they were written.

III. The Letter of Constantine

Soon after the formal dedication of Constantinople on 11 May 330, the Emperor Constantine sent the following letter[25] to Eusebius, Bishop of Caesarea, who quotes the full text in his *Life of Constantine*, iv. 36:

> Νικητὴς Κωνσταντῖνος Μέγιστος Σεβαστὸς Εὐσεβίῳ.
>
> Κατὰ τὴν ἐπώνυμον ἡμῖν πόλιν τῆς τοῦ σωτῆρος θεοῦ συναιρομένης προνοίας μέγιστον πλῆθος ἀνθρώπων τῇ ἁγιωτάτῃ ἐκκλησίᾳ ἀνατέθεικεν ἑαυτό, ὡς πάντων ἐκεῖσε πολλὴν λαμβανόντων αὔξησιν σφόδρα ἄξιον καταφαίνεσθαι καὶ ἐκκλησίας ἐν αὐτῇ κατασκευασθῆναι πλείους. τοιγάρτοι δέδεξο προθυμότατα τὸ δόξαν τῇ ἡμετέρᾳ προαιρέσει. πρέπον γὰρ κατεφάνη τοῦτο δηλῶσαι τῇ σῇ συνέσει, ὅπως ἂν πεντήκοντα σωμάτια ἐν διφθέραις ἐγκατασκεύοις εὐανάγνωστά τε καὶ πρὸς τὴν χρῆσιν εὐμετακόμιστα ὑπὸ τεχνιτῶν καλλιγράφων καὶ ἀκριβῶς τὴν τέχνην ἐπισταμένων γραφῆναι κελεύσειας, τῶν θείων δηλαδὴ γραφῶν, ὧν μάλιστα τήν τ' ἐπισκευὴν καὶ τὴν χρῆσιν τῷ τῆς ἐκκλησίας λόγῳ ἀναγκαίαν εἶναι γινώσκεις. ἀπεστάλη δὲ γράμματα παρὰ τῆς ἡμετέρας ἡμερότητος πρὸς τὸν τῆς διοικήσεως καθολικόν, ὅπως ἄν πάντα τὰ πρὸς τὴν ἐπισκευὴν αὐτῶν ἐπιτήδεια παρασχεῖν φροντίσειεν· ἵνα γὰρ ὡς τάχιστα τὰ γραφέντα σωμάτια κατασκευασθείη, τῆς σῆς ἐπιμελείας ἔργον τοῦτο γενήσεται. καὶ γὰρ δύο δημοσίων ὀχημάτων ἐξουσίαν εἰς διακομιδὴν ἐκ τῆς αὐθεντίας τοῦ γράμματος ἡμῶν τούτου λαβεῖν σε προσήκει. οὕτω γὰρ ἂν μάλιστα τὰ καλῶς γραφέντα καὶ μέχρι τῶν ἡμετέρων ὄψεων

[25] The letter must have been given a date, but Eusebius has not preserved it. It is usually dated 330 or 331 on the grounds that it is likely to have been written fairly soon after the dedication of the City. T. D. Barnes, however, in *The Making of Orthodoxy: Essays in honour of Henry Chadwick* (OUP, 1989), p. 112, says: 'Eusebius dates that [i.e. Constantine's letter thanking Eusebius for his tract on the date of Easter] and the following letter [i.e. the one ordering the 50 bibles] to the time when he was returning to Palestine after his visit to Constantinople in November 335', but in fact Eusebius says nothing to suggest that these two letters are placed in any chronological framework, but are given merely as examples of Constantine's concern for the Church. A date nearer 330 therefore seems more likely. In any case, for the purpose of the present study the exact date is not important.

> ῥᾷστα διακομισθήσεται, ἑνὸς δηλαδὴ τοῦτο πληροῦντος τῶν ἐκ τῆς σῆς ἐκκλησίας διακόνων, ὃς ἐπειδὰν ἀφίκηται πρὸς ἡμᾶς, τῆς ἡμετέρας πειραθήσεται φιλανθρωπίας. ὁ θεός σε διαφυλάξει, ἀδελφὲ ἀγαπητέ.

Is the letter genuine? That is the first question, because in the past it has often been claimed that the Constantinian documents which Eusebius inserted in his *Life of Constantine* were forgeries, either by Eusebius himself, or inserted later. For instance, Pauly-Wissowa, *Real-Encyclopädie*, art. Constantinus, col. 1013, says: 'Die Urkunden fast alle gefälscht oder höchst zweifelhaft.' Then, in 1950, I published a small papyrus fragment in the British Museum which was soon after identified as coming from Constantine's great Edict to the Eastern Provincials, issued in 324 after his final defeat of Licinius, and which Eusebius quotes in full. Although the fragment was so small, the fact that the lines of writing were very long (about 64 letters in col. I, 70–80 in col. II) meant that the amount of text which the fragment guaranteed was quite considerable, amounting to about one-fifth of the Edict.[26] There can thus no longer be any doubt that both the Edict and, by implication, all the other Constantinian documents quoted by Eusebius, including the present letter, are perfectly genuine.[27]

The Emperor's letter is marked by precise instructions and attention to detail, and it may be useful to list them here:

1. Eusebius is to supply 50 copies of the Holy Scriptures[28] for use in the numerous churches now being built in Constantinople.
2. The manuscripts are to be in codex form, written on parchment.
3. They are to be easy to read and transportable, written by expert and highly trained calligraphers.

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[26] Cf. A. H. M. Jones, 'Notes on the Genuineness of the Constantinian Documents in Eusebius's Life of Constantine', *Journal of Ecclesiastical History* 5 (1954), pp. 196–200 (includes, on pp. 198–9, a revised and reconstructed text of the passage).

[27] The whole question is well summarized by F. Winkelmann, 'Zur Geschichte des Authenticitätsproblems der Vita Constantini', *Klio* 40 (1962), pp. 187–243. Despite this Cavallo, op. cit., p. 61 still sounds a note of scepticism: 'Nessuna relazione quindi tra i famosi Vaticano e Sinaitico e la notizia in questione [i.e. the letter of Constantine], alia quale, d'altra parte, non si sa fino a che punto si posse prestar fede: è ben nota infatti la tendenza di Eusebio ad accentuare la "pietà" Constantiniana a detrimento, talvolta, della verità storica.'

[28] It has been suggested that these volumes may have contained only the New Testament; so T. D. Barnes, *Constantine and Eusebius* (Harvard Univ. Press, 1981), p. 125 (but on p. 222 they are called 'copies of the Bible'), or only the Four Gospels (Harry Y. Gamble, *Books and Readers in the early Church* (Yale Univ. Press, 1995), pp. 80, 158–9 and notes 43, 134). Both these suggestions are baseless. When Constantine

4. Orders have been sent to the *Rationalis*[29] (finance officer) of the Diocese, authorizing him to provide Eusebius with everything necessary for the execution of the order, and Eusebius personally is to ensure that it is completed as speedily as possible.
5. Eusebius is authorized to use the present letter to commandeer two wagons of the *Cursus Publicus* for the conveyance of the manuscripts to Constantinople, where Constantine will inspect them.
6. The convoy is to be superintended by one of Eusebius' deacons, who on arrival will be suitably rewarded.

Before we come to the reactions of Eusebius to this letter, there is one small point in the Emperor's letter which I think is worth mentioning, viz. the specification of *two* wagons of the *Cursus Publicus*.[30] The Dioecesis Orientis was a vast province extending from Cilicia and Isauria in South-East Asia Minor to Egypt and Libya, and from Mesopotamia to Cyprus. It contained two of the greatest cities of the ancient world, Alexandria and Antioch, and Constantine obviously thought that with the resources of such an area behind him, Eusebius would have no difficulty in running off the 50 bibles, which could therefore be forwarded to Constantinople in a single consignment. Because of the urgency of the order, it was obviously envisaged that the MSS would be sent by the express service, the *Cursus Velox*, for which the maximum load was 1000 (Roman) pounds, conveyed in a four-wheeled wagon called a *raeda*, drawn by a team of mules. The Roman pound was equivalent to 327.45 grammes, and the maximum load of one wagon would therefore be 327 kilogrammes. The bibles were to be accompanied by one of Eusebius' deacons, and if we allow 80 kg. for this, we are left with only 247 kg. This divided by 50 gives about 5 kg., and although I can see no means of estimating the weight of such huge volumes I am sure

specifies the 'Holy Scriptures' without qualification this must mean the entire Bible, both Old and New Testaments, as is made clear in the Festal Letter of Athanasius of A.D. 367, where he speaks of the 'Holy Scriptures' and then proceeds to list them book by book, including the whole of the Old and New Testaments. Moreover, since the manuscripts were intended for church use, and since from the earliest times church services have included readings from the Old Testament, it is obvious that complete bibles were intended here. Finally, if Constantine had wanted copies of the gospels only, or the New Testament only, he would have said so.

[29] On the Diocesan *Rationales* and the funds they controlled see A. H. M. Jones, *The Later Roman Empire* (1964), vol. 1, p. 408 and vol. 3, p. 104, n. 43.

[30] On the *Cursus Publicus* see E. J. Holmberg, *Zur Geschichte des Cursus Publicus* (Uppsala, 1933); A. H. M. Jones, op. cit., vol. 2, pp. 830–5 and Index, p. 1491.

that, with all the necessary packing, each would have weighed far more than this, which is why Constantine carefully specified *two* wagons. Here there is one further point. Why did not Constantine simply authorize Eusebius to use the *Cursus* 'as necessary'? The reason is simple. The *Cursus*, which was enormously expensive, was intended not only for the conveyance of freight, but also for the passage of persons such as provincial governors and high officials, and the Imperial commissioners, the *agentes in rebus*, travelling about the Empire. Because of its speed, safety, and reliability the system was constantly under threat of abuse by private persons who tried to obtain free transport on it through the offices of influential persons, and everything had to be done to keep this in check. It must therefore have been routine to specify exactly the number of conveyances involved. As Jones says of the *agentes in rebus*:[31] 'it was their business to see that no one used the post (i.e. the *Cursus Publicus*) without a warrant, or demanded facilities in excess of what his warrant entitled him to receive'.

I may add that it seems to me unlikely that any forger would have been able to make the calculations given above, or would even have thought of doing so!

We must now turn back to Eusebius. There can be no doubt that he received the Emperor's letter with a mixture of gratification and consternation—gratification that the order had been given to him, and consternation as he realized the magnitude of the task awaiting him. As is stated above, Constantine clearly expected all 50 manuscripts to be sent off in a single consignment. But this was manifestly impossible. How could Eusebius, at a moment's notice, recruit 50 calligraphers of the highest class,[32] provide them with enough manuscripts for them to copy, and, almost immediately, produce the vast amount of parchment which would be required? On the other hand, if he proceeded as quickly as possible with what resources he could muster, and, over a period, gradually built up the number of

[31] A. H. M. Jones, op. cit., vol. 2, p. 578.

[32] It does not appear that trained scribes of the high degree of skill required would have been available in large numbers, if we may compare the situation in Antioch later in the century. When Libanius wrote a panegyric on Strategius Musonianus, Praetorian Prefect of the East 354–358, the Prefect rounded up ten scribes to make copies for circulation, and Libanius, though appreciating the compliment, noted that other copying in the Capital came almost to a standstill (A. F. Norman, 'The Book Trade in Fourth-Century Antioch', *Journal of Roman Studies* 80 (1960), pp. 122–6).

manuscripts to the required 50, the inevitable delay might provoke an explosion of the Emperor's wrath. What he did, therefore, will be given in his own words:

> Ταῦτα μὲν οὖν βασιλεὺς διεκελεύετο, αὐτίκα δ' ἔργον ἐπηκολούθει τῷ λόγῳ, ἐν πολυτελῶς ἠσκημένοις τεύχεσιν τρισσὰ καὶ τετρασσὰ διαπεμψάντων ἡμῶν . . .

The closing words of this passage have been the subject of an extraordinary variety of interpretations, some of them so bizarre as to make one almost doubt the sanity of their proponents.[33] Thus, it has been suggested that the words meant that the bibles were written with three (like Vaticanus) or four (like Sinaiticus) columns to the page; that they were copied in quires of three or four bifolia; that they were multi-volume bibles, in three or four volumes; or—even more fantastically—that they were polyglot bibles, in three or four languages, or even 'harmonies of three or four Gospels'!

That there has been such a wide variety of proposals results from the fact that their proposers have not troubled to look at documents in which these supposedly mysterious words τρισσὰ καὶ τετρασσὰ are found. They are in fact familiar to every papyrologist because they occur in legal documents executed in a number of copies with the intention—usually literally expressed—that each party should receive a copy. I will quote two typical examples:

> P. Oxy. 1278 (A.D. 214)
> κύριον τὸ ὁμολόγημα τρισσὸν γραφὲν πρὸς τὸ ἕκαστον μέρος ἔχειν μοναχόν.
> P. Lond. 978 (A.D. 331)
> ἡ διαίρεσις κυρία τετρασσὴ γραφεῖσα ὁμότυπος πρὸς τὸ ἑκάστωι ἡμῶν εἶναι μοναχόν.

In the above examples the participle used is the aorist passive of γραφειν, meaning *written* in so many copies. But in Eusebius the participle is διαπεμψάντων (ἡμῶν), and the only possible meaning is 'we dispatched [them] in three and four copies', i.e. by threes and fours.[34] All other explanations are nonsense. And the reason *why* Eusebius mentions the fact is that it is obviously *not* what the Emperor had intended. Faced with the impossibility of copying all 50 manuscripts simultaneously, Eusebius decided to concentrate his resources on

[33] A good selection of these is given by Carl Wendel, *Zentralblatt für Bibliothekswesen* 56 (1939), pp. 165–75.

[34] This, the correct explanation, was first given by R. Devreesse, *Introduction à l'étude des manuscrits grecs*, p. 125.

producing a few manuscripts as quickly as possible, and these he sent off, accompanied, no doubt, with a letter of explanation, as proof of his readiness to execute the Emperor's orders as far as it was possible to do so.

The only problem which I can see in the foregoing is that in the papyrus examples quoted the adjectives τρισσός, etc., qualify a noun in each case (ὁμολόγημα, διαίρεσις), whereas in Eusebius there is no noun for them to qualify, while διαπεμψάντων ἡμῶν is also left without an object. Obviously what is intended is the manuscripts, τὰ σωμάτια. Either, then, we are to understand τὰ σωμάτια, or, as is possible, τὰ σωμάτια may actually have been written, since immediately after διαπεμψάντων ἡμῶν there is a lacuna in all the manuscripts, and when the text resumes it is on a different subject. None of this, however, affects the overall meaning of the passage.

Eusebius says that he sent the manuscripts ἐν πολυτελῶς ἠσκημένοις τεύχεσιν. I take these to be finely made and perhaps ornamented wooden book-boxes, one for each manuscript. This would certainly have been a very sensible precaution, affording maximum protection for the precious manuscripts during their long overland journey to Constantinople—something overlooked by Constantine's staff. Provision of the boxes would not have delayed production of the manuscripts, as it could have been put out to tender.

We now come to the crucial question: are Vaticanus or Sinaiticus, or both (since they were undoubtedly written at Caesarea in the middle of the fourth century) to be connected with the order of Constantine? Whenever this suggestion is made, it is immediately countered by the fact that Sinaiticus at least was certainly still in Caesarea two centuries later. This is true, but overlooks one crucial fact: Sinaiticus was never completed, and therefore could not in any case have been sent to Constantinople. Proof that it was not completed, and possible reasons for this, will be considered in the next section.

IV. The Abandonment of Sinaiticus and the Reduction in Format

The crucial fact in the history of Sinaiticus is that, when virtually complete, work on it was suddenly abandoned. The uncompleted manuscript could not therefore be bound up, and must have remained, a pile of loose leaves, in the scriptorium at Caesarea.

Of the fact itself there is no possible doubt. As was pointed out in *Scribes and Correctors* (pp. 7–9), the original quire numeration allows for a whole quire between the Old Testament and the New. The Old Testament concludes with Job, ending with the last leaf of quire 72. In the earlier part of the New Testament the original quire numbers have in many cases been shorn off in the course of binding, but from quire 83 onwards they are mostly intact, though often partly erased by the writer of the later, continuous numeration. Enough remains, however, to make it quite certain that the first quire of the New Testament was numbered οδ (= 74), and indeed, as we noted, traces of this can still be seen on the top edge of the first leaf of the quire in exactly the position where it would have been expected. The quires on either side of this point are in perfect condition, and it is thus impossible that quire 73 could have simply dropped out of the manuscript.

What, then, has happened? Various suggestions have been made (e.g. a simple mistake in numbering), but the only answer which we found convincing was that the quire had been intended to contain the Eusebian Canon Tables and the Letter to Carpianus which explains the system, but that for some reason these were never written.

The only objection I can see to the foregoing is that the Canon Tables and the Letter to Carpianus could not have filled a normal quire of eight leaves (= 16 pages). This is true, but it might have been a smaller quire of, say, four leaves since there are other quires of less than eight leaves in the manuscript, viz. 41 (four leaves), 90 (six leaves) and 91 (two leaves).

That the Canon Tables, though projected, were never actually written is confirmed by the fact that although the Ammonian Sections and the Canon Table references (in red, as specified by Eusebius) had been inserted in the gospel text (initially by Scribe A, up to Section 53 of Matthew, thereafter by Scribe D), the numeration in Luke breaks off at section 106, leaving sections 107–342 unnumbered. This omission by Scribe D is uncharacteristic and must be deliberate since he is the most accurate and reliable of the three scribes and seems to have exercised something of a supervisory role. It is, moreover, confirmed by the fact that in the first of the three bifolia rewritten by Scribe D to replace ones where the original scribe, Scribe A, had presumably made some exceptionally extensive error (N.T. ff. 10 + 15, containing Matt. 16:9–18:12, 24:36–26:6) the Canon Table references were *not* added, although they must have been in the original bifolium. This replacement bifolium must therefore have been written *after* the decision had been taken to omit the Canon Tables, and may thus have been part of a last desparate effort to salvage the manuscript, but in vain. Why?

I myself at one time thought[35] that the decision to use copying from dictation instead of visual copying, and the errors resultant therefrom, might have been the cause of the abandonment of the manuscript, but I now think this is unlikely, since the amount of error could only have been ascertained after lengthy collation. Moreover, the existence of the three cancel-bifolia shows that the presence of errors *had* been appreciated and, so far as possible, remedied. I think, therefore, that there must have been some much more fundamental and totally compelling reason for the abandonment of the manuscript, and for this we have to turn to Vaticanus.

If we do so, what immediately attracts attention is the remarkable difference in size between the two volumes, despite the fact that both are manuscripts of the entire Greek bible, written in the same scriptorium at approximately the same time, and possibly even sharing a scribe. Sinaiticus (in the prose books) has four columns of 48 lines to the page, while Vaticanus has three columns of 42 and a much smaller script. But it is in the overall size of the page that the difference is most marked, and needs to be investigated.

In *Scribes and Correctors* (p. 71), Milne and I gave the dimensions of the page of Sinaiticus in inches, viz. about 15 in. (38.1 cm.) for the height, and the breadth varying from 13 1/4 to nearly 14 in. (33.6–35.5 cm.). These figures, which were, of course, taken from the original, are necessarily very approximate since, as can be seen in the facsimile, there is considerable variation not only between one leaf and another, but even within the same leaf.

