A SCRIBE AND HIS MANUSCRIPT: AN INVESTIGATION INTO THE SCRIBAL HABITS OF PAPYRUS 46 (P. CHESTER BEATTY II – P. MICH. INV. 6238)

by

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ABSTRACT

This thesis is an investigation into the scribal habits of $\mathfrak{P}^{46}$, attempting to enrich further the information database about the sociology of ancient book production and to explore how these habits might have affected the transmission of the texts of the New Testament in general and the *corpus Paulinum* in particular. Given this end, this thesis challenges the traditional methods of locating the "scribal habits" of a particular manuscript, specifically methods that are *text*-focused. Crucial to developing a viable methodology is articulating how the conceptual category of "scribal habits" is to be understood before we can sufficiently isolate them. Using an *integrative* approach (i.e., the composite employment of papyrology, codicology, palaeography, and textual criticism), this thesis proposes that "scribal habits" are to be found in *everything* that a particular scribe recurrently did and did not do in the manuscript, encompassing all the stages of its production and its eventual use. In regard to $\mathfrak{P}^{46}$, this thesis finds the scribe in the same league with other ancient scribes as well as idiosyncratic in the ways he used his codex, copied the text of his *exemplar*, and employed existing systems and devices practised within the scribal profession. These scribal characteristics emphasise the "human" face of textual transmission of a "divine" book.

DEDICATION

This thesis is humbly dedicated to my ever-supportive wife and my joy-giving son, **Irene** and **Dayao Nathan-El,** who patiently and lovingly journeyed with me throughout the production of this thesis.

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This thesis is penned under my name, but behind its production are crisscrossing communities of wonderful people from different parts of the world who unselfishly journeyed with me through this stage of my life. While I cannot name them all, it is fitting to acknowledge some of them here.

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This research would have not come into fruition if not for the generous financial assistance and support from many quarters. Grateful acknowledgement is due to the Philippine Bible Society (PBS), the United Bible Societies (UBS), the Bible Society of Singapore (BSS), and the Langham Partnerships (LP), who all contributed to our funds during our stay in Birmingham. Special thanks are due to Dr Nora Lucero (PBS General Secretary), Rev Dr Daud Soesilo (UBS), Dr Lee Soo-Ann and Mr Kay Tham (BSS), Rev Dr Christopher Wright, Rev Dr Ian Shaw, and the LP-UK Scholarship Selection Committee, for facilitating all our financial provisions throughout our stay in the UK.

The University of Birmingham community has immensely contributed to my academic development and in various forms also helped me in my research. From ITSEE, Dr Hugh Houghton has liberally lent me his personal copy of Kenyon's facsimile of $\mathfrak{P}^{46}$ throughout the production of this thesis (he also served as my PhD upgrade reader). Dr Rachel Kevern has supportively exposed me to various minuscule manuscripts in the holdings of IGNTP-ITSEE, honing my transcriptional skills in the process. Our term-time Tuesday seminars with ITSEE colleagues (Dr Philip Burton, Dr Rosalind MacLachlan, Dr Catherine Smith, and other PGRs) have in many respects facilitated refinements of my own research and broadened my appreciation for our chosen field.

Through Prof Parker's encouragement, in 2010 and 2011 I visited the Chester Beatty Library (Dublin, Ireland) and the Papyrological Collection of the University of Michigan (Ann Arbor, Michigan, USA) to conduct personal autopsy of the actual $\mathfrak{P}^{46}$ leaves. I am very grateful to Dr Charles Horton (formerly CBL-Western Collection curator) and his staff as well as to Prof Arthur Verhoogt (PC-UM Executive Director) and Dr Adam Hyatt (former PC-UM Manager) for the permissions granting me access to examine $\mathfrak{P}^{46}$ up-close-and-personal. Images in this thesis are courtesy of the holding institutions.

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While at Birmingham, my family and I attended the Christian Life Centre for our spiritual nurture, through which we have come in contact with the Filipino Christian communities in Birmingham, a convergence that would change our lives forever. For more than four years we have been deeply encouraged and blest by the dedication of our compatriots to the work of the Lord and to one another, challenging me even more to look at my own research context as a venue for enriching our cultural database in a foreign land.

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BY GOD'S GRACE, FOR HIS GLORY!

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ABBREVIATIONS

APF	*Archiv für Papyrusforschung*
AJP	*American Journal of Papyrology*
ANTT	*Arbeiten zur Neutestamentlichen Textforschung*
BASOR	*Bulletin of the American Schools of Oriental Research*
BMQ	*British Museum Quarterly*
BSac	*Bibliotheca Sacra*
CBBPIntro	F.G. Kenyon, *The Chester Beatty Biblical Papyri: Descriptions and Texts of Twelve Manuscripts on Papyrus of the Greek Bible, Fasc. I: Introduction* (London: Emery Walker, 1933).
CBBPIII-1934	F.G. Kenyon, *The Chester Beatty Biblical Papyri: Descriptions and Texts of Twelve Manuscripts on Papyrus of the Greek Bible, Fasc. III: Pauline Epistles and Revelation* (London: Emery Walker, 1934).
CBBPIII-1936	F.G. Kenyon, *The Chester Beatty Biblical Papyri: Descriptions and Texts of Twelve Manuscripts on Papyrus of the Greek Bible, Fasc. III, Supplement: Pauline Epistles, Text* (London: Emery Walker, 1936).
CBBPIII-1937	F.G. Kenyon, *The Chester Beatty Biblical Papyri: Descriptions and Texts of Twelve Manuscripts on Papyrus of the Greek Bible, Fasc. III, Supplement: Pauline Epistles, Plates* (London: Emery Walker, 1937).
CBQ	*Catholic Biblical Quarterly*
CBW-Skeat	J.K. Elliott (ed.), *Collected Biblical Writings of T.C. Skeat* (NovTSupp 113; Leiden/Boston: Brill, 2004).
CP	*Classical Philology*
CQ	*Classical Quarterly*
DNTAP$^{2.1}$	Klaus Junack, E. Güting, U. Nimtz, and Klaus Witte, *Das Neue Testament auf Papyrus, II Die Paulinischen Briefe, Teil I: Rom., I Kor., II Kor* (*Arbeiten zur Neutestamentlichen Textforschung* 12; Berlin and New York: Walter de Gruyter, 1989).
DNTAP$^{2.2}$	Klaus Wachtel and Klaus Witte, *Das Neue Testament Auf Papyrus: Die Paulinischen Briefe II : Teil 2 : Gal, Eph, Phil, Kol, 1 und 2 Thess, 1 und 2 Tim, Tit, Phlm, Hebr* (*Arbeiten Zur Neutestamentlichen Textforschung* 22; Berlin/New York: De Gruyter, 1994).
ETNT	Charles E. Hill and Michael J. Kruger (eds.), *The Early Text of the New Testament* (Oxford: Oxford University Press, 2012).
GMAW^{2}	Eric G. Turner, *Greek Manuscripts of the Ancient World* (2nd revised and enlarged edition; Bulletin Supplement 46; ed. P.J. Parsons; London: University of London-Institute of Classical Studies, 1987).
HTR	*Harvard Theological Review*
JBL	*Journal of Biblical Literature*
JEA	*Journal of Egyptian Archaeology*
JR	*Journal of Religion*
JSNTSS	Journal for the Study of the New Testament Supplement Series
JTS	*Journal of Theological Studies*

LDAB	Leuven Database of Ancient Books
NIGTC	*New International Greek Testament Commentary*
NT	New Testament
NovT	*Novum Testamentum*
NovTSupp	Supplements to *Novum Testamentum*
NTS	*New Testament Studies*
NTT-SD	*New Testament Tools-Studies and Documents*
NTTS	New Testament Tools and Studies
OHP	Roger S. Bagnall (ed.), *Oxford Handbook of Papyrology* (Oxford: OUP, 2009).
PNTTC	Eldon Jay Epp (ed.), *Perspectives on New Testament Textual Criticism Collected Essays, 1962-2004* (*NovTSupp* 116. Leiden/Boston: Brill, 2006).
PTS	Patristische Texte und Studien
SD	Studies and Documents
SH-D	James Royse, *Scribal Habits in Early Greek New Testament Papyri.* Th.D. Dissertation, Graduate Theological Union, 1981.
SH-M	James Royse, *Scribal Habits in Early Greek New Testament Papyri* (New Testament Tools, Studies and Documents 36. Leiden/Boston: Brill, 2008).
SMTCNT	E.C. Colwell (ed.), *Studies in Methodology in Textual Criticism of the New Testament* (NTTS IX; Leiden: Brill, 1969).
SNTSMS	Society of New Testament Studies Monograph Series
SP	*Studia Papyrologica*
TB	*Tyndale Bulletin*
TEC	Eric G. Turner, *The Typology of the Early Codex* (Haney Foundation Series 18; Pennsylvania: University of Pennsylvania, 1977).
TBT	*The Bible Translator*
TCPC	Henry A. Sanders, *A Third-Century Papyrus Codex of the Epistles of Paul* (University of Michigan Studies, Humanistic Series 38; Ann Arbor: University of Michigan Press, 1935).
TCGNT^{2}	Bruce Metzger, *A Textual Commentary on the Greek New Testament* (2nd edition; Stuttgart: United Bible Societies, 1994).
TCS	Text-Critical Studies
TENTS	Text and Editions for New Testament Study
TEDCP	Günther Zuntz, *The Text of the Epistles: A Disquisition upon the Corpus Paulinum—The 1946 Schweich Lectures of the British Academy* (London: OUP for the British Academy, 1953).
THGNT	Klaus Wachtel and Michael W. Holmes, eds., *The Textual History of the Greek New Testament: Changing Views in Contemporary Research* (TCS 8; Atlanta: SBL, 2011).
TNTCR^{1}	Bart Ehrman and Michael W. Holmes, eds., *Text of the New Testament in Contemporary Research: Essays on the Status Quaestionis—A Volume in Honor of Bruce Metzger* (SD 46; Grand Rapids, MI: Eerdmans, 1995).
TNTCR^{2}	Bart Ehrman and Michael W. Holmes, eds., *Text of the New Testament in Contemporary Research: Essays on the Status Quaestionis* (*NTT-SD* 42; Leiden/Boston: Brill, 2013).
TNTC	Tyndale New Testament Commentary
TS	Text and Studies
WUNT	Wissenschaftliche Untersuchungen zum Neuen Testament
ZPE	*Zeitschrift für Papyrologie und Epigraphik*

INTRODUCTION

It has been said that every manuscript has a story to tell. But I say that every manuscript is a story in itself, deeply intertwined with the equally intricate and mysterious meta-narrative of human existence. Some plots of the overarching anecdote have been long gone with the wind, forever lost to obscurity and oblivion. But what remains of the mega-narrative is enough to enamour us with the grandeur of what it once was.

This thesis is a story about the unknown scribe of an ancient papyrus codex, now commonly known as Papyrus 46 ($\mathfrak{P}^{46}$)[1]—presently, the oldest surviving and most extensive manuscript witnessing to the text of the *corpus Paulinum*. A few have already told *some* parts of that fascinating story, avidly narrating particular storylines from that grand narrative. In the pages that follow, I will not be a narrator; I have chosen to be an intent listener instead, believing that the best narrator is the story itself. Hence, from beginning to end, this project is intended to know more about this scribe directly *from* the manuscript that he[2] produced, with the hope that this thesis will contribute in solidifying what we already know about book production enterprise in antiquities, especially as it relates to the transmission of the text of the Pauline

[1] This papyrological number was assigned by Prof. E. von Dobschutz, then official keeper of the registers of NT mss; see Frederic G. Kenyon, *Chester Beatty Biblical Papyri: Descriptions and Text of Twelve Manuscripts on Papyrus of the Greek Bible, fasc. I, Introduction* (London: Emery Walker, 1933), 6. Accordingly, the 112 pages kept in Dublin are officially designated as "P. Chester Beatty II", whilst "P. Michigan Inventory 6238" for the 60 pages kept in Ann Arbor, Michigan.

[2] Without prejudice to the contrary, I am here using the masculine pronoun generically, in recognition of the fact that there were also female scribes in antiquity; on this, see Kim Haines-Eitzen, *Guardians of Letters: Literacy, Power, and the Transmitters of Early Christian Literature* (NY/Oxford: OUP, 2000).

Epistles. Furthermore, this is also an attempt to profile details hitherto undocumented that can give further guidance on how we assess this manuscript from antiquity in particular, and other equally early NT manuscripts in general. More specifically, this project is about the habits of this scribe discernible from every *page* still extant—every page—not only the *texts* that were inscribed on it. In saying that I have in effect laid my cards on the table—this project is *not* about establishing the "original text" of the Pauline Epistles through a particular manuscript, but about its scribe who undisputedly had a significant role in transmitting a particular form of that text. Hence, this is an attempt to contribute to the furtherance of NT scribal studies, a sub-discipline that is fast becoming a genre of its own.

In undertaking this research, I have been stimulated (cognitively and professionally), and to some extent influenced, by the informative notes and publications of previous researchers—many of them respected "giants" in the field. I am always pleased when my own observations and conclusions agree with theirs, but as in any endeavour it is inevitable that in some cases I have taken another path of opinion, based on my own analysis of available and verifiable data.

It will become evident that many of the general data here would seem unnecessarily redundant as the literature already abounds with quotations from the first editors and subsequent students of this codex. But that is precisely the intention of this project: to probe which of the information proliferating in the literature about the scribe of $\mathfrak{P}^{46}$ find the unimpeachable support of the evidence, and therefore should be upheld with confidence, and which ones are wobbly, especially those dealing with proposals based on the evidence of silence, and need to be taken with a grain of salt. Needless to say, there is throughout an interaction with what has been

propagated, deservingly or not, in the literature, reckoning that only a detailed and meticulous investigation of our manuscript will hopefully settle these questions.

Chapter One introduces and situates $\mathfrak{P}^{46}$ in its historical context, locating its place not only in the text-critical map but also in auxiliary disciplines, particularly papyrology, palaeography, and codicology; fields that play crucially important roles in the methodology I adopted for this research project, a theme that I developed in Chapter Two. The adopted methodology—which can be described as an *integrative* approach—proposes that "scribal habits" are not only *textual* in nature but the *total* activity of our scribe observable from the manuscript itself. As such, Chapter Three discusses the various habits of our scribe in terms of the pre-copying activities that went into his codex, with particular focus on the physical aspect of manuscript production that may have affected, to some extent, the inscription of the text onto it. Chapter Four deals with various copying habits observable in and through the inscribed text. I shall conclude with a synthesis of how a proposed *integrative* method of locating scribal habits contributes in the advancement of NT textual criticism in general and scribal studies in particular. A series of Tables and Charts are appended to enable readers to check the bases of my observations and conclusions.

A few notes on some recurring details in this research project are in order. First, despite the presence of other foliation systems, I have used Kenyon's folio designations throughout this thesis (including the appendices) for easy reference and cross-checking, especially when one consults Kenyon's transcriptions and facsimile.[3]

[3] Kenyon's three editions designated each leaf *papyrologically*, whilst Henry Sanders, *A Third-Century Papyrus Codex of the Epistles of Paul* (University of Michigan Studies, Humanistic Series 38; Ann Arbor: University of Michigan Press, 1935), transcribed each page without any information as to fibre directions and foliations (although he duly indicated book titles, chapters, and versifications). On the other hand, Muenster's *INTF-VMR*[2] designated the pages *codicologically*, retaining the terms "recto" and "verso" in reference to the right-hand side and left-hand side of an open codex,

Hence, citation of particular pages is essentially represented by the *siglum* "f08^{r}" with reference to: f = foliation; 08 = folio number; ^{r} = recto (^{v} = verso), indicating the side with the horizontal strips ("verso" with the vertical). "F08^{r}-l^{01}" specifies the particular line where a word/s under discussion may be found. When the citation is "f08" only, the reference is to the whole leaf, its front and its back. Since Kenyon used the terms "verso" and "recto" papyrologically, when the sides of our codex are in focus, "f08^{vrs}" and "f08^{rls}" are employed in reference to the right hand ($^{-rs}$) and left hand ($^{-ls}$) sides of the open codex.

Second, whilst there are already existing transcriptions of the text of $\mathfrak{P}^{46}$, I decided to prepare my own transcription, initially checking it against Kenyon's facsimile and Michigan's online digital images, then further checked during personal autopsy of the actual pages. This may be "reinventing the wheel", but this decision had been taken since Kenyon's transcription as well as Sanders' (and some instances in other recent transcriptions) are not always precisely reliable.[4] My own transcription, with some notes, is appended to this thesis (see Appendix O); this will enable others to cross-check (and rectify if need be) the observations and conclusions I have undertaken in this project.

Third, I have used two Greek font types throughout this thesis. The minuscule (Gentium) is used throughout the transcription but I have also used the majuscule (P39LS) for emphasis, especially when a palaeographical detail is at issue.

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respectively, and attaching the sigla "→" and "↓" to indicate fibre orientations; cf. Carl Jaroš, *Das Neue Testament nach den ältesten Griechischen Handschriften die handschriftliche griechische Überlieferung des Neuen Testaments vor Codex Sinaiticus und Codex Vaticanus* (Wien und Würzburg: Echter Verlag, 2006). For an exhaustive discussion on this, see E.G. Turner, *The Terms Recto and Verso: The Anatomy of the Papyrus Roll* (Pap. Brux.; Brussels, 1978), 8-25, 54-60, and 63-65; and Idem, *Greek Papyri: An Introduction* (New Jersey: Princeton University Press, 1980), pp. 4-5 and 14-15.

[4] These instances are noted accordingly in Appendix O.

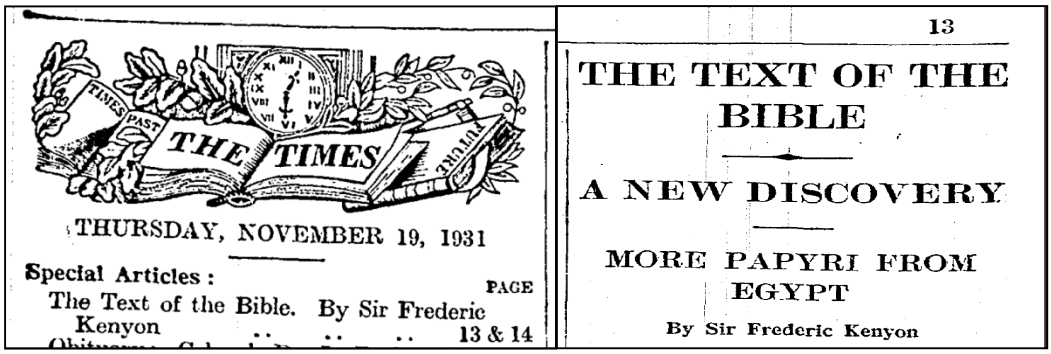
CHAPTER ONE
$\mathfrak{P}^{46}$ IN ITS HISTORICAL FRAMEWORK

> "The last 90 years have been punctuated by the discoveries of manuscripts of prime interest for biblical students… I have now… the privilege of making known a discovery of Biblical manuscripts which rivals any of these in interest and surpasses them all in antiquity… Whether further portions of it exist or not, the collection secured by Mr Chester Beatty is the most remarkable addition to the textual material of the Greek Bible that has been made for many a long day…"
>
> Sir Frederic Kenyon, *The London Times* (19 Nov 1931, p.13)

THE IMPORTANCE OF THE CHESTER BEATTY PAPYRUS OF PAULINE LETTERS

A. 19 November 1931, *The London Times:* An Epoch-Making Discovery

It was not the day's banner story, but the sixth column article of page 13[1] (Fig. 1.1) carrying Sir Frederic Kenyon's announcement about a rare cache of ancient papyri unearthed in Egypt that eventually landed in the philanthropic hands of Mr Alfred Chester Beatty[2] would in no time become a glittering headline of its own, the impact of which reverberates even up to our time. Soon, it would be any textual scholars'


THE TIMES

THURSDAY, NOVEMBER 19, 1931

Special Articles :

The Text of the Bible. By Sir Frederic Kenyon .. .. .. PAGE 13 & 14

13

THE TEXT OF THE BIBLE

A NEW DISCOVERY

MORE PAPYRI FROM EGYPT

By Sir Frederic Kenyon

Figure 1.1 Heading and Column Title of *The London Times*, 19 Nov 1931.

[1] The article is continued to the first column of page 14; see Appendix A.

[2] Unknown to the Beatty camp at the time, the University of Michigan also acquired fragments from the same find. But it took the University some more months to publicly announce the purchase; on this, see Campbell Bonner, "New Biblical Papyri at the University of Michigan," *HTR* 25/2 (Apr 1932): 205-06.

preoccupation, for many good reasons. For one, the pronouncement was described by F.F. Bruce as "the greatest discovery of Biblical manuscripts since Tischendorf discovered the Codex Sinaiticus... For the first time we have an evidence on a sufficiently large scale for the condition of the New Testament text a century before the age of the two great uncials, the Vatican and the Sinaitic codices."[3] F.C. Burkitt, with equal vigour, described the manuscripts' publication as "mark(ing) an epoch in textual history".[4] More than 70 years later, Charles Horton would continue to affirm this, arguing that "As a group the Chester Beatty Biblical Papyri remains the single most important find of early Christian manuscripts so far discovered and individually they have provided scholarship... with direct contact with the formative years of Christianity."[5] But perhaps it was Kenyon's own assessment (gained through many decades of experience with ancient artefacts) that captured the heart of many toward this rare find by appealing more to the pragmatic aspect of the discovery, insofar as the larger and more conservative Christian community was concerned. Expressing encouragement and relief as to the kind of positive witness these new finds bring to modern Christianity, he allayed fears that these ancient artefacts would radically change the text of the NT,[6] saying, "The Chester Beatty papyri have

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[3] F.F. Bruce, "Chester Beatty Papyri," *The Harvester* 11 (1934): 163-64, p. 163; also, H.I. Bell, "Fragments of an Unknown Gospel," *BMQ* 9/3 (Feb 1935): 71-73, p. 71. Albert Pietersma, "Chester Beatty Papyri," *ABD* I: 901-03, p. 901, similarly commented that the Chester Beatty biblical papyri present "important evidence for the text of the Greek Bible as it existed in Egypt prior to the *traditio codicum* (the 'turning in' of Christian books during Diocletianic persecution) and a century or more earlier than the great vellum codices of the 4th century, namely Vaticanus (B) and Sinaiticus (S)."

[4] F.C. Burkitt, "The Chester Beatty Papyri," *JTS* 34 (1933): 363-68, p. 363.

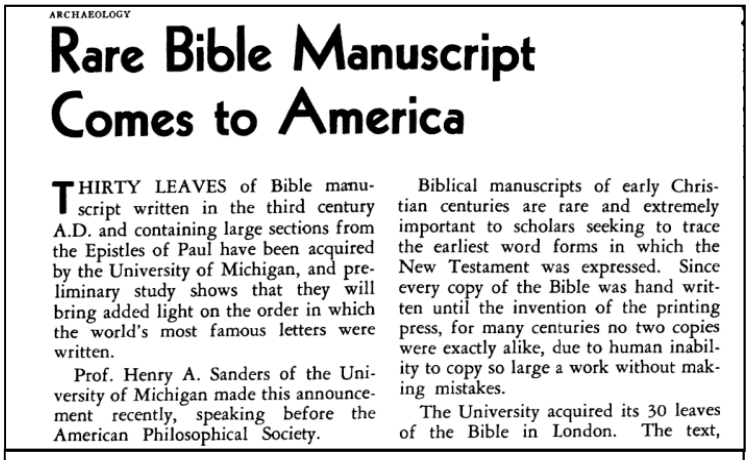
[5] Charles Horton, "The Chester Beatty Biblical Papyri: A Find of the Greatest Importance," in *The Earliest Gospels: The Origins and Transmission of the Earliest Christian Gospel – The Contribution of the Chester Beatty Gospel Codex* $\mathfrak{P}^{45}$ (JSNTSS 258; ed. C. Horton; London: T & T Clark International, 2004), 149-60, p. 149. More recently, together with other Chester Beatty and the Bodmer NT papyri, James M. Robinson, *The Story of the Bodmer Papyri: From the First Monastery's Library in Upper Egypt to Geneva and Dublin* (Eugene, Oregon: Cascade Books, 2011), 3, branded $\mathfrak{P}^{46}$ as one of the "priceless witnesses to the third-century Greek text" of the Bible.

[6] Kenyon, *CBBPIntro*, 14-15, noted, "Textually, the importance of the collection lies in its early date... Hitherto our direct knowledge of the text of the Greek Bible has rested ultimately on manuscripts of the fourth century… Before that date we have had only a few very small fragments, and such evidence

therefore strengthened very materially the basis—already very strong—of our confidence in the text of the New Testament as it has come down to us."[7] In fact, the first time Kenyon presented the find, he underscored their *confirmative* significance:

> The first and most important conclusion derived from the examination of them is the satisfactory one that they confirm the essential soundness of the existing texts. No striking or fundamental variation is shown either in the Old or the New Testament. There are no important omissions or additions of passages, and no variations which affect vital facts or doctrines... But their essential importance is their confirmation, by evidence of an earlier date that was hitherto available, of the integrity of our existing texts. In this respect they are an acquisition of epoch-making value.[8]

But Beatty and Kenyon never had the monopoly of the distinct honour for this rare find. Across the Atlantic, this "scientific" discovery involving an American university would soon become a headliner, too. For instance, in an article published by *Science News Letter*, the news was bannered with a titillating heading, "Rare Bible Manuscript Comes to America"[9] (Fig. 1.2). The same jubilation was expressed by American bible



ARCHAEOLOGY

Rare Bible Manuscript Comes to America

THIRTY LEAVES of Bible manuscript written in the third century A.D. and containing large sections from the Epistles of Paul have been acquired by the University of Michigan, and preliminary study shows that they will bring added light on the order in which the world's most famous letters were written.

Prof. Henry A. Sanders of the University of Michigan made this announcement recently, speaking before the American Philosophical Society.

Biblical manuscripts of early Christian centuries are rare and extremely important to scholars seeking to trace the earliest word forms in which the New Testament was expressed. Since every copy of the Bible was hand written until the invention of the printing press, for many centuries no two copies were exactly alike, due to human inability to copy so large a work without making mistakes.

The University acquired its 30 leaves of the Bible in London. The text,

Figure 1.2 12 May 1934 edition of *The Science News Letter*.

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as can be derived from the early Christian fathers and by deduction from the evidence of the versions. The Chester Beatty papyri carry back the direct tradition well into the third century, and in some instances into the second. Their examination will throw much valuable light on the history of the text of the Greek Bible during the vital period of the two centuries and a half which separate the composition of the canonical scriptures of Christianity from the main authorities on which our knowledge of their text is based."

[7] Frederic Kenyon, *The Bible and Modern Scholarship* (London: John Murray, 1948), 21. Their discovery also effectively dispelled the notion that the codex did not find general acceptance among early Christians until the 4th century. As Frederic Kenyon, *Recent Developments in the Textual Criticism of the Greek Bible: The Schweich Lectures of the British Academy, 1932* (London: British Academy, 1933), 53, again noted, "The Chester Beatty papyri confirm decisively the recognition of a fact… namely, the early use of the codex form of book by the Christian community."

[8] Kenyon, *CBBPIntro*, 15. On the confirmatory significance of the NT papyri in general, see E.J. Epp, "The Papyrus Manuscripts of the New Testament," in *The Text of the New Testament in Contemporary Research: Essays on the* Status Quaestionis, *Second Edition* (*NTS-SD* 42; eds. Bart Ehrman and Michael Holmes; Leiden/Boston: Brill, 2013), 1-39, esp. pp. 24-31.

[9] Anonymous, "Rare Bible Manuscripts come to America," *The Science News Letter* 25/683 (May 12, 1934): 298.

scholars over the find. For instance, upon learning of the purchase by Beatty of 46 more leaves containing the Pauline letters, Duke University's Kenneth Clark positively reported his communication with Kenyon in early October 1935, "Using these (46 folios) to supplement the published materials, it is possible to reconstruct the complete codex and show the portions now recovered and still missing".[10] He further added, "... it will be of some interest to American scholars... to note the reconstruction of that most significant manuscript, and especially this third-century order of the epistles".[11]

The gentlemanly correspondence between the two editors separated by the Atlantic would not escape the deserved accolade of their colleagues. F.A. Spencer, for instance, reviewing the Emery Walker and Ann Arbor publications, had nothing but praise, "... the friendly spirit shown by its editors from the first is one further proof that international cooperation in scholarship is not a theory, but a continuing fact."[12]

Though expressing elsewhere a somewhat concerned voice about the underutilization of papyri in advancing the text-critical cause, E.J. Epp has incisively underscored the value of the Beatty NT papyri, thus: "This discovery is a landmark not because New Testament papyri had not been found before but because the Chester Beatty papyri effected not merely a *quantitative change in the materials* available, but a *qualitative change in the discipline.*"[13] More recently, Barbara Aland, speaking about "textual consciousness", re-affirmed their value, especially as they relate to the social

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[10] Kenneth Clark, "Note on the Beatty-Michigan Pauline Papyrus," *JBL* 55 (1936): 83-84, p. 83.

[11] Clark, "Note on the Beatty-Michigan Pauline Papyrus," 83.

[12] F.A. Spencer, Review of Henry Sanders, *A Third Century Papyrus Codex* and Frederic Kenyon, *The Chester Beatty Biblical Papyri, Fasc. III*, *The Classical Weekly* 30 (Dec 7, 1936): 69-70, p. 70. See also, C.H. Roberts, Review of Henry Sanders, *A Third Century Papyrus Codex*, *JEA* 23 (Jun 1937): 133-34, p.133, where he described this partnership as "a notable example of cooperation among scholars".

[13] Eldon Jay Epp, "Textual Criticism," in *The New Testament and Its Modern Interpreters* (eds. E.J. Epp and G.W. MacRae: Philadelphia: Fortress, 1989), 75-126; repr. as "Decision Points in Past, Present, and Future New Testament Textual Criticism," in *Perspectives on New Testament Textual Criticism* (ed. E.J. Epp; NovTSupp 116; Leiden/Boston: Brill, 2005), 227-84, p. 238 (emphasis his).

history of the nascent Christianity from a particular geographical region. She commented,

> ... it may be said that the Chester Beatty papyri reflect the situation of the New Testament in a particular region of Egypt in the third century, both precisely and revealingly. They attest the significance of the New Testament as a most influential constant in the self-consciousness of the communities… This kind of circumspection is an appropriate attitude to take toward manuscripts such as Chester Beatty papyri, which are accurate in principle, but full of distinctive variants that do no violence to the meaning of the text. These papyri are an extremely informative and eloquent witness for the history of the church in the third century.[14]

B. Papyrus 46: The Earliest Extant Witness to the *Corpus Paulinum*

As to the Beatty leaves of $\mathfrak{P}^{46}$, Kenyon confided, "The second Chester Beatty papyrus contains the Pauline Epistles, and again gives us for the first time proof that these writings were known as early as the third century in a collected form which was impossible so long as the papyrus roll was the only vehicle of publication."[15] Herman Hoskier's assessment further underscored the value of $\mathfrak{P}^{46}$ when he said that "The recent publication of a large portion of the Pauline Epistles... recovered from its tomb in the desert, gives us an opportunity to examine what is perhaps the most solid contribution which the sands of Egypt have provided up to date."[16]

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[14] Barbra Aland, "The Significance of the Chester Beatty Papyri in Early Church History," in *The Earliest Gospels*, 108-21, pp. 120-21. She added, "They are witnesses to the awareness of a distinctive canon of Scripture. They show that alternative sequences of the New Testament writings were possible..., but that the collection of these writings were essentially closed. The Chester Beatty papyri are far from including any apocryphal writings, as is true also of the other early New Testament papyri... Finally, the papyri are a witness to the beginnings of a text consciousness in the community in the sense of the New Testament text to be cited and subject to exegesis..." (p. 121).

From a text-editing point of view, Kurt Aland, "The Greek New Testament: Its Present and Future Editions," *JBL* 87 (June 1968): 179-86, p. 183, equally underscored the importance of the Beatty papyri, intimating that "When the Chester Beatty papyri became known, the need for (a comprehensive critical edition of the Greek NT) was even more urgent". Additionally, on texttype studies, Ernest Colwell, "The Origin of Texttypes of New Testament Manuscripts," in *Early Christian Origins: Studies in Honor of Harold R. Willoughby* (ed. A.P. Wikgren; Chicago: Quadrangle Books, 1961), 128-38; repr. as "Methods in Establishing the Nature of Text-Types of New Testament Manuscripts," in *Studies in Methodology in Textual Criticism of the New Testament* (NTTS IX; ed. E.C. Colwell; Leiden: Brill, 1969), 45-55, p. 45, claimed that "These (Chester Beatty and Bodmer papyri) have revolutionized our understanding of the early history of the manuscript tradition of the Greek New Testament".

[15] Kenyon, *Recent Developments*, 60.

[16] Herman Hoskier, "A Study of the Chester Beatty Codex of the Pauline Epistles," *JTS* 38 (1937): 148-63, pp. 148-49.

Jeremy Duff continued the accolade by prefacing his controversial article with the statement "$\mathfrak{P}^{46}$... is undoubtedly the most important extant papyrus of Paul's letters."[17] However, it was Günther Zuntz who epitomised the textual value of $\mathfrak{P}^{46}$ when he wrote:

> Roughly 250 years separate the archetype from the codex Vaticanus whose evidence Westcott and Hort nonetheless trusted to reach back to it. With the emergence of the Chester Beatty papyrus this gap has been reduced by one-half; moreover, its text evidently derives from exemplars even older...[18]

To a significant extent, it is also to Zuntz's credit that the text of $\mathfrak{P}^{46}$ is highly valued, not only for its early date, but more so for the textual quality it carries:

> The excellent quality of the text represented by our oldest manuscript, $\mathfrak{P}^{46}$, stands out again. As so often above, we must here be careful to distinguish the very poor work of the scribe who penned it and the basic text which he poorly rendered. $\mathfrak{P}^{46}$ abounds with scribal blunders, omissions, and also additions. In some of them the scribe anticipated the errors of later copyists; but the vast majority are his own uncontested property. Once they have been discarded, there remains a text of outstanding (though not absolute) purity.[19]

Having said the foregoing, and putting this "uniquely important manuscript", to quote Zuntz's own descriptor, in its proper historical perspective, we are now ready to chart the history of researches done in the name of this papyrus. What may be said at this juncture is that the discovery and eventual publication of the complete *editio princeps* of $\mathfrak{P}^{46}$ has made the "battle over papyri", as E.J. Epp has put it,[20] more colourful and instructive in many ways. In fact, it has not only enriched our knowledge about the earliest state of the text and canon of the *corpus Paulinum*, but in broader terms has also shed more light on the textual history of the NT.

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[17] Jeremy Duff, "$\mathfrak{P}^{46}$ and the Pastorals: A Misleading Consensus?" *NTS* 44 (1998): 578-90, p. 578.

[18] Günther Zuntz, *The Text of the Epistles: A Disquisition upon the Corpus Paulinum: The 1946 Schweich Lectures of the British Academy* (London: British Academy, 1953), 17.