However, C. R. Gregory quotes some considerably larger figures. In his Prolegomena to Tischendorf's eighth edition of the New Testament, (Pars Prior (Leipzig, 1884), p. 345), he gives the dimensions at 'alt. 43 cm., lat. 37.8 cm.', adding 'Primo maiora erant folia, sed decurtata sunt.' B. M. Metzger, *Manuscripts of the Greek Bible: an Introduction to Greek Palaeography* (Oxford, 1981), p. 76, says of Sinaiticus: 'measuring *when found*, according to Gregory 16 7/8 × 14 7/8 inches (43 × 37.8 cm.), *but now*, according to Milne and Skeat, "15 × 13

[35] 'The Use of Dictation in Ancient Book-Production', *Proceedings of the British Academy* xlii (1956), pp. 196–7 (= A1 above).

1/2 in. (38.1 × 34.5 cm.)"', the words which I have italicized clearly giving the impression that after its discovery by Tischendorf (or after its acquisition by the British Museum?) the leaves were trimmed. Needless to say, nothing of the sort has occurred.[36] But where, then, did Gregory get his measurements? I think I can throw some light on this, for 43 cm. is the *exact* height of Tischendorf's great facsimile of 1862. The width of the facsimile is, however, nearer 40 cm. than 37.8. But there is a further complication. In his *Canon and Text of the New Testament* (1907), p. 333, Gregory says of Sinaiticus: 'This manuscript is in its appearance, when it is thrown open, much like a piece of an old roll. If someone could give us eighty-six centimetres of a corresponding parchment roll it would look just so.' It is obvious that 86 is 43 × 2, so here Gregory is taking 43 cm as the *width of* the leaves, which is clearly impossible. I think this is enough to show that Gregory's measurements are completely worthless and should not be quoted in any description of the manuscript.[37]

Turning back to reality, we find that the maximum area of a bifolium of Sinaiticus is approximately 15 in. × 28 in. = 38.1 cm. × 71 cm. = 2709 cm.^{2} while the area of a bifolium of Vaticanus is 27.5 cm. × 55 cm. = 1512 cm.^{2} only slightly over half that of Sinaiticus.[38] How is this great difference between two manuscripts so closely connected with each other to be explained?

We have now come to the heart of the matter. Are Sinaiticus and Vaticanus (for it must be both or neither) to be connected with the order of Constantine?

Let us begin by assuming that there is *no* connection with the order of Constantine. We should then have to assume that, at some time during the fourth century, the scriptorium at Caesarea was asked to produce, as a matter of urgency, a complete Greek bible. This was

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[36] When the manuscript reached the British Museum on 27 December 1933, it was in exactly the same state as when Tischendorf had found it in 1859, cf. the photographs in *Scribes and Correctors*, Figure I, and p. xi. If anything, the process used by Cockerell for flattening the leaves, described in detail on pp. 84–5, would have had the effect of slightly increasing the measurable dimensions.

[37] Had Gregory ever seen Sinaiticus? He would, of course, have needed to go to St Petersburg for this. But why did he not check with the detached fragment in Leipzig, which would have been freely available?

[38] These figures are for the two manuscripts in their present state, but they were no doubt originally higher in both cases, since the leaves have been trimmed in successive bindings. In the case of Sinaiticus, we estimated that at least 1/2 inch (1.27 cm.) had been trimmed from the foredge, and probably the same from head and tail as well (*Scribes and Correctors*, p. 71).

executed on a grand scale, with four columns of text on pages of exceptional size. When the manuscript was virtually complete, it was suddenly abandoned. Why? If it was because errors had been found in the text, why could not these have been corrected, as they were in fact 200 years later? Instead, a completely new manuscript, Vaticanus, was written in a much smaller format. But why was this reduction in size made when Sinaiticus had already been written on such a lavish scale? And who would have met the cost of producing the abandoned Sinaiticus?

As will have been seen, the supposition that Sinaiticus and Vaticanus are *not* connected with the order of Constantine gives rise to a number of questions to which it is very difficult to find rational answers; and if we now turn to the alternative, that the two manuscripts are so connected, it is remarkable how much more understandable the situation becomes.

There can be no doubt that Eusebius would have dearly loved to be able to supply 50 manuscripts in the magnificent format of Sinaiticus—monumental volumes fully worthy of Imperial patronage. But this proved to be impossible. Why? The clue is given by the great difference in size between Sinaiticus and Vaticanus. It must be borne in mind that at this date papyrus, and not parchment, was the normal writing material in Palestine as it was in Egypt, and although parchment was coming into use, its manufacture would have been on a comparatively small scale.[39] Moreover, parchment-making is a highly skilled and time-consuming process and such an industry cannot be expanded overnight. I do not therefore think that there can be any doubt that it was the huge format of Sinaiticus which caused its abandonment. Even if it had been completed, Sinaiticus could not have been used as one of the 50 manuscripts since Constantine obviously would have expected all 50 to be of similar size, and one so noticeably out of scale would have caused all sorts of difficulties.

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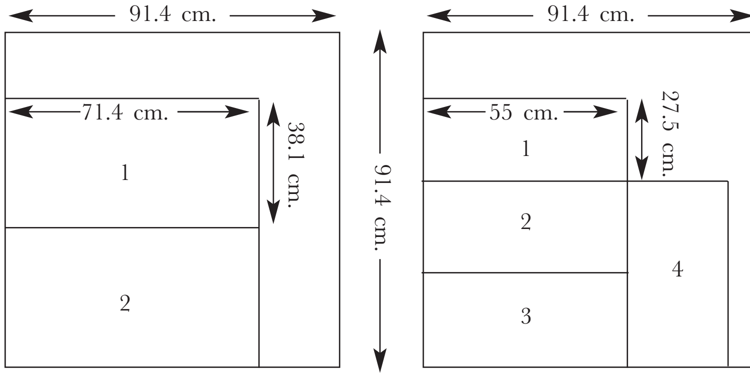
[39] Günther Zuntz, op. cit. [see note 18], p. 42 says 'In Ägypten schrieb man auf Papyrus, nicht auf Pergament' but in fact we know nothing about how the use of parchment developed, or in what localities or circumstances it was preferred to papyrus. Certainly none of the numerous writers who have claimed that both Sinaiticus and Vaticanus were written in Egypt have seen any difficulty in the fact that they were written on parchment. As regards the bibles which Athanasius had written in Alexandria for the Emperor Constans, we are not told whether they were on papyrus or parchment. If, as is suggested by Timothy D. Barnes (*Athanasius and Constantius* (Harvard Univ. Press, 1993)), pp. 39–40, the order was deliberately intended to recall that of Constantine to Eusebius, it is probable that Constans would have specified parchment, which must therefore have been available in Alexandria.



Sinaiticus Vaticanus

The economy achieved by reducing the format to that of Vaticanus can best be shown here in diagram form. As mentioned in *Scribes and Correctors* (pp. 70–1), the parchment of Sinaiticus, where it could be identified, proved to be sheepskin and goatskin, i.e. from small animals, and it is reasonable to assume that the parchment of Vaticanus is similar. According to R. Reed, *Ancient Skins, Parchment and Leather* (1972), p. 120, speaking of goatskin, 'greater cutting areas [i.e. the maximum usable areas] are possible with older animals, and pieces about 3 ft. × 3 ft. (91.4 × 91.4 cm.) can be obtained.' This can be taken as a maximum, and as shown in the diagram here reproduced, while *two* bifolia of Sinaiticus could be cut from one skin, *four* of Vaticanus could be cut, an apparent saving of 50%. The saving would not, in fact, be quite 50% since Vaticanus contains less text to the page than Sinaiticus. However, we must take into account the fact that, in the Old Testament, Vaticanus does not include 1 and 4 Maccabees, which are found in Sinaiticus, and it is also most unlikely that Vaticanus added Barnabas and Hermas after the Apocalypse. When these are taken into account, the saving on parchment becomes almost 50%.

How did Eusebius come to make such a miscalculation about the amount of parchment needed? Here we are entirely in the realm of conjecture. No doubt on receiving the Emperor's letter one of his first actions would have been to contact all known parchment manufacturers and urge them to increase production as far as possible. Possibly they over-estimated their ability to do so, and when existing stocks began to run out, a crisis would have arisen. Alternatively, or additionally, Eusebius might have been able to recruit more scribes than he had originally expected, and without a corresponding increase in the supply of parchment this also would have provoked a crisis.

Another economy, even though minimal, was the decision to omit the Eusebian Canon Tables and, consequently, the section numbers and references in the gospels. We have seen this decision actually taking place in Sinaiticus, and the total omission of the system in Vaticanus proves that the decision was final. There can be no doubt that Eusebius would have wished to include his system in the manuscripts, and the reasons which persuaded him not to do so must have been very powerful ones. The actual savings, both in parchment and in scribal time, were, as has been said, minimal—four leaves of parchment for 50 bibles would have meant a saving of only 200 leaves out of a total of something like 35,000–40,000, while the scribal time saved would only have been a matter of hours. Why, then, did Eusebius find it necessary to take this step? The answer lies, I believe, in the nature of fourth-century ecclesiastical politics, where everyone had to avoid any action which might expose him to criticism from the authorities, above all from the Emperor. Eusebius knew that he was already vulnerable because of his failure to comply literally with Constantine's order for the 50 bibles to be delivered 'as soon as possible' in a single consignment, and he must have realized that he could be accused of vanity if he included his own work to the detriment of the speediest possible execution of the Emperor's orders.

But could such a relatively trivial matter as the inclusion of the Canon Tables have been magnified into a serious accusation against Eusebius? The answer is, most certainly, yes. We have only to think of one of the accusations brought against Athanasius, that agents of his had attacked a Melitian church in a village near Lake Mariut, breaking a chalice, to see how such trivialities could be exploited by enemies. Although Constantine himself heard the charge against Athanasius and dismissed it, the 'chalice incident' continued to be raised again and again, even 20 years later. Thus Eusebius may well have decided that he could not afford to take even the smallest unnecessary risk. Here, once again, we have a situation which is only comprehensible in the context of the order of Constantine. I may add that Zuntz, op. cit. (see note 18), p. 45 n. 137, makes the excellent point that such vacillation regarding the inclusion of the Eusebian apparatus can only have taken place in Caesarea itself.

There is one special feature of Vaticanus which it may be convenient to notice here. It is usually stated that the column of writing contains 42 lines, and this is true of the greater part of the manuscript. However, although the total height of the column remains the same throughout, up to and including p. 334 there are in fact 44 lines to the column. Then, on pp. 335–534 inclusive, there are 42, with a further drop to 40 on pp. 535–54. Finally, at p. 555, the figure reverts to 42 and remains so for the rest of the manuscript. What caused these variations we can only conjecture. It seems to me possible that since Constantine had specified that the manuscripts must be 'easy to read', some of those with 44 lines sent to Constantinople in earlier consignments might have been criticized as failing to meet this requirement because there was insufficient space between the lines of writing. How could this be remedied? Apparently it was decided that since so much of the manuscript had already been written, it was not practicable to alter the total written area of the page (*Schriftspiegel*), and the only alternative was to reduce the number of lines in the column, hence the reduction from 44, first to 42 then to 40. At this point it may have been realized that a 10% drop in the number of lines in the column would entail a 10% drop in the contents of the page as a whole, and consequently a 10% *increase* in the amount of parchment used. If, as I have suggested, supply of parchment was a major problem, this may have motivated the final decision to settle for the compromise figure of 42 lines.[40] Here, yet again, we find something which it would be very difficult to explain in the context of a single manuscript unconnected with the order of Constantine.

Before we leave the order of Constantine, there is one final point which must be made. Manuscripts of the entire Greek bible are extremely rare at any period, and the same applies to complete Latin bibles—Pandects. I will quote two examples here. The Abbot Anastasius in the Nitrian Desert *habebat codicem in pergamenis valde optime scriptum, qui decem et octo valebat solidis. Totum enim Vetus et Novum Testamentum scriptum habebat* (Migne, *Patrologia Graeca*, lxxvii, col. 797). There is a similar story in the *Apophthegmata Patrum* (Migne, *Patrologia Graeca*, lxv, 1451), of an Abbot Gelasius, who also owned a complete bible on parchment, valued at 18 solidi—perhaps the same manuscript, around which various stories had accumulated. Secondly, according to H. Delehaye, *Synaxarium Ecclesiae Constantinopolitanae* (Propylaeum ad

[40] In Sinaiticus there are some variations in the number of lines in the column from the standard 48. Details of these variations, and the reasons for them, are given in *Scribes and Correctors*, p. 76, n. 5 and p. 77, n. 6.

Acta Sanctorum Novembris, Bruxelles (1902), col. 139; cf. also Zuntz, op. cit. (see note 18, p. 11, n. 11) the Church in Nicomedia possessed a complete bible, written (like Vaticanus) with three columns to the page, which, it was claimed, had been written and bequeathed to the church by Lucian of Antioch, who was martyred in Nicomedia in 312. No doubt the church did possess a very ancient manuscript of this kind which it would be natural to connect with their local hero. The important point here is that in all these instances a complete bible was regarded as something wholly exceptional. Even manuscripts containing the entire New Testament are by no means common—there are only about 60, even counting in the old uncials, and even some of these have Revelation in a different hand and thus presumably an addition. Duplacy has calculated that the total number of New Testament manuscripts written in the course of the fourth century was probably in the region of 1500–2000.[41] Obviously the number of complete Greek bibles produced during the same period must have been very much smaller than this. If it was, say, 100, inclusive of the 50 ordered by Constantine, then, statistically, Sinaiticus and Vaticanus each have a 50–50 chance of being one of the Constantinian bibles; and when we look further and find that both these manuscripts were undoubtedly written in Caesarea, the case for identifying them with Constantine's order becomes overwhelming *quite apart from any other arguments*.

We now return to Vaticanus. When completed, the manuscript must have been bound, and finally, ensconced in its τεῦχος, and with two or three sister manuscripts, loaded on to one of the wagons of the *Cursus Velox* for the long journey to Constantinople. Its subsequent history will be considered in Section VI.

V. The Later History of Sinaiticus

We left Sinaiticus lying forlornly, a heap of loose sheets, in the scriptorium at Caesarea. There it remained for some 200 years. This neglect is itself a remarkable fact, illustrating as it does the small

[41] J. Duplacy, 'Histoire des manuscrits et histoire du texte du Nouveau Testament', *New Testament Studies* 12 (1965), p. 127. I am sure that Duplacy's 'manuscrits néotestamentaires' means copies of various sections of the New Testament—Gospels, Epistles, etc., and not complete New Testaments.

demand that there was for complete bibles. It is also remarkable that, with its hundreds of enormous pages of high quality parchment, it escaped the fate of so many disused manuscripts, the attention of the palimpsester.

What finally happened after the manuscript somehow came to light is described in detail by Kirsopp Lake in his introductions to the two volumes of the facsimile and needs only to be summarized here. First, the manuscript, presumably still unbound, was very carefully collated throughout (except for the Epistle of Barnabas) by the corrector known as C^{a}, who not only corrected the thousands of errors made by the original scribes but also made a number of textual alterations intended to bring the manuscript more into line with the type of text then currently in use in Caesarea. C^{a} was followed by another corrector, known as C^{Pamph}, who has inserted two notes, one at the end of 2 Esdras and the other at the end of Esther, stating that the books from 1 Kings to Esther (viz. 1–4 Kings, 1 and 2 Chronicles, 1 and 2 Esdras and Esther) had been collated with a very ancient manuscript which itself had been collated with the original Hexapla of Origen by the martyrs Pamphilus (d. 310) and Antoninus (d. 311) whilst in prison. This revered manuscript may be assumed to have been preserved in Caesarea, and indicates that Sinaiticus was still in the scriptorium there.

The corrector C^{a} provides a further link with Caesarea, for his text agrees very closely in the Epistles with that of a manuscript known as H^{Paul}, which at the end of the Pauline Epistles has a long colophon beginning with the name of Evagrius and ending with a statement that the manuscript had been collated with a copy in the library at Caesarea which was in the autograph of Pamphilus. As Lake says: 'Considering the close textual relationship between cod. H^{Paul} and the corrector C^{a} of the Codex Sinaiticus, it is legitimate to regard this evidence as increasing the probability that during the time the corrector C^{a} was working the Codex Sinaiticus was in the library at Caesarea, in which there were certainly many MSS of Pamphilus, rather than some other library to which a MS of Pamphilus might have been brought.'

Thereafter several other correctors made varying contributions, though whether in Caesarea or elsewhere we cannot tell. Finally, the manuscript must at long last have been bound up: this would be the 'first binding' identified by Cockerell (*Scribes and Correctors*, p. 82).

As regards the date of all this activity, Kirsopp Lake quotes several opinions: 'The latest date suggested is the seventh century, the earliest is the fifth. Sir Frederic Kenyon and Professor Hunt agree in regarding the sixth century as possible, but the former is inclined to accept the seventh as equally possible, while the latter is more disposed to prefer an earlier date.' To this I would only add that the sixth century seems more probable than the seventh: the Persians occupied Palestine from 614 to 629, and after they left the Arab attacks began and they captured Caesarea in 638.

Finally, I do not recollect having seen any discussion of the reason *why* this impressive programme of restoration was so belatedly executed. It can hardly have been for the use of the manuscript in Caesarea, since in that case one might wonder why there was a delay of 200 years in carrying it out. It therefore seems to me much more likely that it was to render the manuscript serviceable in some place *other* than Caesarea to which it was to be sent; and if so, the obvious place would be the Sinai monastery which was to be its eventual home. The monastery was founded by Justinian about the middle of the sixth century, and if one were to use one's imagination one might think of him following in the footsteps of Constantine and ordering the Bishop of Caesarea to supply a copy of the Bible for his foundation. But this is only speculation, and, as Kirsopp Lake says, it may have been years or even centuries before the ill-starred manuscript was to find what might have been expected to be a safe haven in Sinai: sadly, the reality was to prove far otherwise (cf. *Scribes and Correctors*, 'Partial destruction of the manuscript', pp. 81–2).

VI. The Later History of Vaticanus

We may presume that Vaticanus reached Constantinople in safety and, when all 50 manuscripts had been assembled, would have been inspected by Constantine. Thereafter, nothing is known about its history until the fifteenth century. At some time during this immense period the lettering had become faded and difficult to read, and the entire manuscript was therefore traced over to improve its legibility. This was done with great care, but, as already noted, it inevitably affects the style of the original script, as can be seen in the specimen illustrated (Plate 2). When this restoration was carried out is unknown. Possibly it was done before the ninth century, in the course of which the introduction of lectionaries and other service-books rendered these huge bibles obsolete. Thereafter a period of increasing neglect and deterioration followed. The binding collapsed, with the loss of the boards, and leaves were lost at both beginning and end, while a whole gathering fell out from the middle of Psalms. It was in this sorry state that the manuscript finally came to light and was hastily renovated, the missing portions of Genesis, Psalms, and from the middle of Hebrews onwards being replaced in a fifteenth-century hand making no attempt to match the original.