[19] Zuntz, *TEDCP*, 212-13. Note also the agreement of Aland, "Significance of the Chester Beatty Papyri," 116, with Zuntz's textual assessment; also, Michael Holmes, "The Text of $\mathfrak{P}^{46}$: Evidence of the Earliest 'Commentary' on Romans," in *New Testament Manuscripts: Their Texts and Their World* (eds. T.J. Kraus and N. Tobias; TENTS 2; Leiden/Boston: Brill, 2006), 189-206, p. 189, who further elevated the underlying text of $\mathfrak{P}^{46}$: "The *Vorlage* which lay before the scribe of $\mathfrak{P}^{46}$ preserved a text of perhaps *unequalled quality*; indeed, with surprising frequency it alone (or in combination with a very few others) among all extant witnesses preserves the true wording of the Pauline archetype". (Emphasis added).

[20] "Textual Criticism," 103-106; repr. in Epp, "Decision Points," in *PNTTC*, 278-82.

CHAPTER TWO
$\mathfrak{P}^{46}$ AND ITS HISTORY OF RESEARCH

$\mathfrak{P}^{46}$ is a very important resource for NT textual criticism and manuscript studies. Ironically, behind this prestigious accolade, it is nonetheless one of the most misunderstood of manuscripts,[1] suffering from a selective and seemingly utilitarian agenda by some of those who previously analysed or simply appealed to it.[2] Furthermore, its research history tends to focus more on *what was written* (the *text*), and scarcely on *where it was written* (the *physical material*) and *how it was actually written* (the *scribe's actual copying habits*).[3] This chapter highlights this qualitative "discrepancy", in the hope to eventually propose a motive and a method on how the witness evinced by $\mathfrak{P}^{46}$, both as an *ancient artefact* and a *Christian manuscript*, in relation to the scribe who "created" it, can be more holistically appreciated and fairly treated.[4]

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[1] Harry Gamble, Jr., *The Textual History of the Letter to the Romans: A Study in Textual and Literary Criticism* (SD 42; ed. I.A. Sparks; Grand Rapids, MI: Eerdmans, 1977), 34, put it best when he commented, "It cannot generally be said that ($\mathfrak{P}^{46}$) uniquely preserves important readings with claims to originality. Of course many special readings are to be found in it, but the vast majority of these must be set down to scribal error and alleviate conjectural emendation."

[2] Regrettable are those who portray somewhat negatively our codex but obviously rely merely on an incomplete profile (or none at all). For instance, Peter van Minnen, "Dating the Oldest NT Manuscripts," [<http://library.duke.edu/rubenstein/scriptorium/papyrus/texts/manuscripts.html> (14 May 2010)], par. 11, claimed, "With the exception of Sinaiticus, the oldest manuscripts are not complete. Moreover they contain scribal errors of all sorts. *P^{46} is a case in point: it is the manuscript with the largest percentage of blunders on record!*" (Emphasis added). However, this is a bold claim supported by no data!

[3] Scholars' tendency to focus more on the text of a particular manuscript is not distinctive to $\mathfrak{P}^{46}$, but points to an era of scholarship deeply entrenched in the traditional goal of textual criticism, i.e., recovering the original wordings of the NT. On this, see T.J. Kraus and T. Nicklas, "The World of New Testament Manuscripts: 'Every Manuscript Tells a Story'," in *New Testament Manuscripts*, 1-12, esp. pp. 3-4.

[4] Many of the subjects touched in this review, particularly those dealing directly with $\mathfrak{P}^{46}$'s physical features, are discussed in detail in the following chapters; hence, their mention here is kept at a minimum.

I. TRANSCRIPTIONS AND TRANSLATION

I begin this survey of the research history by looking at the subsequent editions published after the announcement of $\mathfrak{P}^{46}$'s discovery. This is appropriate because many of the recurring discussions involving $\mathfrak{P}^{46}$ were initially propounded in these editions, and many others were conceived as offshoots of their discussions, as will become obvious shortly.

A. Forming the Early Opinions on $\mathfrak{P}^{46}$: Kenyon's and Sanders' Editions

In March 1934, Kenyon published a diplomatic edition of ten leaves[5] of $\mathfrak{P}^{46}$, with a limited palaeographical discussion.[6] Aside from the text and (initial) foliation numbers, it also printed book names, modern chapter divisions, and verse numberings in the margins, for easy reference. Although a majuscule manuscript written in *scriptio continua*, the transcription was in minuscule and word-units were separated by a space. Eroded portions of a line were restored,[7] but missing lines at the bottom were not, although Kenyon indicated an approximated number of lost lines. Corrections,[8] *diaereses* on ϒ and ι, breathing marks,[9] reading marks,[10] abbreviated final -ν at line-ends,[11] *nomina sacra*, page numbers, *titloi*,[12] the lone Philippians' *stichos* note (ϹΤΙ^{Χ} ϹΚϵ [=225]), were also printed within the text area. An *apparatus criticus*, collated against the principal MSS in Tischendorf's apparatus, was appended at the bottom of the page.

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[5] These leaves contain (with some missing verses corresponding to the eroded lines at the bottom) Rom 5.17-8.37; 9.22-11.33; Phil 4.14-Col 4.18; 1Thess 1.1; 1.9-2.3; 5.5-9; and 5.23-28.

[6] Frederic Kenyon, *The Chester Beatty Biblical Papyri, fasc. III, Pauline Epistles, Text* (London: Emery Walker, 1934). The text of $\mathfrak{P}^{47}$ was also included in this edition.

[7] Reconstructed characters were enclosed in single square brackets, ([xxx]αβγ[xxx]), whilst uncertain readings were noted with a dot underneath (x̣x̣x̣).

[8] Only four corrections were noted at this point: ελεγει$^{\langle\text{ν}\rangle}$ (Rom 9.25); συνδο$^{\langle\text{υ}\rangle}$λου (Col 1.7); α$^{\langle\text{υ}\rangle}$του (Col 1.20); and νουθετουνθ$^{\langle\text{τ}\rangle}$ες (Col 1.28).

[9] ή in Rom 11.19; ώ in Col 2.12; and ά in Col 2.17.

[10] Note, however, that the reading marks were interchangeably represented by a hanging dot (·) or what seems to be an elongated acute accent (ʹ), which is a bit confusing at times. Also confusing is the unexplained siglum (•) before the start of ll$^{01, 06}$ of "f02^{r}" (= page ϰϡ) and before ll$^{10, 11}$ of "f04^{v}" (= page ϰϹ), which were actually reading marks, too, with thicker ink mixture.

[11] This is represented by an overline above the marked vowel (xxx̄).

[12] ΠΡΟϹ ΚΟΛΑϹϹΑΕΙϹ and ΠΡΟϹ [ΘΕϹϹΑΛΟΝΙ]ϰΕΙϹ [Α].

Despite the limited size of the specimen, Kenyon, seasoned papyrologist that he was, made inferences that would soon occupy other scholars' attention. Three may be immediately cited: a) the Pastorals may have been excluded from $\mathfrak{P}^{46}$;[13] b) Hebrews was part of the collection;[14] and c) $\mathfrak{P}^{46}$ belongs to the *first half* of the third century.[15]

In April 1935, Sanders, under the auspices of the University of Michigan, published what was acquired (in instalments) by the University, involving 30 leaves.[16] Having examined more leaves, Sanders confirmed Kenyon's proposal that $\mathfrak{P}^{46}$ indeed contained Hebrews immediately following Romans,[17] pre-empting any further speculation on the subject. He further confirmed Kenyon's deduction that $\mathfrak{P}^{46}$ indeed was a single-quire codex originally containing 104 leaves. However, he dated $\mathfrak{P}^{46}$ to the *latter* part of the third century, against Kenyon's *early* third century.[18]

Sanders' Introduction is comparatively more extensive in its palaeographical discussion. Except for Chester Beatty's ten leaves (appropriately distinguished and printed without alterations),[19] a fresh transcription of the Michigan leaves was prepared by Sanders.[20] Reading marks were represented by an accent mark (′), elision by an

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[13] Kenyon, *CBBPIII-1934*, vi-vii, argued that statistically the last missing pages are sufficient to contain the missing portions of 1 Thessalonians and the whole of 2 Thessalonians but not enough for the Pastorals, convincing him that the scribe ruled out including Pastorals in this collection.

[14] Kenyon, *CBBPIII-1934*, vii-viii, argued that analogy may be formed between $\mathfrak{P}^{46}$ vis-à-vis the Sahidic version and Codex B which both included Hebrews among the Pauline Epistles. Such proposal is a true mark of scholarly maturity as none of the extant pages known to him at that time contained any portion of the text of Hebrews.

[15] Kenyon, *CBBPIII-1934*, ix, assigned $\mathfrak{P}^{46}$ to the *first half* of the third century, which effectively put this codex in the text-critical map, making it the earliest surviving witness to the *corpus Paulinum*!

[16] Fortunately, Beatty and Kenyon acceded to the inclusion of their ten leaves in Sanders' edition, hence, making available to the public a total of 40 leaves.

[17] Sanders, *TCPC*, 6.

[18] Sanders, *TCPC*, 13-15.

[19] However, Sanders, *TCPC*, 38, noted that where lacunae exist and can be interpreted otherwise, "slight modifications" on the reconstructions were done on Kenyon's transcription of the ten Chester Beatty leaves.

[20] Folio numbers were not indicated but book names and chapter and verse numberings were printed in the left margin. Line numbers (in intervals of 5's) were also printed at the right side of the transcription.

apostrophe (’), and rough breathing by an angular *siglum* “in the form of half an H ⸀”[21] above the marked vowel. Τιτλοι, στιχος notes, line fillers, *nomina sacra*, *diaereses* on ϒ and ι, *dicolon* (:),[22] abbreviated final-ν at line-ends, and corrections were also reflected. An *apparatus criticus* was appended immediately after each folio.[23]

Unlike Kenyon’s, Sanders’ edition provided reconstructions even for missing lines at the bottom, unless the following page is missing.[24] Lines were also broken per word-unit, but unlike Kenyon’s, Sanders’ represented the obvious space-intervals in the codex by leaving at least a two-letter space. Whereas Kenyon did not include any plates,[25] three black and white plates from the Michigan leaves[26] were included in this edition.[27]

Sanders noted the scribe’s tendency to increase the number of characters in the latter quarter of the codex,[28] and consequently used this observation to argue against Kenyon’s proposal that $\mathfrak{P}^{46}$ terminated with 2 Thessalonians. He proposed

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[21] Sanders, *TCPC*, 19, noted that he found 12 cases, which he supposed were used for distinction purposes, i.e., preposition vs relative pronoun or adjective.

[22] The lone *dicolon* is placed after the word αμην of the “floating doxology” in between Rom 16.27 and 16.1.

[23] This apparatus has two sections—the first notes the changes and corrections made by the various hands, and the second shows instances where $\mathfrak{P}^{46}$’s readings diverge from others. Unlike Kenyon, Sanders, *TCPC*, 38, used the Oxford 1880 edition of the *Textus Receptus* as his collation base, justifying that this choice is “for the convenience of the reader”; but see the criticisms against this choice by E.C. Colwell, Review of Henry Sanders, *A Third Century Papyrus Codex*, *JR* 16/1 (Jan 1936): 96-98, p. 96; Idem, Review of Henry Sanders, *A Third Century Papyrus Codex*, *CP* 32/4 (Oct 1937): 385-87, p. 387.

[24] Like Kenyon’s, reconstructions were enclosed in open-close single brackets ([xxx]), whilst uncertain readings with a dot underneath (x̣x̣x̣).

[25] Exception to this is the lone photo provided in Kenyon, *CBBP-Intro*, 11, featuring what would later be known as f90^{r}, containing the latter part of Philippians and beginning of Colossians.

[26] Eventually, high resolution digital imaging of the Michigan leaves was undertaken by the University, and these quality images were posted online for free viewing: http://www.lib.umich.edu/reading/Paul/index.html. The site also provides general discussion on the palaeography of $\mathfrak{P}^{46}$ and a word-for-word transcription. On the other hand, a few photos of the Beatty leaves, but not of comparable quality with the Michigan leaves, are also available at the Chester Beatty Library website, including an English translation of its text (by Dwight Edgar); see, http://www.cbl.ie/getdoc/c6212daf-30bd-4c1a-9e85-2b09839d39d0/1.aspx (and the ensuing links), as well as the recently made available high resolution images at http://csntm.org/Manuscript/View/GA_P46.

[27] As an added feature, Sanders also appended an index of words used in $\mathfrak{P}^{46}$ that are not found in Moulton and Geden’s *Concordance to the New Testament*.

[28] Sanders, *TCPC*, 5-6. Kenyon did not arrive at this conclusion since it would be very difficult (if not impossible) to observe this phenomenon with only ten fragmentary leaves to scrutinise.

instead that the last pages may have contained the Pastorals (except Titus), albeit in "abbreviated" form.[29]

Subsequently, in a review article of Sanders' edition,[30] Kenyon announced that Mr Beatty had acquired additional 46 leaves of $\mathfrak{P}^{46}$ which were already being prepared for publication, together with the 40 leaves previously published.[31] Again the spirit of scholarly cooperation was invoked as Kenyon acknowledged the willing collaboration of Sanders and the University of Michigan for the inclusion of their 30 leaves. This has not escaped the approbation of other scholars.[32]

Kenyon's expanded edition provided a full introduction, integrating additional information that has since come to light, including its palaeographical features.[33] Kenyon this time provided reconstructions even for the missing lines at the bottom

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[29] Sanders, *TCPC*, 10-12. Sander's proposal did not gain support, however, since there is no extant textual evidence attesting to an abbreviated Pastorals; the weight of the evidence simply militates against it; see Frederic Kenyon, *The Chester Beatty Biblical Papyri: Descriptions and Text of the Twelve Manuscripts on Papyrus of the Greek Bible, fasc. III, supplement 3.1, Pauline Epistles, Text* (London: Emery Walker, 1936), xi. C.H. Roberts, Review of Henry Sanders, *A Third-Century Papyrus Codex of Saint Paul*, *JEA* 23/1 (June 1937), 133-34, p. 133, also described this proposal as "highly improbable". What is certain is that the additional 30 Michigan leaves left the discussion open, as to the actual contents of the final pages of $\mathfrak{P}^{46}$. See related discussion in pp. 204-35.

[30] Frederic Kenyon, Review of Henry Sanders, *A Third-Century Papyrus Codex of Saint Paul*, *AJP* 57/1 (1936): 91-95, p. 92.

[31] Kenyon, *CBBPIII-1936*. A year later, the facsimile edition of all the 86 leaves was also published, giving students and scholars alike the access to examine the text and the manuscript that contained it; Idem, *The Chester Beatty Biblical Papyri: Descriptions and Text of the Twelve Manuscripts on Papyrus of the Greek Bible, fasc. III, supplement 3.4, Pauline Epistles, Plates* (London: Emery Walker, 1937).

[32] For instance, Roberts, Review of Sanders, 133, described this collaboration as "a notable example of co-operation among scholars"; see also, Spencer, Review of Sanders and Kenyon, 70.

[33] This time the elongated acute accent mark (*ˊ*) was used generally as *siglum* for the reading; the hanging dots (·) were still printed in some instances though. In addition to Sanders' *dicolon* in Romans, Kenyon also noted another instance in Heb 11.5, between the θανατον and και.

The lone instance of *ancora* in Heb 12.6 was also printed. However, both Kenyon and Sanders failed to print in the text (or misread) the other two occurrences of *ancora* in Heb 8.8 (misread as **c** by both) and 9.14 (missed by Sanders, and noted only in the apparatus by Kenyon).

The newly identified corrections were also printed, although some have been wrongly placed, presumably due to printing limitations at the time. For instance, 2Cor 10.6 ($f70^{v}$-l^{14}) should have been reflected as υμων$^{\eta}$ rather than $^{\eta}$υμων. In Heb 11.21 ($f34^{v}$-l^{08}) the correction $^{\iota\omega\sigma\epsilon\varphi}$ was printed above εκαστον when it should have been above αυ- at the line-end. These misprints are unfortunate as they make sense in context, and therefore can potentially produce misconstrual to the untrained eyes. See also Heb 10.22 (αληθειας $^{\eta\nu}$ instead of αληθει$^{\nu\eta}$ας) and 1Cor 10.21 (τ$^{\alpha}$ρπεζης instead of τρ$^{\alpha}$πεζης).

(up to a maximum of two lost lines), although somewhat irregularly.[34] The *apparatus criticus* was improved, integrating two components: critical *notes on the divergence* of $\mathfrak{P}^{46}$'s text from other manuscripts, (still) collated against Tischendorf's apparatus, and *notes on the corrections* in the text.

With 86 leaves now at his disposal, Kenyon amplified his earlier proposals. First, he reiterated that $\mathfrak{P}^{46}$ terminated after 2 Thessalonians, arguing that Sanders' "abbreviated" Pastorals (sans Titus) is "highly problematical",[35] in view of the absence of any evidence.[36] Second, he reiterated that $\mathfrak{P}^{46}$'s textual affiliation is with the B-text group, qualifying that "the papyrus ranges itself quite definitely with the Alexandrian rather than with the Western group."[37] This opinion would become the dominant view in the field. Third, responding to Sanders' *later* dating, Kenyon reaffirmed his *early* third century dating, arguing that such dating is not "too early", on two grounds: $\mathfrak{P}^{46}$ is a single-quire codex and its *stichos* notes favour a relatively early date.[38]

Most studies on $\mathfrak{P}^{46}$ have been based largely on these early editions. Although intended as transcriptions, they nonetheless traversed many areas and anticipated future debates, encompassing studies on the Pauline collection and canon, scribal habits and tendencies, codicology and palaeography, and the like. Accordingly, some other transcriptions were eventually undertaken, for various reasons, which we shall now discuss, both printed and on-line transcriptions.

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[34] No reconstructions were provided for 70 pages, most of them from the Chester Beatty leaves.

[35] Kenyon, *CBBPIII-1936*, xi.

[36] But Kenyon was cognizant that his view was only *a priori* probable, and therefore willingly accepted the unpleasant scenario that the discussion on the matter was far from close.

[37] Kenyon, *CBBPIII-1936*, xvii. It is noteworthy that Kenyon dedicated eight pages in the Introduction, detailing the textual character of $\mathfrak{P}^{46}$, three pages of which identified at least eighty "noteworthy readings" of the papyrus. He used this analysis to re-assess $\mathfrak{P}^{46}$'s textual relationships.

[38] Kenyon, *CBBPIII-1936*, xiv-xv. Previous to this, Ulrich Wilcken, who Kenyon described at the time as the "first living papyrologist", already gave an even earlier dating for the codex—"*um* 200". This only temporarily settled the issue of dating; ensuing discussions would focus more on $\mathfrak{P}^{46}$'s "earliness" rather than its "lateness". See related discussion in pp. 137-64.

B. Institution-based and other independent Transcriptions

1. Münster-INTF related projects

Das Neue Testament auf Papyrus II comes in two separate parts.[39] *Teil 1* contains Romans, 1 Corinthians, and 2 Corinthians,[40] while *teil 2* contains the rest of the Pauline Epistles and Hebrews.[41] Strictly speaking, this is not a transcription of $\mathfrak{P}^{46}$ per se, but a catalogue of variant readings from different extant papyri containing the Pauline letters.[42] However, since $\mathfrak{P}^{46}$ is the most extensive of all these papyri, this edition may be considered an *improvised* transcription of its text. Thus, the presentation is very different from Kenyon's and Sanders'. Its running text is NA^{26}, then respective papyri readings are glossed underneath each corresponding word-unit;[43] a critical apparatus is provided at the bottom of the page.[44] Generally, this format gives one an easier and more graphic look at how $\mathfrak{P}^{46}$ agrees with or diverges from the texts of NA^{26} and other extant papyri.

The *Münster-Virtual Manuscript Room* version 2 (VMR^2)[45] is dedicated to provide on-line access to users who otherwise do not have the opportunity to examine major NT manuscripts personally. Among other features, it provides diplomatic transcriptions and images of the major manuscripts, including $\mathfrak{P}^{46}$, without the usual palaeographical details mentioned above.

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[39] For reviews of these items, see J.K. Elliott's review articles on *NovT* 31/4 (Oct 1989): 381-83; and *NovT* 37/3 (July 1995): 302-03.

[40] Klaus Junack, E. Güting, U. Nimtz, and Klaus Witte, *Das Neue Testament auf Papyrus, II Die Paulinischen Briefe, Teil I: Rom., I Kor., II Kor.* (*ANTT* 12; Berlin/New York: de Gruyter, 1989).

[41] Klaus Wachtel and Klaus Witte, *Das Neue Testament auf Papyrus II Die Paulinischen Briefe, Teil 2* (*ANTT* 22; Berlin and New York: de Gruyter, 1994).

[42] Note, however, that wherever multiple papyri are extant $\mathfrak{P}^{46}$ is always cited first, except in pages 59-64 of *teil 1* where $\mathfrak{P}^{27}$ comes first, but thereafter is cited after $\mathfrak{P}^{46}$.

[43] The NA^{26} text is presented verse by verse, but corresponding folio numbers and line numbers of the papyri are indicated, so there is very little chance of getting lost. Corrections, breathing marks, reading marks, *nomina sacra*, *ancoras*, *dicola*, accents, and others are reflected as they are in the papyri.

[44] The apparatus has two sub-sections. The upper bottom part is a commentary on the details of the papyrus in question (often with regard to the corrections made by particular hands), and the lower bottom is a critical apparatus showing significant variants from the papyri and major uncials.

[45] http://ntvmr.uni-muenster.de/en_GB/. *VMR*[1] is no longer operational.

2. *Independent Transcriptions*

Independent publications of major NT manuscripts, either as a transcription or used for comparative purposes, have also been undertaken. First, Comfort and Barrett's *corrected* (print) edition of their *The Text of the Earliest New Testament Greek Manuscripts*[46] includes a transcription of $\mathfrak{P}^{46}$ (*cum* reconstructions) and a brief palaeographical discussion.[47] Second, Carl Jaroš's *Das Neue Testament nach den ältesten Griechischen Handschriften*[48] is a digital resource containing $\mathfrak{P}^{46}$'s transcription (*cum* reconstructions) and an introductory palaeographical discussion.[49] Finally, the (unfinished) *New Testament Greek Manuscript* series of Reuben Swanson[50] cited the readings of $\mathfrak{P}^{46}$ along with the other major NT manuscripts, against Codex Vaticanus.

II. $\mathfrak{P}^{46}$ IN THE CONTEXT OF CANONICAL STUDIES

A. Facts of the Case

What physically remains of $\mathfrak{P}^{46}$ unequivocally generated spirited discussions along the line of canonical studies, for obvious reasons.[51] To better appreciate the discussion, it

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[46] P.W. Comfort and D.P. Barrett, eds., *The Text of the Earliest New Testament Greek Manuscripts: A Corrected, Enlarged Edition of The Complete Text of the Earliest New Testament Manuscripts* (Wheaton: Tyndale House, 2001). The transcription of $\mathfrak{P}^{46}$ is found on pages 208-334.

[47] The palaeographical treatment, however, is a bit stretched. Without reducing their value, the transcriptions, especially the reconstructed portions, are also suspicious at times. For reviews of the original and revised editions, see D.C. Parker, Review of Philip Comfort and David Barrett, *The Text of the Earliest Greek NT Manuscripts* [<http://rosetta.reltech.org/TC/vol04/ComfortBarretted1999rev.html> (accessed 1 Mar 2010)]; and Maurice Robinson, Review of Philip Comfort and David Barrett, *Text of the Earliest Greek NT Manuscripts* [http://rosetta.reltech.org/TC/vol06/CB2001rev.html> (accessed 1 Mar 2010)].

[48] Carl Jaroš, *Das Neue Testament nach den ältesten Griechischen Handschriften die handschriftliche griechische Überlieferung des Neuen Testaments vor Codex Sinaiticus und Codex Vaticanus* (Wien und Würzburg: Echter Verlag, 2006).

[49] It presents an image (of less quality) of $\mathfrak{P}^{46}$, then its transcription, with reconstructions and stichometry, followed by another transcription in standard Greek font and a translation in German. For a review, see D.C. Parker, Review of Carl Jaroš, *Das Neue Testament nach den ältesten Griechischen Handschriften* [<http://rosetta.reltech.org/ TC/vol13/Jaros2008rev.pdf> (accessed 1 February 2010)].

[50] Reuben Swanson, *New Testament Greek Manuscripts: Variant Readings arranged in horizontal lines against Codex Vaticanus,* Romans, 1 Corinthians, 2 Corinthians, and Galatians (Wheaton/Pasadena: Tyndale House/William Carey International University Press); these were all published in 2008.

[51] First, because $\mathfrak{P}^{46}$ reflects a non-traditional book sequencing; second, because $\mathfrak{P}^{46}$ remains the earliest and most extensive extant manuscript containing the Letters of Paul; and lastly,

is appropriate at the outset to outline the facts of the case, with regards to the content of the extant portions of $\mathfrak{P}^{46}$, to wit:

a) The extant papyrus is a single-quire codex,[52] containing fragmentary texts of nine epistles, traditionally ascribed to Paul, in this order: Romans, Hebrews, 1 Corinthians, 2 Corinthians, Ephesians, Galatians, Philippians, Colossians, 1 Thessalonians;
b) In its present state, $\mathfrak{P}^{46}$ starts with a mutilated folio containing fragmentary texts of Rom 5.17-6.3 (verso side) and 6.5-14 (recto side), the paginations of which did not survive; but since pagination of the ensuing pages survived, it can be calculated with a considerable degree of certainty that $\mathfrak{P}^{46}$ was originally a single-quire codex of 51 (or 52) sheets (102 [or 104] leaves), suggesting that six (or seven) leaves before the first page and the corresponding leaves at the end were not preserved;
c) The number of characters and lines per page increases toward the latter part;
d) Στιχος notes are appended at the end of each epistle. However, the figures in the notes are larger than the actual number of lines copied.

These facts spawned different hypotheses; some intricately interrelated, others more independently proffered.[53] Accordingly, they immediately bring to fore the questions of *content* and of *sequence*.

B. The Question of Content

1. *On Hebrews*

We begin with the question of content, and here one is greeted by the fact that Hebrews is included in the corpus.[54] F21^{r}–l^{03} (page ΜΑ) bears the title ΠΡΟC ЄΒΡΑΙΟΥC, indicating the commencement of the text of Hebrews, and culminates at

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because $\mathfrak{P}^{46}$ has to some extent anticipated the modern question of "pseudo-Pauline" letters, now most popularly called as the *Pastoral Epistles*.

[52] Most of the first 10 Beatty leaves were still conjoined when acquired by Mr Beatty, corroborating the view that $\mathfrak{P}^{46}$ was originally manufactured as a single-quire codex. For a detailed discussion on this codicological feature, see pp. 79-86.

[53] Since the issue at hand touches on the Pauline canon in general—a matter already well written about in the vast amount of literature—I, thus, limit this section to studies that directly deal with the issues presented by the evidence of $\mathfrak{P}^{46}$. For a recent exhaustive treatment of this matter, see the various articles in S.E. Porter (ed.), *The Pauline Canon* (Pauline Studies I; Boston/Leiden: Brill, 2004).

[54] $\mathfrak{P}^{46}$ is not the only manuscript with Hebrews in the Pauline collection. Kenyon, *CBBPIII-1936*, xi, already noted that MS 1919 has the same arrangement, and that other manuscripts have it either after Thessalonians but before the Pastorals (e.g., ℵABC and Bohairic version) or after the Pastorals or at the conclusion of the thirteen-fold Pauline letters (e.g., D [Paul]). For a fuller discussion, see W.H.P. Hatch, "The Position of Hebrews in the Canon of the New Testament," *HTR* 29/2 (Apr 1936): 133-51. Furthermore, the value of the Hebrews text in $\mathfrak{P}^{46}$ lies also in the amount of the preserved text, for it is comparatively more extensive than other early manuscripts with Hebrews, substantially supplementing an expanded textual base for the analysis of its text.

f38[v] (page **οα**). The presentation is not unusual, but the placement is, for it immediately follows Romans.[55]

William Hatch's 1936 article remains to date the most extensive study of the various locations of Hebrews in the manuscript tradition,[56] identifying at least three locations: a) *among* the epistles addressed to churches, b) *after* the epistles written to churches, and c) at the *end* of the Pauline canon.[57] Categorising $\mathfrak{P}^{46}$ with the first class,[58] he explained that the configuration of evidence is indicative of the provincial origin of the collection reflected by $\mathfrak{P}^{46}$ (i.e., Egypt and Syria),[59] a view earlier insinuated by Kenyon.[60] In short, $\mathfrak{P}^{46}$ reflects the Alexandrian verdict.[61]

Charles Anderson took another step further. Arguing that the evidence[62] for the view that the Pauline corpus and Hebrews circulated independently of each other until the latter's incorporation in the former is inconclusive, he maintained that the exclusion of Hebrews from Marcion's list is predictable since Marcion "had good reason to reject

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[55] The recently catalogued $\mathfrak{P}^{126}$ is believed to also have an unusual placement of Hebrews among the Pauline Epistles; see Claire Clivaz, "A New New Testament Papyrus: $\mathfrak{P}^{126}$ (PSI 1497)," in *Early Christianity* 1 (2010): 158-62, pp. 158-59. The manuscript is too fragmentary, though, to confirm this theory.

[56] See also a summarization, with some updates, of Hatch's article by Bruce Metzger, *A Textual Commentary on the Greek New Testament* (2nd edition; Stuttgart: UBS, 1994), 591-92.

[57] Hatch, "Hebrews in the Canon of the NT," 133. This textual schema, with some updating, was adopted by Paul Ellingworth, *The Epistle to the Hebrews: A Commentary on the Greek Text, NIGTC* (Grand Rapids, MI/Carlisle: Eerdmans/Paternoster, 1993), 6-7.

[58] Hatch, "Hebrews in the Canon of the NT," 133, listed the following in the same class: six minuscules (103, 455, 1961, 1964, 1977, and 1994) as well as in a Syrian Canon dated about 400 A.D.

[59] Shared by Jack Finegan, "The Original Form of the Pauline Collection," *HTR* 49/2 (1956): 85-103, p. 94.

[60] Kenyon, *CBBPIII-1936*, xi, described the inclusion and position of Hebrews in $\mathfrak{P}^{46}$ as "a proof of the high importance assigned to it, and of the unquestioning acceptance of its Pauline authorship"; see also, Frank Beare, "The Text of the Epistle to the Hebrews in $\mathfrak{P}^{46}$," *JBL* 63/4 (Dec 1944): 379-96, p. 381. Surprisingly, despite the fact that the Michigan leaves confirmed the presence of Hebrews in $\mathfrak{P}^{46}$, Sanders did not make any statement regarding the inclusion and placement of Hebrews in $\mathfrak{P}^{46}$.

[61] Almost virtually affirming the Pauline origin of Hebrews, Hatch, "Hebrews in the Canon of the NT," 136, nonetheless quickly qualified that "... (Hebrews) stood among the letters written to churches as early as the third century. If it had not been recognised as in some sense a work of the Apostle, it would not have been given this place in the Pauline corpus. However, it occupied this position only in the East; for in this early period it was not accepted as canonical in the West." See also, Kenyon, *CBBPIII-1936*, xii.

[62] Charles Anderson, "The Epistle to the Hebrews and the Pauline Letter Collection," *HTR* 59/4 (1966): 429-38, referred to Clement of Alexandria, Origen, and $\mathfrak{P}^{46}$ representing the Egyptian/Eastern collection vis-à-vis the canons of Muratori and Marcion as well as 1 Clement representing the Roman/Western collection.

Hebrews" because of its strong association with Jewish thought, something that Marcion was very diligent to abhor.[63] He also explained that its presence in $\mathfrak{P}^{46}$ is a "clear testimony not only to Egyptian acceptance of Hebrews as Pauline, but also to the fact that Hebrews was at that time *in* the Pauline corpus".[64] He then associated $\mathfrak{P}^{46}$ with the testimonies of Clement and Origen proposing that the inclusion "is strong evidence that the Pauline corpus known to Origen and Clement did contain Hebrews, and that they were not arguing for its inclusion but for its right to the place which it already held."[65] Ultimately, Anderson suggested that "Hebrews may have gained admittance to the canon through association with a Pauline letter or letters prior to the formation of the corpus as a whole."[66] Admittedly, Anderson's article is a subtle way of approaching alternatively the difficult question of Hebrews' inclusion in the Pauline collection. His proposal, however, is problematic on certain accounts.[67]

On another tack, Jerome Quinn interpreted the inclusion of Hebrews as something predicated by *practical* rather than *theological* necessities,[68] advancing the hypothesis that the collection of books in $\mathfrak{P}^{46}$ does not point to a Pauline canon as

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[63] Anderson, "Hebrews and the Pauline Letter Collection," 434.

[64] Anderson, "Hebrews and the Pauline Letter Collection," 432. Emphasis his.

[65] Anderson, "Hebrews and the Pauline Letter Collection," 432. Cf. Charles Buck, "The Early Order of the Pauline Corpus," *JBL* 68/4 (Dec 1945): 351-57, p. 356, who suggested that the inclusion of Hebrews and the book arrangement in $\mathfrak{P}^{46}$ "is not a survival of the primitive order but a new development which became possible only when the codex supplanted the roll."

[66] Anderson, "Hebrews and the Pauline Letter Collection," 438.

[67] First, Anderson failed to distinguish, or at least he was not clear, between the concepts of the *original* letters of Paul and the *archetype* of the collected Pauline corpus; he seems to confuse the two. Second, the proposal also bears the burden of explaining the absence of reference from other apostolic fathers who were contemporaries of Clement, particularly Ignatius of Antioch and Polycarp of Smyrna, if there was indeed widespread recognition in the East about the place of Hebrews in the Pauline corpus and ultimately its Pauline origin. Accordingly, the view of Zuntz, *TEDCP*, 15-16, is instructive on this regard: "The Epistle to the Hebrews cannot from the first have been transmitted as an authentic part of the primitive *Corpus Paulinum*, otherwise the absence of references to it in Ignatius and Polycarp would be unaccountable; but since in the Chester Beatty papyrus and in all other manuscripts of the Epistles, it must in some way have early been added to the Greek *corpus* and shared its tradition". See also, F.F. Bruce, "Textual Problem in the Epistle to the Hebrews," in *Scribes and Scriptures: New Testament Essays in Honour of J. Harold Greenlee* (ed. D.A. Black; Winona Lake: Eisenbrauns, 1992), 27-39, p. 27.

[68] Jerome Quinn, "$\mathfrak{P}^{46}$—The Pauline Canon?" *CBQ* 36 (1974): 379-85.

such but simply preserves a widely accepted practice of gathering books together constitutive of Paul's "letters to churches" as opposed to "letters to individuals". He then postulated that the Pastorals and Philemon were not included in $\mathfrak{P}^{46}$ for the simple reason that they appropriately belong to the "letters to individuals".[69] But he quickly qualified that "(T)here is no evidence that the one who compiled this manuscript thought that he had assembled *the* Pauline canon of his time".[70] He interpreted the collection of $\mathfrak{P}^{46}$ as an evidence of a ten-fold Pauline letter collection circulating in Egypt at the turn of the second century, and suggested that the principle of *sequencing* the books was based on stichometric considerations, at which point, he appealed to Finegan's conclusion[71] (see related discussion below).

Quinn's proposal is yet again an innovative attempt to explain the presence of Hebrews (and the absence of the Pastorals) in $\mathfrak{P}^{46}$. In a sense this is a compromise attempt to account for the equally important (canonical) status of the Pastorals.[72] But Quinn's proposal, whilst innovative, suffers from circular arguments, and this therefore renders his proposal suspect.[73] Furthermore, his lack of data directly from

[69] Here Quinn appealed to the view of Kenyon to corroborate his argument, without presenting any evidence or at least an expressed assessment of Kenyon's proposal.

[70] Quinn, "$\mathfrak{P}^{46}$—The Pauline Canon?" 379, 385. Emphasis added.

[71] Finegan, "Original Form of the Pauline Collection," 101, argued, "… as far as present evidence goes, New Testament stichometry began with the epistles of Paul and is older than our oldest extant collection of those epistles. The fact of this very early application of precise methods of length-measurement to the Pauline epistles is in harmony with the theory that considerations of the length of the epistles were from the first the chief principle governing the order in which they were arranged in the collection." However, Finegan's statement in pp. 99-100 seems more relevant to the issue: "Since a uniform figure is not obtained from the data found in $\mathfrak{P}^{46}$, it seems unlikely that this stichometry was first worked out on the basis of this manuscript. Rather the figures appear to represent the transcription to this papyrus of an even earlier set of figures, the precise basis of which cannot at this time be recovered".

[72] Quinn, "The Pauline Canon," 384, suggested, "The scribe of $\mathfrak{P}^{46}$ intended a collection of Pauline letters to the churches. His inclusion of Hebrews witnesses to this concern. There is no evidence that he had any explicit theological criterion that demanded either the inclusion or the exclusion of Philemon or the Pastorals from his codex. He was not compiling *the* Pauline canon". (Emphasis his).

[73] There are two grounds for this: first, his proposal that the Pastorals were excluded from $\mathfrak{P}^{46}$ in view of their nature as "letters to individuals" (as opposed to the inclusion of Hebrews by virtue of its being a "letter to the church") is to a large extent built on Kenyon's inconclusive

the evidence of $\mathfrak{P}^{46}$ militates against his theory. The interesting point he raised, however, is the proposal that *theological* requirements in the collection of Paul's letters intersected with *technological* considerations, i.e., the use of the codex format in the collection of Christian literature in general and the Pauline letters in particular.[74]

2. On the Pastorals

2 Thessalonians is not preserved in $\mathfrak{P}^{46}$ but calculations of its number of characters against the allowable number of characters in the missing pages indicate strongly that it must have been originally included in our papyrus.[75] Hence, Kenyon consistently maintained that $\mathfrak{P}^{46}$ originally terminated after 2 Thessalonians and the remaining pages were left blank. Sanders questioned this and counter-proposed that Philemon and an abbreviated form of the Pastorals might have occupied the lost pages. Nonetheless, the critical consensus swayed toward Kenyon's proposal, including some of the more prominent textual scholars of our time.[76]

Recently, Jeremy Duff cast a challenge against what he called a "misleading consensus", proposing that the increasing number of characters in the last third of the codex indicates *scribal intention* to include the Pastorals in its quire proper, albeit an unsuccessful attempt. Such a proposal should be seriously reconsidered for it has direct consequences for the formation, collection, and canon of the *Corpus Paulinum.* Nevertheless, despite the good intention to include the Pastorals, Duff's proposal suffers methodologically. By the same token, the traditional Kenyon proposal is

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hypothesis that the scribe deliberately did not include the Pastorals; and second, that the book arrangement in $\mathfrak{P}^{46}$ is stichometric is similarly founded on Finegan's unproven hypothesis.

[74] Quinn, "$\mathfrak{P}^{46}$—The Pauline Canon?" 383-84. See related discussion in pp. 204-35.

[75] The proposed calculation formulas for the missing leaves at the beginning and ending of the codex vary (see Table 3-E1 in p. 207). Nonetheless, all these formulas support the observation.

[76] Early adherents of this proposal include Beare, "Text of the Epistle to the Hebrews in $\mathfrak{P}^{46}$," 379-96. For a list of other adherents, see Duff, "$\mathfrak{P}^{46}$ and the Pastorals," 579, n3-4.

equally problematic. Hence, a re-investigation of the evidence (or the lack of it) is not only essential but imperative, especially because the witness of $\mathfrak{P}^{46}$ may prove pivotal in (un-)settling the canonical question of the Pastorals.[77]

It belongs to another study to give definitive words as to the Pauline authorship of Hebrews and the (non-)inclusion of the Pastorals in $\mathfrak{P}^{46}$. Given the limited evidence, I shall content myself with the following thoughts. In the case of Hebrews, perhaps we are in no way nearer to resolving the question of authorship than in the time of Origen, who after giving his opinion on the matter rhetorically asked, "Who in fact did write (Hebrews), God only knows."[78] Indeed we do not know, but what we do know is that the placement of Hebrews in $\mathfrak{P}^{46}$ is something unique that should continue to exercise the mind of those who want to approach the issue of canon from the standpoint of manuscript evidence. In the case of the Pastorals, the more circumspect approach is not to dogmatise unnecessarily the evidence of $\mathfrak{P}^{46}$, for the evidence it offers is indeed very scanty to settle the question categorically.[79]

C. The Question of Sequence

Why did the scribe (re-)locate Hebrews in between Romans and 1 Corinthians in his codex? Why did he put Ephesians before Galatians?[80] Did he adopt a principle of book

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[77] As this question is distinct to $\mathfrak{P}^{46}$, a whole section is dedicated to this subject; see pp. 204-35.

[78] Eusebius, *Historia Ecclesiastica*, 6.25.13-14.

[79] As Epp, "Issues in the Interrelation of New Testament Textual Criticism and Canon," in *The Canon Debate* (eds. L.M. McDonald and J.A. Sanders; Peabody, MA: Hendrickson, 2002), 485-515; repr. in *PNTTC*, 595-639, 619, intimated, "We might all wish that $\mathfrak{P}^{46}$ provided the definitive answer to the presence or absence of the Pastorals in our earliest manuscript of the Pauline letters, but so far it does not." Similarly, Robert Grant, *A Historical Introduction to the New Testament* (London: Collins, 1963), 210-11, confided, "... while we definitely know that the Pauline epistles were arranged peculiarly in the Beatty codex, we do not know that they did not include the Pastorals".

[80] A school of thought, enunciated by Edgar Goodspeed and John Knox, advanced the view that the Pauline collection was first compiled in Ephesus and that the epistle to the Ephesians was the "cover letter" for the whole corpus; see E.J. Goodspeed, *Introduction to the New Testament* (Chicago: University of Chicago Press, 1937), 210-39; John Knox, *Marcion and the New Testament* (Chicago: University of Chicago Press, 1942), 53-73. For a contrary view, see Buck, "The Early Order of the Pauline Corpus," 351-57.

arrangement that is indicative of theological presuppositions? Did the scribe simply fulfil the conventional requirement of book-production of his time insofar as book arrangement in his codex is concerned?

Not a few scholars suppose that the book sequence in $\mathfrak{P}^{46}$ was based on the stichometric principle,[81] that is, the letters were arranged in their decreasing order of length.[82] This convention certainly antedated the Christian movement; hence, we must inevitably assume that this must have functioned in the Christian book production in such a way that is *somewhat* reflective of its history of prior use in the Greek literary domain.[83] Metzger offers four general functions of stichometry: a) to show the length of a treatise or book, b) to provide a standard for payment to the scribe and the pricing of the book, c) to guard against later interpolations and excisions, and d) to permit, through the notation of the *στιχοι* by fifties, the general location of citations.[84]

It is widely held that the presence of stichometric notes in manuscripts is in some way related to the dynamics of the scribal trade, i.e., scribal compensation for services rendered. However, whilst remuneration is an essential professional publishing protocol at the time, it is not implausible that there was something more

[81] Those who supported this view include Anderson, "Hebrews and the Pauline Letter Collection," 432; C.C. McCown, "Codex and Roll in the New Testament," *HTR* 34 (1941): 219-49, p. 245. Bruce Metzger, *Manuscripts of the Greek Bible: An Introduction to Greek Palaeography* (Oxford: OUP, 1981), 64, explained: "The sequence of the Epistles, with Hebrews immediately after Romans, seems to have been dictated in accord with the decreasing lengths of the Epistles"; Idem, *The Text of the New Testament: Its Transmission, Corruption, and Restoration* (3rd edition; Oxford: OUP, 1993), 38. For a visual table of all the extant στιχος notations in $\mathfrak{P}^{46}$, see Appendices B-1 and B-2, pp. 408-11.

[82] This is one physical feature of $\mathfrak{P}^{46}$ that deserves serious thought, especially because $\mathfrak{P}^{46}$'s stichometric notations is the earliest surviving example of NT stichometry, providing a glimpse on how this book production convention was used in earlier times outside of the Greek literary context.

[83] The studies of John Rendel Harris, "Stichometry Part I," *AJP* 4/2 (1883): 133-57; Idem, "Stichometry Part II," *AJP* 4/3 (1883): 309-31; and Charles Graux, "Nouvelles recherches sur la stichométrie," *Revue de philologie de littérature et d'histoire anciennes* 2/2 (Apr 1878): 97-143, although dated already, are still very instructive and worth reviewing when it comes to this matter.

[84] Metzger, *Manuscripts of the Greek Bible*, 39.

to it than just compensation, especially in the Christian context.[85] If this is permitted, then it is not unlikely that these stichometric notes, in a Christian literary context, fulfil multilevel functions, which included, but are not limited to, the assurance that the manuscript is content-wise complete. Stretching this a bit more, it is possible also that this convention had to do with book arrangement in the collection, as suggested by not a few scholars, foremost of whom is Jack Finegan, who proposed that "...the entire Pauline collection was probably contained in a single codex,... and that the principle of relative length is the simple and sufficient clue to (the) order."[86] If this hypothesis is admitted, then our scribe may indeed have followed the principle of decreasing length according to the number of στιχος.

Having said thus, however, I must hasten to add that stichometry as the "governing principle" behind the book arrangement in $\mathfrak{P}^{46}$ seems an inconclusive solution to the conundrum and leaves many questions unanswered. For instance, it remains unexplained why Hebrews is shorter than 1 Corinthians yet was placed before it (i.e., Hebrews has 33 ½ pages while 1 Corinthians has 45 ½ pages [ϹΤΙ$^{\chi}$ ψ =700]), which directly contradicts this presumed "governing principle". The same observation holds true[87] for Ephesians and Galatians. How is this discrepancy to be

[85] There must have been a nobler reason beyond the convention of scribal fees, especially in the context of Christian book production. It is not inappropriate to assume that a certain degree of "devotion" was also in operation even among Christian "professionals" who copied *their* Scriptures, though this should not be dogmatised. This cannot be readily dismissed especially if stichometry also functioned to ascertain the textual completeness of the manuscript being produced. On this devotion, see Harry Gamble, *Books and Readers in the Early Church: A History of Early Christian Texts* (New Haven/London: Yale University Press, 1995), 277, n129.

[86] Finegan, "Original Form of the Pauline Collection," 95.

[87] As far as the scribe who appended the stichometric note is concerned, Galatians is longer than Ephesians, i.e., Ephesians (ΤΙϹ=316) vis-à-vis Galatians (ΤΟΕ=375). David Trobisch, "Structural Markers in New Testament Manuscripts, with special attention to observations in Codex Boernerianus (G 012) and Papyrus 46 of the Letters of Paul," in *Lay-out Markers in Biblical Manuscripts and Ugaritic Tablets* (eds. M. Korpel and J.M. Oesch; Pericope 5; The Netherlands: Royal Van Gorcum, 2005), 177-90, p. 184, contested Kenyon's reading of the στιχος for Galatians, arguing that it should be transcribed as ΤΙϵ (=315) and not ΤΟϵ (=375), which would mean that the placement of Galatians *after* Ephesians is in accord with the

satisfactorily explained then?[88]

Anderson pointed to pragmatic considerations to account for the flaw in the sequence, i.e., to avoid separating the Corinthian letters.[89] David Trobisch threw his support behind this proposal.[90] But it does not hold water either.[91] On the contrary, it only exposes the proposal's handicap, for in the first place Hebrews could have been located *after* 2 Corinthians, as that action would more logically satisfy length requirement without provoking the question of separation of the Corinthian correspondences.

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arrangement principle of decreasing length. However, Trobisch's proposal is palaeographically unsustainable. Based on comparative analysis of other *iotas* in the extant στιχοι, reading the middle character of the *στιχος* number for Galatians as an *iota* is palaeographically against the evidence—*iota* is a single downward stroke with a flattened descender, whilst the letter at issue undoubtedly has more than one stroke due to the thickening of the ink, as well as a closed looping at the bottom indicating that the pen nib formed an upward stroke at that point (see Fig. 2.1). Kenyon's *omicron* reading may have been due to its proximate similarity in formation with that of the first letter (*sigma*) of the στιχος note for Ephesians (Fig. 2.2).

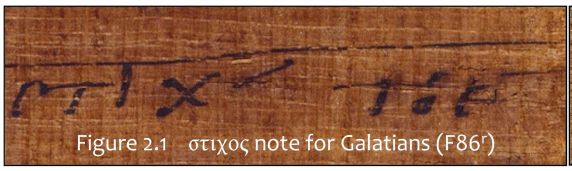


Figure 2.1 στιχος note for Galatians (F86ʳ)

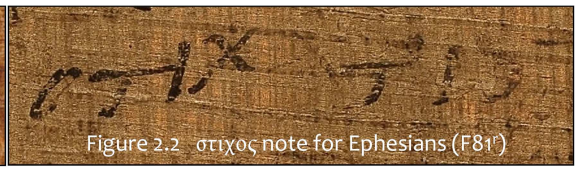


Figure 2.2 στιχος note for Ephesians (F81ʳ)

[88] Craig Koester, *Hebrews*, *ABC* (New York: Double Day, 2001), 21, suggested that the issue might be appreciated from the standpoint of the authorship: "Christians in the East... believed that Paul was the author of Hebrews... ($\mathfrak{P}^{46}$) placed Hebrews after Paul's letter to the Romans, perhaps because of its length and the mention of Italy in Heb 13.24. Christians in the East probably based their understanding of authorship on inferences from Hebrews, rather than on traditions about the author's identity". But this, too, is unsustainable, since it has not taken account of the Ephesians-Galatians arrangement problem. Unfortunately, Koester's discussion was limited to Hebrews.

[89] Anderson, "Hebrews and the Pauline Letter Collection," 432, argued, "In ($\mathfrak{P}^{46}$) the epistles are arranged according to their respective lengths, with Romans first, Hebrews second, 1 Corinthians third, and so on. Whilst Hebrews in fact falls between 1 and 2 Corinthians in length, it was placed before both in $\mathfrak{P}^{46}$, probably *in order to avoid separating the Corinthian letters*". (Emphasis added).

[90] David Trobisch, *Paul's Letter Collection: Tracing the Origins* (Missouri: Quiet Waters, 2001), 17, argued, "Ephesians is longer than Galatians. Hebrews is shorter than 1 Corinthians but longer than 2 Corinthians. Nobody would want to separate the two letters to the Corinthians. An easy solution is to let Hebrews stand before 1 Corinthians. And that is exactly the sequence of letters in $\mathfrak{P}^{46}$. So the producers of $\mathfrak{P}^{46}$ arranged the letters of Paul strictly according to their length..."

[91] Also noted by Tom Dykstra, "A Review of David Trobisch and David Parker on the Origin of the New Testament, the Historical Jesus, and How Manuscripts can Reveal what Texts Conceal," *Journal of Orthodox Center for the Advancement of Biblical Studies* 2/1 (2009) [<http://ocabs.org/journal/index.php/jocabs/article/viewFile/41/16> (accessed 30 Aug 2012)], 19, par. 3.

The problem is further complicated by the fact that the extant *στιχοι* are higher than the actual number of lines in $\mathfrak{P}^{46}$.[92] Stated differently, the stichometric notes are not precise descriptions of the actual lines in this manuscript![93] Sanders already noted this, and suggested that they might have been padded.[94] Furthermore, when compared against other younger manuscripts, $\mathfrak{P}^{46}$'s *στιχοι* are higher.[95] Complications thus come to view, and more questions arise.[96]

Others suggested that the *στιχοι* in $\mathfrak{P}^{46}$ are not meant to be taken literally. For instance, Finegan conceded that these notes do not refer to the actual count *in* $\mathfrak{P}^{46}$ per se, but are representative of "an even earlier set of figures" that was transcribed *into* $\mathfrak{P}^{46}$.[97] Whilst this is possible, the absence of corroborating evidence from other manuscripts of similar provenance and age makes it nothing more than a hypothetical possibility in the meantime.

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[92] This is not distinct to $\mathfrak{P}^{46}$, as already pointed out in 1883 by Harris, "Stichometry I," 134, with regard to MS N-103 at the National Library of Madrid, whose *stichos* notation was also irreconcilable with the actual lines in that manuscript.

[93] Similarly, Harris, "Stichometry I," 148, noted, "Precisely as in the case of total stichometry, we find that these MS notes have no special connexion with the lines or verses of the documents in which they occur; they refer either to older copies, or to fixed and uniform measurements, perhaps to both."

[94] Sanders, *TCPC*, 21-22, and Kenyon, *CBBPIII-1936*, xiii. Of course, this assumes that the stichometric notes were satisfying commercial purposes. Similarly, a manuscript containing a canon list supposedly representing mid-fourth century materials, first brought to public attention by Theodor Mommsen from a private library in Cheltenham, reflects an annotation complaining about the "avarice for gain" apparently referring to the padding of *stichoi*: "*Quoniam indiculum versuum in urbe Roma non ad liquidum, sed et alibi avaritiae causa non habent integrum, per singulos libros computatis syllabis posui numero XVI versum Virgilianum omnibus libris numerum adscribsi*" (Since the index of lines in the city of Rome is not clearly given, and elsewhere also through avarice for gain they do not preserve it in full, I have gone through the books singly, counting sixteen syllables to the line, and have appended to every book the number of the Virgilian hexameters). On this, see Edwin Preuschen, *Analecta: Kürzere Texte zur Geschichte der Alten Kirche und des Kanons, zusammengestellt von Erwin Preuschen* (Leipzig: Mohr, 1893), 138-40; and William Sanday, "The Cheltenham List of the Canonical Books of the New Testament and of the Writing of Cyprian," *Studia Biblica et Ecclesiastica* (Vol. III; Oxford: Clarendon, 1891): 217-325.

[95] For comparisons, see Sanders, *TCPC*, 21-22, and Kenyon, *CBBPIII-1936*, xii-xiii.

[96] For instance, "Was stichometry in the pre-Christian era, especially in the Greek literary context, ever used as a principle for book arrangement in a multi-book collection?" or "Were the stichometric notes referring to the actual number of lines in the manuscript or were they derived from a certain 'standard of length' which were known to the ancients but no longer available at our disposal?" The list of questions goes on, and as answers are offered, more questions are raised.

[97] Finegan, "Original Form of the Pauline Collection," 101.

Resolution of this question is not in sight. Wisdom dictates admitting that the locations of Hebrews and Ephesians in $\mathfrak{P}^{46}$ are anomalies in the textual tradition that defy the principle of stichometry. But there remains a piece of evidence rarely taken into account: the one who appended the stichometric notes, judging by the characteristics of the handwriting, was most certainly *not* the one who copied the *exemplar*, and therefore *did not* arrange the books in this collection. This hand appended the notes *after* the whole manuscript was completed.[98] This evidence should give us an essential hint about the complexity of approaching the matter strictly from a stichometrical perspective. A more extensive study aimed at mining the data derivable from this evidence is therefore in order. Along this line, it might be worthwhile to revisit the suggestion of Charles Buck, who whilst also subscribing to stichometrical principle was very perceptive of other variables at work, particularly the role of "technology" (i.e., the codex format")[99] in the transmission of sacred texts.[100] If Buck's theory holds water, we are then witnessing here a snippet of the social history of the nascent church in view of their Scripture requirement, particularly the emerging intersection of technological development (codex format) and theological dynamics (Scripture collection) in the formation-transmission of the early Christians' sacred Scriptures.[101]

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[98] If Kenyon's estimation is to be trusted, this hand is a third century scribe, perhaps a contemporary of the first hand; see also Zuntz, *TEDCP*, 253.

[99] Buck, "The Early Order of the Pauline Corpus," 356-57, argued, "(T)he arrangement by decreasing length probably first appeared when the adoption of the codex made it possible, and this occurred in all probability not long before the beginning of the third century".

[100] Reacting against the Goodspeed's (and Knox's) "cover letter" theory, Buck, "The Early Order of the Pauline Corpus," 356, asserted, "It may be argued that a Pauline corpus in which the letters are arranged by decreasing length is attested as early as the first half of the third century by the Chester Beatty Papyrus. The order of that corpus is a species of the familiar order of later MSS and printed Bibles, in which not only are I and II Corinthians and I and II Thessalonians treated as four separate letters instead of only two, as formerly, but Hebrews also appears… This arrangement, like all the later arrangements which resemble it, is not a survival of the primitive orders but a new development which became possible only when the codex supplanted the roll".

[101] See relevant discussion on the bibliographical importance of $\mathfrak{P}^{46}$ in pp. 68-87.

III. THE TEXTUAL CHARACTER OF $\mathfrak{P}^{46}$

A. Kenyon's Classification

Kenyon described the preponderant textual affinity of $\mathfrak{P}^{46}$ as tending more toward the Alexandrian text.[102] Interestingly, despite this seeming positive valuation, he closed his textual assessment with a caveat, "($\mathfrak{P}^{46}$) affects the balance of evidence in many cases; and while it can by no means claim a predominant authority..., it shows that the margin of doubt in details is greater than was supposed, and that the exercise of critical judgement and the search for further evidence are still required."[103]

More than once did Kenyon explicitly state that $\mathfrak{P}^{46}$, singularly or with the other Chester Beatty papyri, generally confirms the integrity of the NT text we received, and that $\mathfrak{P}^{46}$ "offers no sensational variants".[104] The first claim is understandable, as we mentioned in the previous chapter;[105] it is the latter that evokes questions. If by "sensational variants" Kenyon meant something theologically aberrant, then by all means $\mathfrak{P}^{46}$ is a faithful witness to a conservative textual tradition. However, if he meant something that betrays *alternative interpretations* of seemingly established readings, then $\mathfrak{P}^{46}$ offers a number of "sensational variants", for it offers readings that are otherwise contrary to the rest of the textual tradition. These alterations are admittedly a small proportion but they are essential for interpretation, and it is this smaller portion which sets out $\mathfrak{P}^{46}$ from the rest; for this we must seek an explanation, because it is now

[102] Kenyon, *CBBPIII-1936*, xvii, described $\mathfrak{P}^{46}$'s textual relationship as ranging closely "with the Alexandrian rather than with the Western group... (although) there remains a respectable minority of agreements with the Western group", and henceforth concluded that "while the Alexandrian group is on the whole the most trustworthy authority for the text of the NT, readings supported by the Western group are at times to be preferred, and should receive consideration on their merits". Most recently, this observation was essentially re-affirmed by James Royse, "The Early Text of Paul (and Hebrews)," in *The Early Text of the New Testament* (eds. C.E. Hill and M. J. Kruger; Oxford: OUP, 2012), 175-203, esp. 181, 202.

[103] Kenyon, *CBBPIII-1936*, xxii.

[104] On $\mathfrak{P}^{46}$, Kenyon, *CBBPIII-1936*, xxii; with the rest of the Beatty papyri, Idem, *CBBPIntro*, 15; also, Idem, *The Bible and Modern Scholarship*, 18-21.

[105] See pp. 7-10, for the *confirmative* value of $\mathfrak{P}^{46}$ in establishing the text of the Pauline Epistles.

acknowledged that in the transmission history, thousands of textual alterations may have emerged because of the theological and other scribal preferences,[106] and all these must be taken into account in exegesis and doctrinal articulation. For this reason alone, this study, in the ensuing chapters, will attempt to identify passages where the kind of text exhibited by $\mathfrak{P}^{46}$ contributes in appreciating its textual configuration, going beyond its designation as simply ranging toward one traditional textual group or "texttype".

B. Günther Zuntz's work on the text of $\mathfrak{P}^{46}$

When we talk of the text of $\mathfrak{P}^{46}$, we cannot but talk highly of Zuntz's contributions. Few will ever dispute that Zuntz's *The Text of the Epistles* is by far the most insightful study of the text of $\mathfrak{P}^{46}$, providing a model on how to employ reasoned eclecticism as a most viable method in arriving as close as possible to the "earliest text" of the Pauline corpus.

Zuntz, against the methodological tide at the time, started with $\mathfrak{P}^{46}$, "the oldest manuscript", describing it as "the decisive material accession"[107] to our resources, and used it as the standard against which all other manuscripts are to be assessed. The result is an avant-garde example of how NT textual criticism is both science and art integrated. And despite the fact that more than half a century has elapsed already, Zuntz's project remains the magisterial work on the Hebrews and 1 Corinthians texts of $\mathfrak{P}^{46}$ and his text-critical judgments on its readings enjoy continuing authority. Hardly anyone will dispute that Zuntz's work on $\mathfrak{P}^{46}$ is an *obra maestra* in its own right. In a nutshell, Zuntz cogently demonstrated that $\mathfrak{P}^{46}$—together with B, 1739,[108] Sahidic and Bohairic versions, Clement of

[106] See related discussion below, pp. 32-36.

[107] Zuntz, *TEDCP*, 11.

[108] But cf. Stephen Carlson, "The Text of Galatians and Its History," PhD Dissertation, Duke University, 2012, 324-26, who argued that, in contrast with Zuntz's assertion, 1739 "hardly ever joins" secondary Alexandrians, and that in Galatians 1739 is more related to common ancestor of Ψ and the Byzantine text as well as the secondary Alexandrians, namely ACP and 1241^{s}. This discrepancy in

Alexandria, and Origen—represents a very early form of text which he described as "proto-Alexandrian" and in many instances comes close to the Pauline archetype—courtesy of "Alexandrian philologists". The frequent reference to Zuntz's work in the ensuing chapters attests to the importance of his contribution in assessing the value of $\mathfrak{P}^{46}$ for text-critical work.[109] His influence is such that any further research on $\mathfrak{P}^{46}$ will need to seek his enlightened guidance with regard to this ancient artefact.[110] But Zuntz would certainly be the first to protest if the research inquiry stops with him, for he consciously welcomed further researches on the whole text of $\mathfrak{P}^{46}$, since he himself acknowledged that his work is only an *initial attempt*, labouring on and drawing some conclusions only from Hebrews and 1 Corinthians texts of $\mathfrak{P}^{46}$.[111] Needless to say, whatever foundation he laid down now needs to be built on.

IV. $\mathfrak{P}^{46}$ IN THE CONTEXT OF SCRIBAL STUDIES

Scribal studies, once a comparatively insignificant component of NT textual studies,[112] has now grown to become a field of its own, producing works, in the last fifty years or

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conclusions underscores the pressing necessity of undertaking a textual valuation of the text of $\mathfrak{P}^{46}$ in its entirety than selectively.

[109] On the significance of Zuntz's contributions, see Eberhard Güting, "The Methodological Contribution of Günther Zuntz to the Text of Hebrews," *NovT* 48/4 (2006): 359-78; and Michael Holmes, "The Text of the Epistles Sixty Years After: An Assessment of Günther Zuntz's Contributions to Text-Critical Methodology and History," in *Transmission and Reception: New Testament and Exegetical Studies* (TS, Third Series 4; eds. J.W. Childers and D.C. Parker; Piscataway, NJ: Gorgias Press, 2006), 89-113. Holmes' catchy caricature is worth quoting, "If a handbook such as Metzger's offers a pedagogically sound introduction, the reading of *Text of the Epistles* is a graduate course on the subject" (p. 94). For an earlier review of Zuntz's *TEDCP*, see R.V.G. Tasker, "The Text of the *Corpus Paulinum*," *NTS* 1/3 (1955): 180-91, p. 191, who concluded his review with a very positive note: "… no student of the text or of the language of the *Corpus Paulinum* can afford to neglect this erudite and lucid piece of scholarship."

[110] A random glance at how critical commentaries on 1Cor and Heb frequently quoted from Zuntz further corroborates this impression. Most recently, in his PhD dissertation, Stephen Carlson used Zuntz's theoretical framework as a major core in his attempt to reconstruct the text of Galatians.

[111] Zuntz, *TEDCP*, 17, explained, "In order not to be drowned in detail we shall confine our examination, in the main, to 1 Corinthians and Hebrews, leaving it to others to amplify and correct the conclusions to which the evidence thus limited may lead us".

[112] This side-lining of the scribes of the manuscripts was largely due to the dominant view that textual variants are primarily a tool to establish the text of the *autographs*; on this analysis, see Ulrich Schmid, "Scribes and Variants—Sociology and Typology," in *Textual Variation: Theological and Social*

so, that have spawned other equally important studies. As Juan Hernández rightly observed, focus on the scribe has now constituted a "genre" of its own.[113]

A. The *Tendenzkritik* of $\mathfrak{P}^{46}$

1. Early Studies on Theological Tendencies and the Epp Model

Herman Hoskier is among the first ones who carried out serious studies on $\mathfrak{P}^{46}$.[114] His first article is significant for our purposes as it allocated an appreciable amount of discussion to the scribe of $\mathfrak{P}^{46}$. Among other things, he noted that the scribe committed a large number of major omissions. Despite this, Hoskier was sympathetic to its scribe, describing him as "not a careless ignoramus, nor somnolent, nor inept, for most scribes... are honourable copyists, and he is not an exception."[115] Hoskier also included discussions of certain readings in $\mathfrak{P}^{46}$ that, in his estimation, have doctrinal implications[116]—a line of thought anticipating the interests of later researches (e.g., Eshbaugh, Ehrman).

Tendencies? Papers from the Fifth Birmingham Colloquium on the Textual Criticism of the New Testament (TS, Third Series 6; eds. H.A.G. Houghton and D.C. Parker; Piscataway, NJ: Gorgias Press, 2008), 1-23, pp. 1-2.

[113] Juan Hernández, Jr., *Scribal Habits and Theological Influences in the Apocalypse* (WUNT 2. Reihe 218; Tübingen: Mohr Siebeck, 2006), 28, "No longer are contemporary textual critics concerned primarily with the quest for the *Urtext* (though this remains a critical presumption in much of their work). Now the very habits of the scribe, once used to get back to the original wording of a passage and construct critical editions of the NT, are used to "reconstruct" the scribe and inform our understanding of his/her? scribal habits, including theological tendencies. Such issues have become so much a part of the current scholarly mainstream that monograph treatments of scribal tendencies constitute their own genre today."

Most recently, in his two consecutive articles, "Scribes and Variants" and "Conceptualizing 'Scribal Performances': Reader's Notes," in *The Textual History of the Greek New Testament: Changing Views in Contemporary Research* (eds. K. Wachtel and M.W. Holmes; TCS 8; Atlanta: SBL, 2011), 49-64, Ulrich Schmid described the two major strands of development in the following terms: scribes as *authors* and scribes as *copyists*. Additionally, in the recent revised version of his *Orthodox Corruption of Scriptures* (Oxford: OUP, 2011), pp. 331-63, Ehrman also appended a passing review of major works on scribal studies that came out after the publication of his first edition in 1993.

[114] Herman Hoskier, "A Study of the Chester Beatty Codex of the Pauline Epistles," *JTS* 38 (1937): 148-63; Idem, *Appendix to an Article on the Chester-Beatty Papyrus of the Pauline Epistles known as* $\mathfrak{P}^{46}$ (Oxford: OUP, 1937); and, Idem, $\mathfrak{P}^{46}$*: Addenda et corrigenda* (Oxford: OUP, 1937); Idem, *A Commentary on the Various Readings in the Text of the Epistle to the Hebrews in the Chester-Beatty Papyrus* $\mathfrak{P}^{46}$, *circa 200 A.D.* (London: Bernard Quaritch, 1938).

[115] Hoskier, "A Study of the Chester Beatty Codex," 150.

[116] Hoskier, "A Study of the Chester Beatty Codex," 158-62.

Prior to Hoskier's $\mathfrak{P}^{46}$ studies, some pre-War projects had already brought to the fore the role of scribal *theological tendencies* in the creation of meaningful variants[117] in the history of textual transmission—a direct challenge to Hort's confidence that no NT variant is theologically motivated.[118] But none[119] confronted the issue more extensively and deliberately than Eldon Jay Epp in his post-War doctoral dissertation, *The Theological Tendency of Codex Bezae*,[120] where he attempted to present the apparent anti-Judaic tendency in Bezae's text of Acts.[121] This ground-breaking work figured prominently in the ensuing decades,[122] mostly among North American textual scholarship,[123] whereby other types of tendencies were soon articulated,

[117] For instance, Kirsopp Lake, *The Influence of Textual Criticism on the Exegesis of the New Testament* (Oxford, 1904); J. Rendel Harris, "Was the Diatesseron Anti-Judaic?" *HTR* 18 (1925): 103-09; Adolf von Harnack, "Zur Textkritik und Christologie der Schriften Johannes," in *Studien zur Geschichte des Neuen Testaments und der alten Kirche,* vol. I, *Zur neutestamentlichen Textkritik (*Berlin: de Gruyter, 1931), 115-27; Donald Wayne Riddle, "Textual Criticism as a Historical Discipline," *ATR* 18 (1936): 220-33.

[118] F.J.A. Hort. *The New Testament in Original Greek: Introduction* (Cambridge and London: MacMillan and Co., 1881), 282-83, where he stated, "It will not be out of place to add here a distinct expression of our belief that even among the numerous unquestionably spurious readings of the New Testament there are no signs of deliberate falsification of the text for dogmatic purposes… Accusations of wilful tampering with the text are accordingly not unfrequent in Christian antiquity; but…wherever they can be verified they prove to be groundless, being in fact hasty and unjust inferences from mere diversities of inherited text."

[119] Here we may mention the post-War studies of Ernst Saunders, "Studies in Doctrinal Influences on the Byzantine Text of the Gospels," *JBL* 71/2 (Jun 1952): 85-92; and Kenneth Clark "The Theological Relevance of Textual Variation in Current Criticism of the Greek NT," *JBL* 85/1 (Mar 1966): 1-16.

[120] E.J. Epp, *The Theological Tendency of Codex Bezae Cantabrigensis in Acts* (SNTSMS 3; Cambridge: CUP, 1966).

[121] By comparing Codex D with the text of Codex B, Epp identified textual variations that may be attributed to the anti-Judaic tendencies of the scribe who copied the text of Acts in Codex D. In a nutshell, Epp argued for this tendency in three areas: 1) the Jews and their leaders were more hostile to Jesus in the Western text, and were assigned a self-incriminating responsibility for his death on the cross, 2) the Jews' response and the importance of Judaism for the emerging faith in Acts was minimised in the Western text, and 3) the Western text portrayed the Jews, and more especially their leaders, as more hostile to the Apostles (persecuting them more vigorously) as would be recorded in other manuscripts.