In 1984 I published a short article entitled 'The Codex Vaticanus in the Fifteenth Century' in *JTS* 35 (1984), pp. 454–65 (= chapter B3 above). As I explained, one of my objects was to draw attention to an article by Father Janko Šagi, S.J., who had conceived the ingenious idea of trying to discover more about the history of Vaticanus by identifying (*a*) manuscripts which had been copied from Vaticanus and (*b*) manuscripts from which the supplementary portions of Vaticanus had been copied. The first part, 'Transcriptiones e codice B', dealt with the claim that in Codex Venetus Marc. Gr. 6 (no. 122 in the list of Greek Old Testament MSS) the books of Esther, Sirach, Judith and Tobit had been copied from Vaticanus. This was examined in the case of Sirach with the aid of Ziegler's 1962 edition, and was shown to be correct. The remaining books were not examined because although it is known that 122 was owned by Bessarion and was probably written for him, there is nothing to prove that he ever owned Vaticanus or whether the transcription was done before or after the manuscript entered the Papal Library. Šagi's alternative plan, of trying to identify the manuscripts from which the supplementary portions of Vaticanus were transcribed, likewise proved fruitless, since although the source of the restored portion of Genesis can be identified as Chisianus R VI 38 (19 in the list of Old Testament MSS), nothing whatever is known about the early history of this manuscript.

Since Šagi's article no further progress seems to have been made in this direction, to judge from what is said in the Introduction to the new facsimile of Vaticanus. There is obviously some sort of connection with Bessarion, but its nature remains uncertain.

Šagi also mentions that the writer of the supplementary portions of Vaticanus cannot be identified with any known fifteenth-century Greek scribe. This statement is said to be based on unpublished researches by Canart, who bad utilized the Bodleian collection of photographs of the work of fifteenth-century Byzantine scribes.

After thus summarizing Šagi's article, I attempted to describe in detail the manner in which Vaticanus was renovated in the fifteenth-century, which I suggested showed signs of haste and changes of plan, particularly the failure to copy the Pastoral Epistles after Hebrews, and finally propounded my own suggestion, viz. that the manuscript might have been brought to Italy by the Greek delegation to the Reunion Council of Florence in 1438–9 as a gift for the Pope. This seemed to me possible since it was known that the Greeks had ransacked libraries and monasteries for patristic manuscripts which might aid them in their theological disputes with the Latins.

After I had myself searched all through the *Acta* of the Council and the narrative of Syropoulos without result, I wrote to the late Father Joseph Gill, the great authority on the Council, who assured me in the most positive manner that the Greeks had made no gifts of books during the course of the Council; he was also very sceptical of my suggestion that Vaticanus might have been brought to Italy from Constantinople as a gift to the Pope.

Having received these statements from such an authority I felt bound to record them in my article, but still felt it worth-while to put forward my own suggestion about Vaticanus. It was fortunate that I did so, because later I discovered to my astonishment that I could have found exactly what I was looking for in Gill's own book, *The Council of Florence* (Cambridge, 1959), pp. 163–4, where he mentions a letter of the Italian humanist, Ambrogio Traversari, which had been published by Mercati in an article, 'Ultimi contributi alla storia degli umanisti. Fasc. 1: Traversariana', in *Studi e Testi* 90 (1939), pp. 24–6.[42] In this letter, written in Florence between 11 March and 7 April 1438, Traversari describes to a friend how he had had an interview with the Greek Emperor, and had seen some of the manuscripts which he had brought with him, and also refers to other manuscripts brought by the Greeks. Stating that he is repeating what he had said in an earlier letter, he goes on:

> Tria me volumina vidisse apud Graecorum Imperatorem significabam praestantissima: Platonis unum, ubi omnia ipsius venustissime scripta haberentur; Plutarchi potius molem quam volumen, in quo itidem omnia ipsius haberentur; et Aristotelis non aeque pulchrum, ubi in omnia ipsius opera notiora . . . comentum habebatur. Pollicitus est

[42] Cf. also Ihor Ševčenko, 'Intellectual Repercussions of the Council of Florence', *Church History* 24 (1955), pp. 291–323.

> Imperator ipse cuncta quae attulit in conspectum daturum . . . Multa se invenisse mirabili studio et diligentia sedula ille gloriatur, et Diodori g[rande?] volumen et Dionysii Alicarnassei et plurimorum quae in ocio nobis aperiet . . . Cum Niceno Archiepiscopo [= Bessarion] singularis eruditionis ac meriti viro magna mihi familiaritas est . . . pauca secum detulisse deprehendi sed magnam librorum molem Mothone [Methone or Modon, the Venetian stronghold on the coast of the Morea, where the Greek ships had put in during their voyage to Italy] reliquisse. Perrexi tamen inquirere, et Strabonis duo maxima volumina se illic reliquisse professus est . . . Adducor tamen in spem ea convehenda . . . Cyrilli magnum volumen contra Iulianum Apostatam habet quod et transcribendum curabimus si membranas invenire poterimus. XV, ni fallor, libris opus illud absolvitur. Mathematica plura apud illum offendi, Euclidem et Ptholomeum manu sua scriptum cum figuris aptissimis . . . Euclidis tria opuscula praeter consuetum et commune illud de Geometria opus, Apud Ephesinum [= Marcus Eugenicus, the greatest opponent of the Union] aeque eruditum plura offendi . . .

It is certain that in bringing all these manuscripts the Greeks had some other motive than an altruistic desire to foster classical studies in Italy, and this must have been to distribute them as gifts in the expectation that they would receive something in return. As Gregory says:[43] We know from the Bible that in the East a gift demands a return, and that this return may under given circumstances be extraordinarily like a good round price for the nominal gift.' To the Greeks, whose financial stringencies were a constant theme of the Council, such 'gifts' must have provided a most valuable opportunity to replenish their resources. Since they were being regularly supported by papal funds, a gift from them to the Pope was an absolute necessity. And what better gift could there be than a manuscript of the complete Greek bible, obviously of the greatest antiquity, specially repaired and bound up for presentation to him?[44]

Such was the situation in which my own suggestion was put forward. 'La supposition est séduisante, mais manque de base objective' is the comment on it in the introduction to the new facsimile

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[43] *Canon and Text of the New Testament* (1907), p. 331.

[44] Throughout the ages copies of the Holy Scriptures have been favoured gifts exchanged between potentates and Church leaders, and very many examples could be quoted. At the historic meeting between Pope Paul VI and the Archbishop of Canterbury in Rome on 23 March 1966 the Pope presented Dr Ramsey with a copy of the facsimile of the New Testament portion of the Codex Vaticanus which had been produced in 1965 for distribution to bishops attending the Second Vatican Council. This volume, suitably inscribed, is in Lambeth Palace Library.

of Vaticanus, and this is, of course, true. There is, however, one further point which should be borne in mind. In my article I referred to the possibility that Vaticanus might have been one of the 50 bibles ordered by Constantine, remarking that this could only be a suggestion, since 'even if it was in Constantinople in 1438[45] it would not follow that it was there 1100 years earlier.' Now, however, we know that it *was* in Constantinople 1100 years earlier, and to that extent my theory is, I think, strengthened.

In any case, whether or not Vaticanus was one of the manuscripts which the Greeks carried with them when they set sail from Constantinople on 27 November 1437, there can be no possible doubt that it was the transfer, by whatever route, from Constantinople to the Vatican, from New Rome to Old Rome, which saved it from total destruction. I think that Constantine would have approved.

Plate 1

This shows the colophon designs at the end of Mark in Sinaiticus (by Scribe D) and at the end of Deuteronomy in Vaticanus (by Hand A), to demonstrate their remarkable resemblance. This resemblance is further illustrated by showing the vertical member of the Mark colophon alongside the corresponding vertical member in Vaticanus. As will be seen, the two designs are virtually identical. Note also that what in *Scribes and Correctors* (pp. 28, 87–8), is called the 'running spiral' design is here used in both manuscripts to fill up the last uncompleted line of text.

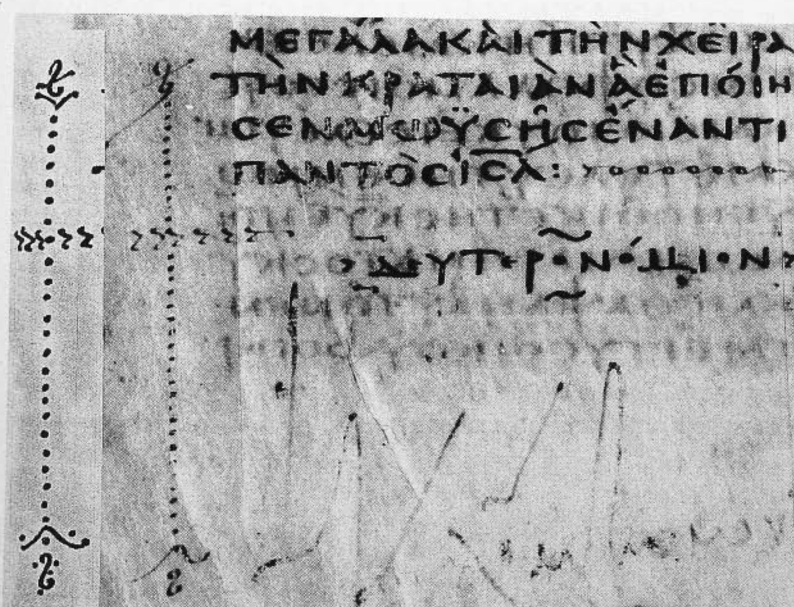
Plate 2

This shows one of the very few places in Vaticanus where the original script can be seen (p. 1479, col. II, lines 32–9; the text is 2 Corinthians 3:15–16). As explained in the text, the scribe had accidentally written a passage twice over, and the restorer of the writing had inked over only the repetition. The illustration, here enlarged, shows that the horizontal strokes had faded away, giving a ghostly effect, while the tracing over has completely altered the character of the script so that comparison with Sinaiticus is not possible. Below is shown the same specimen of Vaticanus, but with the missing hor-

[45] The date should have been 1437.

izontal lines drawn in, various external marks such as the brackets indicating omission removed, and the whole reduced to the original size of the MS. This is as far as we can get to seeing the original script of Vaticanus, and resemblances to individual traits of Sinaiticus can be picked out, e.g. the long tail of *rho*, impinging on the line below, which is characteristic of Scribe D. Nevertheless there is one marked difference. In Sinaiticus, in the case of all three scribes, the letters are tightly packed, sometimes actually touching or, if *tau*, or *upsilon* are involved, even overlapping. In this respect Vaticanus differs radically, letters being carefully separated throughout. In the right-hand margin can be seen one of the section numbers discussed above.


CTACIC·KAIOY
ΔΕΝΙΟΥΔΕΝΕΙ
ΠΟΝΕΦΟΒΟΥΝ
ΤΟΓΑΡ:
ΕΥΑΓΓΕ
ΛΙΟΝ


ΜΕΓΑΛΑΚΑΙΤΗΝΧΕΙΡΑ
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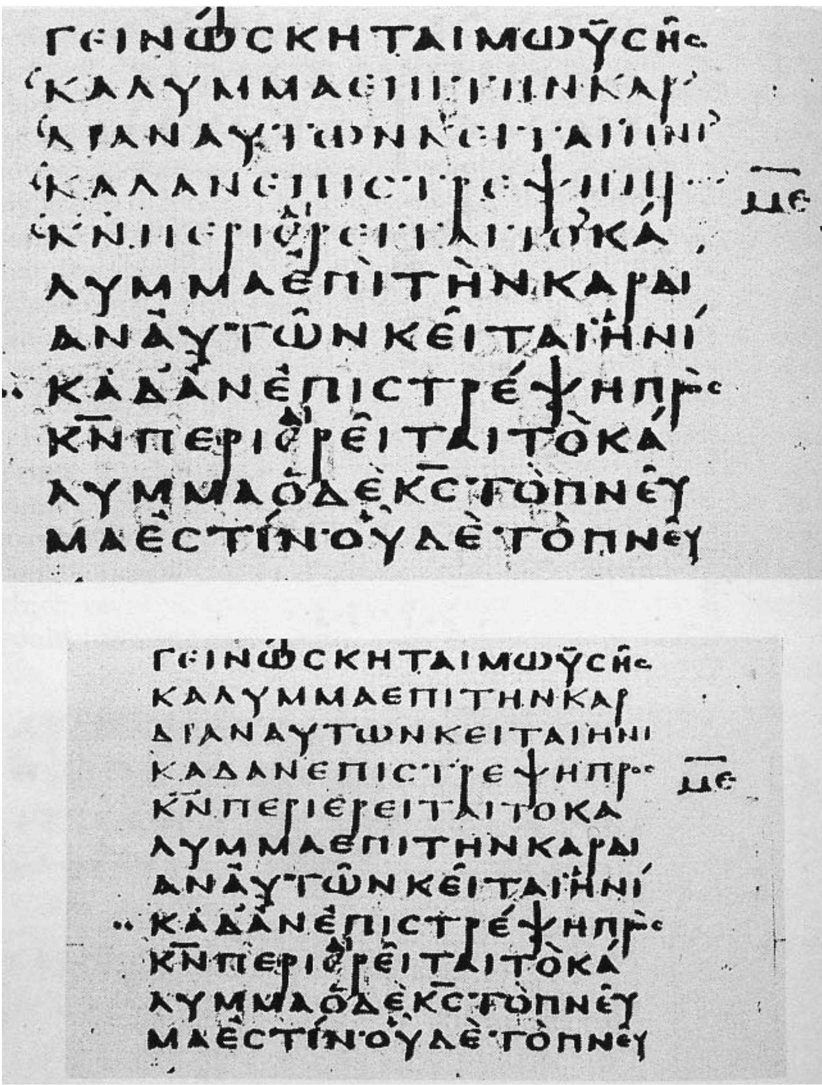

8

THE LAST CHAPTER IN THE HISTORY OF THE CODEX SINAITICUS

In 1975 an extraordinary discovery was made in the Monastery of St. Catherine on Mount Sinai. In a recess in the wall of the Monastery, the very existence of which was unknown to the present-day community, a vast quantity of manuscripts and fragments of manuscripts was discovered, full details of which are now being published.[1] The roof of the cavity had collapsed, showering its contents with dust, earth and stones, so that clearing the area and retrieving the manuscripts proved very arduous, and it was only after 44 days of very hard labour that the Sacrist of the Monastery, the late Archimandrite Sophronios, who worked almost single-handed, completed the task. The volume describing the material in Greek has now appeared, and forms the basis of the present note.

The Report states (p. 20) that it would be unwise to speculate on the origins of this collection of material pending the discovery of written records, but in the meantime it may be useful to put together such evidence as is at present available and to see what conclusions can be drawn from it.

In May 1844 the Biblical scholar Constantine Tischendorf visited the Monastery, where he remained for eight days. He was to make further visits in 1853 and 1859. According to his account,[2] it was one day in 1844, while he was working in the Library of the Monastery, that he noticed on the floor a large basket filled with manuscript fragments. He asked if he might examine them, and the Librarian, Cyril, gave his permission, saying that they were rubbish which was to be destroyed by burning it in the ovens of the Monastery, adding that two similar basketfuls had already been so disposed of. Among these fragments Tischendorf found 129 leaves in Greek which he identified as coming from a manuscript of the Old Testament and which, to judge from the appearance of the script, could not

[1] Τὰ Νέα Εὑρήματα τοῦ Σινᾶ (Athens, 1998).
[2] C. von Tischendorf, *Die Sinaibibel* (Leipzig, 1871), pp. 3–4.

be later than the fourth century, and thus the earliest Biblical manuscript he had ever seen. As the leaves were destined for destruction, he asked if he might keep them, but at this point the attitude of the Librarian changed, evidently because he realised that they might be of value, and eventually Tischendorf was permitted to take only one-third of the whole—43 leaves. These he took back to Germany and later published.

Tischendorf must have realised at the time that somewhere in the Monastery there was presumably a large collection of such, to the monks, useless material, which was being transported, basketful by basketful, to the ovens for destruction. Whether he asked to see this collection, and was refused, or whether he decided, diplomatically, to be contented with the prize he had so unexpectedly secured, we do not know, for he tells us no more. What he did do, however, was to prove of the utmost importance, for he strongly advised the monks to search for more leaves of the manuscript and preserve them carefully. Of course, at this time all the leaves he had seen were from various historical and prophetic books of the Old Testament, and he could have had no idea that any part of the New Testament was included in the manuscript.

We now know that the monks did indeed follow Tischendorf's advice, with spectacular results, recovering not only much more of the Old Testament but the New Testament absolutely complete, together with the Epistle of Barnabas and the early part of the "Shepherd" of Hermas. All this the monks then attempted to bind up, and this is the "second binding" described by Cockerell.[3] The monks got as far as sewing the leaves into quires, and then sewing the quires together. They then attached to the back two broad bands which were evidently intended to be attached to the binding boards. By this stage, however, the volume had become very out of shape. As Cockerell describes it, "While the fore-edge is roughly square, the spine is badly out of shape. When the spine is straightened up, as in the new binding, the fore-edge naturally becomes irregular. It is quite possible that this later binding was never actually completed. The sewing threads were deliberately cut from the bands, perhaps with a view to a fresh start." However, by this time the monks seem

[3] H. J. M. Milne and T. C. Skeat, *Scribes and Correctors of the Codex Sinaiticus* (London, British Museum, 1938), p. 83. For the portions of the MS. destroyed before the intervention of Tischendorf see *op. cit.*, pp. 1–6, 81–2.

to have realised that their primary objective, of securing the leaves against further loss, had been obtained, and they took no further action. It was in this state that Tischendorf saw the manuscript on his final visit to Sinai in 1859.

Among the great mass of fragments found in 1975 were twelve complete leaves of the Codex Sinaiticus, together with some fragments. Either, then, as is quite possible, the monks missed these during their search in the 1840s through the vast mass of fragments, or, having found them, they decided not to include them in the binding because they were only stray fragments providing no continuous text.

This is not the place to pass judgments, but perhaps I may say that, as it seems to me, both the monks and Tischendorf deserve our deepest gratitude, Tischendorf for having alerted the monks to the importance of the manuscript, and the monks for having undertaken the daunting task of searching through the vast mass of material with such spectacular results, and then doing everything in their power to safeguard the manuscript against further loss. If we accept the statement of Uspensky, that he saw the codex in 1845, the monks must have worked very hard to complete their search and bind up the results in so short a period.

C

TEXTUAL VARIANTS

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1

THE LILIES OF THE FIELD

"Consider the lilies of the field, how they grow . . ." There is no need to emphasize here the familiarity of these words to every ear, or the abiding power of their simple and exquisite imagery. To dissect such a passage with the scalpel of textual criticism seems at first sight to verge on sacrilege; and yet these apparently straightforward words do indeed set a scientific problem, on which some new evidence has recently come to light.

The examination of the Codex Sinaiticus lately carried out in the British Museum involved the scrutiny of every correction recorded by Tischendorf; this included passages where the scribe of the manuscript was writing over an erasure, and in such cases an attempt was always made, as a matter of routine, to identify the erased text. As a rule this was easy enough, for in nearly every instance it turned out that the scribe, or the reader dictating to him, had made some quite obvious blunder, devoid of textual significance. There remained, nevertheless, a few places where, for one reason or another, the original text eluded us; such a passage was Mt 6:28, where the manuscript now reads:

ΤΑΚΡΙΝΑΤΟΥ
ΑΓΡΟΥΠΩΣΑΥΞΑ
ΝΟΥΣΙΝΟΥΚΟΠΙ
ΩΣΙΝΟΥΔΕΝΗ >
ΘΟΥΣΙΝ.