[122] Hernández, *Scribal Habits and Theological Influences*, 35, best described Epp's masterpiece, "A close reading of this work will reveal a textual critic who combines a mastery of both primary and secondary sources with creativity, clarity, and acumen. But more importantly, Epp's work offered the first full scale treatment of a NT book within a given MS from the perspective of theological tendencies."

[123] In fact, two of Epp's immediate doctoral students worked also along this framework: G.E. Rice, "The Alteration of Luke's Tradition by the Textual Variants in Codex Bezae," PhD. Dissertation, Case Western Reserve University, 1974, and Howard Eshbaugh, "Theological Variants in the Western Text of the Pauline Corpus," Ph.D. Dissertation, Case Western Reserve University, 1975.

underscoring, to a large extent, the social histories reflected in the history of textual transmission.[124]

But this new focus is not without methodological problems.[125] For instance, attempts to elevate some variants to the status of a "tendency" without due regard to other equally dominant "tendencies" cast doubts on the integrity of the exercise, especially in the absence of sound empirical controls. It is possible, therefore, that the "perceived" tendencies of a particular scribe might soon be proved to be in fact the camouflaged tendencies "imposed" upon the *text* (consciously or unconsciously) by modern-day critics.[126] Needless to say, viable empirical controls are necessary to avoid anachronisms.[127]

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[124] We may mention here some of the more important studies:

a. Mikeal Parsons, "A Christological Tendency in $\mathfrak{P}^{75}$," *JBL* 105 (1986): 463-79;
b. Bart Ehrman, *The Orthodox Corruption of Scripture: The Effect of Early Christological Controversies on the Text of the NT* (Oxford: OUP, 1993; updated with a new Afterword, Oxford: OUP, 2011); Idem, *Misquoting Jesus: The Story Behind Who Changed the Bible and Why* (San Francisco: HarperSanFrancisco, 2005);
c. Wayne Kannaday, *Apologetic Discourse and the Scribal Tradition: Evidence of the Influence of Apologetic Interests on the Text of the Canonical Gospels* (TCS 5; ed. J. Adair; Atlanta: SBL, 2004); Idem, "'Are Your Intentions Honorable?' Apologetic Interests and the Scribal Revision of Jesus in the Canonical Gospels," *TC: A Journal of Biblical Textual Criticism* <http://rosetta.reltech.org/TC/v11/Kannaday2006.html> (Accessed 25 Feb 2010);
d. Kimberly Haines-Eitzen, *Guardians of Letters*.

For non-North Americans, we may mention Peter Head, "Christology and Textual Transmission: Reverential Alterations in the Synoptic Gospels," *NovT* 35 (1993): 105-29. Some other recent articles along this line include: Michael Wade Martin, "Defending the 'Western Non-Interpolations': The Case for an Anti-Separationist *Tendenz* in the Longer Alexandrian Readings," *JBL* 124 (2005): 269-94; and Dominika A. Kurek-Chomycz, "Is There an "Anti-Priscan" Tendency in the Manuscripts? Some Textual Problems with Prisca and Aquila," *JBL* 125 (2006): 107-28.

[125] Jeffrey Childers and Curt Niccum, "'Anti-Feminist' Tendency in the 'Western' Text of Acts?" in *Essays on Women in Earliest Christianity* (Vol. I; ed. C. Osburn; MO: College Press, 1993), 469-92. See also R.P.C. Hanson, "The Ideology of Codex Bezae in Acts," *NTS* 14 (1967-68): 282-86.

[126] For instance, Ben Witherington III's "Anti-Feminist Tendencies of the 'Western' Text in Acts," *JBL* 103 (1984): 82-84, has been severely criticised for its anachronistic labelling; see Bruce Metzger and Bart Ehrman, *Text of the New Testament: Its Transmission, Corruption, and Restoration* (4th ed.; Oxford/New York: OUP, 2005), 290, n48.

[127] In similar vein, C.F.D. Moule, "Some Observations on *Tendenzkritik*," in *Jesus and the Politics of His Day* (ed. E. Bammel; Cambridge: CUP, 1984), 91-100, cautioned exegetes in blindly using "tendency criticism" especially when "perceived tendencies" are contrary to direct material evidences. Accordingly, a very apt advise along this line comes from Kirsopp Lake, *The Text of the New Testament* (6th ed.; London: S. New, 1933), 5, "The critic has always to be ready to revise his judgment. He ought to be suspicious of readings but far more suspicious of his own conclusions."

Despite its shortcomings, however, what this new emphasis underscored is a new *motive* for studying textual variances aside from the classical goal of establishing the "original reading"; it has placed variant readings on equal footings. The text's *social* history becomes an essential component in the discussion of its *textual* history. More importantly, it breathed life into the scribes who "created" the texts, so to speak, with all their mysterious distinctiveness, making them more active participants in the text-production dialogue.[128] It has effectively cast the scribes in the limelight.

2. On the Theological Tendencies of $\mathfrak{P}^{46}$

Aside from Hoskier's works, Howard Eshbaugh also attempted to investigate the theological tendencies of $\mathfrak{P}^{46}$.[129] Citing six selected readings[130] from the Galatians text of $\mathfrak{P}^{46}$ he deemed laden with theological biases, he proposed that $\mathfrak{P}^{46}$'s "scribe-theologian" had a *subordinationist* Christology. Although his methodology is not well developed,[131] Eshbaugh's attempt represented a new stage in studying the text of $\mathfrak{P}^{46}$—he elevated the discussion of its text in relation to a known socio-theological history. Accordingly, Bart Ehrman, operating on the same assumption, identified scattered variants from $\mathfrak{P}^{46}$ (along with other MSS) which he believed were motivated by "proto-orthodox" agenda.[132]

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[128] For a more recent look at this slant in textual studies, see relevant articles in Houghton and Parker, *Textual Variation: Theological and Social Tendencies.*

[129] Howard Eshbaugh, "Textual Variants and Theology: A Study of the Galatians Text in $\mathfrak{P}^{46}$," *JSNT* 3 (1979): 60-72; repr. *New Testament Text and Language: A Sheffield Reader* (Biblical Seminar 4; eds. S.E. Porter and C. Evans; Sheffield: Academic Press, 1997), 81-91.

[130] Gal 1.6 (f81^{r}-I^{22} ε̣ν̣ [χαριτι]); 2.20 (f82^{v}-I^{25} του θυ̅ και χρυ̅); 3.17 (f83^{v}-I^{02} omitting εις χριστον after υπο του θυ̅), 19 (f83^{v}-II$^{07-08}$ τι ουν ο νομος των πραξε||ων); 4.6 (f84^{r}-II$^{08-09}$ το πνα̅||αυτου), and 7 (f84^{r}-II$^{10-11}$ κληρονο||μος δια θυ̅).

[131] See also the critique of Eshbaugh's methodology by James Royse, *Scribal Habits in Early Greek New Testament Papyri* (*NTT-SD* 36; Leiden/Boston: Brill, 2008), pp. 355-57, esp. 357, where he concluded, "All in all, then, the readings cited by Eshbaugh do not seem adequate to sustain his thesis."

[132] In the first edition of his *Orthodox Corruption*, Ehrman used the following passages in $\mathfrak{P}^{46}$: Rom 6.11 (p. 163); Heb 1.3 (p. 150), 8 (p. 265); 2.9 (pp. 149-51); 1Cor 10.9 (p. 89); 15.47 (p. 95); 2Cor 1.9 (p. 115, n194); Eph 4.15 (pp. 268-69); Gal 2.20 (p. 86); and Col 2.2 (p. 267).

B. "Singular Readings" as "the portal" to Scribal Habits[133]

1. The Colwell Method

Methodologically, subsequent researches on scribal tendencies have developed from creative isolation of select variants, from a particular manuscript (Epp, Eshbaugh, Parson, etc.) or from a pool of manuscripts (Ehrman, Kannaday, Haines-Eitzen), perceived to be "theologically-motivated" to a more empirically controlled direction. This movement was the result of a methodological shift focusing on identifying "singular readings" of particular manuscripts to establish scribal habits. For this, credit goes to Ernest Cadman Colwell.[134] Unlike Epp's approach to scribal theological tendencies, Colwell's method cut across continental scholarship, influencing even some of the noted European(-based) textual critics.[135] But whilst this method was a major step forward, the aim was to study these "singular readings" still in light of the establishment of the "original reading". For instance, Epp emphasised that "singular

[133] Since reviews and critical valuations of Colwell's and Royse's methods already abound in the literature, I shall limit my discussion of these models to the features relevant for our present purposes. For reviews of these methods, see E.J. Epp, "Issues in New Testament Textual Criticism: Moving from Nineteenth Century to the Twenty-First Century," in *Rethinking New Testament Textual Criticism* (ed. D.A. Black; Grand Rapids, MI: Baker, 2002), 17-76, esp. pp. 25-31; repr. in *PNTTC*, 641-97 ; Idem, "Traditional 'Canons' of New Testament Textual Criticism: Their Value, Validity, and Viability—or Lack Thereof," in *THGNT*, 79-127, esp. pp. 106-16; Hernández, *Scribal Habits and Theological Influences*, 28-48; Kyoung Shik Min, *Die früheste Überlieferung des Matthäusevangeliums (bis zum 3./4. Jh.) Edition und Untersuchung* (*ANTT* 34; Berlin/New York: de Gruyter, 2005), 32-37; Dirk Jongkind, *Scribal Habits of Codex Sinaiticus* (TS Third Series, Vol. 5; Piscataway, NJ: Gorgias Press, 2007), esp. pp. 134-43; and David Parker, Review of James Royse, *Scribal Habits in Early Greek New Testament Papyri*, *BASOR* 46 (2009): 255-58.

[134] E.C. Colwell, "Scribal Habits in Early Papyri: A Study in the Corruption of Text," in *The Bible in Modern Scholarship* (ed. P. Hyatt; Nashville: Abingdon, 1965), 370-89; repr. as "Method in Evaluating Scribal Habits: A Study of $\mathfrak{P}^{45}$, $\mathfrak{P}^{66}$, $\mathfrak{P}^{75}$,"in *Studies in Methodology*, 106-24.

[135] For instance, Peter Head, "Observations on Early Papyri of the Synoptic Gospels, especially on the Scribal Habits," *Bib* 71 (1990): 240-7; Idem, "The Habits of NT Copyists: Singular Readings in the Early Fragmentary Papyri of John," *Bib* 85 (2004): 399-408; and Idem, "Scribal Behaviour and Theological Tendencies in Singular Readings in P. Bodmer II ($\mathfrak{P}^{66}$)," in *Textual Variation: Theological and Social Tendencies*, 55-74; J.K. Elliott, "Singular Readings in the Gospel Text of $\mathfrak{P}^{45}$," in *The Earliest Gospels*, 122-31 (with caveats on p. 123); and Dirk Jongkind, "Singular Readings in Sinaiticus: The Possible, The Impossible, and the Nature of Copying," in *Textual Variation: Theological and Social Tendencies,* 35-54. Interestingly, almost all of the articles in Part II of *The Early Text of the New Testament*, 83-258, have appealed to the "singular readings" of particular manuscripts, in relation to a NT book or corpus, to isolate scribal peculiarities. This shows how far this methodology has influenced many (younger?) NT textual critics, in regard to their view of the early stage of NT text vis-à-vis scribal studies.

readings" are *insignificant* as far as the broader text-critical task is concerned,[136] so that although it is a theoretical possibility,[137] it cannot be expected that the original reading has been preserved in only one manuscript.[138] But he immediately stressed that an informed knowledge of this scribal phenomenon certainly aids in understanding individual manuscripts better, "both in terms of the habits of its scribe and in terms of any stylistic and ideological biases."[139] This has been based on the theory advanced by Colwell that scribal tendencies are best detected in the singular readings[140] since they are "scribal creations".[141]

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[136] E.J. Epp, "Toward the Clarification of the Term Textual Variant," in *Studies in NT Language and Text: Essays in Honour of G.D. Kilpatrick on the occasion of his 65th Birthday* (*NovTSupp* 44; ed. J.K. Elliott; Leiden: Brill, 1978), 153-73; repr. in *Studies in the Theory and Method of New Testament Textual Criticism* (SD 45; eds. E.J. Epp and G.D. Fee; Grand Rapids, MI: Eerdmans, 1993), 59. (Citations from the latter).

[137] See Epp, "Toward the Clarification," 53; and G.D. Fee, "Rigorous or Reasoned Eclecticism – Which?" in J.K. Elliott's *Studies in New Testament Language and Text*, 174-97; repr. in *Studies in the Theory and Method of New Testament Textual Criticism*, 124-40. (Citations from the latter).

[138] But cf. J.K. Elliott, *The Greek Text of the Epistles to Timothy and Titus* (SD 36; Salt Lake: University of Utah Press, 1968), 10-11, who argued, "there is no reason when an original reading should not have been preserved in only one manuscript", although he also rightly cautioned that "(W)hen a weakly attested reading is accepted as the true reading, it must be shown why and how the variant came about, and why it was so widely accepted". See also, Hernández, *Scribal Habits and Theological Tendencies,* 47; and Timo Flink, *Textual Dilemma: Studies in the Second-Century Text of the New Testament* (University of Joensuu Publications in Theology 21; Joensuu: University of Joensuu, 2009), 14.

Fee, "Rigorous or Reasoned Eclecticism," 130-31, was open to that possibility as well, arguing, "The fact that $\mathfrak{P}^{75}$ (sometimes with $\mathfrak{P}^{45}$) and $\mathfrak{P}^{46}$ have eliminated what were once singular or nearly singular readings of B at some points where these MSS in combination seem to preserve the original text, and the fact that each ($\mathfrak{P}^{45}$ $\mathfrak{P}^{75}$ B) has been judged as a careful preservation of a very early type of text, should cause one to allow the *possibility* that any of them in a singular reading best represents this text-type… (I)f $\mathfrak{P}^{46}$, $\mathfrak{P}^{75}$ or B does preserve the original text in a singular reading, consideration given to such a reading… rests chiefly on the judgment as to the generally excellent quality of these MSS".

[139] Epp, "Toward the Clarification," 59. Accordingly, E.C. Colwell and E. Tune, "Method in Classifying and Evaluating Variant Readings," in *SMTCNT*, 104, argued, "A study of singular readings will reveal habits and inclinations that will aid in the appraisal of his readings which are not singulars."

[140] Colwell, "Method in Evaluating Scribal Habits," 108, defined "singular readings" as readings which have "no Greek support in the critical apparatus of Tischendorf's 8th edition".

[141] Colwell, "Method in Evaluating Scribal Habits," 108, "Since in most readings the student cannot determine whether or not the scribe copied or originated the reading, this study is restricted to singular readings (readings without other manuscript support) on the assumption that these readings are the creation of the scribe". See also Moisés Silva, "The Text of Galatians: Evidence from the Earliest Greek Manuscripts," in *Scribes and Scriptures: New Testament Essays in Honour of J. Harold Greenlee* (ed. D.A. Black; Wynona Lake: Eisenbrauns, 1993), 17-25, p. 18. Accordingly, Colwell's study of three extensive papyri was an attempt to provide a systematic glimpse as to how a study on scribal proclivities evinced from the "singular readings" can help text-critics ascertain the environments and conditions where these scribes operated and how these circumstances eventually affected their respective outputs.

One of those who immediately employed this new conceptual method is Larry Hurtado. In 1973, under the supervision of E.J. Epp, Hurtado worked on the text of the Gospel of Mark in Codex Washingtonianus and employed Colwell's model to a large extent.[142] Having isolated and analysed the "singular readings"[143] and some important non-singulars in Codex W, he ultimately characterised its scribe as one who exercised considerable editorial freedom, whose main interest was to produce a text that was easy to read and as intelligible as possible—a hint that Codex W might have been prepared for popular (public) reading.[144] However, it was in another PhD dissertation that Colwell's "singular reading" method would become most popular and trend-setting: James Royse's.

2. The Royse Method

Colwell's approach was extensively pursued by James Royse in his doctoral dissertation[145] where he took the "singular readings" of six non-fragmentary papyri, including $\mathfrak{P}^{46}$, to profile their scribal habits.[146] To achieve this purpose, the isolated "singular readings" were then categorised into "insignificant singulars" (i.e., orthographic and nonsense singulars) and "significant singulars" (i.e., omission, addition, harmonised, transposed,

[142] Larry Hurtado, "Codex Washingtonianus in the Gospel of Mark: Its Textual Relationships and Scribal Characteristics," PhD Dissertation, Case Western Reserve University, 1973. This was eventually published as *Text-Critical Methodology and the Pre-Caesarean Text: Codex W in the Gospel of Mark* (SD 43; Grand Rapids, MI: Eerdmans, 1981).

[143] Hurtado, *Text-Critical Methodology*, 67, describes "singular readings" as "readings found in only one MS", without any clear indication of any particular collation base.

[144] Also, Hurtado cogently demonstrated that the textual evidence provided by $\mathfrak{P}^{45}$ and W strongly goes against the prevailing notion at the time that $\mathfrak{P}^{45}$ and W etc. belonged to the "Caesarean texttype". Conversely, he argued that W and $\mathfrak{P}^{45}$ (and to some extent f^{13}) form a group, unrelated with the manuscripts considered to be representative of the "Caesarean" text.

[145] James Royse, "Scribal Habits in Early Greek New Testament Papyri," ThD Dissertation, Graduate Theological Union-Berkeley, 1981; now updated and published as *Scribal Habits in Early Greek New Testament Papyri* (*NTT-SD* 36; Leiden/Boston: Brill, 2008). For a related study, see also his "Scribal Tendencies in the Transmission of the Text of the New Testament," in *The Text of the NT in Contemporary Research: Essays on the Status Quaestionis* (SD 46; eds. B.D. Ehrman and M.W. Holmes; Grand Rapids, MI: Eerdmans, 1995), 239- 52; and its updated version in *TNTCR*^{2}, 461-78.

[146] This is not to suggest that there are no significant differences in the methodological details between the two. For these differences, see Jongkind, *Scribal Habits of Codex Sinaiticus*, 134-41; and Hernández, *Scribal Habits and Theological Influences*, 42-46.

and substituted singulars). But of these categories, Royse highlighted his findings in relation to the omission singulars in the analysed papyri. Collectively, he concluded that the high percentage of omissions in these papyri casts doubts on the venerable text-critical maxim of *lectio brevior potior* (preference for the shorter reading)—enunciated by Enlightenment textual historians but most eminently by J.J. Griesbach,[147] a canon devised before any pre-fourth century NT papyrus was ever discovered—since these papyri tended more to omit than expand their texts.[148] Royse eventually proposed that all things being equal, one should prefer the longer reading, at least in the period of the early papyri.[149] As to $\mathfrak{P}^{46}$ in particular, Royse concluded the following:

a. The scribe makes very many errors in spelling, demonstrating a great variety of confusions of similar sounds.
b. The scribe makes a number of errors that result in nonsense, despite frequent correction by him of his text. Many of these seem to arise from his faulty understanding of what he is copying, resulting in a high density of nonsense in context readings. In particular, he rather often errs when he encounters abbreviations of *nomina sacra*.
c. The scribe has a very marked tendency to omit portions of the text, most often only one word but longer phrases also. Some of these are due to scribal leaps, but most seem to have arisen from simple oversight or carelessness. The additions are not as frequent, and are often the results of harmonizations to the context, although there are three examples of conflation from readings found in different textual groups.
d. There are comparatively few transpositions, and these tend to be rather short, probably because the scribe cannot master the sense of what he is copying.
e. As we have seen throughout, our scribe makes many errors affecting the grammar of what he is trying to copy. Many of these are simple slips, often caused (as it seems) by the influence of the context. But some changes are more systematic or betray perhaps a deliberate attempt to improve on his *Vorlage*. Few, if any, of these have any claim to serious

[147] See English translation in Metzger and Ehrman, *Text of the New Testament*, 166.

[148] But cf. Silva, "Text of Galatians," 17-25, who similarly analysed the text of $\mathfrak{P}^{46}$ (*cum* codices ℵ, B, and A) in relation to the addition and omission variants committed by its scribe. Whilst the data derived from this study, by his own admission, are hardly conclusive, he nonetheless raised a very important point in defence of the traditional maxim of preference for the shorter reading, particularly those that fall within Griesbach's exemptions; also, Idem, "Response," in *Rethinking New Testament Textual Criticism*, 141-50, pp. 145-46. See also the recent discussion by E.J. Epp, "Traditional 'Canons' of New Testament Textual Criticism: Their Value, Validity, and Viability—or Lack Thereof," in *THGNT*, 79-127, esp. 106-16.

[149] Royse, *SH-D*, 593-615; Idem, "Scribal Tendencies," 246. The same conclusion was reached by Peter Head in his two studies analysing fragmentary papyri containing the gospels; see his "Observations on Early Papyri of the Synoptic Gospels," 240-47, and his "The Habits of NT Copyists," 399-408. Head, "Habits of NT Copyists," 399, 400, defined "singular readings" as "reading unique to the particular manuscripts", and indicated that his collation bases were NA[27], Tischendorf[8], von Soden, and Swanson's. Additionally, both Hernández (*Scribal Habits and Theological Influences*, 193-96) and Jongkind (*Scribal Habits of Codex Sinaiticus*, 246) also reached similar conclusions in their own respective studies on non-papyri manuscripts.

consideration as being older than $\mathfrak{P}^{46}$ itself, but they do indicate a certain awareness by the scribe of what he was writing, and a willingness to alter what he read.

f. Harmonizations of various kinds occur, and by far the most pervasive is harmonization to the immediate context. This influence of the context seems to be the major factor in the scribe's occasional attempts to make stylistic or grammatical improvements.[150]

3. The Importance and the Impediments of the Colwell-Royse Methods

As a consequence of these studies,[151] text-critics began to recognise the strategic importance of studies in scribal habits as a "new direction" in NT textual criticism,[152] particularly in probing further questions of transcriptional probability.[153] This methodological innovation afforded a more critical review of NT text-critical maxims. For the first time in its history, a challenge was systematically made against one of its pillar principles that have, to a large extent, shaped textual decisions for more than 200 years—a fitting credit to the commendable meticulousness of Royse.[154]

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[150] Royse, *SH-D*, 282-83 and *SH-M*, 358.

[151] Another extensive work largely employing the Colwell-Royse methodology, with notable methodological refinements, is Hernández's *Scribal Habits and Theological Influences in the Apocalypse*, which isolated the singular readings in codices ℵ, A, and C (the earliest extensive manuscripts of Revelation), to identify theological tendencies (much like in the mould of Epp and Ehrman's methods, with some refinements). Like Royse, he also noted that these manuscripts produced a "shorter text", taking a swipe yet again at the doctrine of *lectio brevior potior*; and like Epp's method, he discerned that Codex ℵ exhibits an anti-Arian tendency in its singular readings. He defined a "singular reading" as reading "found only in one MS and assumed to have been introduced into the textual history by a scribe" (p. 7, n21). His collation bases include: Tischendorf, Weiss, Schmid, von Soden, Hoskier, Andrew of Caesarea, and NA^{27} (p. 47).

[152] Larry Hurtado, "Developments and Directions in NT Textual Criticism," in *Studies in the Early Text of the Gospels and Acts: The Papers of the First Birmingham Colloquium on the Textual Criticism of the NT* (ed. D.G.K. Taylor; Atlanta: SBL, 1999), 26-48, pp. 36-37; Epp, "Issues in NT Textual Criticism," 17-76, esp. 22-34; Hugh Houghton, "Recent Developments in New Testament Textual Criticism," *Early Christianity* 2.2 (2011): 245-68; J.K. Elliott, "Recent Trends in the Textual Criticism of the New Testament: A New Millennium, A New Beginning?" *BABELAO* 1 (2012): 117-36, p. 131; Charles E. Hill and Michael J. Kruger, "Introduction: In Search of the Earliest Text of the New Testament," in *ETNT*, 1-19, esp. 13-15; among others.

[153] For instance, Hurtado, "Developments and Directions," 36-37, viewed these studies as a necessary reminder from invoking mechanically traditional text-critical maxims in textual decisions, since scribal habits are much more complex than first expected.

[154] However, this should not be taken as foolproof to immediately and fully abandon the maxim. In fact, a kind of "doctrinal stalemate" is now brewing. For critical reviews of Royse's conclusion, see Silva, "Response (to the Essays)," 141-50, esp. 145-50; Jongkind, *Scribal Habits of Codex Sinaiticus*, 138-39; and Parker, Review of James Royse, 257. Edgar Ebojo's "How Persuasive is the 'Persuasive Words of Human Wisdom'? The Shortest Reading in 1 Corinthians 2.4," *TBT Technical Papers* 60 (2009): 1-33, is also an attempt to demonstrate the continuing validity of this venerable maxim.

In this brewing stalemate I find Epp's enlightened advice very instructive, "My own judgment, however, is that at this juncture the discipline is not fully prepared either to drop the shorter reading

Incidentally, Royse's thesis and massive monograph represent the most extensive work on the text of $\mathfrak{P}^{46}$ after Zuntz's classic work.[155] But whether his methodology fully justified his goal of probing the "scribal habits" of $\mathfrak{P}^{46}$ by isolating its "singular readings" is the question that interests us in this thesis. As I see it, the question of methodological details will continue to figure most prominently in future studies on "scribal habits" of particular manuscripts. Certainly, the methodology pioneered by Colwell and refined by Royse is by no means foolproof, and needs to be constantly reviewed and accordingly calibrated (as attempted by Hernández and Jongkind).[156] Two issues may be raised immediately as a methodological critique against this approach: the questions of *definition* and of *presupposition.*

a. The Question of Definition

What qualifies a variant to be considered a "singular reading"?[157] Is the basis for establishing singularity *quantitative* (numerical) or *qualitative* (genetic)? This is methodologically critical as differences among relevant studies in defining what constitutes a singular reading are conspicuous.[158] Hence, a review is in order.

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criterion in favor of a longer reading canon, nor is there sufficient confidence to maintain the shorter reading option without clear accompanying recognition of the longer reading criterion. It is not an either/or situation but one requiring adjudication case by case. A compromise formulation is necessary, I think, to avoid a stalemate… It both accurately describes our text-critical situation and retains the usefulness of the criterion—or, better, both criteria" ("Traditional Canons," 115-16).

[155] A relevant study on some of the "singular readings" of $\mathfrak{P}^{46}$ without impinging on the domain of scribal habits is Beare, "The Text of the Epistle to the Hebrews in $\mathfrak{P}^{46}$," 384-92, who investigated at least some of the 31 "singular readings" that created interesting implications for the text of Hebrews.

[156] My M.Theol. thesis ("Scribal Tendencies in the Singular Readings of $\mathfrak{P}^{46}$", Trinity Theological College, Singapore, 2006) was also an attempted refinement of the method, and although independently of Hernández, we shared a number of methodological affinities. In a nutshell, by isolating "singular readings" using a more restrictive definition, I identified 611 singular readings (Royse listed 668 in his dissertation) to discern at least three theological tendencies: 1) pro-Gentile and pro-Pauline tendency, 2) pro-orthodox tendency, and 3) misogynist tendency.

[157] Cf. Colwell and Tune, "Method," 96-105; Epp, "Toward the Clarification," 47-61; and Fee, "Rigorous or Reasoned," 124-40.

[158] The range of differences in definition found in the literature, especially amongst those who employed this method, is seen easily in the following table:

Hort, who first propounded this scribal phenomenon, defined it as readings "which have no other direct attestations whatever".[159] Clearly, he understood singular readings *quantitatively*, i.e., textual divergences that are universally unsupported by the entire textual tradition except by one manuscript.[160] Since then it has become

	DEFINITION	COLLATION BASE
Hort (1881)	"… (readings) which have no other direct attestation whatever…" (p.230)	
Colwell (1965)	"… a reading which has no Greek support in the critical apparatus of Tischendorf's 8th edition." (p.108)	Tischendorf[8] and "recent finds" (pp.108-09)
Hurtado (1981)	"… readings found in only one MS…" (p.67)	
Royse (1981)	"…one which found no Greek manuscript support… (T)hose readings which are here defined as singular have either no Greek manuscript support at all, or, at the most, such support which is very likely coincidental" (p.50)	Tischendorf[8], von Soden, NA[26], UBS[3]; Hoskier; IGNTP-Luke (43-44; 48)
Royse (2008)	"… one that had no continuous-text Greek manuscript support… (T)hose readings that are here defined as singular have either no continuous-text Greek manuscript support at all, or, at the most, such support that is very likely coincidental." (p.74)	von Soden, Clark, NA[25, 26, 27], UBS[3, 4], Aland's *Synopsis*, Legg, IGNTP, *DNTAP*, and Swanson (65)
Parsons (1986)	"… readings found in only one Greek manuscript…" (p.470)	
Epp (1993)	"… a 'reading' found in one NT MS but with the support of no other; *it* is a unique reading as far as our knowledge of NT MSS extends. 'Singular readings'… may be especially useful in assessing the nature and characteristics of an individual MS and its scribe, but 'singular readings' are not genetically or genealogically significant, nor is an original reading to be expected among them… (F)or purposes of discerning… the ideological bias of a NT MS, it will be prudent, if not essential, to adopt the more restrictive view of 'singular readings,' that they must be unique—without support—in the *entire* (known) textual tradition…" (p.59)	
Head (1994)	"… a reading or variant which is unique to the manuscript under investigation (usually defined, pragmatically, as a reading found in neither Tischendorf nor NA26 nor a particularly related MS)." (p.242)	Tischendorf, NA[26]
Head (2000)	"… one that is not known from NA[27], Tischendorf[8], von Soden and Swanson." (p.400)	Tischendorf[8], von Soden, NA[27], and Swanson
Elliott (2004)	Singular (and sub-singulars) "are readings not found in the bulk of other manuscripts…" (p.121)	
Hernández (2006)	"… found only in one MS and assumed to have been introduced into the textual history by a scribe" (p.7, n21)."	Tischendorf, von Soden, Hoskier, Andrew of Caesarea, and NA[27] (p.47)
Hernández (2009)*	"… Greek readings that occur only in Codex Sinaiticus and that find no support in any witnesses—Greek or otherwise—listed in the apparatuses of Tischendorf, von Soden, Hoskier, Schmid, UBS[4], and NA[27]." (p.252)	Tischendorf, von Soden, Hoskier, Schmid, UBS[4], and NA[27]
Jongkind (2007)	"… a reading for which no outside Greek support can be found" (p.131)	
Flink (2009)	"A singular reading is a unique reading to one single textual witness, one that has no direct attestation anywhere else, including versions and fathers." (p.14)	

*Juan Hernández, "Scribal Tendencies in the Apocalypse: Starting the Conversation," in *Jewish and Christian Scripture as Artifact and Canon* (eds. Craig Evans and H. Daniel Zacharias; London/NY: T&T Clark, 2009), 248-60.

[159] Hort, *Introduction*, 230.

[160] Note that Hort, *Introduction*, 230, distinctly differentiated a *singular reading* from *subsingular reading*, defining the latter as readings which "have only secondary support, namely, that of inferior Greek MSS, of Versions, or of Fathers, or of combinations of documentary authorities of these kinds". See also Zuntz, *TEDCP*, 39.

customary to adopt this definition,[161] until Colwell proposed a rather more tradition-specific definition, i.e., a "reading which has no *Greek* support in the critical apparatus of Tischendorf's 8th edition".[162] For Colwell, singularity is established when no other *Greek* manuscripts support a particular reading, an understanding that slightly (but with significant implications on the statistical result) departed from Hort's. With some refinements, Royse adopted this pragmatic definition[163] in his study of six *non-fragmentary* papyri,[164] and similar studies on *fragmentary* gospel papyri were undertaken by Head.[165] (Notable also are the subsequent studies by Hernández and Jongkind on some of the more important majuscule manuscripts). Although Royse and Head agree on most points, fundamental differences in definition are apparent.[166] Royse described "singular readings" as those that "have either no Greek manuscript support at all, or, at the most, such support which is very likely coincidental".[167] Clearly, Royse was open to "coincidental agreement"[168] as a possible qualification for

[161] For instance, although Zuntz, *TEDCP*, 39, did not make a forthright definition of what makes up a singular reading with regard to textual boundaries, there are indications that he also understands singular readings as "readings attested by one witness only".

[162] Colwell, *Scribal Habits*, 372-73. Emphasis added.

[163] Royse, *SH-D*, 43 (*SH-M*, 65), explained that the difficulty with Hort's definition "is that it will be exceedingly arduous to determine that a reading is singular… In practice, therefore, it is necessary to relativize the term to some definite body of available evidence".

[164] Royse, *SH-D*, 29-57.

[165] Head, "Observations," 242, explained, "A 'singular reading' is a reading or a variant which is unique to the manuscript under investigation (usually defined, pragmatically, as a reading found in neither Tischendorf nor NA[26] nor a particularly related MS)." Obviously, such definition is an attempt to consider the problem of collation in the method.

[166] For instance, see the definition issues raised by Royse, "Tendencies," 247, n51, on Head's earlier article. Accordingly, in his later article, Head, "The Habits," 400, redefined a "singular reading" to mean "one that is not known from NA[27], Tischendorf[8], von Soden, and Swanson". However, in both articles, Head never mentioned explicitly whether a "singular reading" is Greek text-specific or a rather broader one.

[167] Royse, *SH-D*, 50. With an expanded collation base in place, Royse, *SH-M*, 74, defined "singular reading" as a variant reading "that had no continuous-text Greek manuscript support. But in the absence of such major critical apparatus (Royse was referring here to IGNTP or the *Editio Critica Maior* editions), one can be morally certain that those readings that are here defined as singular have either no continuous-text Greek manuscript support at all, or, at the most, such support that is very likely coincidental."

[168] See also Colwell, "Scribal Habits," 387; Hurtado, *Text-Critical Methodology*, 67-69. But cf. Hernández, *Scribal Habits and Theological Influences*, 47, who excluded this category from his list of singulars in Codex Sinaiticus, but with qualifying discussions.

some readings with versional and patristic support to be counted as "singular readings" provided that "coincidence" can be firmly established.[169]

However, this Greek text-specific definition as a basis for establishing singularity bristles with difficulties.[170] First, the nagging *question of control*, i.e., Are variants recorded in Tischendorf[8] as singular readings still singulars if they are later found to be actually supported by other manuscripts?[171] This is especially true with the advent of the widespread use of computing technology in studying ancient manuscripts, resulting in faster and more accurate manuscript collations. Definitely, "singular readings" in Tischendorf[8] will predictably diminish[172] as soon as the *Editio Critica Maior* becomes available in full.[173] The matter of *quantitative* relationship is thus at issue. Interestingly, Zuntz already anticipated this dilemma.[174] In fact, this problem is best demonstrated by comparing Royse's statistics in his dissertation against his monograph. In the former, he listed 668 singulars[175] for $\mathfrak{P}^{46}$ whilst in the latter, he listed 639.[176] The discrepancy in

[169] Although in practice this is not without problem either, since there are 28 subsingular variants in NA[27] (and NA[26]) that were not included in Royse's dissertation list: Rom. 9.25 (B), 30 (G); 11.22 (B); 15.30 (B), 32a (B), 32b (B); 16.7b (B); Heb. 1.4 (B); 9.19 (D*); 10.7 (D*[.2]), 25 (D*); 12.18 (Ψ), 24 (1505), 28 (bo); 1Cor. 1.8 (B); 11.24 (lat); 13.13 (Cl); 14.39 (B); 15.51 (A[c]); 2Cor. 1.18 (B); 3.5 (B); 12.10 (**א***), 16 (D*); Gal. 1.16 (D*); 3.28 (A); Phil. 1.22 (D*); 2.3 (B); and Col. 1.12 (B).

[170] Epp, "Toward the Clarification," esp. 54-56 and 59-60.

[171] In fact, Royse himself was able to de-list 77 erstwhile singulars from Tischendorf's list in his 1981 dissertation.

[172] This was similarly observed by Hernández, "Scribal Tendencies in the Apocalypse: Starting the Conversation," 250-51, with regard to Bernard Weiss's list of "singular readings" in the Apocalypse text of Codex Sinaiticus.

[173] Houghton, "Recent Developments," 256-57, underscored this point when he noted, "Although the identification of scribal practice has traditionally proceeded on the basis of 'singular readings' peculiar to a manuscript, the number of genuinely unique readings (not taking into account nonsense forms) is being diminished as more manuscripts are transcribed in full. The current definition adopted for a singular reading as one 'which has no Greek support in the critical apparatus of Tischendorf's 8th edition' will have to be reviewed with the publication of the *ECM*".

[174] Zuntz, *TEDCP*, 39-40, commented, "An assiduous search of the textual material indeed leads to a steady reduction of the number of readings attested by one witness only; obvious scribal errors apart, the number of truly 'singular' readings is small indeed and new finds are liable to reduce it still further".

[175] Broken down to 141 orthographic, 56 nonsense, and 471 significant singulars.

[176] Broken down to 124 orthographic, 63 nonsense, and 452 significant singulars.

figures bears out our criticism of the lack of an airtight definition of the term most central to the method.

Second, this also begs the *question of textual parameters* insofar as manuscript support is concerned, i.e., "Is a singular reading in the [known] Greek manuscript tradition still a 'singular' reading when it has support of manuscripts of the ancient versions?"[177] Thus, the matter of *qualitative* (genetic) relationship is at issue. For instance, Epp rightly inquired whether the continuous Greek text-specific definition adopted by Colwell and Royse is tenable in the case of Codex D where most of the singular readings are naturally unsupported by Greek mss because of its genetic relationship with "Western texttype", and therefore obviously are supported more by "Western" versional mss than Greek-text manuscripts. Because of this latter difficulty, however, Epp suggested that for purposes of establishing the scribe's ideological proclivities and biases, it is essential to have a rather more rigid textual parameter in formulating the definition of singular reading, i.e., "textual divergence not merely without other Greek support, but also without ancient versional support or support in the patristic quotations of the NT".[178] Undeniably, Epp's recommendation is shaded with some Hortian influence.

Third is a "material" (physical) question, i.e., Should reconstructed readings be included or excluded in the list of "singular readings"? In his list of "singular readings" in $\mathfrak{P}^{46}$ that are omissions due to leaps, Royse, despite recognising the highly speculative

[177] Epp, "Toward the Clarification," 52. Equally, Elliot, "Singular Readings," 123, clearly favoured a quantitative treatment for establishing singularity, "(singular readings) are readings not found in the bulk of other manuscripts". We must note here the decision taken by Hernández, *Scribal Habits and Theological Influences*, 47, to *exclude* readings with versional support, which would have been otherwise included in Royse's methodology.

[178] Epp, "Clarification," 60. See also, Hurtado, *Text-Critical Methodology*, 69, n9.

nature of reconstruction,[179] still included three cases of reconstructed readings: Rom 12.8; 14.18; and Phil 1.1.[180] But does this accurately reflect the data?

b. The Question of Presupposition

The *question of presupposition* is a more serious concern, for the term "singular reading" in its *strictest* sense is intrinsically a very arbitrary category as it cannot be probed with utmost certainty whether a reading presently dubbed as "singular reading" is in fact historically unattested in the vast number of manuscripts that have never survived the test of time.[181] This is further complicated by the fact that, despite their discoveries, there are still many minuscules that are yet to be fully studied and transcribed.[182] This underscores the question of collation base.

Yet another pressing difficulty is the advocates' lack of accounting for the copying context, especially in discriminating between a "singular reading" as a *scribal creation* and as a reading already present in the scribe's textual base,[183] i.e., *exemplaric variation.*[184] For instance, are incremental omissions (*homoioteleuton* and *homoioarcton*) and dittographies listed as "singular readings" genuinely the *creation* of the scribe because they are not shared by any other manuscript? But what about haplographies and dittographies in the same manuscript that are shared by others? Or is it methodologically easier to assume

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[179] For his justifications for their inclusion, see Royse, *SH-M*, 280-81, n476, 283, n482 and n483.

[180] No attempt was made to check if this is also true for the other papyri that Royse analysed.

[181] Parsons, "Christological Tendency," 470, cautiously (but rightly) observed, "Singular readings constitute only a relative category at best, since new manuscript discoveries could remove the 'singularity' of any reading"; see also, Aland, "The Significance of the Chester Beatty Papyri," 110, n12.

[182] Elliott, "Singular Readings," 123. One only needs to visit VMR[2] to witness the magnitude of collation work needing to be done.

[183] Houghton, "Recent Developments," 257, also underscored this point when he commented, "… the presence of a particular form in the first-hand text of a given manuscript cannot necessarily be ascribed to the copyist's choosing but may have been inherited from the exemplar: the characteristics isolated by the study of singular and sub-singular readings apply not so much to the scribe as to the form of text found in the manuscript. Only the study of corrections and other annotations provides firm evidence for the intervention of individuals." Although to the last sentence, I would add that "*all*" corrections and annotations, not only selective ones, should be studied in their entirety.

[184] For the use of this term in this thesis, see pp. 248-66.

that variations of this type are faithful reproductions of the readings of the scribe's *exemplar* and not the scribe's own creation? The question thus is, "Is it really methodologically justifiable to impute a particular habit (especially with *negative* undertones) to a scribe of a specific manuscript if the evidence can point to multiple possible culprits?" Satisfactory answer to this question does not come easy. Hort, for instance, whilst recognising the need to discriminate (from the list of "singular readings") between a scribe's "individualisms" and "inherited" readings (what he calls as "ancestral" text), could appeal only to "approximation", not methodological certainty, in view of the fact that the *exemplars* no longer exist.[185] Because of this also, Hort was less strict in his ascription of who started the "singular readings" that are "mere individualisms", arguing that they may have "originated with the scribe or one of his immediate predecessors".[186]

Furthermore, if a *copying by dictation* context is assumed, were these "singular readings" spawned by the oral reading of the *lector* and not by the copying scribe?[187] And here we can easily find potential examples via the pattern-less interchanges of orthographic forms, e.g., **αι** to **ε**, **ο** to **ω**, and *vice-versa*. Do orthographic variations of this nature truly reflect the inabilities of a scribe? Or can these be attributed to this oral

[185] Hort, *Introduction*, 231-32: "On the other hand the singular readings of a document may always be due either to inheritance from a more or less remote ancestry, which may be of any degree of purity, or to quite recent corruption, or, which is much the commonest case, partly to the one, partly to the other. Whatever a document has inherited of the autograph text is of necessity included in its proper or ancestral text; and in order to ascertain the character of those of its singular readings which belong to its ancestral text, we must sift away as far as possible those other singular readings which are mere individualisms, so to speak, originating with the scribe or one of his immediate predecessors. Complete discrimination is of course impossible in the absence of the exemplar or exemplars; but every approximation to it is a gain." Conversely, Royse, *SH-M*, 40, was more optimistic in his citation of Hort, "Hort grants that this discrimination may often be difficult or even impossible to carry out, but asserts that one can often succeed."

[186] Hort, *Introduction*, 232; see also the related caveats mentioned by Jongkind, "Singular Readings in Sinaiticus," 35-36.

[187] The recognition of the latter point is increasingly becoming more important in the discussion of textual transmission, and it would be unwise to leave this out of the equation. On this, see Thomas Wayment, "The Scribal Characteristics of the Freer Pauline Codex," in *The Freer Biblical Manuscripts: Fresh Studies of an American Treasure Trove* (ed. L. Hurtado; TCS 6; Atlanta: SBL, 2006), 251-62, esp. pp. 253-57. See also, pp. 249-51 of this thesis for related discussion on scribal copying by dictation.

component in the transmission history that was already reflected in the text of our scribe's *exemplar*?

Having said the foregoing, it becomes rather clear that there is practically no way of genuinely ensuring that a reading presently dubbed as "singular" (or any variant reading for that matter) was indeed the *actual* creation of the scribe of a manuscript being analysed or a product of a faithful reproduction of the *exemplar*.[188]

The difficulties (if not impossibilities) in establishing "singularity", either because of the lack of air-tight definition or because of the problem of collation, undermine the very presupposition by which this method of locating scribal habits was founded. On the contrary, it seems more likely that, as Parker suggested, *manuscript loss* may have caused the "singularity" of a reading rather than due to the *creation* of individual scribes.[189] This becomes more telling as one notes the very slim survival rate of Christian and biblical manuscripts from the second-third centuries.[190]

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[188] A kind of "compromise" view is held by Tommy Wasserman, "The Early Text of Matthew," in *Early Text of the New Testament*, 83-107, p. 85, who argued, "Obvious errors and singular readings can more confidently be attributed to the scribe, especially if there is a discernible pattern… On the other hand, non-singular readings may also be creations of the scribe, and agreement with other witnesses coincidental." Whilst this may sound a bit more palatable proposal, the methodological criticism outlined here remains un-addressed though, for it similarly presupposes that "scribal habits" is based on "peculiarity" more than "recurrence". Interestingly, Hernández, "Scribal Tendencies in the Apocalypse," 252, acknowledged the conceptual difficulties attached with the idea of "singular readings" as scribal creation: "all of the singulars considered in this study are 'created readings', insofar as they are obvious departures from the 'original'. This does not mean, however, that the two scribes of Sinaiticus created all of them. Some, no doubt, were already in the *exemplar* or *exemplars* of the scribes".

[189] Parker, Review of Royse, *Singular Habits*, 256. The caveats given by Elliott, "Singular Readings in the Gospel text of $\mathfrak{P}^{45}$," 123, in using this method are also very instructive, "Singular (or even 'sub-singular') are dangerous words. What we mean by these terms is that these are readings not found in the bulk of other manuscripts, but we must remember that the vast majority of other manuscripts, especially minuscules, have not been read in their entirety. So, it may be argued, today's singular readings could tomorrow turn into a reading shared by other recently read manuscripts… The other thing that must be said at the outset is that even if our current extant fund of manuscripts reveals that a reading in one manuscript is unique, singular and distinctive, that does not of course mean that it was ever thus. The sheer chance of survival may deny our ever knowing if that distinctive reading once shared (commonly) in its own day. All we may do is to say that of the manuscripts that happen to be extant today we have at this or that verse a text otherwise unattested elsewhere".

[190] For a summary list of surviving Christian manuscripts during the second and third centuries, see Roger S. Bagnall, *Early Christian Books in Egypt* (Princeton: Princeton University Press, 2009), 17.

In any case, it is my contention that "singular readings", whilst they may provide a creative way of categorising some of a particular manuscript's textual variations and errors, are *not* necessarily "scribal creations" as such but unique *textual patterns* extractable from that extant text;[191] at best, they point to the *de facto* state of a manuscript's text as it survived the test of time, which may partially give an idea about a particular scribe's "copying habits".[192]

On top of all this, it seems to me that the major hurdle to fully profile the "habits" of a particular scribe is a categorical one, i.e., what do we exactly mean by "scribal habits"? What constitutes a scribal habit? Does the concept "scribal habits" exclusively refer to the textual peculiarities of a particular manuscript or does it broadly refer to all the derivable recurring patterns of everything where scribal activity and participation might have been involved?

Habits imply "recurrence and frequency", not necessarily "uniqueness". Scribal habits are everything *in* the manuscript that betray the proclivities and practices of the scribe who produced that particular manuscript, inclusive of all the technical stages of its production,[193] and maybe extended to its recoverable immediate reception history.[194] Scribal habits do not need to be shared by no one else, and therefore need not necessarily be isolated against the manuscript tradition. As in the natural world order, human habits

[191] Whilst the call of Silva, "Text of Galatians," 23, for a broader base of variants "to know the profile of a manuscript" is commendable, his judgement to use "singular readings" as the criterion for coming with a "completely accurate profile of individual *scribal habits*" is nonetheless regrettable.

[192] This view is somewhat akin to the judgment of Min, *Die früheste Überlieferung des Matthäusevangeliums*, 36, "*Singulärlesarten lassen nur die Schreibgewohnheiten eines Kopisten erkennen.*"

[193] This includes, but not limited to, the processes of decision-making on which text/s to copy and the desired general lay-out, codex(/roll) production (deciding on quiring format, pasting/cutting of papyrus to the desired size, arranging of sides [r-r or v-v or r-v or v-r], laying, folding, boring of threading holes, stitching, etc.), choosing-sharpening of writing implements, ink mixing (and its attendant processes), ruling, actual copying, reading of *exemplar*, correcting, putting of protective cover, etc.

[194] Reference is made here to the reading marks in $\mathfrak{P}^{46}$, suggesting how the lay-out and text of $\mathfrak{P}^{46}$ was eventually construed by its users. On this subject, see fuller discussion in pp. 182-93.

may also be shared by others notwithstanding their differences in circumstance, locality/geography, and time. By the same token, scribal habits need not be the exclusive domain of one particular scribe only, hence, the quest for the "singular readings" of a particular manuscript. A scribe's traceable habits may be shared by other scribes of the same era or even of a later generation, and such agreement does not disqualify that scribe as a temporal possessor of that particular habit. Sometimes those habits are activities bound within the scribal trade itself (codex production, ruling, aesthetics, use of *nomina sacra*, etc.), some have to do with the textual tradition (special readings, including big block relocations, etc.), and some betray the scribe's own copying habits (occurrences of textual errors due to the physical details of the material itself, etc.).

David Parker once noted, "(O)bservation of scribal habits may lead us to certain conclusions with regard to a manuscript's *exemplar*. *If* we are able to isolate certain scribal habits, we will be in a position to decide that such distinctive readings of a manuscript are not to be regarded as the text of its ancestor. But this has to be refined. The problem is similar to the criterion of dissimilarity in Life of Jesus research, in that the method only works *if* we can be sure that the habits of the scribe are different from those of the exemplar, and of earlier copies. Under certain circumstances, we are able to establish this. But we have to suspect always that the scribes of a particular period may share similar habits, as a result of their education, preoccupations and working customs."[195] Whilst there is a sense of confidence in Parker's tone, it is nonetheless a very conditional statement, dependent upon the level of certainty we can amass in locating scribal habits of particular manuscripts.

How then are we to locate the scribal habits of $\mathfrak{P}^{46}$?

[195] D.C. Parker, "Scribal Tendencies and the Mechanics of Book Production" in *Textual Variation: Theological and Social Tendencies*, 173-83, p. 174. Emphasis added.

V. (RE-)INVESTIGATING THE EVIDENCE: TOWARD AN INTEGRATIVE APPROACH TO THE STUDY OF THE SCRIBAL HABITS OF $\mathfrak{P}^{46}$

A. The Imbalance in Assessing the Evidence of $\mathfrak{P}^{46}$

A keen observer will have realised by now that most of the studies undertaken thus far with regard to $\mathfrak{P}^{46}$ almost exclusively, with very few exceptions,[196] have to do with the *text* it reflects,[197] and very little with its *paratextual* features. Such a *text-focused* history, to a certain degree, is explicable because $\mathfrak{P}^{46}$ is thus far still the widely-received earliest extant witness attesting to the Pauline Letters.[198] Naturally, a high degree of sentiment results from this very fact, especially when the text of $\mathfrak{P}^{46}$ is viewed against the traditional goal of NT textual criticism.[199] However, since its

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[196] For instance, T.C. Skeat, "Did Paul Write to 'Bishops and Deacons' at Philippi? A Note on Philippians 1.1," *NovT* 37 (1995): 12-15; repr. in *Collected Biblical Writings of T.C. Skeat* (NovTSupp CXIII; ed. J.K. Elliott; Leiden/Boston: Brill, 2004), 258-61, who exemplified a textual study vis-à-vis calculable codicological evidences. But even this study is also a bit unsatisfactory, since Skeat's analysis was based mainly on Kenyon's transcription; on this, see our critique in p. 108, n169.

[197] This interest on the text of $\mathfrak{P}^{46}$ is further made evident by the fact that some scholars even commented on the reconstructed portions; for instance, G.D. Kilpatrick, "The Chester Beatty Papyrus $\mathfrak{P}^{46}$ and Hebrews xi.4," *JTS* 62 (1941): 68-69; T.J. Finney, "A Proposed Reconstruction of Hebrews 7.28a in $\mathfrak{P}^{46}$," *NTS* 40 (1994): 472-73; S.R. Pickering, "Hebrews 7.28: Priest or High Priest," *NTTRU* 2 (1994): 93; and Skeat, "Did Paul Write to 'Bishops and Deacons' at Philippi?" 258-61. cf. Holmes' "The Text of $\mathfrak{P}^{46}$," where he argued that some of $\mathfrak{P}^{46}$'s distinctive readings in Romans might have been an evidence of an early form of a "commentary".

[198] But whether the witness of $\mathfrak{P}^{46}$ is fully utilised in providing definitive judgments as to the "primitive" text of the Pauline epistles was raised by Søren Giversen, "The Pauline Epistles on Papyrus," in *Die Paulinische Literatur und Theologie/The Pauline Literature and Theology* (ed. S. Pedersen; Göttingen: Vandenhoech & Ruprecht, 1980), 211-12. Of course, given the historical framework from which major NT papyri were discovered, $\mathfrak{P}^{46}$ was initially treated with a wait-and-see attitude by textual critics operating on the presupposition that the NT text was already established by the Enlightenment project. As E.J. Epp, "The Papyrus Manuscripts of the New Testament," in *TNTCR*[1], 3-21; repr. *PNTTC*, 411-35, pp. 422-23, rightly observed, "(T)he first series of New Testament papyri did not produce instant or widespread changes in the critical texts of the New Testament; on the contrary, even after the discovery of the Chester Beatty papyri…, these early papyrus artifacts of the New Testament text were often treated not so much as welcome illuminators of textual history, but more as intruders or even irritants to an already well-established and quite satisfactory understanding of the history of the text".

[199] The pursuit for the traditional goal of textual criticism had somehow influenced this marked editorial preference for the text. S.E. Porter, "Pericope Markers in Some Early Greek New Testament Manuscripts," in *Lay-out Markers in Biblical Manuscripts and Ugaritic Inscriptions* (eds. M. Korpel and J.M. Oesch; Pericope 5; The Netherlands: Royal Van Gorcum, 2005), 161-76, pp. 161-62, keenly observed that "… editors are so concerned to establish the text for the purpose of collation with other texts that they often pass by distinctive features of particular manuscripts. In other words, there is a greater concern for the text itself, almost in an abstract sense, than there is for the particularities of individual manuscripts,

discovery in 1931, we have yet to behold a work on $\mathfrak{P}^{46}$ that does not only deal with its text, but also treats $\mathfrak{P}^{46}$ both as an ancient manuscript *and* a Christian text, with *metatextual* components.[200] In fact, only a handful of manuscripts have been treated in such a way;[201] perhaps the best known example to date is Codex Sinaiticus. Those *metatextual* features equally deserve the scholar's time and attention, for they are in many ways more definitive indicators of actual scribal activities. It is equally important to ask the following: Why did the scribes of our earliest surviving manuscripts use *nomina sacra*?[202] Or why would some scribes set the text of their manuscripts in *cruciform* pattern,[203] or why do some of our surviving manuscripts bear

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with all of their differences in handwriting, size, and accompanying palaeographical features." See also Tobias Nicklas, "Zur historischen und theologischen Bedeutung der Erforschung neutestamentlicher Textgeschichte," *NTS* 48 (2002): 145-58, p. 145, who expressed his dismay that NT textual criticism is generally considered only as a "*Hilfsmittel auf der Suche nach dem 'Urtext'*".

[200] Here I must emphasise at the outset that $\mathfrak{P}^{46}$ was written on papyrus, gathered in a codex, in a single-quire manner, inscribed in a calligraphic hand, reflecting different kinds of inking density, with very few punctuation, with varying numbers of lines per page and fluctuating number of characters per line, with στιχοι contradicting the actual number of lines copied, with one opening without pagination, with non-traditional book arrangement, so on and so forth. Like its text, these features also deserve to be seriously analysed. One must resist the temptation of going directly to the text and completely ignore these features. More importantly, any diverging pattern derived from these naturally point to scribal activities that help in painting a more complete portrait of our scribe.

[201] As Bruce Metzger confided, "Besides the collation of the text of the New Testament manuscripts, it is also necessary for scholars to examine carefully all aspects of the physical make-up of the documents. Such codicological examination involves a painstaking study of the preparation of the parchment, the ruling pattern, the gatherings and sewing of the codex, as well as the detailed palaeographical analysis. Only a very few manuscripts have received such a minute scrutiny"; see "The Future of New Testament Textual Studies," in *The Bible as Book: The Transmission of the Greek Text* (eds. S. McKendrick and O. O'Sullivan; London/Michigan: British Library/The Scriptorium, 2003), 201-08, p. 204,

[202] The writings of Larry Hurtado on *nomina sacra* provide informative data in appreciating how this convention might have played in the scribal trade and in the life of the nascent Church; see fuller discussion of the *nomina sacra* in $\mathfrak{P}^{46}$ in pp. 323-66.

[203] Does this format betray the magical, hence non-liturgical, use of Christian manuscripts? If so, why and how did this get into the manuscript tradition and what role did scribes play in this manuscript-production context? For introductory discussion on this, see E.A. Judge, "The Magical Use of Scripture in the Papyri," in *Perspectives on Language and Text: Essays and Poems in Honor of Francis I. Andersen's Sixtieth Birthday* (eds. E.W. Conrad and E.G. Newing; Winona Lake: Eisenbrauns, 1987), 339-49. See also, Tommy Wasserman, "$\mathfrak{P}^{78}$ (*P.Oxy.* XXXIV 2684): The Epistle of Jude on an Amulet?" in *New Testament Manuscripts*, 137-60; and Theodore de Bruyn, "Papyri, Parchments, Ostraca and Tablets written with Biblical Texts in Greek and Used as Amulets: A Preliminary List," in *Early Christian Manuscripts: Examples of Applied Method and Approach* (eds. T.J. Kraus and T. Nicklas; *TENTS* 5; Leiden: Brill, 2010), 145-89.

the inscription (from other later hands) "ἑρμηνείαι" or "προσερμηνείαι"?[204] Or why would later manuscript users put markers (i.e., reading marks) that seem to represent sense-units? And a whole host of other pertinent questions. These features, although integral parts of the transmission history of that particular manuscript, are unfortunately no longer accessible in the printed Greek text editions, hence, somehow we have lost a great deal of appreciation for the "forms" in which these copies of the *sacred text* were transmitted to us. Lest one forget, the text of 𝔓[46] (and all other manuscripts for that matter) was not independent of the physical vehicle that carried it, through which it survived the rigors of age, destruction, as well as the function it might have played along the way. The essence of the text is inseparable from its material texture.[205] When we speak of scribal habits, we are not only dealing with *what* a particular scribe wrote but equally with *where*, *how* and *why* this scribe wrote it that way. This underscores the need to look at manuscripts as ancient artefacts and not only as inconsequential containers of "sacred texts". As Hort advocated, "Knowledge of documents should precede final judgments upon readings."[206]

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[204] Bruce Metzger, "Greek Manuscripts of John's Gospel with 'Hermeneiai'," in *Text and Testimony: Essays in honour of A.F.J. Klijn* (eds. T. Baarda, et al; Kampen: Kok, 1988), 162-69, p. 162, argued that this feature was used as a "means for telling fortunes". See also, P.W. van der Horst, "*Sortes:* Sacred Books as Instant Oracles in Late Antiquity," in *The Use of Sacred Books in the Ancient World* (eds. L.V. Rutgers et al; Contributions to Biblical Exegesis and Theology 22; Leuven: Peeters, 1998), 143-73. From a text-editing perspective, see D.C. Parker, "Manuscripts of John's Gospel with *Hermeneiai*" in *Transmission and Reception*, 48-68; repr. in *David C. Parker: Manuscripts, Texts, Theology: Collected Papers* (ed. D.C. Parker; *ANTT* 40; Berlin/NY: de Gruyter, 2009), 121-38.

[205] Ulrich Schmid, "Scribes and Variants," 23, shares similar sentiment, "… do not separate variants under scrutiny from their physical container nor their socio-cultural context of literary production/reproduction in early antiquity. Before using the fashionable 'some scribes changed' prose, ask yourself: who contributed what and when to a manuscript?"

[206] Hort, *Introduction*, 31. Frederic Kenyon, *Books and Readers in Ancient Greece and Rome* (2nd ed.; Oxford: Clarendon, 1951), 40, was certainly correct when he asserted, "The external form of books has at all times affected and been affected by their contents. The materials available for writing have facilitated or impeded the output of literature. Fashion and convenience have dictated the size and shape of books, and thereby have affected the scale and character of their contents."

Conversely, any codico-palaeographical study of $\mathfrak{P}^{46}$ must go beyond its physical features;[207] palaeography and codicology should correspondingly relate their findings in relation to its text,[208] if we are to appreciate better the social history behind the scribe and the text he produced.[209] It needs no further elaboration that there should be no dichotomy between codico-palaeographical and textual studies, as they are not mutually exclusive disciplines. We cannot speak about the textual relationships of $\mathfrak{P}^{46}$ and its textual character without talking about its physical relationships with other papyri that were similarly unearthed from the sands of Egypt or elsewhere.[210] Hence, any sensible study on the scribe of $\mathfrak{P}^{46}$ must inevitably start with its physical features and how these features conceivably affected the production and the transmission of the text that it reflects. By doing this, we can achieve a kind of microcosmic social history within $\mathfrak{P}^{46}$—revealing how its scribe and the other

[207] We may perhaps be allowed to cite the appeal of Kenneth W. Clark, "Manuscripts belong to Archaeology," *BASOR* 122 (Apr 1951): 7-9, p. 8, before leading Western and Oriental archaeologists when he put forward the arguments for expanding the scope of archaeology to include biblical manuscripts: "(But) archaeology itself is a discipline broad enough to embrace a wide range of interests and a long reach of time. It is evident that a broadening interest has emphasized the New Testament and the Roman periods... If all these factors resolve in a logical conclusion, it is that archaeological institutions should actively embrace the field of manuscript research as an integral part of their responsibility and service".

[208] As David Parker, *An Introduction to the New Testament Manuscripts and their Texts* (Cambridge: CUP, 2008), 33, rightly underscored, "Palaeography is widely conceived to be the process of dating and localising manuscripts. Their subsequent history is then viewed as a matter for the historian of the book. However, it will be observed that the palaeographer is extremely interested in the further history of the manuscripts, because of the evidence available from it for the study of yet more manuscripts, with regard to influence and development. The work of the best palaeographers… includes codicology and includes book history as well as knowledge of the texts. There is not yet a discipline of book history within the world of New Testament manuscript studies."

[209] On this point, I concur with Kenyon, *Books and Readers*, 41, "It is therefore of importance to know, as fully as the extant evidence permits, the form of book which was prevalent in the ancient Greek world… It also has a bearing on textual criticism, since the restoration of corrupt passages is to some extent conditioned by the habits of ancient scribes. No excuse therefore seems to be required for setting out, even in somewhat minute detail, the present state of our knowledge with regard to the material of Greek books and the habitual practices of their transcribers."

[210] Clark, "Manuscripts belong to Archaeology," 7-9, rightly suggested that manuscripts are not the exclusive domain of textual scholars, but should be an inter-disciplinary concern. Along this line, the works of Eric G. Turner come to the fore, particularly his *Typology of the Early Codex* (Haney Foundation Series 18; Pennsylvania: University of Pennsylvania, 1977) and *Greek Manuscripts of the Ancient World* (2nd revised and enlarged edition; Bulletin Supplement 46; ed. P.J. Parsons; London: University of London-Institute of Classical Studies, 1987).

hands that worked on it contributed to the inscription and transmission of its text, producing the textual complex that we now have, and enriching our knowledge of how all these factors might have intersected at one point of human history.

B. Toward an Integrative Method in locating the Scribal Habits of $\mathfrak{P}^{46}$

In attempting to locate the scribal habits of $\mathfrak{P}^{46}$, I employed generally the methodological model of David Parker in his incisive but integrative study of Codex Bezae[211] as an ancient manuscript *with* Christian text, highlighting the necessity of treating NT manuscripts in tandem with palaeography and codicology, i.e., as manuscripts *and* as texts.[212]

Parker has placed a premium on the important points that individual witnesses must be examined first *as a manuscript* before setting them in the larger spectrum of manuscript tradition, and second, that we should seriously consider individual manuscripts as physical objects in themselves. With regard to the first, he maintained that a manuscript should be examined "to assess the character of the scribe, to enquire into the nature of the tradition from which the copy is derived, and to attempt to show why this manuscript is what it is."[213] This means that *any* ancient manuscript is intrinsically important; its worth does not depend on whether it readily helps or not in the establishment of the "original text". With regard to the second, Parker rightly underscored that "documents consist of more than the texts they contain, and their layout, their design and the material of which they are made, their ink and script, their marginalia and the ornamentation, paintings and bindings with which they may have been adorned all provide evidence about cultural as well as religious history and even cast

[211] D.C. Parker, *Codex Bezae: An Early Christian Manuscript and Its Text* (Cambridge: CUP, 1992).

[212] Parker's *Introduction to New Testament Manuscripts and their Texts* is equally important in the utilisation of his model for this thesis.

[213] Parker, *Codex Bezae*, 1-2.

light on economic, social and political matters."[214] This highlights our point that any worthy study of a manuscript should be integrative of all the features derivable from the manuscript itself.[215]

Having said that, it is not my intention in this project to unnecessarily reinvent or deconstruct the wheel skilfully fashioned by the earlier students of $\mathfrak{P}^{46}$, especially Zuntz and Royse; they have made their own contributions so well. I do intend nevertheless to connect their textual and text-based scribal analyses more closely to its physical features. Using Parker's conceptual model,[216] this thesis is an attempt to conduct an integrative study of $\mathfrak{P}^{46}$, aiming to learn more about the scribe who produced it and the habits he left traceable in this manuscript. As a matter of definition, I am using integrative approach in this research as the methodological integration of papyrology, codicology, palaeography, and textual criticism; of the inseparable connection of $\mathfrak{P}^{46}$'s *physical*, *textual*, and *paratextual* features; of *what* was written and *where* and *how* it was written.[217] This investigation includes looking at the leaves, the folia, the different copying conventions and *sigla* and their placement in the text, the spacing, the inks and inking practices, the laying-cutting-stitching of and the fibre orientation of the papyrus leaves, the ruling, and other *paratextual* components vis-à-vis the final copied text—its errors and its subsequent

[214] Parker, *New Testament Manuscripts and their Texts*, 7-8.

[215] Thomas Kraus, "'Parchment or Papyrus?': Some Remarks about the Significance of Writing Material when Assessing Manuscripts," in *Ad Fontes: Original Manuscripts and Their Significance for Studying Early Christianity—Selected Essays* (Texts and Edition for New Testament Study 3; ed. S.E. Porter and W.J. Porter; Leiden/Boston: Brill, 2007), 13-24, pp. 13-14, rightly put it, "(O)nly the compilation of papyrological and palaeographical data can form the basis for decisive grounds and conclusions that refer to, for instance, the date of writing, scribal convention and habits, the potential purpose, and the real people behind each fragment or manuscript."

[216] To a certain extent, the dissertation monograph of Jongkind, *Scribal Habits of Codex Sinaiticus*, similarly traverses toward the direction of Parker's model.

[217] In this regard, I concur with Kraus, "'Parchment or Papyrus'," 20, underscoring the discrepancy among textual scholars in executing value judgment on manuscripts, "Only if the artifact is considered and looked at in an appropriate way may one gradually get closer to what is really somewhere hidden behind papyri, parchments, ostraca, wood tablets and the like: the socio-cultural conditions of the time and above all the real people of a time long ago, of whom we desparately (sic) seek to know more than we do now."

corrections, its agreements with and divergences from other manuscripts, its normalities and its idiosyncrasies, etc. To establish *scribal habits*, recurring patterns must be detected from both the derivable material *and* textual evidences, regardless of whether they are singularly attested or supported by other manuscripts. Whilst textual analysis reveals the copying errors of the scribe, an integrative analysis takes the investigation deeper as to the reasons or factors why those textual errors were most conceivably committed, insofar as the physical material is concerned. Whilst textual analysis may suggest tendencies, papyrological-codicological analyses (in)validate those suggestions, as the case maybe.

In pursuing this aim, I did not only work with the transcriptions (and facsimile) of Kenyon and Sanders—our very aim necessitates that both the details of the text and the writing material of $\mathfrak{P}^{46}$ are taken into account as completely as possible. Hence, I primarily worked from high-quality digital images of the Michigan (and a few Dublin) leaves. Working on the actual manuscripts at the first instance is the desiderata of all researchers, but working with manipulatable digital images is perhaps the second best working environment. On various occasions, having completed my initial transcription and analyses, I visited the sites where the actual leaves are permanently housed, enabling me to recheck the accuracy of my transcriptions and analyses directly from the images. My up-close and personal contact with the actual leaves made me more meticulously attentive to the fascinating details our "narrator" was dramatically revealing bit by bit as I flipped from one page to another—in this context, this thesis is in a way also a memoire of that life-changing encounter.

CHAPTER THREE
PAPYRUS 46 AS AN ANCIENT MANUSCRIPT: THE PHYSICAL FEATURES OF $\mathfrak{P}^{46}$

INTRODUCTION

The open page of any biblical manuscript prominently highlights what it contains and intends to transmit through that content. But a manuscript contains more than just scribbles of text considered by the early Christian communities as "Scriptures" that govern the rule of their faith. In fact, any ancient manuscript contains and transmits *both* the inscribed texts *and* the whole world of scribal ethos and culture bound up with it. This chapter analyses the physical and paratextual features of $\mathfrak{P}^{46}$, aiming to reveal recurring scribal patterns or features in the production, composition, and subsequent use of the manuscript, that are clearly attributable to the scribe who "gave birth" to it.

SECTION ONE
"SINGLE-QUIRE PAPYRUS CODEX"
THE CONSTITUTIVE ELEMENTS OF $\mathfrak{P}^{46}$

INTRODUCTION

When its discovery was publicised, $\mathfrak{P}^{46}$ became an overnight sensation, morphing from obscurity to become one of the oldest surviving examples of a papyrus artefact! But that is just a speck in a very broad canvass, for the codicological minutiae $\mathfrak{P}^{46}$ evinces are very instructive for comparative analyses across similar biblical (and literary) documents from antiquities.[1] Let me begin with the obvious: the pages of $\mathfrak{P}^{46}$ are made up of papyrus strips.[2]

I. MADE OF PAPYRUS…

That $\mathfrak{P}^{46}$ is a papyrus manuscript[3] immediately raises questions of methodological importance to NT textual criticism. Foremost is the issue of forming a judgement in terms of manuscripts' material composition vis-à-vis the text they reflect, i.e., Should papyrus manuscripts be given *automatisch Bedeutung*[4] despite the fact that the integration

[1] For instance, Kenyon, *CBBPIntro*, 9, referring to the Chester Beatty biblical papyri, asserted, "This group of manuscripts makes a notable addition to our knowledge of the methods of book production in the early centuries of the Christian era. All have one characteristic in common—that they are codices, not rolls; and it is for the early history of the codex form of the book that they are so important."