The words αὐξάνουσιν to νήθουσιν are written over an erasure, and the natural conclusion was that the scribe had either transposed κοπιῶσιν and νήθουσιν, or had substituted the singular for the plural number. But in spite of every effort, the traces of earlier writing could not be made to fit either hypothesis, and it looked as though the mystery would have to be left unsolved.

Some months later, however, while glancing at the fragments of an uncanonical Gospel from Oxyrhynchus (P. Oxy. iv. 655), the following passage attracted my attention. As printed by the editors, ll. 7–10 of the papyrus run:

[ση]σθε. [πολ]λῷ κρεί[σ-
[σον]ές [ἐστε] τῶν [κρί-
νων ἅτι[να α]ὐξά-
νει οὐδὲ ν[ήθ]ει.[.

As I read this, it struck me that ἅτινα αὐξάνει οὐδὲ νήθει is really intolerable Greek for "which grow but spin not" (Grenfell and Hunt's own translation). οὐδέ implies a preceding negative, or negative idea at least, whereas here αὐξάνει is strongly positive. Even if the sentence be syntactically correct, the weakness of the phraseology when compared with Matthew cannot be denied. But is αὐξάνει in fact an inevitable reading? Examination of the facsimile (the original is now in Harvard University) disclosed that the second α of αὐξάνει is a mere speck on the broken edge of the papyrus, and might well have been followed by another letter or two. From this it was but a step to conjecture that the original reading was ἅτι[να ο]ὐ ξα[ί]νει οὐδὲ ν[ήθ]ει, "which neither card nor spin", giving an excellent sense without doing violence to the remnants of writing visible.

There now seemed to be an outside chance that the papyrus might in some way help to solve the problem in the Sinaiticus. So far were expectations exceeded, indeed, that, with the assistance of the ultraviolet lamp, every letter of the original (erased) writing was eventually deciphered as follows:

ΤΑΚΡΙΝΑΤΟΥ
ΑΓΡΟΥΠΩΣΟΥΞΕ
ΝΟΥΣΙΝΟΥΔΕΝΗ
ΘΟΥΣΙΝΟΥΔΕΚΟΠΙ
ΩΣΙΝ

This seemed to indicate that οὐ ξαίνει in the Oxyrhynchus papyrus was something more than a mere conjecture. Yet how could there be any textual connection between two documents so different in every way? Could οὐ ξαίνειν in each case be an independent corruption of αὐξάνειν? This seemed hardly likely, for the phonetic similarity is by no means strong. On the other hand, it seemed still less likely that οὐ ξαίνειν was the primitive reading, universally corrupted to αὐξάνειν. In fact, a first glance at the critical editions of the NT appeared to shew αὐξάνειν in an impregnable position. Every Greek manuscript, every version, every Patristic quotation, is on its side, while against it we have the solitary witness of S* (= Sinaiticus).

But this is Matthew. When we turn to the Lukan version (Lc

12:27) we find a very different position, for here the authorities are sharply divided, as follows:—

πῶς αὐξάνει οὐ κοπιᾷ οὐδὲ νήθει	SAB plur. *f q* vg. sah. boh. arm. etc., syr. pesch. W-H txt.
πῶς οὔτε νήθει οὔτε ὑφαίνει.	Marcion Clement D *a d* syr. sin. cur. Tisch txt Soden txt.

quomodo crescunt (non crescunt [*sic*] *i*) non laborant neque (non *ff*²) neunt neque texunt *b ff*² *i l r*.

quomodo crescunt et florescunt neque laborant neque neunt *e*.

It is clear enough that πῶς οὔτε νήθει οὔτε ὑφαίνει is the original text of Luke, and that the common reading of the Greek manuscripts, both 'Neutral' and other, results from assimilation to Matthew. This assimilation, too, certainly took place at a very early date, since so many of the Old Latin manuscripts shew a conflation of the original and assimilated readings. But in deciding between the claims of οὐ ξαίνει and αὐξάνει it does not much help to learn that the original text of Luke included neither, though this does suggest that all is not well with αὐξάνει in Matthew.

We are thus thrown back on the internal evidence, the propriety of the words themselves. οὐ ξαίνει gives impeccable sense. αὐξάνει, on the other hand, if not actually redundant, adds very little. We expect to be asked, not to reflect on the growth of the lilies, but on the appearance of the lilies themselves. That this is a valid criticism can be seen by comparing the parallel 'logion' of the 'fowls of the air'. Here we find ἐμβλέψατε εἰς τὰ πετεινὰ τοῦ οὐρανοῦ (Mt 6:26), or κατανοήσατε τοὺς κόρακας (Lc 12:24), but not, for example, κατανοήσατε τοὺς κόρακας πῶς πέτονται. The unsuitability of αὐξάνει was clearly felt by the scribe of *e*, who added *et florescunt* after *crescunt*, in an effort to bring the subject back to the appearance of the lilies. Moreover, if αὐξάνει be the true reading, the divergence between Matthew and Luke remains inexplicable—though of course it does not necessarily follow from this that αὐξάνει is wrong.

Let us now test the reverse hypothesis, and assume that οὐ ξαίνει is the original reading. Corruption of οὐ ξαίνει to αὐξάνει would leave οὐδὲ νήθει in the air, exposed to the very objections which have been made against Grenfell and Hunt's restoration of P. Oxy. iv. 655. The balance of the sentence could only be restored by inserting a negative verb; hence οὐ κοπιᾷ οὐδὲ νήθει in Matthew, οὔτε νήθει οὔτε ὑφαίνει in Luke, οὐ . . . νήθει being part of the original reading and therefore the constant factor. A small point in favour of this view is the vagueness of κοπιᾷ, natural enough in a stop-gap. As for Luke, in dropping αὐξάνει altogether he may well have been influenced by the same considerations which have been urged against it above.

If this reconstruction is correct, the corruption of οὐ ξαίνει to αὐξάνει took place at a very early date, before either Matthew or Luke were written—in other words, it belongs to the textual history of "Q". Preservation of οὐ ξαίνει in such a work as P. Oxy. iv. 655, which shews every mark of primitiveness, is not surprising; but we should certainly not expect to find it in any manuscript of the Gospels, and its presence in the Sinaiticus is indeed remarkable. Whatever the explanation, it is clearly no scribal aberration, but the result of an acute piece of textual criticism. Either it was inserted on the order of an editor who had found it in some uncanonical Sayings-collection like P. Oxy. iv.655, or, as I am more inclined to think, it was a brilliant conjectural emendation, perhaps by the same daring critic who decreed the excision of John 21:25.

2

ΑΡΤΟΝ ΦΑΓΕΙΝ: A NOTE ON MARK 3:20–21

In Mark 3:14–19 we are given a list of the Twelve Apostles who, we are told, had been chosen by Jesus to be his companions (ἵνα ὦϲιν μετ' αὐτοῦ) and to be sent out on evangelising missions, during which they would also have power to cast out evil spirits. There can be no doubt that attendance on Jesus was their primary function, since although one mission by them is rather perfunctorily described, apart from this they appear as constantly in attendance on Jesus, right up to the final scene in Gethsemane.

Immediately after this we are told (verses 20–21 in Nestle-Aland's 27th edition): καὶ ἔρχεται εἰϲ οἶκον· καὶ ϲυνέρχεται πάλιν ὁ ὄχλοϲ ὥϲτε μὴ δύναϲθαι αὐτοὺϲ μηδὲ ἄρτον φαγεῖν. καὶ ἀκούϲαντεϲ οἱ παρ' αὐτοῦ ἐξῆλθον κρατῆϲαι αὐτόν· ἔλεγον γὰρ ὅτι ἐξέϲτη.

Problems arise right at the outset, because although καὶ ἔρχεται εἰϲ οἶκον stands at the beginning of verse 20 in Nestle-Aland[27] and also in UBS[4] (so also in Westcott and Hort, and hence R(evised) V(ersion), in Textus Receptus, and hence A(uthorised) V(ersion) and also R(evised) E(nglish) B(ible), the Preface to which states that the verse-division of A.V. has been followed), these words come at the *end* of verse 19: and there is a further complication in the text itself, for instead of the singular ἔρχεται there is widespread attestation for the plural ἔρχονται (S^2 ACL f 13 M(ajority) T(ext) lat syr^{ph}) or εἰϲέρχονται (D). Thus, A.V., for instance, has "and *they* went into an house." This is clearly wrong, since it implies that the Twelve Apostles, who had just been listed, were accompanying Jesus, although it is clear from the sequel that Jesus was alone, and therefore had to be rescued by his family and friends (οἱ παρ' αὐτοῦ). The conclusion is inescapable, that the Apostles were *not* there when most needed because they had *not* yet been appointed—indeed, it may well have been the riot here described which prompted their recruitment.

Further difficulty arises with the words καὶ ἔρχεται εἰϲ οἰκον, translated "And he cometh into a house" (R.V.) or "he entered a house" (R.E.B.). Both, I believe, are wrong. οἶκοϲ certainly means "house", but it also has the specific meaning of "home." Thus, the adverb οἴκοι means "at home", οἴκαδε means "homewards", etc. The modern Greek cπίτι has the same meaning of "house" and "home." In any case, if Jesus had got into a house, why was he in such danger from the mob? My suggestion is that the words mean simply that Jesus "came home," i.e., to Nazareth, which he himself described as his πατρίc, as also did the local population (Matthew 13:55–57).[1]

We now come to the crucial phrase telling us that such a mob collected ὥcτε μὴ δύναcθαι αὐτοὺc μηδὲ ἄρτον φαγεῖν. The words ἄρτον φαγεῖν must surely be corrupt, for they make no conceivable sense. Why should the mob have wanted to *eat*, let alone eat *bread*? They were not out in the wilderness, but their own home town; and in any case they did not want to eat—they wanted to see Jesus, to get near him, to touch him, since it had been rumoured that even the touch of his garments could confer healing.

It is my belief that the original was not ὥcτε μὴ δύναcθαι αὐτοὺc μηδὲ ἄρτον φαγεῖν, but ὥcτε μὴ δύναcθαι αὐτὸν μηδὲ φανῆναι, i.e., that Jesus could not even be seen—he had disappeared into the crowd. Everyone knows how dangerous it is to be caught up in the middle of a large and excited crowd, out of control, and the danger was fully appreciated at the time; cf. Luke 12:1: ἐπιcυναχθειcῶν τῶν μυριάδων τοῦ ὄχλου ὥcτε καταπατεῖν ἀλλήλουc, while Jesus, who, according to an ancient tradition, was somewhat short of stature, would have been particularly at risk. In any case, as has been said, the Apostles were clearly nowhere to be seen.

It is my belief that the primary corruption was from αὐτόν to ἄρτον, and it was only after this that φανῆναι was altered to φαγεῖν to provide some kind of sense. But, it will be argued, how could such different words as αὐτόν and ἄρτον have been mistaken for each other? The answer is, very easily. Consider the following readings in Sinaiticus where, in each case, it is the corrector C[a] who gives the right word:

[1] English is, I believe, one of the few languages which make a clear distinction, inherited from Anglo-Saxon, between "house" and "home", and this has crossed the Atlantic, for it was an American, John Howard Payne, who wrote the words of "Home, sweet home." In these circumstances it is remarkable how reluctant English translators have been to use the word "home" even where it is the obvious meaning. For instance, at Mark 8:3: καὶ ἐὰν ἀπολύcω αὐτοὺc νήcτειc εἰc οἶκον αὐτῶν (no variation in the Greek), A.V. has "And if I send them away fasting to their own houses," R.V. "And if I send them away fasting to their home," R.E.B. cuts out the verbiage and at last provides an idiomatic version with "If I send them home hungry."

I Maccabees 3:1:	αρτου.	C^a αυτου
Psalms 104:16:	αυτου.	C^a αρτου
Proverbs 20:13:	αυτων.	C^a αρτων

That is as may be, the critic will say, but how could such totally different words as φανῆναι and φαγεῖν possibly be confused? Once again let us look at Sinaiticus, at James 5:3, where we find:

φαινετε (i.e., itacistic for φαινεται) S* solus.[2]
φαγεται rell.

A final difficulty is provided by the word ἐξέϲτη. This has usually been applied to Jesus, meaning that he was out of his mind, and that his family and friends said this to quieten the mob and defuse the situation. This would certainly have been a sensible thing to do, but it is perhaps not surprising that some critics have applied these words to the mob, i.e., telling them that they were out of their minds. However, this is hardly likely to have had a calming effect, nor do I see how it can be reasonably obtained from the Greek.

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[2] Not noted in Nestle-Aland 27th ed., but it is in the new Editio Critica Maior, *ad loc.*, p. 84.

3

A NOTE ON ΠΥΓΜΗ IN MARK 7:3

In *Zeitschrift für die neutestamentliche Wissenschaft*, lx (1969), pp. 182–198, Professor Martin Hengel published a masterly survey of the various explanations which have been proposed of the mysterious word πυγμῇ in Mark 7:3, in the phrase ἐὰν μὴ πυγμῇ νίψωνται τὰς χεῖρας, and propounded a solution of his own. It is not my intention to consider yet again these explanations, but to draw attention to one cardinal fact which, so far as I know, no commentator has noticed, namely that the word πυγμῇ is totally otiose. All that Mark is saying is that Jews, or at any rate strict Jews, wash their hands before eating, whereas some of the disciples were observed not to do so. The exact extent of the washing, whether it was to the wrist or the elbow, the position of the hands during the washing, the quantity of water used, and so on, are all beside the point, as can readily be seen from the parallel account in Matt. 15:1–20.

I would suggest that the unnecessary πυγμῇ is in fact the result of a very simple scribal error. Let us suppose that Mark, in writing out the text of his Gospel, came to the passage in question, and that ἐὰν μὴ happened to come at the end of a line. Before continuing on the next line, his attention was momentarily distracted, and when he resumed he accidentally repeated ἐαν μὴ at the beginning of the new line, and then, immediately realizing his mistake, cancelled the unnecessary letters by blotting them out or crossing them through, but in such a way that the last two letters μη were still partially recognizable.

Mark's manuscript, probably written in a rapid cursive, would have been given to a professional scribe to make a fair copy which would become the archetype of the Gospel. When he came to the cancelled ἐὰν μὴ he could not make up his mind whether the letters were intended to be cancelled or whether they had suffered some accidental damage. The fact that the final letters μη could still be distinguished inclined him to the second possibility, and he then tried to think of a possible word of about five letters ending in μη and somehow connected with hands or washing. Unluckily he thought of πυγμη which he wrote down in his text, with the result that the entire manuscript tradition was saddled with this nonsensical reading.[1]

There is a remarkable parallel between this case and that of the preposterous word δευτεροπρώτῳ in Luke 6:1, on which I contributed a note in *Novum Testamentum*, xxx. 2 (1988), pp. 103–6 (= chapter C5, below). In this, following a suggestion of Burkitt, I showed that the word could be explained, and indeed could only be explained, as the result of a scribal error.

What these two examples admirably illustrate is the remarkable fidelity of the scribes of our Scriptures, who continued to copy out what they saw before them even though it made no sense.[2] It is my belief that as a part of their training scribes were given one golden rule: 'Never omit.' The reason for this is obvious, for if a scribe, faced with an apparent corruption in the text he was copying, put down exactly what he saw, or thought he could see, there was always the chance that someone might be able to emend the text, whereas if he omitted the difficult word, whatever was there, or should have been there would inevitably be lost, perhaps for ever.

While it is now too late to exclude πυγμῇ and δευτεροπρώτῳ from our texts of the Gospels, future translators who decide to ignore the fictitious words can at least feel reassured.

[1] The textual evidence in favour of πυγμῇ is virtually unanimous since the variant πυκνά is clearly an attempt to make some kind of sense out of nonsense. The only authorities to omit the word are Δ, a ninth-century St. Gall MS., and the Sinaitic Syriac and Sahidic versions, the translators of which presumably realized, correctly, that πυγμῇ was nonsense.

[2] The same point is made by Professor Hengel, op. cit., p. 183.

4

ST. MARK 16:8: A MODERN GREEK PARALLEL

Recent issues of *JTS* have borne witness to a revival of the theory that St. Mark intentionally concluded his Gospel at 16:8,[1] and it therefore seems an appropriate moment to draw attention to a very similar 'ending' in a medieval Greek composition. This is the metrical paraphrase of Genesis and Exodus by Georgios Chumnos of Candia, written about the year 1500. The poem has never been printed in full, but a selection of the apocryphal episodes with which the narrative is liberally interlarded has been published by F. H. Marshall under the title *Old Testament Legends, from a Greek Poem on Genesis and Exodus by Georgios Chumnos*, Cambridge University Press, 1925. The poem ends with the Assumption of Moses, and the final couplet (Marshall, p. 109) runs:

> Λοιπὸν αὐτὸν ἐσκέπασεν, καὶ ὁ Μωϋσῆς ἐχάθη
> καὶ ἀπὸ τὸν φόβον ὁ λαὸς ὅλος ἐπαραπάρθη.

i.e. 'Then it [the cloud] covered him, and Moses was lost to sight, and all the people were distraught with fear.'

The significance of φόβος in this context seems to come very close to that of 'reverential awe' postulated by Mr. Willoughby Allen. And be that as it may, these lines indicate that a latter-day Greek author, whose level of intelligence and literary abilities cannot have been so very different from those of St. Mark, saw no objection to concluding his work on such a note. In any event, is not the determination to demand an 'ending'—and much more a 'happy ending'—to St. Mark's Gospel something of a *petitio principii*? There are many abrupt, or seemingly abrupt, endings in literature, and it may be instructive to recall what has been written of one of the best-known English examples, the ending of *Piers Plowman*:[2]

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[1] See the review by R. H. Lightfoot; *J.T.S.* xlvi. 217–24, and his *Locality and Doctrine in the Gospels*, chs. 1 and 2; Willoughby C. Allen, 'St. Mark xvi. 8. "They were Afraid." Why?', *J.T.S.* xlvii. 46–9; his 'Fear in St. Mark', *J.T.S.* xlviii. 201–3; and L. J. D. Richardson, 'St. Mark xvi. 8', *J.T.S.* xlix. 144 f.

[2] *Piers the Plowman*, ed. W. W. Skeat, vol. ii, 1886, p. lvi.

> Dr. Whitaker has suggested that the poem is not perfect; that it must have been designed to have a more satisfactory ending, and one not so suggestive of disappointment and gloom. I am convinced that this opinion is erroneous; not so much because all the MSS. have here the word *Explicit*, but from the very nature of the case. What other ending can there be? or rather, the end is not yet. We may be defeated, yet not cast down; we may be dying, and yet live. We are all still pilgrims upon earth. *That* is the truth which the author's mighty genius would impress upon us in his parting words. Just as the poet wakes in ecstasy at the end of the poem of Do-bet, where he dreams of that which has been already accomplished, so here he wakes in tears, at the thought of how much remains to be done. So far from ending carelessly, he seems to me to have ceased speaking at the right moment, and to have managed a very difficult matter with consummate skill.

And it is in something of the same spirit that another scholar, who likewise held that the ending of *Piers Plowman* was just and true, once said: 'I was never very careful to find a peroration for my lectures; the conclusion in which nothing is concluded has always seemed to me the most admirable.'[3]

Finally, as regards a parallel between the 'abrupt' ending of the Gospel and its hardly less 'abrupt' beginning, Dr. Lightfoot's remarks in *J.T.S.* xlvi. 223–4 can be left to speak for themselves; but I suggest that some new light has been given by A. Wikgren's interpretation of ἀρχή in 1:1 as 'elements' or 'essentials'.[4] If the Gospel as a whole is the 'First Steps' in Christian instruction, may not this conception modify our expectations regarding the manner of its termination? Perhaps the truth may be found by regarding 16:8 not as 'The End', but rather 'The End of the Beginning'.

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[3] W. P. Ker, quoted in R. W. Chambers, *Man's Unconquerable Mind*, 1939, p. 405.
[4] *Journal of Biblical Literature*, lxi, 1942, 11–20.

5

THE 'SECOND-FIRST' SABBATH (LUKE 6:1): THE FINAL SOLUTION

Every few years a new theory is propounded to explain the mysterious word δευτεροπρώτῳ in Luke 6:1. The purpose of the present note is not to discuss these, but to draw attention to a suggestion made many years ago by F. C. Burkitt, and to demonstrate that this provides not only a possible solution, but the only possible solution.[1]

In *The Gospel History and its Transmission*, 3rd edition, 1911, p. 80, note 1, Burkitt wrote: "The date of the events recorded in Mk iii 5 ff. cannot be accurately determined, but it is reasonable to suppose that it was shortly after the occasion on which the disciples had plucked the ears of corn on the Sabbath (Mk ii 23 ff.). This story, placed as it is somewhere near the shore of the Sea of Galilee, implies a date somewhere in April or May. Lk vi 1 does not tell us any more than the parallel in Mark. The textual evidence makes it certain that the δευτεροπρώτῳ of the Byzantine and some Western texts is not genuine, and even if it were accepted it does not seem to correspond to any known Jewish expression. Probably an ancient Western scribe wrote ЄΝϹΑΒΒΑΤΩΒΑΤΩ by dittography, and $\overline{\text{ΒΑ}}$ΤΩ was erroneously expanded into δευτερο-πρώτῳ."

As will be seen, Burkitt did not suggest how the dittography might have arisen. No doubt there are a number of possible ways, and if I put forward one here, it is purely for the sake of illustration.

I suggest that either the original manuscript of the Gospel or an early archetype showed the text written something like this:

[1] For a conspectus of the theories which have been advanced (not, however, mentioning Burkitt's solution) see G. W. Buchanan and Charles Wolfe, "The 'Second-First Sabbath' (Luke 6:1)", *Journal of Biblical Literature*, 97, 1978, pp. 259–262. The suggestion of E. Delebecque there recorded, *viz.* that the disciples walked "forcibly" (βίᾳ) through the cornfields, and that βίᾳ was mistakenly expanded to δευτεροπρώτῳ has been taken further by him in his *Etudes grecques sur l'Evangile de Luc*, Paris, 1976, pp. 71–76. An article not mentioned by Buchanan and Wolfe is E. Mezger, "Le sabbat 'second-premier' de Luc", *Theologische Zeitschrift*, 32, 1976, pp. 138–143. Mezger interprets δευτεροπρώτῳ as meaning "the second sabbath of the first month", *viz.* the month Nisan, and calculates that the incident took place on 29 March 32 A.D., which was indeed a Saturday!

ΛΑΙΟϹΧΡΗϹΤΟϹΕϹΤΙΝΕΓΕΝΕΤΟΔΕΕΝϹΑΒ
ΒΑΤΩΔΙΑΠΟΡΕΥΕϹΘΑΙΑΥΤΟΝΔΙΑϹΠΟΡΙΜΩΝ

I then suggest that the letters ΒΑΤΩ at the beginning of the second line were somehow damaged and partly obliterated. When whoever checked the text came to the passage, he did not bother to wash out and re-write the damaged letters, but simply added ΒΑΤΩ to the end of the preceding line. When a copy was made from this manuscript, the careful and conscientious, but not very intelligent, scribe, after copying as far as ϹΑΒΒΑΤΩ, came to the partially defaced letters ΒΑΤΩ at the beginning of the second line. As they had not been formally cancelled, he did not feel authorised to ignore them, and copied them into his text, producing something like this:

ΕΓΕΝΕΤΟΔΕΕΝϹΑΒΒΑΤΩΒΑΤΩΔΙΑΠΟΡΕΥΕϹΘΑΙ

Once this stage had been reached, the manner in which the dittography had arisen was no longer obvious, and it would have been much more difficult for a scribe or editor to eject ΒΑΤΩ. But since this was clearly nonsensical, subsequent copyists were faced with the choice of either omitting the letters, or trying to extract some sense from them.

The letters of the alphabet which the Greeks used as numerals could stand equally for cardinals or ordinals, in any number or case, and there would thus be no difficulty in interpreting the Β as δευτέρῳ and the Α as πρώτῳ, or, taken together, δευτεροπρώτῳ, had such a word existed. The following ΤΩ was, strictly speaking, unnecessary, but may have been thought of as an indication that the adjective was to agree with σαββάτῳ.

But, it may be objected, is this explanation any more than just one more attempted solution of the problem? Has it any more validity than the rest of its numerous competitors? To this question the answer must be a resounding Yes. Once it has been accepted that δευτεροπρώτῳ is a feasible expansion of ΒΑΤΩ, it surely cannot be a mere coincidence that we find exactly the letters βατω in the conclusion of the immediately preceding σαββάτῳ. That this is accidental is so incredible that it could not be accepted on any evidence whatsoever.

It follows that δευτεροπρώτῳ is a "ghost-word", to use the convenient phrase coined by my grandfather, Prof. W. W. Skeat, just over a century ago. In an address to the Philological Society on 21 May 1886, he defined "ghost-words" as "words which never had any real existence, being mere coinages due to the blunders of printers or scribes, or to the perfervid imaginations of ignorant or blundering editors. . . . As it is convenient to have a short name for words of this character, I shall take leave to call them "ghost-words". Like ghosts, we may seem to see them, or may fancy that they exist; but they have no real entity. We cannot grasp them; when we would do so, they disappear."[2]

It is not necessary here to go into the textual evidence for δευτεροπρώτῳ, or to attempt to explain why so many authorities include, while others omit, the fictitious word. Certainly by the end of the 4th century it must have been equally common and unchallenged in both Western and Eastern Christendom, since we find Jerome writing to Gregory Nazianzen to ask for an explanation of the word, to which Gregory replied that it would embarrass Jerome before the whole Church to do so. Jerome concluded that Gregory was bluffing, and that he did not know either.[3]

What at first sight seems so extraordinary is that critics as acute as Jerome and Gregory should not have realised that δευτεροπρώτῳ was sheer nonsense. I would suggest that the explanation lies in a widespread and enduring human trait which Lynn Thorndike, in his *History of Magic and Experimental Science* has aptly termed "the desire, almost, to believe the unbelievable." This is perfectly exemplified by one of the examples in Prof. Skeat's paper, which is so illuminating that it must be quoted in full:

> Another extraordinary instance is that of the ghost-verb *to morse*. As a substantive, the word is real, and means a walrus, for which it is the Russian name; but as a verb, the word is spectral. It occurs, I believe, in all but a few editions of Sir Walter Scott's novel of The Monastery, chap. x., where we have this sentence: 'Hardened wretch (said father Eustace) art thou but this instant delivered from death, and dost thou so soon *morse* thoughts of slaughter?' The word has been lately discussed in Notes and Queries, in the Sixth Series, ix. 507, x. 34, 97, 195; and in the Seventh Series, i. 199. The question was definitely settled by Mr. Fenwick, son-in-law of the late Sir Thomas Phillipps, who possesses the original MS. He says: 'The word *nurse* is very legibly written, and there can be no doubt that it is *nurse*.' This is a most instructive instance, as proving that a false form, if once introduced, can maintain itself through countless editions without detection, or at any rate without correction. Many readers have supposed it to be

[2] *Philological Society's Transactions*, 1885–6, pp. 351–52.
[3] *PL.* 22. 534, letter 52.

excellent Lowland Scotch, and it is not a little curious to find that, in Notes and Queries, 6 S. x. 97, the reading *morse* is explained, upheld, and etymologically accounted for by two independent correspondents, who refer it to the Lat. *mordere*, to bite. One explains it as 'to prime,' as when one primes a musket, from O. Fr. *amorce*, powder for the touch-hole (Cotgrave): and the other by to bite, to gnaw, hence 'to indulge in biting, stinging, or gnawing thoughts of slaughter.' The latter says: 'That the word as a misprint should have been printed and read by millions for fifty years without being challenged and altered exceeds the bounds of probability.' Yet this very thing has actually happened, and it is not so very surprising. Many admire what they cannot understand, and uphold all that is paradoxical. It must be added that, in a few editions, as e.g. in one printed in 1871, the word rightly appears as *nurse*, a reading which may have been due to a slight exercise of common sense. The correction is obvious enough to any reflecting mind. I draw the conclusion that any ghost-word, if of plausible appearance, will be greedily accepted and even defended."[4]

[4] *Philological Society's Transactions*, 1885–6, pp. 353–54. The word *morse* still appears in an edition of *The Monastery* printed in 1969 (J. M. Dent's *Everyman's Library* series, vol. 136, p. 127.

6

DID PAUL WRITE TO "BISHOPS AND DEACONS" AT PHILIPPI? A NOTE ON PHILIPPIANS 1:1

Every commentary on the Epistle to the Philippians discusses the remarkable addition to the address of the words σὺν ἐπισκόποις καὶ διακόνοις, this being the only passage in the Pauline corpus which appears to recognise any form of church hierarchy. It may therefore be interesting to consider the text of what is certainly the most ancient surviving manuscript of the Epistle, the Chester Beatty papyrus codex of the Pauline Epistles (and Hebrews) denominated P 46 in the list of New Testament papyri, generally agreed to have been written about the year 200.[1]

Reference to Kenyon's edition[2] at first sight provides a total disappointment, since apart from the first seven words, the remainder of the first verse is lost through the destruction of the lower part of the codex. The purpose of this note is to suggest that the position may not be quite so hopeless as it at first sight appears.

We must first determine how many lines of text are lost at the foot of the page, and in order to discover this we must ascertain the total height of the column of writing. Can this be calculated?

As the facsimile shows, the scribe aligned the top lines of the columns on facing pages, it is therefore probable that he will have similarly aligned the last lines of the columns on both pages. In order to check this, we can estimate the number of lines lost in a variety of pages on the assumption that there was no substantial variation of text in the lost portion, and that the number of letters to the line was within the limits suggested by the extant portion. When this has been done we can estimate the total height of the column of writing.[3]

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[1] See, most recently, *Das Neue Testament auf Papyrus: II, Die paulinischen Briefe*, Teil I, Rom., I Kor., 2 Kor. = Arbeiten zur neutestamentlichen Textforschung Band 12, Berlin & New York, 1989, pp. xl–xlvi.

[2] F. G. Kenyon, *The Chester Beatty Biblical Papyri*, Fasciculus III, Supplement, Pauline Epistles, London, 1937.

[3] The average height of a line of writing, *i.e.* the height of the writing itself plus the space between it and the next line, is very constant at approximately 7 mm.

On this basis I have calculated figures for the total height of the column of writing in a succession of openings (*i.e.* pairs of facing pages) in the proximity of the beginning of Philippians, with the following results. All measurements are in millimetres, and are taken as near as possible to the central fold of the book. This is essential, since the lines of writing are often far from horizontal, and obviously the scribe, for the sake of appearance, will have tried to align the *beginning* of the last line on the right-hand page with the *end* of the last line of the left-hand, *i.e.* facing, page.

In the following table, the plus sign links pairs of facing pages, while x denotes the unknown quantity, the height of the column containing the end of Galatians and the commencement of Philippians.

Folio References	Height in Millimetres
ff. $79^v + 80r$	207 + 206
$80^v + 81^r$	208 + 208
$81^v + 82^r$	207 + 207
$82^v + 83^r$	210 + 211
$83^v + 84^r$	200 + 200
$84^v + 85^r$	205 + 205
$85^v + 86^r$	208 + x
$86^v + 87^r$	211 + 210
$87^v + 88^r$	207 + 209
$88^v + 89^r$	204 + 204
$89^v + 90^r$	205 + 206
$90^v + 91^r$	198 + 200
$91^v + 92^r$	206 + 204

As will be seen, although there is considerable variety in the total height of the column of writing, from 198 to 211 mm., the columns on facing pages are the same within a millimetre or two. We can therefore conclude that the height of the column containing the beginning of Philippians was approximately 208 mm. And if we assume that lines were spaced at about the same intervals as in the extant portion of the page, we can calculate that there were precisely five lines, neither more nor less, lost after the first line of the Epistle.

Addition or subtraction of a single line therefore makes a noticeable difference in the height of the column.

This first line, as printed by Kenyon, ended with πᾶσιν, and this is certainly correct, since the beginning of the next word, τοῖς, is clearly visible in the facsimile at the beginning of the second line, which Kenyon did not attempt to decipher, although some traces remain. From τοῖς to ἐπὶ τῇ in verse 5, where the column certainly ended because the next word, over the page, is κοινωνίᾳ, the text comprises 207 letters, and if this was contained in the five lines mentioned above, there was an average of 41.4 letters to the line. Such an average is quite impossible if we compare the upper part of the page concerned, containing the conclusion of Galatians. This consists of lines of 38, 36, 32, 38, 37, 33, 34, 35, 30, 32, 32, 33, 33, 31, 34, 35, 32, 33 and 31 letters, an average of 33.6, while the first line of Philippians certainly had no more than 35 letters. Clearly, therefore, something has been omitted.

Before proceeding further, however, we must return to the second line of the Epistle. As stated above, this certainly began with τοῖς, after which the text should have run ἁγίοις ἐν Χρ(ιστ)ῷ Ἰη(σο)ῦ. If we examine the facsimile we can see that after the presumed τοῖς there is a lacuna which could have accommodated ἁγίοις, followed by a certain epsilon, which must come from ἐν. This should have been followed by Χρ(ιστ)ῷ Ἰη(σο)ῦ but this was certainly not the case, since if so the horizontal lines surmounting the *nomina sacra*, or at least traces of them, would have been visible. On the contrary, immediately after ἐν is a vertical stroke which can only represent a phi, the first letter of Φιλίπποις.

Obviously what has happened is that the scribe has skipped from the first τοῖς to the second τοῖς, a type of error to which he is particularly prone, Kenyon's apparatus noting examples at Hebrews 8:8, 8:12, 9:14, 12:6–7; I Corinthians 1:25, 6:12, 15:40; 2 Corinthians 1:6, 5:15, 8:18–19, 11:12; Ephesians 1:3; Philippians 3:10, 4:17; Colossians 3:1–2.[4] In consequence, the word following τοις must have been ουσιν, not αγιοις, and indeed the facsimile shows clear signs of the final nu. We are certain of the text as far as Φιλίπποις, and this could have been followed by σὺν ἐπισκόποις but hardly more than this if the line was of normal length. From this point to κοινωνίᾳ

[4] Kenyon describes these omissions as "per homoiotel(euton)" but in fact in every case it is not identity of termination, but identity of a complete word or words that is involved. A later hand has corrected all but one of the omissions in Hebrews, but nowhere else.

in verse 5 comprises 156 letters, for which only four lines are now available, giving an average of 39 letters to the line—still far too high to be acceptable. If, however, the words σὺν ἐπισκόποις καὶ διακόνοις had been omitted, this would reduce the number of letters to be accommodated to 144. Of these, χάρις ὑμῖν καὶ could have found place in line 2, leaving 132 for the remaining four lines and giving an acceptable average of 33.

At first sight this might appear a strong argument for the omission of the words σὺν ἐπισκόποις καὶ διακόνοις. There is, however, another candidate for omission to consider, *viz.* from πάσῃ in verse 3 to πάσῃ in verse 4. This would have occasioned the loss of 24 letters, almost identical with the loss caused by the omission of σὺν ἐπισκόποις καὶ διακόνοις. That this omission would have produced nonsense is no argument, since this is the result of most of the scribe's omissions. For example, at Philippians 4:17 he compresses οὐχ ὅτι ἐπιζητῶ τὸ δόμα, ἀλλ' ἐπιζητῶ τὸν καρπὸν to οὐχ ὅτι ἐπιζητῶ τὸν καρπὸν, thereby reversing the sense.

Thus, while there is overwhelming evidence to show that one or other of these two omissions must have occurred, it is impossible to decide which, and although it still remains possible that the scribe omitted the words σὺν ἐπισκόποις καὶ διακόνοις, this represents only an even chance, and the result of our investigation must therefore be: *non liquet.*

7

'ESPECIALLY THE PARCHMENTS': A NOTE ON 2 TIMOTHY 4:13

> τὸν φαιλόνην ὃν ἀπέλιπον ἐν Τρωάδι παρὰ Κάρπῳ ἐρχόμενος φέρε, καὶ τὰ βιβλία, μάλιστα τὰς μεμβράνας.

So far as I am aware there has never been any difference of opinion about the significance of the last three words of this verse, translated in A.V. '*but* especially the parchments' ('but' being italicized as not represented in the Greek), 'especially the parchments' in R.V., and 'above all my notebooks' in the N(ew) E(nglish) B(ible), whose editors recognized that μεμβράναι was the Latin name for the parchment notebook used by the Romans for notes, memoranda, or rough drafts, and that Paul merely transliterated the word because it was a Roman development having no Greek equivalent.[1]

But what precisely was the nature of Paul's request? What I may call the traditional view seems to be that the implied meaning was 'Bring all the books if you can, but if this is not possible, at least be sure to bring the notebooks'. Spelt out in this way this would have been a perfectly reasonable request for Paul to have made: but the fact remains that he did not make it.

We can hardly imagine that Paul would have carried an extensive library round with him. The typical form of book familiar both to Gentiles and to Jews of the Diaspora was the papyrus roll, which was light, compact, and easily transported. If these were the βιβλία, why should Paul have anticipated any possible difficulty in Timothy bringing them with him?

My own suggestion is that μάλιστα in this passage,[2] instead of differentiating the βιβλία from the μεμβράναι, in fact equates them,

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[1] There is a good example of the use of μεμβράναι in common Greek speech in a second-century papyrus letter published by H. C. Youtie, P. Petaus 30: Δεῖος γενόμενος παρ' ἡμῖν ἐπέδειξεν μὲν ἡμῖν τὰς μεμβράνας ἕξ. ἐκεῖθεν μὲν οὐδὲν ἐξελεξάμεθα, ἄλλα δὲ ὀκτὼ ἀντεβάλομεν, εἰς ἃ ἔδωκα ἐπὶ λόγον δραχμὰς ρ.

[2] There are, of course, very many passages where μάλιστα has its usual meaning of 'especially' or 'particularly'. A large selection is given by C. Spicq, *Saint Paul: Les Épîtres Pastorales*, 4th edn. (1969), pp. 509–10.

at least to the extent of defining or particularizing the general term βιβλία, and that an idiomatic English translation would be 'the books—I mean the parchment notebooks'.