[2] For a chemical analysis of the papyrus pith, see Arie Wellert, "The Reconstruction of Papyrus Manufacture: A Preliminary Investigation," *Studies in Conservation* 34/1 (Feb 1989): 1-8, esp. pp. 3-6.

[3] Literature in this area abounds but a few may be mentioned. Dated but still useful handbooks include Wilhelm Schubart, *Das Buch bei den Griechen und Römern (*Zweite umgearbeitete Auflage; Berlin und Leipzig: bereinigung Wissenschaftlicher Verleger; de Gruyter & Co., 1921); Kenyon, *The Palaeography of the Greek Papyri* (Oxford: Clarendon, 1899); Naphtali Lewis, *Papyrus in Classical Antiquity* (Oxford: OUP, 1974); Eric G. Turner, *Greek Papyri: An Introduction* (2nd edition; Oxford: Clarendon, 1980); Idem, *Greek Manuscripts of the Ancient World* (*GMAW*[2]). The most recent extensive resource is Roger S. Bagnall, ed., *Oxford Handbook of Papyrology* (Oxford: OUP, 2009).

[4] A phrase used in NA[26], 12*. In the English translation of Barbara Aland and Kurt Aland, *The Text of the New Testament: An Introduction to the Critical Editions and to the Theory and Practice of Modern*

of their texts into our modern printed critical editions has not essentially altered the textual landscape of NT, more than 100 years after Westcott-Hort capitalised on 4th century parchment codices?[5] Thomas Kraus' inquiry candidly underscores this tension,

> (B)y focusing solely on the writing material as the criterion, manuscripts are automatically put into a category that exegetes and textual critics often rashly accept as superior to other categories and as more significant for the reconstruction of the Greek New Testament. But why should papyri *per se* have preserved a more reliable text and be more important than parchment manuscripts dated to the same period of time or even older?[6]

In view of this inquiry, we therefore must ask whether the value of $\mathfrak{P}^{46}$ to textual criticism rests on its material or whether there is something inherently valuable in this manuscript. I cannot agree more with Kraus and Epp's view that value judgment upon manuscripts should not be made solely on the basis of their material. But here I must immediately make some qualifications. I absolutely do not wish to unilaterally impute special textual import upon $\mathfrak{P}^{46}$ simply because it is a papyrus manuscript—that is definitely putting the carriage before the horse. Certainly, the credibility of its readings needs to be adjudicated by the standards of sound text-critical analyses. On the other hand, $\mathfrak{P}^{46}$, I think, presents a very good (if not quite *unique*) case for a truly serious study

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Textual Criticism (rev. and enl. ed.; trans. Errol Rhodes; Grand Rapids, MI: Eerdmans, 1989), 84, 93, this was translated as "inherent significance", referring particularly to papyri dated 3rd-4th centuries.

[5] An instructive dialogue on the matter is between Kurt Aland, *Repertorium der griechischen christlichen Papyri I: Biblische Papyri* (PTS 18; Berlin-New York: de Gruyter, 1976); E.J. Epp, "The Papyrus Manuscripts of the New Testament," in *TNTCR*[1], 3-21 (repr. *PNTTC*, 411-35; and updated in *TNTCR*[2], pp.1-39); and, Stanley Porter, "Why so Many Holes in the Papyrological Evidence for the Greek New Testament," in *The Bible as Book*, 167-86, esp. pp. 168-73. Also, note the continuing question along this line echoed by Elliott, "Recent Trends in the Textual Criticism of the New Testament, p. 119.

[6] Kraus, *Ad Fontes*, 16. He further commented as a case in point, "It is incomprehensible why, for instance, $\mathfrak{P}^{74}$ (P. Bodmer XVII) from the 7th century should gain more attention and attain more significance, as it was done in the course of its reception, than the parchment leaf 0165 (P. Berol. inv. 13271) from the 5th century just because the first is written on papyrus and thus is placed in the first category of manuscripts which leads to a more prominent position rather than the latter, placed somewhat later in the last in a rather inconspicuous position" (p. 16).

A related view is held by J.K. Elliott, "The Early Text of the Catholic Epistles" in *ETNT*, 223-24, who stated, "…we need to ask why it is that papyri are privileged by most textual critics and editors of an *apparatus criticus*. Who first promoted their supremely great authority?... Some papyri have obviously achieved their greatness just because of their age… But most papyri have had their greatness thrust upon them, whatever the dates allocated to them by palaeographers…, partly because the gullible believe that there is an unwarranted magic associated with their having been written on papyrus."

of its physical attributes, first, because it is dated early,[7] *and* secondly, it is comparatively better-preserved than other comparably early manuscripts, in terms of its contents.[8] Therefore, any study of its material configurations must be taken at its face value without necessarily instantaneously influencing the judgment on the text it reflects—this is still to be established later vis-à-vis the broader textual history. Thus, these two factors justify a dedicated in-depth inquiry as to the "physicality" of $\mathfrak{P}^{46}$, i.e., every minute detail extractable from it, and what implications it potentially presents, first for the broader study of the manuscript tradition in regard to the sociology of ancient book production, and secondly, on how its physical features as a papyrus manuscript possibly affected the rise of "special readings" now embedded in its pages that may be vital in the discussion (if not resolution) of particular variation problems in the textual tradition, whether it helps positively or negatively in our quest for the "original text" of the *Corpus Paulinum* in particular. At the end of the day, it might prove beneficial to appreciate NT textual history from the perspective of the "fibres"—the silent witnesses to our scribe's fidelity and foibles.

In its present state, $\mathfrak{P}^{46}$ is splendidly preserved compared to other equally early manuscripts with the text/s of the Pauline Epistles, although it already bears palpable marks of discoloration, breakages,[9] and wormholes[10] in some of its pages, and continuing

[7] The dating of $\mathfrak{P}^{46}$ and its undergirding history of research is discussed in Section Three "Scripts and Style: A Paleographical Analysis of $\mathfrak{P}^{46}$", pp. 137-64.

[8] For a graphic interlinear comparison of various papyri, with Pauline Epistles and Hebrews, in terms of the extent of their content, as well as a reasonable introduction to each papyrus, including dating, see *DNTAP*$^{2.1}$ and *DNTAP*$^{2.2}$.

[9] Leaves with breakages (both slight and severe) within the text area of the page include: f08, f11, f12, f13, f14, f15, f17, f22-f30, f32-f36, f38-f42, f49-f50, f52-f54, f56, f63-f65, f68-f69, f72, f75-f76, f81, f83, f89, and f92. Most of these breakages are vertical, with f75 as the most severe, almost separating the page into two fragments. Leaves with horizontal breakages include f08, f14-f15, f17, f29-f30, and f39 (most severely damaged). Fortunately, all damaged portions with texts can be easily reconstructed with high degree of certainty.

erosion in the outer and bottom edges (an inevitable result of the conservation process).[11] Apart from these immediately observable general physical blemishes, our papyrus is in good shape and is a very good specimen for conducting full-scale papyrological analysis.

Like any other ancient papyri, the material of which $\mathfrak{P}^{46}$ is made evidently went through the normal production process, i.e., stripping off the protective rind, cutting the soft pith into preferred size, laying the vertical and horizontal strips at right angles to each other, hammering, pressing, drying, pumicing, and pasting.[12] But whilst the end-product is generally good, it is still imperfect; production-related blemishes reveal themselves in the face of close-in autopsy. Prudent reflection, nonetheless, suggests that these blemishes are a potential processing window enabling us to view the level of attention exerted by our scribe on the material—but only actual details can corroborate this assertion.

Many of its leaves betray a recurring presence of small gaps between horizontal fibres.[13] Many of these small gaps are not readily apparent to the naked eye, and detectable only with the aid of a magnifying glass. Some, however, are very conspicuous, so much so that our scribe consciously avoided them (e.g., f30^{r} [Fig. 3-1.1A]), but at times wrote characters on them when it was already

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[10] Wormholes hitting portions of texts include (ranged entries indicate holes with similar worming patterns): f31, f32-f33, f35v, f36r, f37, f41, f49-f59, f60-f61, f62-f63, f66-f69, f70-f74, f83, f84-f85, and f86-f92. All these wormholes must have not been originally present, but resulted from a long non-use, as evidenced by the similar holing patterns. Nonetheless, affected texts can all be reconstructed with high degree of certainty.

[11] One excellent example is f21. Comparing Kenyon's 1937 facsimile and the actual leaf reveals that a fragment of 2.1(W) x 3.7(L) cm on the lower outer margin area has disappeared already! This must have resulted when the conservators changed the mounting glass (no one knows how many times already since its original mounting). Fortunately, as it was already outside the text area, no text was affected. Ironically, whilst f21 lost a fragment, f27 on the other hand "gained" five small fragments!

[12] On this manufacturing process, see Lewis, *Papyrus in Classical Antiquity*, esp. 34-69; but also, Adam Bülow-Jacobsen, "Writing Materials in the Ancient World," in *OHP*, 3-29, esp. 4-10.

[13] On how these small gaps might have come about during the manufacturing process, see Bülow-Jacobsen, "Writing Materials in the Ancient World," 8-10, where he compared the experiments conducted by I.H.M. Hendriks, Hassan Ragab, and Corrado Basile.

inevitable (e.g., f25[r] [Fig. 3-1.1B]),[14] although it meant that the characters are technically already written on the inner vertical layer exposed on the horizontal side due to the gaps.



Figure 3-1.1A F30[r]-II[07-12], showing the avoided horizontal gaps between lines 08-11.



Figure 3-1.1B F25[r]-II[07-13], showing the avoided horizontal gap between l[07] and l[08], and the unavoidable gap in l[12].

Generally, the writing surfaces of 𝔓[46] appear to be neatly pumiced, and must have presented little difficulty when the texts were written on them. However, there are some exceptions. Although all the Beatty and Michigan leaves are now mounted on glasses one can still have a general sense that some pages are comparatively not properly pumiced, as can be evidenced by the effect they had on the written text—finely pumiced surfaces generally have calligraphically beautiful scripts[15] whilst those that are not (especially those on the side with vertical fibres), although intelligible still, reflect badly written scripts,

[14] F16[r] is also representative of this phenomenon.
[15] For instance, f20[r], f21[r], f22[r], f24[v], f40[r], f74[r], and f76[r].

most likely due to the coarser surface[16] that prevented the proportionate settling of the ink properties (e.g., f29^{v} and f30^{v}).

The presence of a lacuna (0.9[H] x 0.5[B] cm) in f52 must have been pre-copying, as the scribe consciously avoided it in both the opposite pages, although resulting in mid-word space-gaps (Fig. 3-1.2).

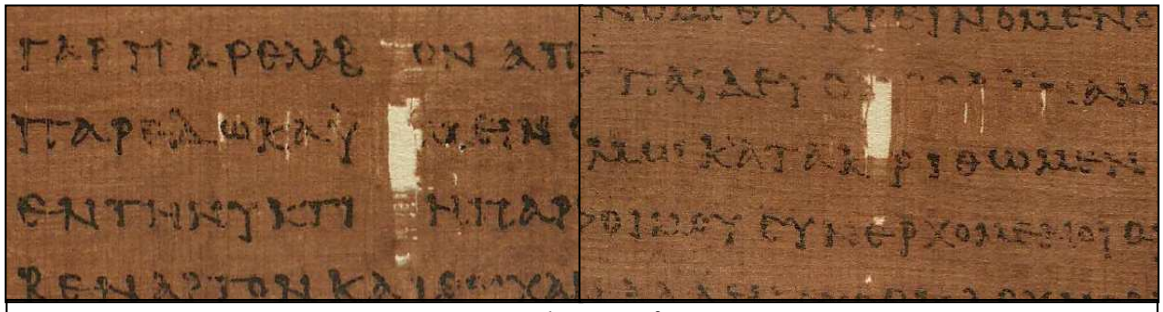


Figure 3-1.2 F52^{v} and f52^{r}, showing the lacuna in II$^{16\text{-}17}$ and II$^{17\text{-}18}$ respectively, causing mid-word space-gaps (e.g., ϋ[ ]μειν and κατακ[ ]ριθωμεν).

The perpendicular arrangements of the layered strips are by no means perfect either; defects are evident everywhere.[17] This is not to say, however, that other papyri are without blemish of this sort,[18] but only to underscore how this physical imperfection aesthetically affected the inscription of the text upon our papyrus. Hence, the rising or falling direction of the fibre strands is easily exposed once the texts have been written. Blemishes in the vertical strips are less obvious than the horizontal strips since it is likely that our scribe made use of the horizontal fibres as default ruling guides where it was convenient and possible.[19] But this is not always the case. There are intermittent instances where our scribe neatly started

[16] Of course, other factors, such as the scribe's state of being at the time of copying, as well as the horizontal fibre directions in a given page, may have cumulatively contributed to this.

[17] For instance, see f65^{v} where a 0.2-0.3 cm vertical fibre on the left-side area was stripped-off from top-to-bottom even *before* the scribe copied anything on it, resulting in some mid-word gaps.

[18] See W.J. Tait, "Guidelines and Borders in Demotic Papyri," in *Papyrus: Structure and Usage—British Museum Occasional Papers 60* (ed. M.L. Bierbrier; London: British Museum, 1986), 63-89, p. 68.

[19] Like Sanders, *TCPC*, 5, I did not find any indications that ruling lines were drawn by our scribe as a writing guide, but he was more likely "guided" by the horizontal fibres, due to his tendency to go by the fibre direction. Similar scribal tendency among the demotic and hieratic papyri rolls before second century A.D. was observed by Tait, "Guidelines and Borders in Demotic Papyri," 67.

straight lines but ended up with lines that are aesthetically poor because he followed the fibre directions,[20] which is exactly what happened in f82^{v} (Fig. 3-1.3).[21]

Figure 3-1.3 F82^{v} showing the lower portion lines "going with" the fibre direction.



Conversely, there are instances where our scribe self-corrected this "ruling mistake". For instance, in f40^{v}, where our scribe initially followed the "ruling" but having

[20] This of course is not distinctive to the scribe of 𝔓^{46}; other scribes were equally liable to this "mistake". For instance, column 12 of B.M. Pap. 131 (containing Aristotle's *On the Constitution of Athens*, and dated to late 1st century A.D.), shows how its scribe followed the horizontal fibres producing left-to-right sloping upward lines (plate available in Turner, *GMAW*^{2}, 102). See also, f03^{r} of 𝔓^{66}.

[21] This is a side with vertical strands but the horizontal fibres from the back-page are prominently detectable. The first five lines are generally straight, but l^{06} down the page tended to rise up, following the horizontal fibre flow. Interestingly, our scribe did not have this problem when he was writing his text on f82^{r}. See also f16 where this same thing happened.

detected the upward direction of the horizontal strands from the other side immediately straightened his line, correcting himself as it were. On the other hand, sometimes our scribe, consciously or unconsciously, followed the fibres' direction only to find himself doing correction halfway through the page already, as in f14^{r} where he started with a falling line following the fibre directions, and was able to arrest the downward flow only in l^{10}.[22] At any rate, this self-correcting awareness, although not consistently displayed throughout, is generally indicative of the coordination of our scribe's good eyesight, alert writing hand, and a processing mind, characteristic of an experienced scribe. Furthermore, that the scribe did not mechanically follow the horizontal strands as guide at every turn shows yet again, at least for the most part, the scribe's level of attention to the physical details presented by the manuscript. F60^{r} is a further case in point where, despite the fact that horizontal fibre directions of two κολλήματα are both rising to the middle where the κολλήσις is located, our scribe still successfully negotiated the leaf.[23]

As to the vertical strands, there are a few instances where the imperfections are very pronounced.[24] Consider, for instance, f16^{v} (Fig. 3-1.4 [next page]), where we noted at the upper left-side area what seem to be two uneven (oversized) vertical strands, one of which invasively protruded down to l^{05}; this might have been a replacement strand (also f14^{v}, f26^{v}, f27^{v}, f45^{v}, f53^{v}, f69^{v}). Vertical strands of f45 are also very badly pasted, especially the right-side portion where the κολλήσις is located (see also f38^{v}); a portion of a middle strand was stripped off and projected in a right-upward direction, producing space-gaps between letters,[25] which might have caused the omission of the particle αν in l^{05}.

[22] See also f51^{r}.

[23] F54^{r} is almost of similar circumstance but the joining is more pronounced, and as a consequence wordbreaks and space-gaps unavoidably transpired in almost every line.

[24] See related discussion in p. 195.

[25] As a consequence to the text lay-out, this produced quite a big space-gap (about 0.7 cm) between μητι and εκ of l^{05}, and caused a word-break in l^{04} with αποσ<0.4cm>τερειτε; see also f14^{v}.

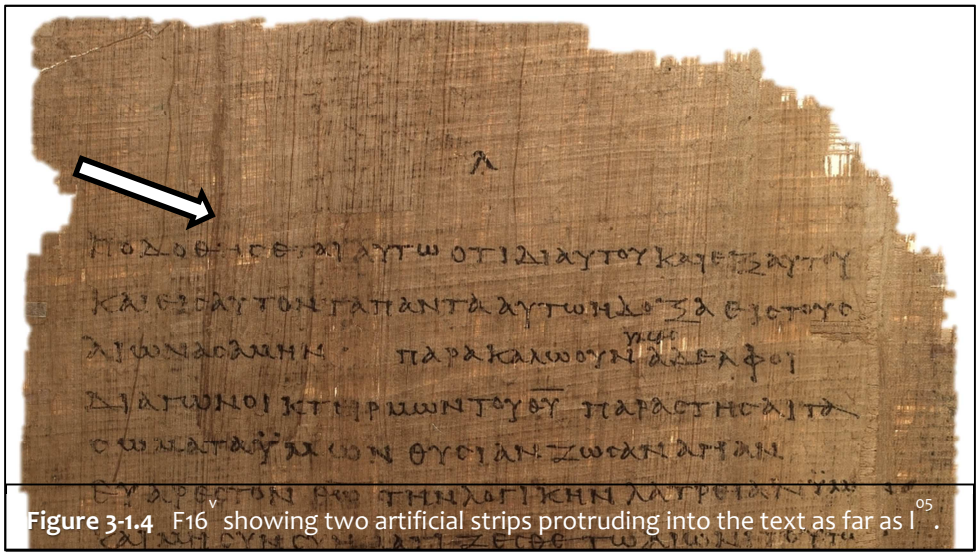


Figure 3-1.4 F16[v] showing two artificial strips protruding into the text as far as I[05].

More can still be said about these production-related blemishes in each of the pages of $\mathfrak{P}^{46}$, and we will continue doing this at appropriate junctures. But the foregoing is already sufficient at the moment to show that the materials used to produce $\mathfrak{P}^{46}$, like any other manuscripts, had physical deficiencies. The more important question, however, is whether it ever mattered really to our scribe that he was using imperfect raw materials in creating his manuscript? Are there relics in the material itself that are symptomatic that they bothered him in the process of his copying task? Looking at its codicological details is the way to go.

II. …CONSTRUCTED AS A CODEX

A. The Importance of the Chester Beatty Papyri for Codicological Studies

1. The Paradigm Shift: Early Christians' Preference for the Codex Format[26]

The advent of the Chester Beatty papyri, along with other finds of comparative age, radicalised the way scholars viewed the economy of early Christian book production,

[26] This subsection serves only to introduce how the codicological details of $\mathfrak{P}^{46}$ have been used in the literature pertinent to the discussion of this important subject, which is a broad topic in itself; hence, discussion here is mainly summative. For an exhaustive treatment of the subject, see Larry Hurtado, *The Earliest Christian Artifacts: Manuscripts and Christian Origins* (Grand Rapids, MI/Cambridge: Eerdmans, 2006), esp. pp. 43-93, and pertinent materials cited therein.

particularly in the preferred format by which they transmitted *their* sacred Scripture.[27] To borrow from Roberts and Skeat's study, prior opinions on the origin and development of the codex were "antiquated" by the series of important manuscript finds in Egypt and elsewhere.[28] Two questions, intricately connected to each other, immediately come to view; first, the question of *preference*, i.e., "Why was the codex format preferred over the roll by the early Christians?",[29] and second, the question of *material*, i.e., "Was the preferred format written foremost in parchment or papyrus?"[30] Various suggestions have been advanced to address these questions, and therefore we need only to consult standard works on the field.[31] It must be noted, however, that $\mathfrak{P}^{46}$, along with the other Chester Beatty papyri, has particular importance since "(t)hey are likewise the most significant discovery of books in codex form ever made, for they are the earliest codices of which any considerable portions have been preserved."[32]

Skeat critiqued the notion of earlier scholarship that the ubiquity of the papyrus rolls during the Greek and Roman periods meant similar preference by the early Christian communities.[33] In contrast, he emphasized that the treasure trove of manuscripts yielded by the sands of Egypt profoundly altered all this. In fact, in 1949,

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[27] On the immediate effect of papyri discoveries, see C.C. McCown, "Codex and Roll in the New Testament," 219-49; Idem, "The Earliest Christian Books," *BA* 6/2 (May 1943): 21-31; T.C. Skeat, "Early Christian Book Production: Papyri and Manuscripts," in *Cambridge History of the Bible, Volume 2: The West from the Fathers to the Reformation* (ed. G.W. Lampe; Cambridge: CUP, 1969), 54-79, 512-13; repr. in *CBW-Skeat*, 33-59. In reference to how the Chester Beatty papyri significantly helped in tipping the balance in favour of this opinion, see Kenyon, *Books and Readers*, 98-99.

[28] C.H. Roberts and T.C. Skeat, *The Birth of the Codex* (London: The British Academy, 1983), 1.

[29] Parker, *NT Manuscripts and their Texts*, 17, underscored the significance of such question in this fashion, "This is a very important question, because implicit within the answer to it are observations about the role and status of the writings in the earliest Christian communities".

[30] On this latter question, see Turner, *TEC*, 35-42.

[31] For instance, Roberts and Skeat, *Birth of the Codex*, and Turner, *TEC.* To this may be mentioned Harry Gamble's *Books and Readers*, which presents an extensive summary of the state of affairs until 1995, including his own proposals that will become evident in the ensuing pages.

[32] McCown, "Codex and Roll in the NT," 230.

[33] Skeat, "Early Christian Book-Production," 33-59. See also, H.I. Bell and T.C. Skeat, *Fragments of an Unknown Gospel and Other Early Christian Papyri* (London: British Museum, 1935), 2.

Roberts, reviewing the *status quaestionis* regarding the implications of growing papyrus discoveries at the time, argued that it was the codex format that captured early Christians' predilection despite the fact that roll was already widely used.[34] He drew this conclusion from the available data at the time, that almost all of the Christian Scripture texts discovered in Egypt, dated from 2nd-4th centuries (then numbering to 116), were in the codex format (not rolls as previously supposed),[35] as against the scarcity of its use along a parallel historical and geographical timeframe in the Greek and Latin literature, which instead favoured the roll until the fifth century.[36]

More than a decade earlier, Kenyon already intimated that the twelve Chester Beatty biblical papyri "not only... confirm the belief that *the Christian community was addicted to the codex rather than 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".[37] Whilst more

[34] C.H. Roberts, "The Christian Book and the Papyri," *JTS* 50 (1949): 155-68. This view was further articulated by Roberts in his article, "The Codex," *PBA* 40 (1954): 169-204, which was in turn revised and expanded in Roberts and Skeat, *Birth of the Codex*, esp. pp. 35-44; see also, McCown, "Codex and Roll in the NT," 228. Note, however, the important observation made by Larry Hurtado, "Early Christian Manuscripts as Artifacts," in *Jewish and Christian Scripture as Artifact and Canon* (eds. Craig A. Evans and H. Daniel Zacharias; Library of Second Temple Studies 13; London/NY: T&T Clark, 2009), 66-81, esp. 74-75, that whilst the early Christians preferred the codex for their emerging "biblical texts" they still demonstrated "greater readiness" to use the roll for their "other texts" (e.g., theological tractates, liturgical texts, etc.); see also his "Manuscripts and the Sociology of Early Christian Reading," in *ETNT*, pp. 49-62, esp. 55-56.

[35] Roberts, "The Christian Book and the Papyri," 157, 158, also underscored the point that rolls with NT texts (he mentioned three only) were all re-used rolls, i.e., the texts were written on the verso side of the roll. More recently, analysing more manuscripts, Hurtado, *Earliest Christian Artifacts*, 57, again underscored this point, "… there are no second-century Christian copies of writings that became part of the Christian canon on rolls. Indisputably, in the entire body of Christian manuscripts of the second and third centuries there is no instance of a New Testament writing copied onto the recto side of a roll."

[36] However, this changeover from roll to codex did not happen overnight, but rather gradually, perhaps spanning a timeframe of about 150 years or more. On this point and on the strategic roles of early Christian libraries, especially that of Caesarea, in the eventual preponderance of the codex, see Anthony Grafton and Megan Williams, *Christianity and the Transformation of the Book: Origin, Eusebius, and the Library of Caesarea* (Cambridge, Mass/London: Belknap Press of the Harvard University Press, 2006), 10-15.

[37] Kenyon, *CBBP-Intro*, 12. (Emphasis added). Similarly, Gamble, *Books and Readers*, 49, noted, "... early Christianity had an almost exclusive preference for the codex as the medium of its own writings and thus departed early and widely from the established bibliographic convention of its own environment." See also, Turner, *TEC*, 4.

than 60 years have elapsed since Kenyon and Robert's incisive appraisals,[38] the scenario essentially remains unaltered,[39] despite the sporadic voices vigorously criticising the tendentious attempts in NT scholarship to assign earlier dates to fragmentary Christian manuscripts.[40] The codex unambiguously won the day.

2. $\mathfrak{P}^{46}$, *the Codex Format, and the Collection and Canon of the* Corpus Paulinum

Various proposals, either as reasons or results,[41] have been advanced already in the literature attempting to account for this technological paradigm shift and other related matters. But since the "full pattern of data" has not been fully engaged, as intimated by Hurtado,[42] I shall here engage only discussions where the witness of $\mathfrak{P}^{46}$ has been directly or indirectly appealed to, and assess whether its evidence has been utilised appropriately.[43]

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[38] Other earlier statistical analyses with similar results include R.A. Pack, *Greek and Latin Literary Texts from Greco-Roman Egypt* (2nd edition; Ann Arbor: University of Michigan Press, 1965); and T. Kleberg, *Buchandel und Verlagswesen in der Antike* (Darmstadt: Wissenchaftliche Buchgesselshaft, 1967), 83-84.

[39] William A. Johnson, "The Ancient Book," in *OHP*, 256-81, p. 266, citing data from LDAB as of 2006, noted that, in general, by 2nd century 90% of the surviving books are in roll format, but by the 4th century 80% account for codices over rolls, and by 6th century, the changeover is complete. Regarding NT manuscripts in particular, the McQuarie University's Papyri from the Rise of Christianity in Egypt Project, listed (as of 2005) 61 NT papyri that are in codex format and four in re-used rolls ($\mathfrak{P}^{12}$, $\mathfrak{P}^{13}$, $\mathfrak{P}^{18}$, and $\mathfrak{P}^{22}$); [see <http://www.acrc.mq.edu.au/PCE/docs/pceconspectus.pdf> (accessed 13 January 2012)]. See also Hurtado, *Earliest Christian Artifacts*, 44-49 and 90-93, where he provided helpful statistical graphs showing the paradigm shift from roll to codex by century. Of course, appraisals of this type greatly hinge on the reliability of the dating system used for these manuscripts. For a critique of using palaeography as a *sole* dating tool, see Brent Nongbri, "The Use and Abuse of $\mathfrak{P}^{52}$: Papyrological Pitfalls in the Dating of the Fourth Gospel," *HTR* 98 (2005): 23-48.

[40] For instance, despite his very critical stance on the tendency of some scholars to go for earlier dating of fragmentary manuscripts, Bagnall, *Early Christian Books in Egypt*, 71-78, nonetheless arrived at the same impression: "…Christian books in these centuries (3rd-4th) are far more likely to be codices than rolls, quite the reverse of what we find with classical literature" (p. 74).

[41] Gamble, *Books and Readers*, 66, rightly warned against the failure to distinguish between *reasons* for the adaptation of codex and *consequences* (*results*) that were facilitated by the early Christians' preference for codex over the roll.

[42] Hurtado, "Manuscripts and the Sociology of Early Christian Reading," 55.

[43] Without oversimplifying the intricacies involved in the issue, it is fair to assume in the meantime that early Christian *preference* for the codex cannot be satisfactorily attributed to a single factor but to a confluence of many factors, which must have been predicated on *both* practical *and* theological considerations. Needless to say, it is unwise to make the claim exclusively in favour of one over the other, as viable reasons can be derived from both. For summaries and relevant critiques of the proposed advantages of a codex over roll, see Skeat, "Early Christian Book-Production," 44-53; Roberts

One of the suggested practical advantages of the codex is that in terms of *text management*, as compared with the roll, almost twice the amount of writing can be accommodated on the same amount of material in a codex format.[44] For instance, testing the validity of this suggestion, Skeat estimated that in its present codex form $\mathfrak{P}^{46}$ would need only about 1570 cm (=15.7 meters) to accommodate the ten letters of Paul, whereas 2,806 cm would be required were they written in a roll, or about 29 meters of continuous papyrus roll.[45] Admittedly, implicit in this rough calculation[46] is another proposed factor, i.e., *cost-advantage*,[47] since the cost differential between a roll and a codex seems demonstrable in this example, that is, the codex is more economical than the roll.[48] If indeed the early Christians belonged to the poorer classes and with limited educational background, economy would have been an enchanting motivation. But there are a number of problems attached to these proposals.

Whilst it may be demonstrated that the codex format has cost advantage over the roll, the difference is not substantial enough to overturn the advantages offered by

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and Skeat, *Birth of the Codex*, 45-53; Gamble, *Books and Readers*, 54-56; Hurtado, *Earliest Christian Artifacts,* 63-67; Parker, *NT Manuscripts and their Texts*, 17; and Johnson, "The Ancient Book," 265-67.

Advocating a contrary position, Bagnall, *Early Christian Books in Egypt*, 86-87, rejected both theological and practical considerations as the major factors in the change-over from roll to codex in antiquities, arguing that it is attributable more logically to the "spread of Roman habits and technologies throughout the empire" (p. 87), or what he calls *Romanization*. For a rebuttal of this proposal, see Larry Hurtado, "A Review of Bagnall's *Early Christian Books in Egypt*," *Review of Biblical Literature 01/2010* (http://www.bookreviews.org/pdf/7289_7933.pdf), especially paragraph 12.

[44] See E.M. Thompson, *Introduction to Greek and Latin Palaeography* (Oxford: Clarendon, 1912), 51.

[45] T.C. Skeat, "The Length of the Standard Papyrus Roll and the Cost-Advantage of the Codex," *ZPE* 45 (1982): 169-76; repr. *CBW-Skeat*, 65-70, pp. 68-70.

[46] See also similar cost comparison mentioned by Roberts and Skeat, *Birth of the Codex*, 46.

[47] T.C. Skeat, "Was Papyrus regarded as 'Cheap' or 'Expensive' in the Ancient World?" *Aegyptus* 75 (995): 75-93; repr. *CBW-Skeat*, 88-105.

[48] Based on his calculation, lumping all other manuscript-production expenses, Skeat estimated that the cost difference between the $\mathfrak{P}^{46}$ codex and the hypothetical $\mathfrak{P}^{46}$ roll would be a savings of 26%; see Elliott, *CBW-Skeat*, 70. Of course, Skeat doubts that economy factor alone hardly accounts for the shift from roll to codex.

the roll.[49] In fact, a random look at the pages of 𝔓[46] (as well as the other more extensive 2nd-3rd NT papyri for that matter) strongly suggests that its scribe did not seem to exhibit concern for economy since there are plenty of "white spaces" on its writing surfaces,[50] i.e., wide margins,[51] constant generous line spaces and script size, ornamented τιτλοι, and other paratextual features that occupy spaces other than the text.[52] It is also important to point out that the supposed cost advantage in Skeat's comparative figures derived from 𝔓[46] can only be appreciated insofar as the *material* and *copying fees*[53] are concerned, since ancient codex-production heavily exacted other non-monetary requirements (i.e., technical skills) from the scribe/s more than what a roll-production would, as keenly noted by Johnson and Hurtado.[54]

More strikingly, that the economy factor appealed strongly to the early Christians also presupposes an inadequate characterization of the socio-economic affiliations of the early Christians,[55] as more recent studies[56] have shown convincingly that whilst there were probably many Christians who were poor, there were also who

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[49] See especially the points raised by Roberts and Skeat, *Birth of the Codex*, 45-46, along this line; noted also by Hurtado, *Earliest Christian Artifacts*, 63-64.

[50] This point has been underscored already by Roberts and Skeat, *Birth of the Codex*, 46-47; Hurtado, *Earliest Christian Artifacts*, 64; and Gamble, *Books and Readers*, 55.

[51] As Johnson, "The Ancient Book," 267, cautiously noted that wide-margined codices "make(s) it unlikely that this (economy factor) was an important ancient consideration"; see also, Hurtado, *Earliest Christian Artifacts*, 64.

[52] This is not to suggest that these features were unimportant but only to point that this scribe (and others for that matter) seems to have greater readiness to lavish these features with more space than what they strictly required. The importance of these features in profiling the habits of our scribe is underlined at appropriate junctures.

[53] That is, whether the Letters of Paul were written on a codex or a roll the fee would be the same, dependent on the prevailing remuneration system of the day, particularly in relation to the *στιχοι*.

[54] William Johnson, *Bookrolls and Scribes in Oxyrhynchus* (Toronto: University of Toronto Press, 2004), 86, "While the codex has obvious advantages… the roll trumps the codex in one very important respect, namely, ease of construction. Codex production brings in its wake the need for specialty skills, such as the knowledge of how to fashion and plan quires, sew bindings, craft and attach the covers. Bookroll production, by contrast, is nearly trivial." Hurtado, *Earliest Christian Artifacts*, 64-65.

[55] On this, see Gamble, *Books and Readers*, 54-55; Hurtado, *Earliest Christian Artifacts*, 68-69.

[56] For instance, Abraham J. Malherbe, *Social Aspects of Early Christianity* (Baton Rouge: Louisiana State University Press, 1977), esp. 29-59; and Wayne A. Meeks, *The First Urban Christians: The Social World of the Apostle Paul* (New Haven: Yale University Press, 1983).

were of affluent means, literate,[57] and occupied positions of influence,[58] and therefore had capability and equal access to whatever the roll may have had offered. Hence, this cumulative matrix of issues does show that economic reasons alone cannot fully account for the early Christians' attraction to the codex over the roll.