We can attempt to test this hypothesis by looking at other passages in which μάλιστα seems to introduce some kind of definition or qualification. One such passage is Titus 1:10–11: εἰσὶ γὰρ πολλοὶ καὶ ἀνυπότακτοι ματαιολόγοι καὶ φρεναπάτα, μάλιστα οἱ ἐκ περιτομῆς. The final words are translated 'specially they of the circumcision' in both A.V. and R.V. and 'especially among Jewish converts' in N.E.B. It is difficult to say what precisely was in the minds of the A.V. and R.V. translators beyond giving a literal rendering of the Greek. Presumably they thought that what Paul had intended to say was that while there are many ματαιολόγοι and φρεναπάται in Crete, the majority were to be found among the ἐκ περιτομῆς. Certainly the N.E.B. takes this view, and spells it out in so many words. But here again we must note that this is what Paul did *not* say, since had this been his intention he would have needed to write ἐν τοῖς ἐκ περιτομῆς. My suggestion is therefore that here again μάλιστα introduces a definition, and that Paul was identifying the whole group of Jewish converts with the troublemakers: on this basis an English translation would be 'in other words, the Jewish converts'.

My third example is 1 Timothy 4:10, ἠλπίκαμεν ἐπὶ θεῷ ζῶντι, ὅς ἐστι σωτὴρ πάντων ἀνθρώπων, μάλιστα πιστῶν, translated 'who is the Saviour of all men, specially of those that believe' in A.V. and also R.V. (except for the substitution of 'them' for 'those') and 'the Saviour of all men—the Saviour, above all, of believers' in N.E.B. On my hypothesis this should be rendered 'God, who gives salvation to all men—that is to say, to all who believe in Him'. This in fact gives better sense, since although God is the potential Saviour of all, He can only be the Saviour of those who accept him. The extended N.E.B. translation indicates the difficulty the editors experienced in finding a meaningful rendering of μάλιστα πιστῶν and their solution comes very close to that propounded here.

The significance of μάλιστα to which I have drawn attention[3] is one which is unlikely to occur in formal prose, since precisions or definitions of this kind would normally be ironed out in the course of composition and would not find their way into the final text. On

[3] Dr. Caird has suggested that support for my view might be sought in the question τί μάλιστα; which according to Liddell and Scott means 'What precisely?'

the other hand, this is exactly the kind of locution which one might expect to find in a letter, particularly if dictated (the normal practice in the ancient world) or written without premeditation; and I would suggest that it is no coincidence that the examples I have quoted come from the Epistles. It seems logical therefore to seek for further examples among Greek letters on papyrus.

My first example comes from the correspondence of the farmer Lucius Bellenus Gemellus, of Euhemeria in the Arsinoite nome, edited by Grenfell and Hunt in *Fayum Towns and their Papyri*, 1900. No. 118 is a letter from Gemellus, written on 6 November A.D. 110, giving various commissions, including the following, in which the erratic spelling has been normalized: καὶ ἀγόρασον ἡμῖν εἰς ἀποστολὴν τοῖς Ἰσιείοις οἷς ἔχομεν συνήθειαν πέμπειν, μάλιστα τοῖς στρατηγοῖς. The editors translate 'Buy us some presents for the Isis festival for the persons we are accustomed to send to, especially the strategi', the strategi in question being the civil governors of the three divisions of the Arsinoite nome. In this, as in other letters of his, Gemellus always gives his orders with precision and clarity, and I suggest that he realized that a phrase like 'the persons we are accustomed to send to' was too vague, and that he added the words μάλιστα τοῖς στρατηγοῖς, 'in other words the strategi', to put the matter beyond doubt.

The second example, of rather a different character, is Oxyrhynchus Papyrus 1411, a copy of a proclamation by the Strategus of the Oxyrhynchite nome in A.D. 260, during the brief reign of the usurpers Macrianus and Quietus, ordering the local banks to accept the coinage issued by the usurpers, which they had been reluctant to do. He in fact commands the bankers πᾶν νόμισμα προσίεσθαι πλὴν μάλιστα παρατύπου καὶ κιβδήλου, which the editors translate 'to accept and exchange all coin except the absolutely spurious and counterfeit'. It is difficult to see how one forged coin can be more spurious than another, and I would suggest that after writing (or dictating) πᾶν νόμισμα the Strategus realized that he would have to exclude forgeries, and therefore added the modification introduced by πλὴν μάλιστα. The translation should therefore read 'except, that is, anything forged or fraudulent'.

An even clearer example occurs in Oxyrhynchus Papyrus 3253, a letter from a certain Zoilos to a local agent, dated by the editors third to fourth century. In lines 14–21 we find the following passage which despite some mutilation and irregular syntax is quite intelligible: καὶ περὶ τούτου η . . ạτο ὁ μεικρὸς Π̣[α]γένης ὥς τινων πινώντων

(l. πεινώντων) ἐν τῷ ἐποικίῳ μάλ̣ι̣στα Λου[. .]υ. μάθε οὖν καὶ ποίησον α̣[ὐτο]ῖς δοθῆναι ὑπὸ Βησαρίωνος εἰς διατροφὰς ὀλίγα σιτάρια ἐπιδείξας αὐτῷ τὰ γράμματά μου. The unread verb at the beginning is obviously something like 'said' or 'wrote', while the proper name following μάλιστα might be Λούπου or Λουκίου. The editors translate 'Also about this little Pagenes[4] . . . that some were going hungry, especially in the settlement of Lu . . s. Find out and see to it that a little grain is given them for food by Besarion, showing him my letters.'[5] They were evidently troubled by the position of μάλιστα, since they comment 'It seems better to translate "some were going hungry, especially in the settlement of L." than "some were going hungry in the settlement, especially L."' But the important point to notice is that Zoilos does *not* write μάλιστα ἐν τῷ ἐποικίῳ Λου[. .]υ, and in my opinion this is a certain example of μάλιστα introducing a definition, i.e. Zoilos first wrote ἐν τῷ ἐποικίῳ, meaning the village which would be familiar both to him and his agent, and then added 'I mean Lou . . u' so as to obviate any possible misunderstanding.

My final example from the papyri is Oxyrhynchus papyrus 3302, a petition to the Prefect of Egypt written in A.D. 300–1 by a lady who claims to have been unjustly dispossessed of some property which had been bequeathed to her. The operative passage runs ἔτι μὴν καὶ τῶν καταλελιμμένων μοι μάλιστα ὑπ' αὐτοῦ τοῦ πατρὸς ὑπαρχόντων ὑπὸ βιαίων καὶ δυναστῶν παρανόμως κρατηθέντων, which the editor translates 'and when the property left to me, especially that left to me by my father, was illegally detained by violent and influential persons'. It will be seen that μάλιστα ὑπ' αὐτοῦ τοῦ πατρός is hardly sufficient warrant for 'especially that left to me by my father', nor is it clear how property can be 'especially' bequeathed. I would therefore suggest that here once more we have a definition or particularization, added in this case to add force to an appeal *ad misericordiam*: the translation should thus be 'when the property left to me—in fact by my own father—was illegally detained etc.'

I have suggested that examples of this use of μάλιστα might be expected to occur in texts which are dictated, i.e. in direct speech, and I shall therefore quote one example of this kind. This is in the

[4] Rather 'Pagenes the younger', the use of μέγας and μικρός for 'older' and 'younger' being an Egyptian locution, cf. J. G. Tait, *Greek Ostraca*, p. 40, no. 237, note.

[5] Better, 'this letter of mine' since it is obvious that the reference is to the present letter.

Acts of St. Eudoxius, martyred under Diocletian (Migne, *Patr. Gr.* 115, 624 C), where the Prefect (of Melitene in Cappadocia) addresses the Saint as follows: μετεστειλάμεθά σου τὴν περιφάνειαν ὥστε καὶ τοῖς βασιλικοῖς ὑπηρετῆσαι προστάγμασι καὶ τοῖς θεοῖς τὴν προσήκουσαν θυσίαν προσενεγκεῖν, μάλιστα τῷ πατρὶ θεῶν τῷ μεγάλῳ Διί, Ἀπόλλωνί τε καὶ τῇ φίλῃ παρθένῳ Ἀρτέμιδι. In this context μάλιστα can hardly mean 'especially' since one cannot sacrifice 'especially' to a deity: either one sacrifices or one does not. I suggest therefore that the Prefect, having specified the vague τοῖς θεοῖς, felt that he ought to spell out precisely what he himself would regard as required for compliance with the edict.

So far the significance of 2 Timothy 4:13 has been considered from the philological point of view, but I would suggest that a very small point of textual criticism is also involved. While the great majority of the Greek manuscripts read μάλιστα τὰς μεμβράνας, a few, including D* 69. 462. 489 have μάλιστα δὲ τὰς μεμβράνας, and this variant is reflected in some Latin manuscripts which have *maxime autem* or *maxime vero*. It seems clear to me that the δέ was inserted by someone who took what I have called the traditional view, i.e. the meaning 'but especially' and in so doing anticipated the 'but' added by the translators of the Authorised Version.

There has been endless speculation about the nature of the βιβλία and the supposedly differentiated μεμβράναι; a selection of suggestions which have been made is given by Spicq, op. cit., pp. 814–16. We have now made some progress by showing that Paul was only asking for the μεμβράναι, but we are no nearer to being able to guess what they contained—certainly not literary works of any kind, which rules out a great many earlier suggestions, but probably notes or memoranda such as lists of Christians in various communities. This is one small step forwards. Another point of possible value arises from my suggestion that this use of μάλιστα is characteristic of direct speech or writings based thereon, such as private letters. If this observation is correct, one might surmise that the Epistles in which it occurs, whatever their authorship, are, in origin at least, genuine letters and not deliberate forgeries or pastiches, since composition of these would not be likely to provide opportunity for the locution here discussed.[6]

[6] I am indebted to the Revd. Professor C. F. D. Moule for a number of helpful comments, but I am solely responsible for the contents of this article.

APPENDIXES

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APPENDIX A

THE FORMATION OF THE FOUR-GOSPEL CODEX: A DRAMATIZED ACCOUNT OF HOW IT MAY HAVE COME ABOUT

Scene: A house in Antioch.
Time: A day in May, 90 A.D.
Present: The Bishops of Antioch, Alexandria, Ephesus, Corinth and Rome.

Antioch: As host of this Conference may I open the proceedings by welcoming you all; and now that the sailing season has been open for some weeks I hope that those of you who came by sea had pleasant journeys.

The aim of the Conference, if I may put it very briefly, is to consider the proliferation of Gospels; to consider what recommendations, if any, we should make; and how these might be implemented. But first of all we need a Chairman, and I should like to propose Alexandria, both because of his city's acknowledged pre-eminence in all bibliographical matters and also because he is, I believe, rather less committed than some of us in regard to particular Gospels.

All: Agreed.

Alexandria: As Antioch has said, our problem is the proliferation of Gospels, or works calling themselves such. I doubt whether any of us would care to say how many Gospels there are in circulation, not to mention other, less successful, efforts which have existed in the past, and may have been available to other writers, but have themselves dropped out of use. Of course there are the well-known Gospels which circulate widely and are familiar to all of us here, and I propose to refer to these by their traditional designations of Mark, Matthew, Luke and John even though there may be doubts about their authorship in some cases. In addition there are Gospels such as the Gospel of the Egyptians and the Gospel of the Hebrews which have achieved wide acceptance, but only in certain limited areas. I am sure there are plenty more, and of course a new Gospel may appear any day, since anyone can sit down and write a Gospel and publish it and hope it will achieve circulation.

Is this multiplicity of Gospels a threat to our Mission? From one point of view one might say No, and that the sum total of these works gives us a fuller picture of Our Lord's life and teaching than can be gained from any one of them. As Our Lord himself said, In His Father's house there are many mansions. I myself have some sympathy with this view. Certainly if a new Gospel were to emerge based on long-forgotten sources and giving new and, so far as we could judge, authentic information about Our Lord we would welcome it with open arms. However, I think we must agree that this is unlikely. What *is* likely is the continuing production of Gospels designed to propagate views which the Churches have rejected, and which seek to achieve popularity by including bizarre or sensational anecdotes of Our Lord's life, or drifting off into eschatological or apocalyptic fantasies. We cannot of course prevent the production of such "Gospels", but we can, and perhaps ought, to specify those which embody the true teaching of Christ and his Church.

Of course the ideal situation would be that we should possess one single Gospel, incorporating all the surviving information and universally accepted. I should like us to consider whether this is still a possibility. As I see it, there are two ways in which we might arrive at such a situation. Firstly, we might commission a completely new Gospel on comprehensive lines, and hope that with our authority and support it would in time replace all others. After all, is this not precisely what Luke intended, and said so?

Corinth: I am not so sure about this. Of course, every biographer hopes that his work will be the definitive one. And it is certainly true that Luke undertook a great deal of research among such earlier sources as were available to him. But I am not quite convinced that he intended his Gospel to lead to the disappearance of his predecessors.

Alexandria: In any case, is my suggestion practicable? I can myself see enormous difficulties and objections. Writing a Gospel is no mere hack-work or a suitable production for a committee. Each of the four Gospels bears the imprint of its author's personality and beliefs, and I doubt whether we could find anyone with the same total commitment to-day. In any case, the immense popularity of the four Gospels would, I think, make it impossible for any new work to compete with them with any chance of success. Can we then agree to reject this proposition?

All: Agreed.

Alexandria: My second suggestion is, I think, more practicable since it builds upon the very popularity of the four Gospels to which I have alluded. This would be to take the four Gospels as they stand and weave them into a single continuous narrative.

This might be possible, though I can foresee immense practical problems. For instance, when two or more Gospels describe what is obviously the same incident in slightly different terms, or even with some factual discrepancies, which version is to be chosen? How can the different time-scales be reconciled? And over and above all this, from the literary point of view the particular style of each writer and the message which he is trying to communicate would be hopelessly obscured. What are your views?

Rome: I feel it would be quite impracticable to expect my Church to give up Mark in favour of such a patchwork. There is so little of the factual information in Mark which is not found in some form or other in Matthew or Luke, or both, that there would be very little of Mark left in such a composite work. At the same time, as Alexandria has pointed out, the essential message of the Gospel would be destroyed in the process.

Ephesus: I also feel that the suggestion is impracticable. John is in so many ways unique among the Gospels that I do not see how it could be combined with the others. And if fragmented in the manner proposed his contribution to our faith would be irreparably damaged.

Corinth: I too find the suggestion impossible to accept. The difficulties are much too great. For instance, I would defy anyone to harmonise the genealogies of Luke and Matthew.

Antioch: Such a single Gospel, if it could be produced in an Aramaic version, might be useful in our efforts—not very successful so far—to spread the Gospel message further East, where Greek is much less used and Aramaic is the *lingus franca*. But we are considering here Greek-speaking Churches, and for them my feelings are the same as those of the rest of you. I fear therefore that we must accept that both alternatives tabled by Alexandria for discussion are impracticable. We are thus left, inevitably, with a plurality of Gospels and to this extent this Conference has been a failure. Perhaps our Chairman would like to give us his views on this?

Alexandria: I would not call this Conference a failure, for the discussions have been most interesting and stimulating. But if, as Antioch says, we are obliged to admit a plurality of Gospels, can we at least limit the number in some way? I do not think there is much doubt that the most widely used Gospels are those going under the names of Mark, Luke, Matthew and John. Perhaps we could try to ensure that copies of these are available in all Churches.

Rome: We could certainly do this by circularising individual Churches and sending them copies of any of these Gospels which they do not already have. But what we really need is some way of ensuring that in every Church all four Gospels are not merely available but are actually used. For example, if a Church is at present using John and is quite happy with it, and we send out copies of Mark, Luke and Matthew, is there not a danger that they will simply be put aside in a cupboard and remain shut up in their book-cases?

Alexandria: I agree we should try to find a way to popularise all four Gospels equally—and, by implication, discourage the use of others. I wonder whether it would be possible to include all four Gospels in a single roll, and send copies of such rolls to the principal Churches? It would then be impossible to neglect any of the four, as might happen if they were on separate rolls. I am afraid a very long roll would be needed, but this might be worth investigating. Antioch, are you in touch with any good scribes?

Antioch: Yes indeed, I have a very good scribe who works for me, although he is not as yet a Christian. But I am not sure that expert advice is required at this stage. As it seems to me, all we have to do is to mobilise rolls of all four Gospels and measure them so as to get the total length. I am sure I can locate the four rolls without difficulty. Perhaps our Chairman would assist me in the calculations, and we can meet again to-morrow.

(Next day)

Antioch: I am afraid I was too optimistic about the simplicity of the problem. The four rolls which I collected turned out to be of all shapes and sizes, and there was also great variety in the size of the writing. For instance, John was written in a very tiny hand and was actually two feet shorter than Mark, although we all know Mark to be the briefest of the Gospels. It is clear therefore that a more scientific approach is required, and I shall after all have to invoke the aid of my friendly scribe. As the investigation may take some time, perhaps we could meet again the day after to-morrow.

(Two days later)

Antioch: Now at last I have reliable information to give you. As I had suspected, a very considerable amount of work was involved, and my scribe had to call upon some of his colleagues in order to produce the result by to-day. First, it was necessary to calculate the number of notional lines, which as you are aware form the basis of scribes' remuneration, for each Gospel and this meant marking up my four copies. The results are interesting, since although as I mentioned before we all knew Mark to be the shortest, the relationship of all four is something new. Here is the list of figures, copies of which I give to each of you.

Mark	1494	lines	)
John	1903	lines	) TOTAL: 8345 lines
Matthew	2397	lines	)
Luke	2551	lines	)

As you will see, I have arranged the Gospels in order of length. I shall have something to say later on about the question of order. And I may add that the rolls had no titles, except in some cases simply 'Gospel' and in order to distinguish them I wrote the name of the author, or supposed author, at the head of each roll. I then asked my scribe to calculate the length of a roll which would be required to accommodate 8345 lines. At this point he raised all sorts of questions—what was to be the height and breadth of the columns of writing, how much space should be left between columns, what type of script was to be used, space for titles, paragraphing, and so on. As I am no expert in such matters I told him to use the normal standards in use for any literary prose work, and to allow for a script easy to read and of reasonable size. He has now given me his calculations. On the basis I gave him, a roll containing 8345 lines would be approximately 160 Roman feet in length. I asked him to give the figure in Roman feet because this is a standard familiar to you all which can easily be converted into any local measures.

I am afraid this is a great disappointment. As we had feared might be the case, this is an impossible length for a single roll, and there is no doubt that the idea of a roll containing all four Gospels must be abandoned.

Alexandria: Is that really so? I have seen rolls well over a hundred feet long in common use on Government offices, and these seem to present no problem. And of course some reduction could be effected by using taller and wider columns and a smaller script.

Antioch: That is true, but my scribe did not think that the reduction would be very significant. As regards script, for instance if the writing is smaller the columns must be narrower or the book becomes very hard to read; and more and narrower columns means more spaces between them, which tends to nullify the saving through the script. As regards Government offices, I imagine these rolls are used only for occasional reference, not continuous reading, and are not really relevant. Can we then all agree that the idea is impracticable?

All: Agreed.

Antioch: It seems then, unless anyone can think of an alternative, that we have come to the end of the road. Although these discussions have been useful, and we have at least agreed to confer a kind of seal of approval on these four Gospels. I fear this Conference has not been very productive.