On another matter, some have proposed, as we shall discuss later, that the format by which $\mathfrak{P}^{46}$ was transmitted is inseparable from the discussion of the collection and canon of the Pauline corpus, that is, the collation of the Pauline letters into a single collection (the *corpus Paulinum*) was made possible by the advent of this technological innovation. It is further argued that the usefulness of the codex for random access, its economy, and its capability to absorb larger blocks of literature into one container[59] cumulatively must have given birth to the idea of clustering related genres together. This seems deducible from the way NT books were grouped together in the extant papyrus manuscripts, i.e., Gospels (as in $\mathfrak{P}^{75}$; sometimes Gospels-Acts as in $\mathfrak{P}^{45}$), Pauline Epistles (as in $\mathfrak{P}^{46}$), Catholic Epistles (as in $\mathfrak{P}^{74}$),[60] and so on.[61] However, whilst this proposal is not far-fetched, it accompanies resultant questions that continue to perplex us. On the one side are all the possible advantages that the codex format affords and on the other are the

[57] On this, see the insightful discussion of early Christian literacy by Gamble, *Books and Readers*, 1-41.

[58] Haines-Eitzen, *Guardians of Letters,* 1-21, argued for some kind of "networking" within and among the scribal trade and how they might have wielded influence even among the wealthy sectors of the society, who adhered to the Christian faith; but cf. the point made by William Harris, "Why did the Codex Supplant the Book-Roll?" in *Renaissance Society and Culture: Essays in Honor of Eugene F. Rice, Jr.* (eds. J. Monfasani and R.G. Musto; NY: Italica Press, 1991), 71-85, pp.73-75.

See also Grafton and Williams, *Christianity and the Transformation of the Book*, 14-15, who argued that another embedded (powerful) meaning accompanied by the codex format is the social impact of Christian cultural influence or what they call *social capital.*

[59] This capability to carry multiple texts, of course, is also true for rolls, as can be seen from some extant OT rolls from Qumran, e.g., Murabaat Gen-Ex-Num^{a}, 4QGen-Ex^{a}; 4QpaleoGen-Ex^{l}; 4QExod-Lev^{f}; 1QpaleoLev-Num^{a}; and 4QLev-Num^{a}. To these may be added the Greek OT Nahal Hever (8HevXII gr) containing the Minor Prophets.

[60] Note that $\mathfrak{P}^{74}$ also includes Acts.

[61] To a large extent, this is the thesis of David Trobisch's concept of "Canonical Edition"; on this, see his *The First Edition of the New Testament* (Oxford: OUP, 2000), esp. 68-77. For a contrary view, see Michael Holmes, "Text and Transmission in the Second Century," in *The Reliability of the New Testament*, 61-79, esp. 62-65.

recurring questions of *authorship*, *collection*, and *canon*. Though I am not convinced that the suggestive iconographic effect of the codex as opposed to the Jewish roll paved the way for the preference for the codex, Peter Katz's side comment is not out of line: "... recent thinking has more than once felt induced to recur to a theological explanation of archaeological facts, where more outward argument proved no longer valid."[62]

It has been suggested by Gamble that early Christians' predisposition for the codex necessarily assumes that some decisive and trendsetting developments (whether gradual or instant) have transpired in the process of Scripture production and transmission very early in the history of the nascent Church that eventually put the codex format way above the roll.[63] He further suggested that it is not unlikely that preference for it was *also* predicated by theological considerations.[64] Congruent to this suggestion is Hurtado's observation that this preference for the codex format has been a "conscious and deliberate" decision, especially as it relates to the ritual/liturgical function of the Christians' copies of their Scriptures.[65] He equally stressed the point that although the early Christians preferred the codex format, yet they did not totally abandon the roll format, especially for their other literary requirements. He argued,

> Clearly, Christians preferred the codex generally, but they felt free to use rolls sometimes, at least for *some texts.* In particular, in the earliest extant artifacts of their book practice, it appears that Christians strongly preferred the codex for *those writings that they regarded as scripture* (or, at least, writings that were coming to be widely so regarded).[66]

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[62] Peter Katz, "The Early Christians' Use of Codices instead of Rolls," *JTS* 46 (1945): 63-65, p. 65.

[63] Gamble, *Books and Readers*, 58. Although disagreeing with their proposal, Gamble's suggestion builds on the main premise of Roberts and Skeat's theory: "So striking an effect (i.e., the shift from roll to codex) must have had a cause of comparable weight" ("Birth of the Codex," 53).

[64] Gamble, *Books and Readers*, 58. See also, Larry Hurtado, "Manuscripts and the Sociology of Early Christian Reading," 56.

[65] Hurtado, "Manuscripts and the Sociology of Early Christian Reading," 56, "Early Christians cannot have been unconscious that their preferred book-form was out of step with the larger book culture of the time. Indeed, the evidence suggests a particularly deliberate effort to move away from the bookroll for copies of texts that were intended to function in their assemblies as scripture, as part of their ritual culture, as texts that were associated closely with their gathered worship settings." See also his *Earliest Christian Artifacts*, 60.

[66] Hurtado, *Earliest Christian Artifacts*, 57.

In the case of $\mathfrak{P}^{46}$, Buck has already suggested that $\mathfrak{P}^{46}$'s book arrangement might have been influenced by its format.[67] On the other hand, Finegan went beyond that, suggesting the probability that the archetypal Pauline corpus was originally published in a codex rather than a roll.[68] This is not the only time such a proposal was made, although with more nuances.[69] And if economic and cultural conditions were already ripe for the use and circulation of the codex within the Roman Empire prior the birth of the Church, then Finegan's proposal is worth reflecting upon. In fact, Finegan finds an ally in Zuntz, who in no amount of doubt proposed that the editor of the archetype of the *corpus Paulinum* originally copied the collected letters in a codex format.[70] The major problem with Zuntz's proposal, however, is its brevity of treatment.

It was Gamble who fully developed the idea and organized a systematic enunciation of evidence for the view that the Pauline letter collection was initially made accessible in codex format.[71] Combining church-historical and text-critical acumen, Gamble argued that the authority vested in Paul's letters from the beginning

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[67] Buck, "Early Order of the Pauline Corpus," 356.

[68] Finegan, "Original Form of the Pauline Collection," 88, argued, "… since the codex form of book was available in the first century A.D., since extant examples show that it was in use for Christian books and collections of books from the second century and probably earlier, and since it was immensely preferable to the roll for any book or group of books which it was desired to consult frequently, every probability speaks for the conclusion that Paul's collected letters were published originally in a codex rather than a roll".

[69] It would be remembered that Roberts, "The Codex," 187-91, also proposed, although with very little acceptance, that the Gospel of Mark was originally written in a parchment codex; for a contrary view, see Gamble, *Books and Readers*, 56-59, as well as the references he cited. On another instance, Roberts and Skeat, *Birth of the Codex*, 59-60, suggested that the Gospel traditions… were first written sporadically in "papyrus tablets" which were in due time put together as a collection of Jesus' Sayings.

More recently, Skeat, "The Origin of the Christian Codex," *ZPE* 102 (1994): 263-68; repr. in *CBW-Skeat*, 79-87, as an innovation of their proposal in the *Birth of the Codex*, proposed that only a codex form, not a roll, has the capability to hold together as one the four gospels, and therefore this served as a powerful motivation for the early Christians to adopt the codex for their Scripture requirements. This has been augmented by his other article, "The Oldest Manuscript of the Four Gospels?" *NTS* 43 (1997): 1-34; repr. in *CBW-Skeat*, 158-92; but cf. Graham Stanton, *Jesus and Gospel* (Cambridge: CUP, 2004), 168.

[70] Zuntz, *TEDCP*, 14-15.

[71] Gamble, *Books and Readers*, 58-66, identified two factors that must have led to this: first, the concept of letters sent to *seven churches* (as in the Muratorian canon), implying universal authority, and second, the decreasing order of book arrangement in Marcion and in $\mathfrak{P}^{46}$.

makes them the most likely candidates for the "decisive and precedent-setting development" factor, which ultimately led to the eventual Christian predilection for the codex. To support this thesis, Gamble appealed to Skeat's measurement analysis of $\mathfrak{P}^{46}$ mentioned above arguing that if putting the Pauline Epistles on a roll requires more papyrus sheets than what a codex format requires (hence, not only more expensive but also cumbersome), then the principle of custom and convenience that the roll was known to offer is defied in such context.[72] He explained instead that "If Paul's letters were transcribed in a single book, as the features of the earliest recoverable edition required, that book must have been a codex, not a roll."[73] He concluded, "This coming together of transcriptional need and religious authority in the Pauline letter collection and nowhere else makes it nearly certain that the codex was introduced into Christian usage as the vehicle of a primitive edition of the *corpus Paulinum*."[74]

3. Pitfalls and Potentials of using the bibliographical data of $\mathfrak{P}^{46}$

One cannot help but commend the meticulous care with which Gamble was able to marshal his proposal, including the use of $\mathfrak{P}^{46}$. In many ways, this is an "improvement" on Roberts' and Skeat's proposals to account for the early Christians' preference for the codex. That being said, however, there seems to me more in this claim than the evidence truly can accommodate, especially if seen in the bigger context of the surviving NT papyri.[75] For one, given the fact that there are more extant early codices of the four gospels than of the Pauline Epistles,[76] it seems counterintuitive to suppose that the

[72] Gamble excluded Hebrews from his own calculations and came up with the 24 meters (or 80 feet). He believes that a roll of this length is "extremely unlikely" (p. 63).

[73] Gamble, *Books and Readers*, 63.

[74] Gamble, *Books and Readers*, 63.

[75] See also, Stanton, *Jesus and Gospel*, 168-69.

[76] For some statistics, see Epp, "The Codex and Literacy in Early Christianity and at Oxyrhynchus: Issues raised by Harry Y. Gamble's *Books and Readers in the Early Church*," *Critical Review of Books on Religion* 11 (1998): 15-37; repr. in *PNTTC*, 521-50, p. 532; but also see NA[28], 792-99, for the most updated list.

collection of the Pauline Letters into a single corpus acted as the authoritative impetus (i.e., Gamble's "precedent setting" factor) for the early Christians' attraction to the codex.[77] If at all, it seems more sensible to suggest that the four gospels rather than the Pauline corpus influenced the early Christians' widespread adoption of the codex format. Hence, like Roberts' and Skeat's earlier proposals, some aspects of Gamble's proposal are equally speculative, as already identified by Epp.[78]

It becomes evident that there is always the danger of speculation in discussing the bibliographical importance of $\mathfrak{P}^{46}$ (and other manuscripts for that matter) from the *exclusive* standpoint of explaining the origin of early Christians' preference for the codex. The specificity required from the available evidence to support these proposals naturally militates against them. We need to accept the sad fact that given the present state of things insofar as the evidence at our disposal is concerned, borrowing from the very words of Roberts and Skeat, all proposals "must necessarily be conjectural".[79]

On the other hand, much is to be added to our information reservoir if we first take at face value whatever codicological data $\mathfrak{P}^{46}$ presents. Any productive analysis must commence from what is incontrovertibly given: $\mathfrak{P}^{46}$ is a codex. How this information will help us paint the portrait of the scribe who "gave birth" to this papyrus codex justifies this sub-section. Hence, what follows reveals in details some of its codicological features that are seldom discussed or that have remained unexplored even inside the closed circles of papyrologists and palaeographers, and some may even be corrective of earlier impressions about $\mathfrak{P}^{46}$, which shall be underscored at appropriate junctures.

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[77] On this, see Epp, "The Codex and Literacy in Early Christianity and at Oxyrhynchus," 525-30; and Stanton, *Jesus and Gospel*, 84, n81.

[78] Epp, "The Codex and Literacy in Early Christianity and at Oxyrhynchus," 525-30.

[79] Roberts and Skeat, *Birth of the Codex*, 62.

B. $\mathfrak{P}^{46}$ and its Codicological Details

Did our scribe himself construct $\mathfrak{P}^{46}$ into a codex? Frankly, my own assessment of this rather difficult question is a bit deductive only. With the evidence at hand, it is extremely difficult to answer this question; $\mathfrak{P}^{46}$ simply does not give any definitive indication as to *who* constructed it as a codex. Nonetheless, the upside is that our codex itself provides a viewing deck as to *how* it was constructed.

$\mathfrak{P}^{46}$ is unambiguously a single-quire codex of more than 50 leaves.[80] However, it is theoretically incomplete to say that $\mathfrak{P}^{46}$ is a codex. The truth is $\mathfrak{P}^{46}$ is *both* a codex *and* a roll. This calls for explanation.

Whilst already a formed codex when discovered, $\mathfrak{P}^{46}$ nevertheless is like other papyrus codices in that it was constructed out of pre-manufactured papyrus rolls. The recurring presence of κολλήσεις or joins[81] (Fig. 3-1.5) throughout is the single but most decisive indicator divulging the incontrovertible truth that the ultimate source-material for the folded sheets used in constructing our codex was a papyrus roll (or more precisely *rolls*, as we shall argue later) which was cut into the size required by the scribe-user and then bound to form the codex that it is now.[82] To borrow Turner's rather graphic description, like other papyrus codices, the sheets of $\mathfrak{P}^{46}$ were "cannibalized from a

[80] See more discussion on this method of sheet-gathering below, pp. 87-109. Accordingly, the traditional suggestion as to the number of constituent sheets forming $\mathfrak{P}^{46}$ is 52 (so Kenyon and Sanders). However, a palaeographical-codicological reinvestigation of the evidence yields an alternative scenario, which I shall discuss in length in Section Five, esp. pp. 228-34.

[81] Throughout this project, I use the term κολλήσις (κολλήσεις plural) to refer to the actual joins in $\mathfrak{P}^{46}$, to distinguish it from the sheets to be joined forming a roll. I shall call the latter κολλήμα (κολλήματα plural). This necessary distinction follows Turner's definitions, *TEC*, 44, 47. Accordingly, I am using the term "sheet" to refer to the basic unit of papyrus cut from a roll(s) that is folded in the middle to form a codex; each sheet forms four pages when folded.

[82] As Johnson, "The Ancient Book," 265, correctly noted, κολλήσις signals that "a roll (is) the ultimate source for the sheets that make up the quires". See also the systematically ordered codicological discussion of James M. Robinson, "Future of Papyrus Codicology," in *The Future of Coptic Studies* (ed. R. McL. Wilson; Leiden: Brill, 1978), 23-70, where informative details about the presence of κολλήσεις in major papyrus manuscripts played a dominant role in his discussion.

roll(s)". That being the case, two pertinent questions now come to light: How many rolls were used to construct 𝔓[46] into a codex? Are there scribal habits derivable from or as a result of this manufacturing fact? These questions are inter-connected, but I shall reserve the discussion of the latter for the next section as it deserves broader treatment.

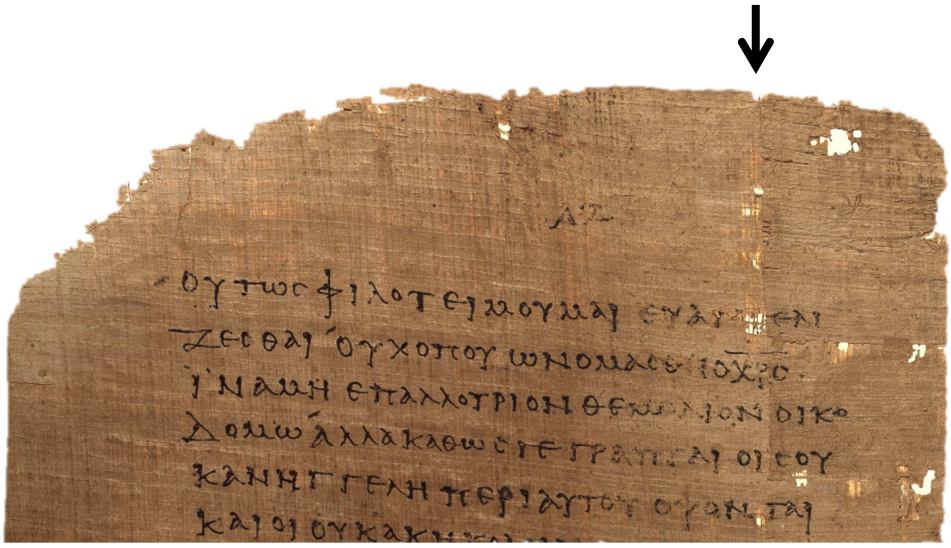


Figure 3-1.5 F19[r] showing the point of pasting between two κολλήματα, with the right overlap on top of the left.

To the modern mind, the least laborious and intricate method of codex-construction is to pre-order individual manufactured papyrus sheets according to the pre-determined size of the codex to be constructed.[83] This is surely the most ideal scenario, conveniently avoiding the recurring presence of κολλήσεις, therefore rendering elegance to the pages.[84] However, this is not the case for 𝔓[46] (and many others). The sheets of 𝔓[46] lack the elegance of the Manichean papyri, and the high grade rating accorded to some especially manufactured papyri without evident joins[85]—κολλήσεις

[83] In fact, this seems to have been the position of earlier papyrologists up until the time of W. Schubart, *Das Buch bei den Griechen und Römern* (3rd edition; Berlin/Leipzig: de Gruyter, 1921), 21-22. But see the critiques by Lewis, *Papyrus in Classical Antiquity*, 129-34; and Skeat, "Was Papyrus regarded as 'Cheap' or 'Expensive'," 89-90; and Johnson, *Bookrolls and Scribes*, 88.

[84] Whilst there are documented cases of this scenario, this is more of an exception than rule. On this, see Turner, *TEC*, 44, 47-50; also noted by Johnson, "Ancient Books," 265.

[85] For some of these manuscripts, see Turner, *TEC*, 43-54.

abound in $\mathfrak{P}^{46}$! Of the 43 extant sheets, I found with a high level of certainty 37 with κολλήσεις,[86] three with possible κολλήσεις,[87] two difficult cases due to the fragmentary nature of the extant sheets,[88] and one with either an extremely difficult case to detect or possibly no κολλήσις at all (folios 17 & 88).[89] Accordingly, there are more sheets with two[90] κολλήσεις than with only one.[91] Turner implied that the frequency of κολλήσεις in the pages of particular manuscripts may have to do with the kind of prestige endowed by the end-users upon the manuscript. Alternatively, this frequency may also reveal other pertinent information about our codex. As we shall see later, this asymmetrical κολλήσις distribution in $\mathfrak{P}^{46}$ is suggestive of the number of rolls used in its construction.

The size (breadth) of the individual κολλήμα varies. Although none of the extant sheets is completely preserved, some reasonable measurement profiling is possible. In the 43 extant sheets I have identified 67 κολλήματα but nine of these are not particularly helpful due to their fragmentary state,[92] leaving us with 58 κολλήματα to analyse.

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[86] These sheets include: Folios 13 & 92, 14 & 91, 15 & 90, 16 & 89, 19 & 86, 20 & 85, 21 & 84, 22 & 83, 23 & 82, 24 & 81, 25 & 80, 26 & 79, 27 & 78, 28 & 77, 29 & 76, 30 & 75, 31 & 74, 32 & 73, 33 & 72, 34 & 71, 35 & 70, 36 & 69, 37 & 68, 38 & 67, 39 & 66, 40 & 65, 41 & 64, 42 & 63, 43 & 62, 44 & 61, 45 & 60, 46 & 59, 47 & 58, 48 & 57, 49 & 56, 50 & 55, 51 & 54, and 52 & 53.

[87] Folios 12 & 93, 13 & 92, and 18 & 87. Accordingly, Hurtado, *Earliest Christian Artifacts*, 165, n32, noted that although roll-makers were so skilful in pasting sheets together and that the join must have not bothered scribes in writing characters on them, κολλήσεις are "potential point for damage". However in the case of $\mathfrak{P}^{46}$, I have not found any κολλήσις at the fold of a bifolium that led to the opposite leaves of the bifolium separating. But I am inclined to think that the κολλήσις(?) in f18 might have caused the loss of the good portion of that leaf.

[88] Folios 08 & 97 and 11 & 94.

[89] On how these κολλήσεις were detected, see Section Two, "κολλήσεις in $\mathfrak{P}^{46}$: Scribal Naiveté or Sophistication?" For a list of their locations, see Appendices C-1 and C-2.

[90] Sheets with two κολλήσεις include (in conjugate pairs): Folios 13 & 92, 14 & 91, 15 & 90, 16 & 89, 19 & 86, 22 & 83, 24 & 81, 26 & 79, 27 & 78, 28 & 77, 29 & 76, 32 & 73, 33 & 72, 34 & 71, 35 & 70, 37 & 68, 38 & 67, 40 & 65, 41 & 64, 43 & 62, 45 & 60, 46 & 59, 48 & 57, and 50 & 55.

[91] Sheets with only one κολλήσις include (in conjugate pairs): Folios 12 & 93, 18 & 87, 20 & 85, 21 & 84, 23 & 82, 25 & 80, 30 & 75, 31 & 74 , 36 & 69, 39 & 66, 42 & 63, 44 & 61, 47 & 58, 49 & 56, 51 & 54, and 52 & 53.

[92] This includes f08 & f97, f11 & f94, f12^{r}, f92^{r}, f14^{r}, f16 & f89, f17 & f88, f18^{r}, and f19^{r}. I shall use the category "uncertain" when referring to these particular κολλήματα, and "certain" to refer to the 58 others.

The measurement data derived from the extant sheets may be represented in the following sequence of measurements (in centimetres),[93] including the nine that are "uncertain" (marked with "?"):[94]

> 13.4?/18.8?/13.2?/13.6?/7.4?/16.6/10.1/17.1/9.4?/17.5/12.9?/30.3/12.2?/29.7?/10.3/20.3/30.5/30.0/31.4/17.0/16.0/32.8/12.5/20.6/13.3/19.1/13.7/20.6/14.1/18.3/15.1/33.6/15.1/18.1/11.1/19.2/13.9/18.3/14.8/18.2/15.3/17.2/19.1/16.7/19.5/29/3.1/19/16.8/18.8/16.7/16.3/18.9/17.4/17.6/17.9/16.7/18.4/17.9/17.6/19.3/18.7/17.6/18.6/17.6/18.2/17.9

Turner noted that the distances between κολλήσεις "tend to be fairly close—18 or 16 cm is a much more usual distance than the 27 cm."[95] However, this does not fit well in the case of $\mathfrak{P}^{46}$. In fact, at first glance the sequence elicits no detectable measurement pattern, especially if one looks at the fluctuating breadth per related κολλήματα. This becomes more evident when placed in a graph (Table 3-A1). Note that in κολλήματα #1-#46 (i.e., f08 & f97 to f39 & f66), whilst there seems to be *almost* similar κολλήματα breadth, the alternating intervals nonetheless do not allow patterns to be established—there is simply no regularity. The smallest breadth ("certain")[96] within this range is at 10.1 (κολλήμα #7 [f15^{r}]) and the broadest is at 33.6 (κολλήμα #32 [f74^{r}]). Both the broad sheets and small sheets are found anywhere within the range. In fact, the broader κολλήματα are scattered pattern-less[97] and so are the smaller ones.[98] Within this range the extant sheet sizes measure from 29.0-31.1 cm in breadth.

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[93] The figures exclude the overlaps, which may have been between 2.0-2.5 centimetres.

[94] The sequence is arranged according to the sheet formation of the codex, i.e., starting from the bottommost extant sheet (f08 & f97) up to the uppermost or the centrefold (f52 & f53), measured from left to right.

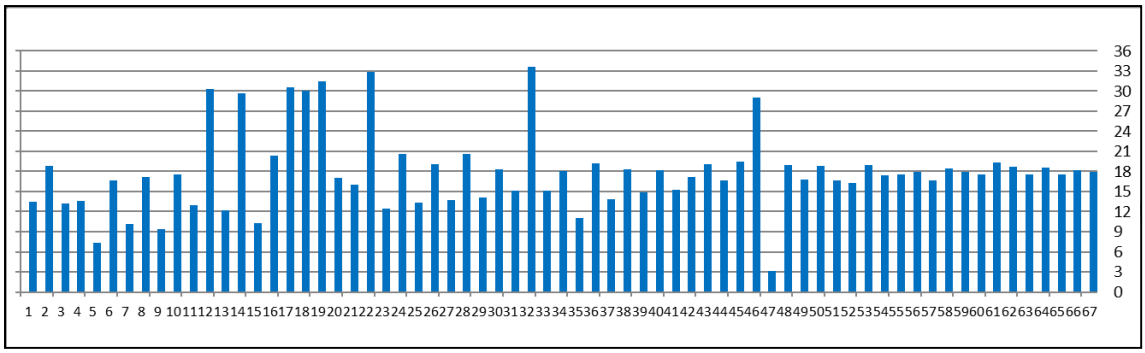
[95] Turner, *TEC*, 48.

[96] Whilst κολλήματα #1, #2, #3, #4, #5, #9, and #11 are equally smaller ones, their sizes, however, are "uncertain".

[97] Κολλήματα #12, #14, #17, #18, #19, #32, and #46.

[98] Κολλήματα #7, #13, #15, #23, and #35.

TABLE 3-A1
GRAPH SHOWING DISTANCE BETWEEN κολλήματα



However, what this table also reveals is the intriguing presence of an anomalously small κολλήμα at 3.1 cm[99] (κολλήμα #47 [f66^{r} & f40^{r}], see Fig. 3-1.6), which is much smaller compared to the smallest κολλήματα within this given range. Turner observed that "from time to time the (codex) maker slipped in a sheet of less than normal dimensions"[100] into the series of κολλήματα. However, a "cheat sheet" of 3.1 cm is comparatively smaller than the samples Turner analysed; its size is bafflingly anomalous. What then could have been its function in this context?

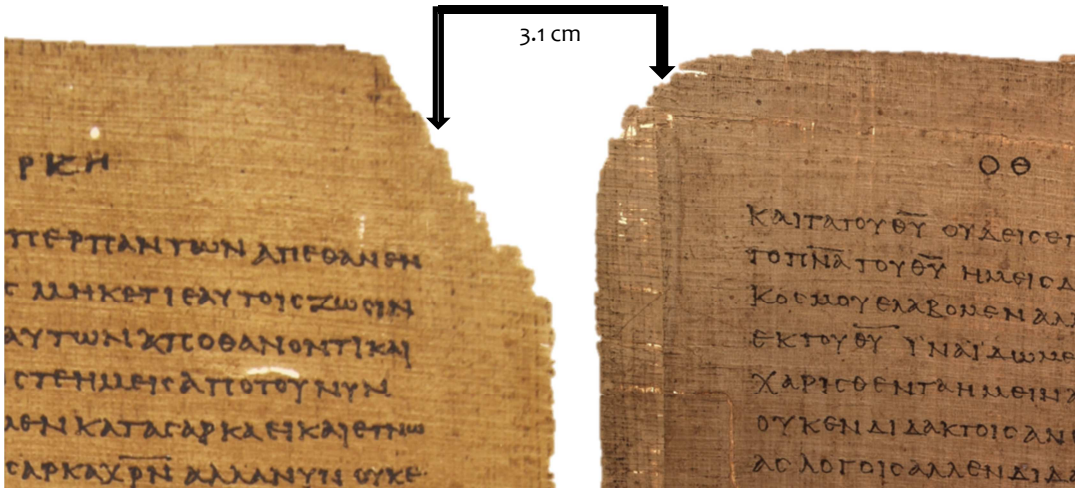


Figure 3-1.6 F40^{r} (R) and f66^{r} (L), showing the very narrow distance between the two κολλήσεις.

[99] Its original breadth size must have been bigger by at least one more centimetre, accounting for the portion that was taken away through the cut when the codex was trimmed.

[100] Turner, *TEC*, 47.

The answer seems to be found in the next range of κολλήματα, i.e., #48-#67 (f40 & f65 to f52 & f53). Note that, in contrast with the first cluster, the array of κολλήματα in the second range-group exhibits more regularity in the distance between each κολλήσις, ranging from 16.3-19.3 cm. It is plausible to presume that this group of κολλήματα originally had roughly the same size before they were pasted together forming a roll. The comparatively slight discrepancy in their present sizes may perhaps be explained by the mechanical fact that this codex was trimmed, with the middle sheets receiving the cut—all these second group κολλήματα are middle sheets, whose breadth sizes range from 26.2-28.7 cm. This seems to imply that when folded as a single-quire, the amount trimmed from these sheets may have been cumulatively up to about 1.25 cm.[101]

What can be derived from this observation is the suggestion that these two ranges of κολλήματα represent two different rolls used in the construction of our codex. The unusually small κολλήμα #47 is difficult to explain apart from taking this as a sort of a transitional sheet joining the two rolls together.[102] The first roll is characterized by uneven width sizes of each κολλήμα whilst the second is much more consistent. Admittedly, it is extremely difficult to account for the various sizes of the κολλήματα in the first group. But taking the total dimensions of these two groups may yield yet important information about the material make-up of $\mathfrak{P}^{46}$. Adding up all the extant sheets in the first group, representing the outer sheets, yields a total length dimension of 821.7 cm (about 27 feet or 8.2 metres),[103] whilst the second group totals to

[101] Note that this figure refers only to the sheets within this second group. Full details on this phenomenon is discussed in the next sub-section, "…Gathered into a Single-quire", pp. 87-109.

[102] If this κολλήμα is the last in the first roll, its small size might be explained by the possibility that the bigger portion of it was cut for another (perhaps documentary) use. But this suggestion, although not impossible, I admit, is conjectural.

[103] This sum total excludes the eroded portions of extant sheets as well as the missing sheets covering the texts of Rom 1.1-5.17b and the corresponding 2 Thessalonians and the texts of whatever that followed it (if any).

388.3 cm (about 12.7 feet or 3.9 metres) or less than half the size of the first roll. Assuming for argument's sake that Pliny's statement "(T)here are never more than twenty sheets in a roll"[104] is plausible[105]—which Skeat approximated to be at an average of 340 cm per standard roll[106]—what we then have in $\mathfrak{P}^{46}$ are two different roll-types based on measurement:[107] the inner sheets (second group) might have been an example of a standard χάρτης/τόμος (20 sheet-roll measuring about 340 cm) and the outer sheets (first group) as a χάρτης τρίτομος (60 sheet-roll measuring around 1,020 cm).[108] This information is not without consequence to the scribe responsible for inscribing the text onto the codex, for in a single-quire format the burden is heavily placed on him to work out in advance how many sheets he needs for his codex... ideally before he actually starts writing or at least before he reached the left-hand page of the central sheet (i.e., $f52^{r}$), for theoretically he can no longer add any more sheets once the right-hand page (i.e., $f53^{r}$) has been written on.[109] Either our scribe depended on the number of sheets in his *exemplar* for the calculation model, or he used some kind of material-text calculation concomitant with the type of manuscript being used (roll or codex), or the scribal trade itself already provided a mechanical convention on how to calculate the texts, i.e., *stichometry*.[110] Whichever way, this means that our scribe must have some form of

[104] Pliny, *Naturalis Historia*, paragraph 77, "*numquam plures scapo quam vicenae*".

[105] For contrary views, see Lewis, *Papyrus in Classical Antiquity*, 54-55, who broached up the suggestion that the Latin *scapo* might have meant "stalk" which may mean that "a single stalk of the papyrus plant could yield up to twenty sheets of the paper" (55). See also, Turner, *Greek Papyri*, 4.

[106] Skeat, "Length of the Standard Roll," 66.

[107] For the various lengths of rolls, see Skeat, "Length of a Standard Roll," 66.

[108] This calculation makes perfect sense if we add to the 821.7 cm the total dimension for the missing pages, which I approximate to be around 31± cm/sheet multiplied by the number of missing pages or a total of 1007 cm. Hence, 821.7 + (31x6) =1007 cm.

[109] Although *de facto* the scribe can still add extra sheet/s at the end, as some of the Nag Hammadi single-quire codices exhibit; on this, see James M. Robinson, *The Facsimile Edition of the Nag Hammadi Codices, Introduction* (Leiden: Brill, 1984) 41-44, 52-53. However, this would have aesthetic backlash, leaving blank leaves at the beginning.

[110] Without providing detailed analysis, Trobisch, *First Edition of the New Testament*, 143, n102, believed that our scribe committed a grave miscalculation in this regard, "The scribe of ($\mathfrak{P}^{46}$), which

intervention in the production process of this codex, he might have given the pre-calculated figure to the codex manufacturer... or he himself might have constructed this codex from the rolls available to him.

C. Κολλήσεις and the Text of $\mathfrak{P}^{46}$: Codicological Analysis Compliments Textual Studies

But κολλήσις presence in $\mathfrak{P}^{46}$ is not only a codicological datum; it can also be a window on how to alternatively appreciate (if not enlighten) particular textual variations reflected in our codex, especially those that can be palaeographically explained. As this rightly falls on the domains of the textual aspect, I shall develop this point further in Chapter Four, but one example will suffice for the time being.

F79^{r}-l^{04} (covering the last two clauses of Eph 4.28) reads ΤΟ ΑΓΑΘΟΝ ΙΝ ΕΧΗ ΜΕΤΑΔΙΔΟΝΑΙ ΤΩ ΧΡΕΙΑΝ. The textual glitch here is the reading ΙΝ—an error for the conjunction ΙΝΑ. Royse listed this as a "singular reading", exemplifying loss of a vowel due to an elision.[111] However, this case is palaeographically explicable. Looking at the actual page rather than a transcription, it becomes rather more likely that this error emerged due to an interruption of the normal copying process--the scribe re-sharpened his pen/quill (or changed his pen) too soon, leaving the final alpha out. This is corroborated by the fact that this is the only occurrence of the incomplete ἵνα throughout the extant pages of $\mathfrak{P}^{46}$. The change in ink density between the first four lines (until ΙΝ) and the following lines is unmistakable. But more strikingly, the page reveals why the scribe re-sharpened his pen/quill at this particular point: the presence of the κολλήσις on the actual spot where the final alpha should have been written (Fig. 3-1.7). Needless to say, integrating the study of the physical (codicological-

consists of a single layer, did not calculate correctly the number of papyrus sheets needed. In the second half of the book he had to add more lines per page and more letters per line to accommodate the text, but he had little success." Whether there is merit to this proposal, I shall discuss in Section Five.

[111] Royse, *SH-M*, 253.

palaeographical) features of particular manuscripts vis-à-vis textual study can certainly open up new portals for appreciating textual variations in the transmission history of the text of the New Testament.



Figure 3-1.7 F79^{r} showing the point of joins between the two κολλήματα. Notice the difference in ink densities between $\mathrm{II}^{01\text{-}03}$ and $\mathrm{II}^{05\text{-}08}$, and the variation in I^{04} due to the κολλήσις.

III. …GATHERED INTO A SINGLE QUIRE

Ancient codices were gathered in more than one way, and Turner's exhaustive discussion[112] remains the standard against which any study of a manuscript quiring system must be set. His section on the different types of sheet-formations needs no repetition, but this sub-section is intended to see how $\mathfrak{P}^{46}$ fits into (or critiques) his proposed system of codex constructions, by exploring all the derivable data from the way its sheets were gathered.