Rome: I have one suggestion to make, though I put it forward with great diffidence. You all know the parchment memorandum-books which we use for casual notes and rough drafts. These are very useful, and also economical because the ink can be easily wiped off with a sponge and the note-book used again. Recently similar note-books have been made with papyrus, because it is more readily available. One special advantage is that you can write on both sides of the material, and this makes them very compact. Recently there have been proposals in Rome for using the same format for works of literature. Of course educated people hold up their hands in horror at the at the idea, but the advantage of compactness is undeniable. Might not something on these lines solve our problem?

Antioch: Certainly I will ask my scribe to look into this, though I expect the idea of such a book will be as novel to him as it is to me. Certainly it would appear that this would reduce the amount of papyrus used by half. By the way, perhaps Rome could tell us whether any new word has been coined to describe the new format; you could hardly call such a book a *volumen*, could you?

Rome: It has been proposed to call such a book a codex, because it derives, through the parchment note-book, from the collection of wooden writing-tablets customarily called a codex. Perhaps we could use this term in our present discussions, especially as I cannot think of any Greek equivalent.

Antioch: Codex then it shall be. But to revert to the saving of half the amount of papyrus, could not this be more simply effected by writing on both sides of the roll—what is commonly called an opisthograph roll? This would reduce the length of our imaginary roll from 120 Roman feet to 60—still very long, but perhaps not impossibly so.

Alexandria: I would strongly oppose any idea of opisthograph rolls. Friends of mine in the Library say they have constant trouble with them, because they deteriorate so rapidly. The reason for this is that when one is reading what I shall call the front side—the side which carries the writing in a normal roll—the hands are all the time handling and rubbing against the writing on the back side. In a normal roll, of course, the hands do not touch the writing at all. Opisthograph rolls are also very awkward for the reader, since when you get to the end of the front side you have to turn the roll over, and the position of the hands holding the roll is reversed, that is to say the rolled-up portions of the roll are now below the surface you are inspecting instead of rising above it. You could of course re-roll the roll in the reverse direction on coming to the end of the front side, but this is difficult and time-consuming because papyrus has a natural tendency to roll up in the direction in which it has always been kept ever since manufacture.

Antioch: That sounds a pretty comprehensive condemnation of opisthograph rolls. I propose therefore to consult my scribe and see if he can produce a specimen codex for us to consider tomorrow. This is certainly a revolutionary idea: but are we not ourselves, as Christians, in some sense revolutionaries?

(Next day)

Antioch: I promised you a specimen codex for our consideration today, but in fact my scribe has produced five dummy codices, which I will hand round. I call them dummies because as you will at once see they are almost entirely blank. Of course in the short time since our meeting yesterday it was quite impossible to transcribe all four Gospels five times ever, so what my scribe has done is just to write the first and last two or three lines of each Gospel in the appropriate position in the codex. The calculation of the number of notional lines in each Gospel made it possible to do this very accurately. You will see, too, that I have kept the Gospels in order of length, without prejudice to any different order which we may decide upon later on.

These dummies are designed to contain a total of 280 columns of text, as you can see for yourselves since the scribe has numbered the positions which they would occupy in the finished product. He has also based his calculations on a slightly smaller and more compact script than the one he used in working out the length of the four-Gospel roll. The columns will also be rather wider because the lines will run right across the available space.

Alexandria: This is certainly a most remarkable innovation, and will no doubt raise a few eyebrows when I return home. I imagine that what the scribe did was to cut 70 pieces of papyrus of identical size, stitch them down the centre, and then fold them over.

I observe that because of the bulk of the book the sheets which were on top of the pile (and are now in the centre) project beyond those at the ends, producing a wedge-like shape. This projecting portion might be susceptible to damage. I wonder whether this can be avoided.

Antioch: Yes, the edge can be cut square, though this would mean that the sheets in the centre of the codex become slightly narrower and will accommodate less text. However, this can easily be allowed for in the calculations which must precede the writing of any codex.

Corinth: This codex certainly seems much better than the monstrous roll we were threatened with. But isn't it still rather bulky for the average Christian?

Ephesus: I don't think at the moment we are considering the average Christian, but rather Churches, and for this purpose the four-Gospel codex seems an excellent idea. The individual Christian will probably be quite satisfied with the single Gospel of his choice, for I take it that there is no intention to discourage the circulation of separate Gospels for private use?

Antioch: No, certainly not. So long as Churches possess the four-Gospel codices, and transcribe them as necessary, the continued circulation of individual Gospels should not affect our objective. Can we therefore now formally approve the proposal?

All: Agreed.

Rome: I am very glad that my suggestion has been found useful. With these dummies as models we can each of us arrange to put the work of transcription in hand. No doubt there will be criticisms as there always are for any innovation, but I do not expect any serious problems.

Antioch: I mentioned before that I intended to raise the question of the order in which the Gospels should appear. So far, as you know I have simply placed them in order of length. Can anyone suggest a more logical, or defensible method?

Corinth: Do we need to have a fixed order? As long as the codices contain all four Gospels I don't see that it matters what order they are in.

Alexandria: I think it does matter. If you pick up a codex to find a particular passage it will be a nuisance to have to find out first what order the Gospels are in. And anyway it is good literary practice to have a fixed order. If, for example, you pick up a roll of the Idylls of Theocritus or the Mimes of Herondas you will find they are always in the same order, presumably because this was the order of the archetype; and this order is reinforced by numbering the individual items. However, I don't fancy numbering the Gospels.

Antioch: Can we then consider the order? I myself, of course, would like Matthew to come first.

Rome: I should naturally like Mark to come first, and this could be justified on historical grounds, since there is no doubt that Mark is the earliest. However, Matthew was an Apostle so I should not object provided Mark came second.

Antioch: If we may tentatively agree on Matthew and Mark, this leaves us with Luke and John—or should it be John and Luke?

Ephesus: John is unique both in content and style, and I would agree that it is probably the latest in time. It is true that John was an Apostle like Matthew and his Gospel should therefore be given the same priority. However, there is honour in the end as well as in the beginning, so I should not object to John being placed last.

Corinth: This leaves Luke in third place. I see no objection to this. He should certainly be in close proximity to Mark upon whom he draws so largely.

Antioch: Can we then agree on the order Matthew, Mark, Luke, John

All: Agreed.

Antioch: There is one further point. The four Gospels were not published with any specific titles—I suppose because each writer regarded his work as *The* Gospel. But in a four-Gospel codex they must be named in some way. Shall we just put the name of the writer at the head of each, as I had to do when handing out the manuscripts to my scribe?

Alexandria: I would disagree with that. Everyone knows that the proper place for a title is at the end of a work. Should we not simply give the title "Gospel of Matthew" (etc.) at the end of each?

Antioch: I have doubts about "Gospel of Matthew", although it is commonly so referred to. The genitive suggests that Matthew was the author of the Good News instead of merely chronicling it. If the Gospel is "of" anybody, it is the Gospel of Our Lord Jesus Christ, as Mark actually says at the very beginning of his work. I would prefer, since all four are putting forth the Gospel, to word the titles "Gospel according to Matthew" (etc.). Can we agree on this?

All: Agreed.

APPENDIX B

THE ARRIVAL OF THE FIFTY BIBLES IN CONSTANTINOPLE

Constantine told Eusebius that, when all the fifty manuscripts of the Bible which he had ordered had been safely delivered to Constantinople, he himself would inspect them, and there can be no possible doubt that he did so. The delivery of the last three or four manuscripts must therefore have been a very special occasion, since it enabled Constantine, at last, to carry out his expressed intention of inspecting the full fifty manuscripts.

In these circumstances it is, I think, legitimate for us to reflect on this, one of the most extraordinary events in the history of manuscripts, and to try to imagine the course of events. Thus, we may imagine the fifty great manuscripts, shown open, and laid out on long tables, covered, no doubt, with some rich material such as silk or tapestry, and accompanied by the ornamental book-boxes, the πολυτελῶς ἠσκημένα τεύχη which Eusebius says he provided to afford maximum protection for the manuscripts during their long overland journey from Caesarea to Constantinople. What else, if anything, would have been on the tables? Here we have only our imagination to rely on. Were there, for instance, vessels of gold or silver, *objets d'art*, pictures? Surely there must have been flowers, to offset the stark simplicity of the great manuscripts.

Then there would have come the great moment—the arrival of the Emperor accompanied by court officials and guards, and, of course, the Bishop of Constantinople and all the local clergy, each of them, no doubt, hoping to acquire one of the Bibles. The Caesarean deacon who accompanied the final delivery of manuscripts must have been there, to answer questions or to draw attention to special features. We can picture the Emperor walking up and down between the long tables, stopping to turn over the leaves of one of the manuscripts or picking up another to examine the binding, and, when he finally declared himself satisfied, everyone must have breathed a sigh of relief.

Besides the Emperor himself, there might have been present other members of the Imperial family. Constantine's three sons were, I believe, at this time all in important positions away from Constantinople, but there would have been other members, such as Delmatius and Hannibalianus, then in high favour, but destined, like so many others, to perish in the blood-bath which followed the death of Constantine. Of the few survivors, there might have been one, at this time a boy of three or four, no doubt in the charge of a nurse, who was destined, thirty years later, to succeed to the throne; to abjure the religion which Constantine had espoused; and to be known to posterity as Julian the Apostate.

Eusebius himself, I think we may be sure, was not there; otherwise he could hardly have failed to record the fact. He had, after all, failed to carry out to the letter Constantine's orders that all fifty manuscripts should be delivered in a single consignment, and he must have experienced an enormous feeling of relief at having escaped the usual consequences of such failure.

Such, then, is the picture which I have tried to recreate here, and it is above all essential to realise that it, or something very like it, must actually have occurred, irrespective of whether the Codex Sinaiticus or the Codex Vaticanus, or both, or neither, were among the fifty manuscripts. According to my reconstruction, Vaticanus is the sole survivor of that historic occasion, and the officials of the Vatican Library may, if they wish, like to reflect that they have on their shelves a manuscript which has been personally inspected by Constantine the Great.

APPENDIX C

T. C. SKEAT ON THE DATING AND ORIGIN OF CODEX VATICANUS

J. K. Elliott

Biblical scholars are used to working with the text of Codex Sinaiticus and Codex Vaticanus. We sometimes need to remind ourselves just how unique these manuscripts are.

Both are codices on parchment that originally included the whole of the Bible. Even complete copies of the New Testament are rare: my count is only sixty manuscripts out of 5,000 New Testament manuscripts and not all those sixty were originally composed as complete manuscripts; in some cases one of the sections was added by a different and later hand. Then the age of these manuscripts is remarkable—they are our oldest Bibles in Greek. (Their dates will be considered shortly.) The fact that they contain not only the New Testament but the complete Bible in Greek makes these, together with Codex Alexandrinus and Codex Ephraemi Rescriptus exceptional. Even Latin pandects are rare. The fifty Bibles ordered by Constantine (about which more below) must therefore have been a very high production of all the complete Bibles written during the fourth century or, indeed, ever written.

The commonly agreed dates for Codex Vaticanus and Codex Sinaiticus are fourth century; Alexandrinus and Ephraemi Rescriptus are from the fifth century. Cavallo[1] suggested dates of 350 for Codex Vaticanus and 360 for Codex Sinaiticus—those suggestions by a famed expert ought to be weighed carefully. Kenyon[2] gives the date as "early fourth century" for both.

We ought to remind ourselves what was happening in the Christian world at that time.

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[1] G. Cavallo, *Ricerche sulla maiuscola biblica* (Florence, 1967 = *Studi e testi di papirologia* 2) pp. 52–6, 60–1.

[2] F. G. Kenyon, *The Text of the Greek Bible* 3rd ed. by A. W. Adams (London, 1975) esp. pp. 78, 85.

There was a growing consensus about the content of the Christian scriptures—the finally agreed canon was being shaped. It may plausibly be argued that texts like Codex Sinaiticus and Codex Vaticanus were written precisely as templates to show which books ought to be included within one set of covers, and thus to provide concrete examples of the lists that were being produced by the likes of Athanasius in his 39th Festal Letter of 367. In this letter (written in Alexandria) we have a very early example of a listing of the books of the Old and New Testament. We shall return to that letter soon.

We are informed that the sequence of the New Testament books in the Festal Letter bears a close resemblance to Codex Vaticanus. In the Old Testament the order of the canonical books in Athanasius' letter agrees with that in Vaticanus,[3] but the form in which the New Testament books appear in the manuscript of Codex Vaticanus agrees with the sequence in only the Greek form of the Festal Letter. In the Sahidic Coptic (and hence an Egyptian) form of Athanasius' letter Hebrews comes between 2 Corinthians and Galatians. That is close to, but not identical with a form known to a scribe who copied a series of marginal numerations into Vaticanus. This chapter numbering in Vaticanus is illogical because Ephesians begins at number 70 yet follows Romans-Galatians which ends at 58, but it implies that its predecessor, in Alexandria (so it is often argued), had Hebrews (numbered 59–69) after Galatians and before Ephesians, and thus bears comparison with the order in the Sahidic version of the Festal Letter. However, it is not exactly the same. The important point about these numbers is that they are not the work of either of the scribes of the manuscript but were added later, possibly in Constantinople.

Some deduce from these facts that Codex Vaticanus may have been written in Alexandria but, as we shall see below, if Codex Vaticanus shares a common provenance with Codex Sinaiticus, which is certain, the completely different order of not only the New Testament

[3] Vaticanus has none of the books of Maccabees; Sinaiticus has 1 and 4 Maccabees; Alexandrinus has 1–4 Maccabees. Then there is the different order: B has the poetic books of the Old Testament preceding the prophetic as in the Festal Letter and Codex Vaticanus ends the Old Testament with Daniel, Sinaiticus ends with the poetic books concluding with Job, Alexandrinus also has the poetic books after the prophetic books but ends with Sirach. The textual character of the manuscripts differs both within the manuscript (cf. Vaticanus in Ezekiel and Isaiah), and between Sinaiticus and Vaticanus, because the writers of the manuscripts used a variety of different exemplars.

but also the Old Testament books in Codex Sinaiticus must mean that they cannot be from Egypt as Sinaiticus does not share common sequences with Athanasius' lists. The contents of Sinaiticus also differ from Vaticanus. Thus the argument used to imply an Egyptian origin of Vaticanus based on the Festal Letter cannot be made to apply to Sinaiticus. The Festal Letter may have been reproducing what by 367 had become established practice, at least in Egypt, but it ought to be considered that the letter may be defending the *contents* of the canon rather than a particular sequence of those contents.[4]

The different sequences in Codex Sinaiticus and Codex Vaticanus, and the different contents alert us to the fact that these were pioneering times when books and collections of books were being gathered together from previously independent and isolated codices to form what was intended to be an authoritative and demonstrable assemblage of books that defined the compass of the Christian canon in Greek.[5]

About the same time Jerome was at work doing a similar thing for the Latin Bible by assembling previously separate Latin texts of the Old Testament and the New Testament (and in his case, of course, by also translating several of them) to form a definitive Bible for distribution to Latin-speaking Christendom, just as Codex Vaticanus and Codex Sinaiticus could have had the effect of convincing the Greek-speaking churches to accept their library of texts.

The other events that come to mind—and are often referred to in discussions about the provenance of these two manuscripts, Codex Sinaiticus and Codex Vaticanus—are significant:

[4] The earliest New Testament witnesses show scant regard for any one agreed sequence. For example, compare the sequence of the Gospels Matthew John Luke Mark in the fifth century manuscripts D W with the order Matthew Mark John Luke in the Curetonian Syriac, and Matthew Luke Mark John in the Ambrosiaster. Hebrews follows Romans in Papyrus 46 *c*. 200.

[5] Athanasius permitted the inclusion of the Didache and the Shepherd of Hermas in his list. Hermas and the Epistle of Barnabas are included in Codex Sinaiticus suggesting it was written in an area *not* influenced by Athanasius. Codex Alexandrinus of the following century contains 1 and 2 Clement. Therefore a direct influence of the Festal letter on these early manuscripts seems unlikely, and indeed would of course have been impossible if the dates for the writing of Sinaiticus and of Vaticanus, argued for in this paper, are correct. All we may say is that these sources bear witness to a gelling of ecclesiastical opinion in the fourth-fifth centuries, although the situation remained fluid.

1) The Emperor Constantine sometime between 331–335 wrote to Eusebius, Bishop of Caesarea asking for fifty copies of the Bible for the new churches in his recently founded capital.
2) The doubtlessly imitative request to Athanasius by Constantine's son Constans for copies of the scriptures. Athanasius could have acceded to such a request between 23 November 337, after having returned to Alexandria in triumph following his first exile from Trier, and 16 April 339, when he fled from Egypt for his long second exile in Rome.[6] It would certainly have been impossible for him to have found two expert calligraphers[7] in Rome had he already started that second exile, or to have had appropriate texts to hand in Rome for those scribes to have worked from. He could easily have furnished Constans with manuscripts from Alexandria and had them sent to Constans, whose headquarters at that time were probably in Naissus (modern Niš in Serbia)[8] Wherever Athanasius wrote or found the manuscript(s) he makes it clear that he complied with the request and eventually sent them off to Constans.

The records of both events have survived[9] and we note the following points from them. In Athanasius' case he confirms that he sent these Bibles. As far as Eusebius is concerned, the precise details of the request by Constantine and the processes for its execution and delivery seem historically accurate; his account does not read like an exaggerated fiction encouraged by Eusebius' hero-worship of Cons-

[6] Cf. T. D. Barnes, *Athanasius and Constantius* (Harvard University Press, 1993) pp. 36, 46.

[7] There were two hands responsible for the writing of Vaticanus, scribes A and B. Scribe A wrote Gen 46:28–1 Kingdoms 19:11 (pp. 41–334) and Psalms—Tobit (pp. 625–944). In prose passages he began each new paragraph on a new line with the initial letters intruding into the left margin. Scribe B wrote 1 Kingdoms 19:11—2 Esdras and Hosea-Daniel and the New Testament: for the New Testament and in the prophetic books this scribe sometimes but not always began a new paragraph on a new line. There are other differences between scribes A and B. For example, both had different ways of drawing decorated tailpieces and of filling up short lines. Both had some distinctive spelling and punctuation. See T. S. Pattie, "The Creation of the Great Codices" in J. L. Sharpe III and Kimberley van Kampen (eds.) *The Bible as Book: The Manuscript Tradition* (London: The British Library, 1998) pp. 61–72, based on H. J. M. Milne and T. C. Skeat, *Scribes and Correctors of Codex Sinaiticus* (London, 1938) pp. 87–90.

[8] See Barnes *op. cit.* p. 224.

[9] Athanasius *Apologia ad Constantium* 4.2 ed. Szymusiak; Eusebius *Vita Constantini* IV 36–37 ed. Winkelmann GCS (Berlin, 1975) pp. 133–5.

tantine. Devreesse[10] suggested that the supposedly enigmatic τρισσὰ καὶ τετρασσὰ διαπεμψάντων ἡμων (meaning 'in dispatches of threes and fours') was probably an excuse to explain that he (Eusebius) had not been able to fulfil Constantine's original demand for the fifty Bibles to be sent as a single consignment.

It is tempting to try to discover if the manuscripts referred to in these sources have survived. Are any of our extant codices examples of the manuscripts sent by Eusebius or by Athanasius?

The recent revival of interest in Codex Vaticanus may justifiably be due to the splendid new facsimile edition. But scholarly circles have also been confronted by a magisterial article by the veteran papyrologist T. C. Skeat that appeared in the centennial number of *JTS*.[11] In it he argues that Codex Sinaiticus and Codex Vaticanus are indeed two sole surviving examples of the manuscripts copied in the 330s to comply with Constantine's order (even though Skeat argues that Codex Sinaiticus itself was never actually sent, not only because it was a copy full of faults but because its format proved impracticably huge to serve as a model). Skeat also argued, as Kirsopp Lake had originally done, that both were manuscripts written in Palestinian Caesarea.

Theodore Cressy Skeat was an assistant keeper at the British Museum in 1933 when Codex Sinaiticus arrived there following its purchase by the British government and people. It was he and his colleague H. J. M. Milne who published the long-lasting and much-quoted *Scribes and Correctors of the Codex Sinaiticus* (London, 1938). Then in 1984 he published his article "The Codex Vaticanus in the Fifteenth Century" in *JTS* 35 pp. 454–65 (= B3 above).

So, for over a period of nearly sixty years Skeat has been working on and with two of our most famous Greek Bibles, Sinaiticus and Vaticanus. And it is his latest thinking on one of these manuscripts that I am promoting here, although lacking his eloquence and depth of learning and experience. The bulk of this paper is based substantially on his 1999 *JTS* piece and on some further thoughts that he has shared with me in the lively exchange of correspondence that we have engaged in for many years.

[10] R. Devreesse, *Introduction à l'étude des manuscrits grecs* (Paris, 1954) p. 125.
[11] "The Codex Sinaiticus, the Codex Vaticanus and Constantine" *JTS* 50 (1999) pp. 583–625.

The *JTS* piece is reproduced in this volume.[12] Skeat had sent a draft of that article to the Vatican library in 1996 and had been in contact with the library since that date, so he was somewhat surprised and disappointed that a promotional brochure issued in 2000 by the Vatican to announce the forthcoming facsimile states that Vatican Gr 1209 had been written much later than the 330s[13] and specifies that "Il codice fu *probabilmente* trascritto in Egitto (italics mine)".

The Vatican's view may be traced to Devreesse who, referring to the Bibles ordered by Constantine, says:[14] "Il est infiniment probable que de ces Bibles de Césarée rien n'existe plus. Le Vaticanus et le Sinaiticus seraient, en tout cas, seuls à considérer, mais leur date est vraisemblement postérieure au premier tiers du IV[e] siècle . . . quant au Sinaiticus, il semble égyptien d'origine".

The introductory matter to the new facsimile is in three parts each by a different author. In Pierre-Maurice Bogaert's introduction to the Old Testament we read "on tiendra . . . *pour possible* (italics mine) une origine alexandrine et égyptienne de B" and he agrees with the consensus date of the 4th century. Stephen Pisano's introduction to the New Testament agrees that "It is the most commonly accepted opinion that Codex Vaticanus is Egyptian, and was most likely produced in Alexandria itself". Commonly held opinions are not by definition correct opinions.

Even though those views are against Skeat's position we note the authors' modest and often nuanced opinions about the date and provenance of the manuscript.

In the newspaper *Osservatore Romano* of Feb. 27th., 2000 an article written by Paul Canart, Vice-Prefect of the Vatican Library, on the occasion of the presentation of a copy of the facsimile to His Holiness, gives Skeat's *JTS* article very full and sympathetic treatment. The article notes Skeat's arguments favouring the provenance of Codex Sinaiticus and Codex Vaticanus in Caesarea and their early dates.

Among Skeat's persuasive arguments is the constant message that no-one working in this area should forget that Codex Sinaiticus and Codex Vaticanus are from the same scriptorium. The common ori-

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[12] Chapter B7 above.

[13] The brochure as originally distributed specified ". . . risalente a circa il 380 d.C". We were subsequently informed that this was a typographical *erratum*!

[14] *Op. cit.* p. 125, cf. p. 153.

gins of Codex Sinaiticus and Codex Vaticanus have been regarded as axiomatic from the days of Tischendorf through Lake to the present and no responsible New Testament scholar should ignore this fact. Among his proofs are:

i) The very close resemblance of the colophon design at the end of Deuteronomy (in Codex Vaticanus) with that at the end of Mark in Codex Sinaiticus.[15] [This Skeat identifies as his strongest argument and one which must be understood and recognised.]

ii) Possibly Codex Sinaiticus shares a scribe with Codex Vaticanus. Two of their hands may be identical. This is a disputed point because the re-inking of Codex Vaticanus at a later date (probably ninth-tenth centuries) makes it difficult to examine carefully the hand of the original scribes. Tischendorf thought hand D of Codex Sinaiticus was the same as hand B of Codex Vaticanus but Milne and Skeat argued[16] that the closest resemblance was between scribe D of Codex Sinaiticus and scribe A of Codex Vaticanus and that, even if they are not the same, "the identity of the scribal tradition stands beyond dispute". Cavallo agreed with Milne and Skeat. However, this is not a point Skeat himself would now wish to dwell upon.

   [We must remember that the colophon designs were not re-inked, although the lettering was.]

iii) Another relevant consideration is the fact that Vaticanus and Sinaiticus both end their text of Mark with the same verse. One of the features of Codex Sinaiticus and Codex Vaticanus is that they, virtually alone among New Testament manuscripts, end Mark at 16:8 (even though it is plausible that the scribe of Codex Vaticanus was hesitant to do so.)[17] Sinaiticus does not provide any evidence for the continuing of the text after verse 8, and did not do so even before the re-writing of the bifolium, the error which provoked the re-writing being in the text of Luke 1.

[15] Parts of the relevant pages are reproduced by Skeat in his *JTS* piece as Plate 1

[16] *Scribes and Correctors* Appendix I (pp. 87–90) "Scribes of the Codex Vaticanus" headed "Have B and Aleph a Scribe in Common?".

[17] He left an uncharacteristically large space after Mark 16:8 before resuming with Luke at the beginning of the next column but one. The intervening space would not actually have sufficed to be filled with the section commonly numbered 16:9–20 but it is in fact symbolic of the fact that this text has been reluctantly omitted.

If these two manuscripts were among the fifty written for Constantine we need to ask if this shortened text of Mark, ending at 16:8, was a common feature of all of these specially commissioned codices and, if so, why this textual variant did not influence the subsequent manuscript tradition more decisively than the mere addition in a handful of manuscripts of obeli or asterisks or notes alongside vv. 9–20 to the effect that some ancient authorities lacked the passage. As we know, nearly all manuscripts of Mark include 16:9–20. Possibly the other forty-eight or forty-nine copies differed from Vaticanus and Sinaiticus in this regard, or possibly other text types came to dominate the traditions even in Constantinople. Early readers may also have recognised the difficulty of accepting the originality of a text of Mark that terminated in verse 8 in such an abrupt and strange way.

Streeter,[18] albeit in the context of his now generally discredited theories about the Caesarean text-type, notes that when Jerome was in Constantinople (*c.* 380) he found that the authorities there advocated the text of Lucian—in effect the Byzantine text type—precisely because this included the longer ending to Mark. The discredited fifty copies would then, according to Streeter, have been despatched for use in provincial monasteries and churches. In any case, complete Bibles did not become fashionable until the invention of printing, possibly because such bulky volumes proved themselves impracticable. That may explain why these fifty manuscripts (assuming they resembled Vaticanus at the end of Mark) exerted no influence on other manuscripts over the ending of Mark.

But, the important point of all this is that whatever we say about the provenance of Codex Sinaiticus must also apply to Codex Vaticanus and *vice versa*. The similarity of their scripts also makes their dates of writing remarkably close to one another. [If two of the hands in Sinaiticus and Vaticanus are the same then, of course, that confirms a similar date and place of composition too.]

Obviously when we look at the text of Codex Sinaiticus and Codex Vaticanus it is clear that they are no mere *Abschriften* of the same exemplar, or copies one of the other:

1) The difference in their contents and the differences in the sequence of the texts have already been referred to but need not militate against a common scriptorium.

[18] B. H. Streeter, *The Four Gospels* (London, 1924) p. 103.

2) The texts are not identical. There are many differences apparent when these two manuscripts are collated against each other or against a common base text. This suggests that, if Skeat is right in saying that they were originally composed in compliance with Constantine's request, the method of production was not the simultaneous mass production of copies from dictation.[19] If Caesarea were the place in which this work was undertaken, individual scribes doubtless used the many manuscripts available.[20] Some of these may well have shared common textual characteristics with Egyptian manuscripts. Zuntz[21] accepts this point in defending the argument that the two manuscripts were written in Caesarea to fulfil Constantine's request by saying that many different manuscripts would have been assembled for the task and that it would have been unlikely that all the fifty manuscripts would have been copied from the same exemplars in a short duration.
3) The layout differs (three columns per page for the non-poetic books in Codex Vaticanus and four columns per page in Codex Sinaiticus) but such a difference may merely be a result of Codex Sinaiticus having been designed as a larger format book. [That overambitious scale resulted in its having been abandoned as the model for subsequent copies written to satisfy and fulfil Constantine's request for fifty copies.]

So, the physical differences between the two codices and their differing contents need not argue against their common origin in the same scriptorium.

Now we turn to the likely provenance of these manuscripts and the case that they were both written in Palestinian Caesarea.[22]

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[19] This does not imply that individual scribes did not dictate the words of the exemplar to themselves *sotto voce* or that public dictation never took place. Skeat makes a strong case for the use of dictation in his article "The Use of Dictation in Ancient Book Production" in *The Proceedings of the British Academy* XLII (1956) pp. 179–204 (and chapter A3 above) and see G. Zuntz, "Die Überlieferung der Evangelien" in B. Aland and K. Wachtel (eds.), *Lukian von Antioch und der Text der Evangelien* (*Abhandlungen der Heidelberger Akademie der Wissenschaften: Philosophisch-historische Klasse* 2 (Heidelberg, 1995) pp. 26–55 here note 134.

[20] On the probable contents of that library see Andrew James Carriker, *The Library of Eusebius of Caesarea* (Leiden, 2003) (= *VC Supplements* 67).

[21] *Op. cit.* p. 44.

[22] P.-M. Bogaert, "Aux origins de la fixation du canon: Scriptoria, listes et titres. Le *Vaticanus* et la stichométrie de Mommsen" in J.-M. Auwers and H. J. de Jonge (eds.), *The Biblical Canons* (BETL 163; Leuven, 2003) pp. 153–76 esp. pp. 155–6 where Bogaert agrees with the points made by Skeat.

1) Sinaiticus seems to have been in Caesarea in the sixth century when parts of it were collated against a Biblical manuscript used by Pamphilus and Antoninus which, before their martyrdom in Palestine in 309, they had corrected against the Hexapla of Origen. Notes in Sinaiticus at the end of 2 Esdras and at the end of Esther explain this. The sixth century corrections were presumably executed in the library of Pamphilus in Caesarea.
2) Codex Sinaiticus has links with the sixth century manuscript 015 (H^{Paul}). 015 at the end of Paul notes that this manuscript too was corrected against the copy (in Caesarea) of the manuscript used by Pamphilus.
3) Codex Sinaiticus and Vaticanus share a distinctive chapter division in Acts related to the so-called Euthalian material, found in certain other codices. Euthalian material was associated with Caesarea, and this implies that our two codices spent some time there.[23] The Armenian tradition contains Euthaliana and that version also has strong links with Caesarea.
4) More importantly, Codex Sinaiticus has certain readings that are strongly suggestive of Palestinian provenance. The reading Ἀντιπατρίδα for πατρίδα at Matthew 13:54 suggests that a Caesarean-based scribe erroneously wrote the name of a nearby town. The reading Καισαρίας at Acts 8:5 is even stronger evidence that the writer was in Caesarea. There is another similar variant: Ἵππον replaces Joppa at 1 Macc. 14:5. Here the Palestinian scribe may have been thinking of the nearby town Hippos on Lake Galilee.
5) The Eusebian section numbers in Codex Sinaiticus were added by the original scribes (initially by scribe A, then by scribe D, but Luke was never completed), and it is more likely that these were known and copied in the early 4th century in Caesarea than in, say, Alexandria.

As far as Vaticanus is concerned, it was bound in red when the manuscript reached Rome in the fifteenth century, and is so described in the Vatican Library's 1475 catalogue ("Biblia. Ex membr(anis) in rubeo") and in its 1481 catalogue ("In primo banco bibliothecae graece. Biblia in tribus columnis ex membranis in rubeo"). It seems that this leather binding has not survived: had it done so it may

[23] *Pace* J. H. Ropes in F. J. Foakes Jackson and Kirsopp Lake (eds.), *The Beginnings of Christianity* Part I *The Acts of the Apostles* III *The Text* (London, 1926) p. xliii.

have been possible to prove if that type of binding was distinctive and characteristic of fifteenth Constantinople. Such a proof would clinch the argument where and when it was covered and where it came to Rome from. A possible rewarding line of investigation is to link the additions to Vaticanus with a scribe from Constantinople. A recent attempt to identify the fifteenth-century hand as that of a known Constantinopolitan calligrapher, John Eugenikos, has not convinced Canart and we have yet to find our man—further attempts to search for the identity of this scribe continue. Canart in his prolegomenon to Codex Vaticanus in the introductory booklet to the new facsimile states that the "motifs [des bandeaux colorés et les initiales qui marquent le début de chaque livre] sont ceux de *la décoration constantinopolitaine* du X^{e} siècle, mais dans un traitement abâtardi et une exécution maladroite qui seraient plus explicables au XI^{e} ou au XII^{e} siècle, voire plus tard (italics mine)". So, if Vaticanus was in Constantinople in the fifteenth century and if it also betrays characteristics of tenth- to eleventh-century Constantinople as well, it is plausible that it had been there ever since Eusebius despatched it from Caesarea.

As far as competing places of origin for the composition of the two manuscripts are concerned, the strongest alternative (and the one favoured in the introduction to the new facsimile) is Alexandria. That is often based on the several grounds. These are noted below with counter-arguments attached:

1) The suggestion has been made that Codex Vaticanus was one of the Bibles sent from Alexandria by Athanasius to Constans has already been referred to. But if Vaticanus had been sent from Alexandria to Niš we need to ask how, when and why it got to Rome in the fifteenth century.
2) The text of Vaticanus resembles the text-type of certain third century Egyptian manuscripts, notably P75. But this need not be a decisive argument in favour of Alexandria and against Caesarea. As Zuntz reminds us,[24] Caesarea was a centre of Alexandrian scholarship—the two cities were not so far from each other: we need think only of the link from Origen through Pamphilus to Eusebius himself. Also to be remembered is the fact that manuscripts older than Vaticanus and Sinaiticus are papyri, which virtually

[24] *Op. cit.* especially p. 40.

all come from Egypt. We do not have comparable third century witnesses from other places, such as, for example, Caesarea.

3) Hexaplaric influences in Vaticanus such as the addition of obeli and asterisks in Isaiah, Zechariah, Malachi and Jeremiah are sometimes given as evidence of an Egyptian provenance. But they reflect only Egyptian *influence* that could plausibly have reached Caesarea through the person of even Origen himself.
4) Earlier arguments, by Lake and others, emphasise that certain features of the script of Codex Sinaiticus are Egyptian (the alleged Coptic mu, a cursive xi and a strangely formed omega) but these have been dismissed by no less an authority than Cavallo[25] and by Milne and Skeat[26] as not decisive.

So, the arguments for Alexandria are not watertight. Another of the arguments against Alexandria as the place of writing for Codex Sinaiticus and Codex Vaticanus is, as we have already noted, the continuing presence of Codex Sinaiticus in Caesarea in the sixth century, a presence which Skeat explains as its having been there since its composition, because it was never completed and therefore not included among the manuscripts sent to Constantine.

As a curiosum we ought to mention a third contender as the place of composition of Vaticanus, namely Rome. This was put forwarded by Hort and by Wettstein but has found little favour. More recently Hahneman has repeated this extraordinary suggestion.[27] Arguments based on alleged Latinisms in the manuscript are not persuasive. In any case it is the essential *Greek* character of Vaticanus which requires it to have been written in—and then used in, and preserved in—a Greek-speaking milieu. Among these distinctively Greek features are:

1) A Greek autograph by a monk named Clement was written on pp. 238 and 624, possibly as late as the fifteenth century.
2) Tremas and iotas were added later, when the manuscript was re-inked.
3) Extended scholia in a twelfth- to thirteenth-century Greek hand were added on, *inter alia* pages 1205, 1206 and 1239.

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25 *Op. cit.* p. 66.
26 *Scribes and Correctors* pp. 24–7 and see plate 31.
27 G. M. Hahneman, *The Muratorian Fragment and the Development of the Canon* (Oxford, 1992) pp. 164–5.

4) At Hebrews 1:3 there is an amusing note in Greek against the variant reading φανέρων found only in B* B^2 (plus coincidentally Serapion):[28] ἀμαθέστατε καὶ κακέ, ἄφες τὸν παλαίον, μὴ μεταποίει.
5) The text of the manuscript was re-inked (as we have noted earlier). This occurred in perhaps the tenth century or slightly earlier and implies that the text was still being used and read by Greek speakers or readers. Apparently Byzantine scribes continued to use majuscule even for non-liturgical works.
6) In a gloss the word sophia is explained in Greek at the beginning of Proverbs.
7) Section numbers have been added and these are Greek numerals. There is no evidence that a Greek manuscript would have been so treated in a church like Rome that had abandoned Greek by the beginning of the fourth century.

All those points would need to be addressed by anyone with the temerity to propose a provenance such as Rome.

To conclude we merely summarize Skeat's views on the later history of the two manuscripts:

Codex Sinaiticus

This was not sent to Constantinople. It was abandoned after the format of the Bibles was reduced. It therefore remained in Caesarea. Having been corrected in the sixth century it was sent to the newly founded monastery of Saint Catherine's on Mount Sinai where it remained until Tischendorf rescued it in the nineteenth century.

Codex Vaticanus

The manuscript, having at some stage been neglected and having lost pages, lay abandoned in Constantinople, possibly because its text did not conform to the ecclesiastically approved norm. Then in the fifteenth century it shows signs of having been hastily reconditioned. Despite the additions required in the New Testament and written in a cursive hand, the Pastoral Epistles were inexplicably left out. But the whole of the codex was rebound and sent to Rome, perhaps in

[28] This ought not to be used as an argument in favour of an Alexandrian provenance for Vaticanus.

time for the Council of Florence (1438–9). The publicity brochure for the new facsimile admits that "La storia di questo codice resta comunque avvolta nel mistero, fino alla sua prima sicura attestazione presso la Bibliotheca papale, nella seconda metà de secolo XV" but goes on to say "Secondo un'ipotesi piuttosto suggestiva, il Codice Vaticano B giunse in Occidente nell'anno 1438, durante il Concilio di Firenze, come dono dell' imperatore bizantino Giovanni VIII al pontifice Eugenio IV". That had been Skeat's position in his 1984 article in *JTS* and is also approved of in Canart's article in the *Osservatore Romano*, where the hypothesis is described as 'seducente'—although that seductiveness is described in Canart's introduction in the booklet accompanying the facsimile as lacking any objectivity.

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