A. Its Attributes as a Single-quire Codex

The advent of the Chester Beatty papyri into the papyrological scene made a notable addition to the specialists' knowledge of sheet-gathering system since the Chester Beatty

[112] See especially, Turner, *TEC*, 55-68.

biblical papyri exhibit multiple patterns.[113] For instance, P. Chester Beatty V (Genesis) is in multiples of 10 leaves, P. Chester Beatty XII (Enoch) in quires of 12 leaves, whilst P. Chester Beatty VII (Isaiah), IX (Ezekiel and Esther) and X (Daniel) are single-gathering codices.[114] Other NT papyri single-quire codices include $\mathfrak{P}^{5}$, $\mathfrak{P}^{47(?)}$, and $\mathfrak{P}^{75}$.[115]

As a single-quire papyrus codex, the fibre orientation of each sheet of $\mathfrak{P}^{46}$ is uniformly alternating. Sides with horizontal fibres face upward and sides with the vertical face downward.[116] Hence, when fully constructed, the right-hand pages (^{rs}) of the first half of the codex all have vertical (↓) fibres whilst the left-hand pages (^{ls}) all have the horizontal (→) strands. This sequence is reversed at the page immediately after the mid-point, for at the centrefold both the left-hand and the right-hand pages have the horizontal side, hence:

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[113] For the bibliographical importance of the Chester Beatty biblical papyri, see Kenyon, *CBBPIntro*, 9-13. Note also that Kenyon's justifications for his early 3rd century dating of our papyrus includes its being a "large single-quire" (*CBBPIII-1936*, xv), which seems to be supported by Turner, *TEC*, 65, when he commend with confidence, "But there are at least twenty certain instances (which included $\mathfrak{P}^{46}$) of what I have called the normal order… and some of these are definitely to be dated to the third century after Christ." Without citing his source directly, Kraus, *Ad Fontes*, 33, also said "… single-quire papyrus… as a rule are dated to the third century (or slightly earlier or slightly later)."

[114] Kenyon, *CBBPIntro*, 11. For other single-quire papyri (both Christian and literary), see Turner, *TEC*, 58-60, and the more exhaustive database of Leuven Database of Ancient Books, available online at <http://www.trismegistos.org/ldab/list_all.php?p=1>. Accordingly, H.I. Bell, "Early Codices from Egypt", in *The Library* N.S., X, (1904): 307ff, p.307, suggested that *PGM v P. Lond. 46* is also a single-quire codex; but see Turner, *TEC,* 59, n**, for a critique of this proposal.

[115] It has been suggested by T.C. Skeat, 'The Oldest Manuscript of the Four Gospels?' *NTS* 43 (1997): 1–34, p.32; repr. in *CBW- Skeat,* 158–92, that $\mathfrak{P}^{4+64,67}$ is also a single-quire codex; but see S.D. Charlesworth's critique of this proposal in his "T.C. Skeat, $\mathfrak{P}^{64+67}$ and $\mathfrak{P}^{4}$, and the Problem of Fiber Orientation in Codicological Reconstruction," *NTS* 53 (2007): 582–604. Also, Kraus, *Ad Fontes*, 33, listed the following as single-quire codices: $\mathfrak{P}^{1}$, $\mathfrak{P}^{9}$, $\mathfrak{P}^{15}$, $\mathfrak{P}^{28}$, $\mathfrak{P}^{29}$, $\mathfrak{P}^{30}$, and $\mathfrak{P}^{72}$. However, with the exceptions of $\mathfrak{P}^{30}$, and $\mathfrak{P}^{72}$, these are single fragments only and cannot be independently confirmed. On other Christian manuscripts, see W. Schubart, *Einführung in die Papyruskunde* (Berlin: Weidmannsche Buchhandlung, 1918), 55; and, Idem, *Das Buch bei den Griechen und Römern,* 129; Herbert Thompson, *The Gospel of St John according to the Earliest Coptic Manuscripts* (Publications of the Egyptian Research Account and British School of Archaeology in Egypt 36; London: British School of Archaeology in Egypt, 1924), xii; and Campbell Bonner, ed., *A Papyrus Codex of the Shepherd of Hermas (Similitudes 2-9), with a Fragment of the Mandates* (University of Michigan Studies, Humanistic Series XXII; Ann Arbor: University of Michigan Press, 1934), 11. Robinson, "Future of Papyrus Codicology," 24, also reported that except for Codex I (consisting of three quires), all the Nag Hammadi codices demonstrate the single-quiring system.

[116] It is possible that this type of arrangement is a relic of the roll where the side with the vertical fibres is placed at the outer side, supposedly because having it in the inside has the natural risk of detachment; see Bülow-Jacobsen, "Writing Materials in the Ancient Word," 21.

$$\ldots\downarrow^{rs}\rightarrow^{ls}\downarrow^{rs}\rightarrow^{ls}\downarrow^{rs}\rightarrow^{ls}\ \|\ \rightarrow^{rs}\downarrow^{ls}\rightarrow^{rs}\downarrow^{ls}\rightarrow^{rs}\downarrow^{ls}\ldots$$ [117]

This arrangement is the natural consequence of a single-quire format where the codex-maker uniformly piled up the papyrus sheets in the stack, the sides with → face up and sides with ↓ face down.[118] This alternating feature is best demonstrated in Table 3-A2 showing how the sheets of $\mathfrak{P}^{46}$ are arranged and how this arrangement, to a certain extent, determined the formation of its content (*text*):

TABLE 3-A2
SHEETS FORMATION AND CONTENT OF $\mathfrak{P}^{46}$

A	B	C	D	E
EXTANT SHEET REFERENCE	**FOLIATION[119] & FIBRE ORIENTATION**	**EXTANT CONTENT**	**FOLIATION & FIBRE ORIENTATION**	**EXTANT CONTENT**
	Fo1^{v}-Fo7^{r}	*Rom 1.1-5.16: missing*	*F98^{r}-F104^{v}*	*Missing; contents after 2 Thessalonians uncertain*
#1	Fo8^{v}	Rom 5.17-6.3	F97^{v}	1 Thess 5.23-28[120]
	Fo8^{r}	Rom 6.5-14	F97^{r}	1 Thess 5.5-9
	Fo9^{v}-F10^{r}	*Rom 6.15-8.14: missing*	*F95^{v}-F96^{r}*	*1 Thess 2.9-5.4: missing*
#2	F11^{v}	Rom 8.15-25	F94^{v}	1 Thess 1.8-2.3
	F11^{r}	Rom 8.27-35	F94^{r}	*Col 4.16-18; 1 Thess 1.1-2*
#3	F12^{v}	Rom 8.37-9.9	F93^{v}	Col 4.3-12
	F12^{r}	Rom 9.9-22	F93^{r}	Col 3.13-25
#4	F13^{v}	Rom 9.22-32	F92^{v}	Col 2.22-3.11
	F13^{r}	Rom 10.1-11	F92^{r}	Col 2.8-19
#5	F14^{v}	Rom 10.12-11.2	F91^{v}	Col 1.27-2.7
	F14^{r}	Rom 11.3-12	F91^{r}	Col 1.16-25
#6	F15^{v}	Rom 11.13-22	F90^{v}	Col 1.5-13
	F15^{r}	Rom 11.24-33	F90^{r}	*Phil 4.14-23; Col 1.1-2*
#7	F16^{v}	Rom 11.35-12.9	F89^{v}	Phil 4.2-12
	F16^{r}	Rom 12.10-13.1	F89^{r}	Phil 3.10-21
#8	F17^{v}	Rom 13.2-11	F88^{v}	Phil 2.29-3.8
	F17^{r}	Rom 13.12-14.8	F88^{r}	Phil 2.14-27
#9	F18^{v}	Rom 14.9-21	F87^{v}	Phil 1.30-2.12
	F18^{r}	Rom 14.22-15.10	F87^{r}	Phil 1.17-28
#10	F19^{v}	Rom 15.11-19	F86^{v}	Phil 1.5-15
	F19^{r}	Rom 15.20-29	F86^{r}	*Gal 6.10-18; Phil 1.1*
#11	F20^{v}	Rom 15.29-16.3	F85^{v}	Gal 5.20-6.8
	F20^{r}	Rom 16.4-13	F85^{r}	Gal 5.2-17

[117] Although the original outermost sheet did not survive, it is not illogical to assume that the very first leaf, i.e., fo1^{v} (along with the rest of the right-hand side pages of the missing leaves of Romans), was also a side with the vertical strands.

[118] This fibre-orientation distinction is necessary in view of the information provided by Turner, *TEC*, 65, that the following single-quire codices are arranged otherwise: Mississippi Crosby codex, a miniature codex in Oslo (*Symbolae Osloenses* XXIV), and P. Rylands 1 28.

[119] Although I hold another opinion as to how many sheets are missing, corresponding to the beginning and last pages of our codex, I have retained the traditional codicological foliation designation for $\mathfrak{P}^{46}$, for purposes of cross-referencing with Kenyon.

[120] 1Thess text occupies only 8 lines of the page, hence, 22-24 more lines are still available, most likely to have contained the τιτλος and the beginning texts of 2Thess Chapter One.

#12	F21^{v}	Rom 16.14-23	F84^{v}	Gal 4.20-5.1
	F21^{r}	*Rom 16.23; Heb 1.1-7*	F84^{r}	Gal 4.2-18
#13	F22^{v}	Heb 1.7-2.3	F83^{v}	Gal 3.16-29
	F22^{r}	Heb 2.3-11	F83^{r}	Gal 3.2-15
#14	F23^{v}	Heb 2.11-3.3	F82^{v}	Gal 2.12-21
	F23^{r}	Heb 3.3-13	F82^{r}	Gal 1.23-2.10
#15	F24^{v}	Heb 3.14-4.4	F81^{v}	Gal 1.10-22
	F24^{r}	Heb 4.4-14	F81^{r}	*Eph 6.20-24; Gal 1.1-8*
#16	F25^{v}	Heb 4.14-5.7	F80^{v}	Eph 6.8-18
	F25^{r}	Heb 5.8-6.4	F80^{r}	Eph 5.26-6.6
#17	F26^{v}	Heb 6.4-13	F79^{v}	Eph 5.8-25
	F26^{r}	Heb 6.13-7.1	F79^{r}	Eph 4.26-5.6
#18	F27^{v}	Heb 7.2-10	F78^{v}	Eph 4.15-25
	F27^{r}	Heb 7.11-20	F78^{r}	Eph 4.2-14
#19	F28^{v}	Heb 7.20-28	F77^{v}	Eph 3.11-4.1
	F28^{r}	Heb 7.28-8.8	F77^{r}	Eph 2.21-3.10
#20	F29^{v}	Heb 8.9-9.2	F76^{v}	Eph 2.10-20
	F29^{r}	Heb 9.2-9	F76^{r}	Eph 1.21-2.8
#21	F30^{v}	Heb 9.10-16	F75^{v}	Eph 1.12-20
	F30^{r}	Heb 9.18-26	F75^{r}	*Eph 1.1-11*
#22	F31^{v}	Heb 9.26-10.8	F74^{v}	2Cor 13.5-13
	F31^{r}	Heb 10.8-20	F74^{r}	2Cor 12.18-13.5
#23	F32^{v}	Heb 10.22-30	F73^{v}	2Cor 12.10-18
	F32^{r}	Heb 10.32-11.3	F73^{r}	2Cor 11.33-12.9
#24	F33^{v}	Heb 11.4-9	F72^{v}	2Cor 11.23-32
	F33^{r}	Heb 11.9-17	F72^{r}	2Cor 11.12-22
#25	F34^{v}	Heb 11.18-26	F71^{v}	2Cor 11.3-10
	F34^{r}	Heb 11.26-34	F71^{r}	2Cor 10.11-11.2
#26	F35^{v}	Heb 11.35-12.1	F70^{v}	2Cor 10.1-11
	F35^{r}	Heb 12.2-11	F70^{r}	2Cor 9.7-10.1
#27	F36^{v}	Heb 12.11-21	F69^{v}	2Cor 9.1-7
	F36^{r}	Heb 12.21-13.2	F69^{r}	2Cor 8.13-24
#28	F37^{v}	Heb 13.3-11	F68^{v}	2Cor 8.4-12
	F37^{r}	Heb 13.12-20	F68^{r}	2Cor 7.12-8.3
#29	F38^{v}	*Heb 13.20-25; 1 Cor 1.1-3*	F67^{v}	2Cor 7.5-11
	F38^{r}	1Cor 1.4-14	F67^{r}	2Cor 6.14-7.4
#30	F39^{v}	1Cor 1.14-23	F66^{v}	2Cor 6.3-13
	F39^{r}	1Cor 1.24-2.2	F66^{r}	2Cor 5.14-6.2
#31	F40^{v}	1Cor 2,3-11	F65^{v}	2Cor 5.5-13
	F40^{r}	1Cor 2.11-3.5	F65^{r}	2Cor 4.13-5.4
#32	F41^{v}	1Cor 3,6-15	F64^{v}	2Cor 4.4-12
	F41^{r}	1Cor 3.16-4.3	F64^{r}	2Cor 3.14-4.3
#33	F42^{v}	1Cor 4.4-10	F63^{v}	2Cor 3.5-13
	F42^{r}	1Cor 4.11-19	F63^{r}	2Cor 2.13-3.3
#34	F43^{v}	1Cor 4.20-5.7	F62^{v}	2Cor 2.3-12
	F43^{r}	1Cor 5.8-6.3	F62^{r}	2Cor 1.16-2.1
#35	F44^{v}	1Cor 6.4-12	F61^{v}	2Cor 1.8-15
	F44^{r}	1Cor 6.13-7.3	F61^{r}	*2Cor 1.1-8*
#36	F45^{v}	1Cor 7.4-12	F60^{v}	1Cor 16.12-23
	F45^{r}	1Cor 7.12-19	F60^{r}	1Cor 16.2-12
#37	F46^{v}	1Cor 7.20-29	F59^{v}	1Cor 15.51-16.2
	F46^{r}	1Cor 7.30-37	F59^{r}	1Cor 15.39-50
#38	F47^{v}	1Cor 7.37-8.7	F58^{v}	1Cor 15.28-39
	F47^{r}	1Cor 8.7-9.1	F58^{r}	1Cor 15.17-28
#39	F48^{v}	1Cor 9.4-12	F57^{v}	1Cor 15.6-15
	F48^{r}	1Cor 9.12-20	F57^{r}	1Cor 14.34-15.5
#40	F49^{v}	1Cor 9.20-10.1	F56^{v}	1Cor 14.24-34
	F49^{r}	1Cor 10.1-10	F56^{r}	1Cor 14.16-24
#41	F50^{v}	1Cor 10.11-20	F55^{v}	1Cor 14.6-15
	F50^{r}	1Cor 10.21-30	F55^{r}	1Cor 13.11-14.6
#42	F51^{v}	1Cor 10.31-11.6	F54^{v}	1Cor 13.2-11
	F51^{r}	1Cor 11.7-17	F54^{r}	1Cor 12.24-13.1
#43	F52^{v}	1Cor 11.18-25	F53^{v}	1Cor 12.13-24
	F52^{r}	1Cor 11.26-12.2	F53^{r}	1Cor 12.3-12

This table indicates that sheet #43 (f52 & f53, containing 1Cor 11.18-12.24) was the uppermost in the stack of sheets, and therefore when folded, served as the central sheet, and theoretically as the transition signal for the scribe insofar as copying the text is concerned—it is presumed that at this point, he already knew whether he still had sufficient space for the rest of the texts to be inscribed (due to the mechanical limitations cited above), and to prepare him for available options in case of any unfavourable scenario.

This table also shows that, in consonance with its general aesthetic format, utility rather than beauty takes precedence in the use of its space in regard to starting a new book. It is noteworthy that, with the exception of Hebrews ($f21^{rls}$), all the extant τιτλοι, marking the beginning of a new book, are on the right-hand pages.[121] But this is more of a coincidence than a conscious effort since the scribe never starts inscribing succeeding books on a new page, unless the current page is already fully written on, as in the case of 2Cor and Eph.[122] It would have been more aesthetically beautiful if all the new books start on a new page, but this is simply not the case for $\mathfrak{P}^{46}$. In fact, it is almost certain that the τιτλος of 2Thess (not extant) was inscribed on a left-hand page also ($f94^{vls}$), immediately following the last lines of 1Thess. Hence, it is the utility of the page rather than aesthetics that relatively determined the style of $\mathfrak{P}^{46}$, insofar as book beginnings are concerned.

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[121] That is, Heb-1Cor ($f38^{vrs}$); 2Cor ($f61^{rrs}$); Eph ($f75^{rrs}$); Eph-Gal ($f81^{rrs}$); Gal-Phil ($f86^{rrs}$); Phil-Col ($f90^{rrs}$); Col-1Thes ($f94^{rrs}$).

[122] One may argue that Eph started on a new page ($f75^{r}$) and yet the preceding page ($f74^{v}$) contains the last 22 lines of 2Cor only. However, this suggestion would be misleading. Whilst $f74^{v}$ has 22 lines only (occupying at least 16.1 cm of the text area), in contrast with the average 28 lines to a page in its immediate environment, this page has a *subscriptio* after the last line and the παραγραφος, occupying at least 1.5 cm—hence, a total text area of 18.5 cm, including the intervening space between the παραγραφος and the τιτλος, leaving an average upper margin of 2.7 cm and a hypothetical lower margin of about 7 cm. Had the scribe added Ephesians immediately, only its τιτλος (about 2.5 including intervening space) would have occupied the remaining space, which would not be visually appealing. It seems that the presence of *subscriptio* was not *exemplaric* but more in keeping the lower margin bigger than the upper.

It is yet to be established later how much text was inscribed per leaf, to see whether the pattern of character-density per page is increasing, decreasing, or a mix of it. However, this table already shows that the fate of the texts inscribed on the last missing pages is the same with the texts inscribed on the missing pages (of Romans) at the beginning since these pages were lost as conjugate sheets. (This is also true for the f09-f10 vis-à-vis f95-f96). The lacunae in the beginning can clearly account for the missing texts of Romans before 5.17, but the lacunae after 1Thess 5.28 is a much more complex question, which will surely keep the Pauline scholars busy, particularly those seriously concerned with the canonical question of the Pastorals. Approaching the question of content in the last missing pages of our codex from the canonical viewpoint of the Pastorals is admittedly a valid aim. However, since this question of content is intricately connected with the construction of our codex, it would be ill-advised to approach the issue primarily and exclusively from a canonical standpoint. Consideration of the mechanical dimension of ancient book production, I suggest, will provide hard data that may help resolve this question.

B. The Size of $\mathfrak{P}^{46}$ and the Diminishing Dimension Aspect of a Single-Quire Codex

Another idiosyncrasy of a single-quire codex now needs to be tested in light of the evidence of $\mathfrak{P}^{46}$: the diminishing dimensions of the middle sheets vis-à-vis the outer sheets. To a large degree, this feature is aesthetically occasioned—to avoid protrusions of the middle-sheet fore edges once folded, codex-makers had to skilfully effect trimming.[123] As such, it has been suggested that this onerous feature contributed to the eventual relinquishment of this quiring-method in the

[123] Taking this feature as one of the disadvantages of a single-quire codex, Turner, *TEC*, 57-58, explained, "… if the book is to have an even appearance when closed, it must be trimmed at the fore edge, and this will result in the pages in the middle of the book being narrower than those on the outside."

later centuries in favour of the less demanding multi-quire.[124] Be that as it may, it would be interesting to see how this attribute affected the pages of $\mathfrak{P}^{46}$, especially in light of Epp's comment that this diminishing-size factor is a codicological given that has content (i.e., text) implications since "... pages at the beginning and at the end will be wider and therefore capable of carrying more text than those in the center".[125] This remark becomes more pertinent when viewed against the background of the continuing debate as to what occupied the pages of $\mathfrak{P}^{46}$ that are now missing. In the meantime, it would be instructive to have a detailed look at the actual page dimensions of its extant sheets and then elicit correlations on how these known details can help in resolving this question.

When first examined, based on the first 10 Chester Beatty leaves, Kenyon estimated that $\mathfrak{P}^{46}$'s original sheet size would be about 28 cm x 16.5 cm[126] when fully preserved. On the other hand, Sanders, whilst concurring with Kenyon as to the height, approximated the width at only 15.2 cm.[127] When all the Michigan and the Chester Beatty leaves were fully published, Kenyon reiterated his earlier approximations but rightly added the caveat about its inconstancy, being a single-quire manuscript (an important note lacking in his earlier edition).[128] The difference in the width estimates is explicable—

[124] See McCown, "Codex and Roll in the NT," 233, who argued that "...if heavy, a large, single-quire book would not lie closed. Even if thin, it would bulge near the fold, and the central leaves would project like a wedge. This defect and the tedious process necessary to remedy it may have been the chief reasons for the abandonment of this kind of codex."

[125] Epp, "Issues in the Interrelation of New Testament Textual Criticism and Canon," 611. See also, Skeat, "The Length of Standard Papyrus," 68.

[126] Or 11 x 6 ½ inches, Kenyon, *CBBPIII-1934*, v. Kenyon gave his measurements in inches; Sanders presented the figures in inches first then converted them to centimetres. For consistency purposes, I have converted all their figures to centimetres, rounding the results off to the nearest single figure after the decimal point as necessary.

[127] Or six inches, Sanders, *TCPC*, 5.

[128] Kenyon, *CBBPIII-1936*, ix, "The total size of the page would therefore have been approximately 11 x 6 ½ inches. It will be understood, however, that these measurements cannot have been constant: for when a codex was formed as a single quire, in order to preserve anything like an even edge when the

it is a case of sampling difference. Kenyon derived his conclusion from taking the text area of the largest leaf fragment[129] of a conjoint bifolium from the initial ten leaves, measuring 20.3 (H) x 12.1 (B) cm,[130] then added approximated margin sizes: upper margin = 3.2 cm;[131] lower margin = 4.4 cm;[132] outer margin = at least 2.9 cm;[133] and inner margin = 1.3 cm.[134] Sanders did the same procedure but had different results because he utilized the ones in their possession. For the upper margin, Sanders recorded 3.5 cm[135] when fully preserved; outer margin = 2.5-3.0 cm;[136] inner margin = 1.3 cm;[137] and surmised that the bottom margin is equal to the upper margin. It is unwise to readily dismiss this difference of 1.3 cm (½ inch) as to the width size as negligible,[138] especially in view of the suggested observation that our scribe apparently put in more characters in the latter pages of the codex. This increasing copying "trend" has received different interpretations, and therefore needs to be properly addressed, in due time.

Most of the leaves are imperfectly preserved, with varying degrees of damage at the bottom and fore-edges. A cursory look at Kenyon's facsimile immediately gives one the impression that the second-half leaves suffered more erosions than the first-half, as the most fragmented pages are on this side of the codex. The losses at the bottom can also varyingly account for as many as 1-6 lines

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book was closed, the outer sheets must have been the largest, with a gradual decrease towards the centre."

[129] This is sheet #5 in our list or what is now designated as f14 & f91.

[130] Or 8 x 5 inches.

[131] Or 1 ¼ inches.

[132] Or 1 ¾ inches.

[133] Or 1 1/8 inches.

[134] Or ½ inch.

[135] Or 1 3/8 inches.

[136] Or 1 to 1 3/16 inches.

[137] Or ½ inch.

[138] In fact, in the context of $\mathfrak{P}^{46}$, a 1.3-cm can account for at least 3-5 characters, depending on the complexity of a character's strokes i.e., squarish, looping or simply upright.

lost.[139] With regard to its vertical dimension, the shortest extant leaf at 7.9 cm is f97 (presently the outermost leaf of the second-half), whilst the biggest leaves are f19 and f25 measuring 23.1 cm and 23.2 cm, respectively. Other leaves measuring around 23.0 cm include f22, f23, f29, f32, f37, f42, f43, f44, f45, and f46—all belonging to the first-half of the codex. The average height of the extant first-half leaves (excluding the two outermost extant leaves corresponding to the very fragmentary leaves of the opposite half) is 22.6 cm, whilst 21.7 cm for the opposite half. In terms of measurement,[140] this confirms the observation that the first-half leaves are comparatively better preserved as to height. In fact, the only time that a second-half page is slightly bigger than the first-half is in sheet #30, where f66 measures 22.9 as against f39's 22.7 cm. Table 3-A3 (next page) shows the height dimensions of individual extant leaves.

[139] Analysis of the number of lines lost and preserved shall be discussed in Section Five "The Enigma of the Missing Pages", pp. 204-35.

[140] Two variables must be pointed out in regard to the method of measurement employed in this project, insofar as the extant pages are concerned: 1) the *degree of preservation* and 2) the *point of separation*. On the first, it must be noted that the figures given in various measurement tables are highly dependent on the degree of preservation of each extant sheet. Unless stated otherwise, the figures given are the actual measurement of the extant pages, measured by using the portions with the broadest area, i.e., between the corresponding fore edges (point A to point B). In cases where the corresponding fore edge is eroded, an imaginary point drawn from broadest portion of that section of the page measured perpendicularly has been used as its measurement reference. In the case of f94 which is now preserved in two separate fragments, the bigger fragment was used for the measurement. What needs to be pointed out on the second variable has to do with the fact that the point of separation between conjugate leaves is unequally divided, and must have been dependent on how the antiquities dealers divided them to sell to the highest bidder/s. Hence, it should not be assumed that the broadest measureable size of a certain leaf is equal to its conjugate leaf or that the degree of separation for each sheet is equal. Contra Turner's method, I am of the opinion that the conjugate pairs should be treated as a composite sheet when it comes to measurements, rather than by taking a certain leaf as representative of the whole manuscript. And in this regard, the two variables mentioned become very pertinent.

TABLE 3-A3
HEIGHT DIMENSION PER EXTANT LEAF

	FIRST-HALF LEAVES		SECOND-HALF LEAVES	
A	B	C	D	E
EXTANT SHEET #	FOLIATION REFERENCE	SIZE IN CM	SIZE IN CM	FOLIATION REFERENCE
#1	F08	18.9	7.9	F97
#2	F11	20.6	9.1	F94
#3	F12	22.0	18.0	F93
#4	F13	20.9	18.4	F92
#5	F14	21.0	18.9	F91
#6	F15	19.2	18.9	F90
#7	F16	22.7	20.9	F89
#8	F17	22.8	20.8	F88
#9	F18	22.4	21.3	F87
#10	F19	23.1	21.5	F86
#11	F20	22.9	21.8	F85
#12	F21	22.9	21.8	F84
#13	F22	23.0	21.8	F83
#14	F23	23.0	21.2	F82
#15	F24	22.9	22.2	F81
#16	F25	23.2	22.1	F80
#17	F26	22.9	22.7	F79
#18	F27	22.9	22.6	F78
#19	F28	22.6	22.0	F77
#20	F29	23.0	22.6	F76
#21	F30	22.9	22.0	F75
#22	F31	22.9	22.4	F74
#23	F32	23.0	21.8	F73
#24	F33	23.0	22.3	F72
#25	F34	22.6	22.0	F71
#26	F35	22.8	22.0	F70
#27	F36	22.9	22.3	F69
#28	F37	23.0	22.2	F68
#29	F38	22.9	22.7	F67
#30	F39	22.7	22.9	F66
#31	F40	22.8	22.2	F65
#32	F41	22.9	22.4	F64
#33	F42	23.0	22.3	F63
#34	F43	23.0	22.5	F62
#35	F44	23.0	22.3	F61
#36	F45	23.0	22.2	F60
#37	F46	23.0	22.4	F59
#38	F47	22.5	22.5	F58
#39	F48	22.8	22.2	F57
#40	F49	22.8	22.5	F56
#41	F50	22.7	22.3	F55
#42	F51	22.0	22.2	F54
#43	F52	22.4	22.1	F53

Needless to say, $\mathfrak{P}^{46}$ was bigger originally than 23.2 cm. In fact, Kenyon and Sanders agree that its original height dimension must have been around 28 cm.[141] But how do we confirm the veracity of this suggested dimension? How do we approximate its original size? Is there any derivable scribal habit from the analysis of its height dimensions? The nature of the present problem, admittedly, disallows methodological precision. Only some informed inferences can be drawn from the scanty evidence. One of the ways in which this can be done is to deductively investigate the average number of lines (extant and lost) in the larger leaves mentioned above vis-à-vis the extant height measurement. Whilst this is not without a potential methodological problem, its result can give a clearer (if not more realistic) picture when we approximate the original height dimension of $\mathfrak{P}^{46}$.

Despite their agreement as to the height of $\mathfrak{P}^{46}$, Kenyon and Sanders looked at the upper and lower margins differently. Sanders believed the two margins have equal measurements, whilst Kenyon suggested that bottom margins are bigger than the upper.[142] This must be probed first.

F19^{v}, measuring 23.1 cm, has 26 extant lines but reconstruction shows that it originally contained 27 lines. Presently, the extant text area is 19.1 cm with an average upper margin of about 3.7 cm. To account for the lost line, we must take the average size of the characters and line spaces on this page, which is at 0.3-0.4 cm and 0.4-0.5 cm, respectively. Adding all these figures together yields a sum of 23.7 cm. If Kenyon and Sanders are right that a fully preserved page is about 28

[141] Turner, *TEC*, 20, without providing calculation details, recorded $\mathfrak{P}^{46}$'s height as between 26.5-27 cm. Hurtado, *Earliest Christian Artifacts*, 162, follows Turner's suggestion.

[142] Sanders, *TCPC*, 5; Kenyon, *CBBPIII-1936*, ix.

cm, then the bottom margin for this page would have been 4.3 cm, which is 0.6 cm bigger than the upper margin. The formula, therefore, maybe represented, thus:

	A (extant text area)
+	B (extant upper margin)
+	C (# of lost lines x average script size [0.4 cm])
+	D (# of lost in-between line spaces x average in-between line spaces [0.5 cm])
	E (Total)
-	28 cm (proposed height)
=	F (hypothetical bottom margin)

Using the same formula, results for f19^{r}, f25^{v}, and f25^{r} are shown in the following table:

TABLE 3-A4

A	B	C	D	E	F	G	H	I	J
	Extant Size (H)	**# of Lines**	**# of extant lines**	**# of lost lines**	**Extant Text Area**	**Ave. Upper Margin**	**Ave. Script Size x # of lost lines**	**Ave. Space in-between lines x # of lost spaces**	**Approximate Lower Margin**
F19^{r}	23.1	27	26	1	18.8	3.3	0.4	0.5	5.0 cm
F25^{v}	23.2	27	26	1	19.3	3.5	0.4	0.5	4.3 cm
F25^{r}	23.2	29	29	1	19.1	3.5	0.4	0.5	4.5 cm

Results in column J of this table, along with that of f19^{v}, appear to support Kenyon's proposal rather than Sanders'—the lower margin is indeed bigger than the upper. However, this is admittedly a very limited sample. Table 3-A5, therefore, takes the conjugate leaves[143] of the two biggest leaves, and shows how the upper margins fare with that of the resulting (hypothetical) lower margins:

TABLE 3-A5

A	B	C	D	E	F	G	H	I	J
	Extant Size (H)	**# of Lines**	**# of extant Lines**	**# of lost Lines**	**Extant Text Area**	**Ave. Upper Margin**	**Ave. Script Size x # of lost lines**	**Ave. Space in-between lines x # of lost spaces**	**Approximate Lower Margin**
F80^{r}	22.1	31	27	4	18.8	2.9	1.6	2.0	2.7 cm
F80^{v}	22.1	32	28[144]	4	18.4	2.7	1.6	2.0	3.3 cm
F86^{v}	21.5	31	27	4	18.4	2.7	1.6	2.0	3.3 cm

[143] F86^{r} has been excluded since the page has the transitional τιτλος, making calculations more intricate. Cumulative figures from other leaves, nonetheless, can compensate for this exclusion.

[144] Actually, portions of I^{29} are still extant. But since the bottom tip of the extant characters did not survive, preventing actual measurement, this line is therefore excluded in the calculation.

Except for f80^{r} (with a slightly bigger upper margin), results in column J, for f80^{v} and f86^{v}, also support Kenyon. But an objection might be raised that f80 and f86 are not the biggest leaves in the second-half of the codex. Hence, we investigated further whether the outcome will be different when the biggest leaves (f66, f67, and f79) on this side of the codex are used, and the result is shown in column J below:

TABLE 3-A6

A	B	C	D	E	F	G	H	I	J
	Extant Size (H)	# of Lines	# of extant lines	# of lost lines	Extant Text Area	Ave. Upper Margin	Ave. Script Size x # of lost lines	Ave. Space in-between lines x # of lost spaces	Approximate Lower Margin
F66^{r}	22.9	29	26[145]	3	19.0	3.0	1.2	1.5	3.3 cm
F66^{v}	22.9	30	27	3	19.0	3.0	1.2	1.5	3.3 cm
F67^{r}	22.7	29	26	3	18.9	2.9	1.2	1.5	3.5 cm
F67^{v}	22.7	29	26	3	18.2	3.0	1.2	1.5	4.1 cm
F79^{r}	22.7	29	26[146]	3	18.5	3.0	1.2	1.5	3.8 cm
F79^{v}	22.7	31	28	3	18.8	2.5	1.2	1.5	4.0 cm

In Table 3-A6, figures in column J equally support the results shown in the previous tables.

It would be ideal if all the extant pages are subjected to this formulation, but it seems that this series of samples are sufficient at the moment to throw our support behind Kenyon's suggestion that the lower margin may have been more frequently bigger than the upper margin, and it seems to me that on the whole this was a conscious decision on the part of our scribe insofar as the textual circumstances allow,[147] and therefore could be added to the list of his copying habits, but shared by

[145] Portions of l^{27} are still extant. But since the bottom tip of the extant characters did not survive, this line is therefore excluded.

[146] As in f86^{r}, the bottom tip of the extant characters did not survive, preventing actual measurement, hence, l^{27} is also excluded.

[147] The best example in support of this suggestion is f21^{r} where it reflects the beginning of the text of Hebrews *after* only one line of Romans (και κουαρτος ο αδελφος [=19 characters]). This line could have been easily squeezed in on f21^{v}. However, the text area is already "filled-up" sort of, leaving a lower margin of at least 3.8 cm (or 1.5 inches) as against the upper margin of 3.5 cm.

other scribes also.[148] But a caveat must be registered also—it is unwise to mechanically assume that the lower and upper margins are precisely constant at all times. The varying number of lines inscribed on each page makes this untenable. For instance, based on these same data samples, it appears rather more regularly that pages with a fewer number of lines have bigger lower margins (Table 3-A4) when compared with those having more lines to a page (Tables 3-A5 and 3-A6). But perhaps the most graphic illustration in support of this caveat is the fact that in the latter pages of our codex the scribe's placement of the first line is already one-or-two lines higher than the earlier pages, as can be seen in the two surviving conjoint leaves (Fig. 3-1.8 [next page]).

The same can also be said of f74^{v}, involving the *subscriptio* of 2 Corinthians. In both these cases, application consistency of the "margin rule" was preferred over aesthetic beauty.

[148] This observation conforms to Turner's general margin rule, *TEC*, 25, "A rule of thumb would allow the lower margin to be bigger in proportion of 3:2 than the upper margin". See also, Johnson, *Bookrolls and Scribes*, 86; and Hurtado, *Earliest Christian Artifacts,* 169, n52.



Figure 3-1.8 F14^{r} & f91^{v} (upper) and f15^{r} & f90^{v} (bottom), both showing that the first lines on the right-hand pages are already higher than the left-hand.

Kenyon and Sanders' height suggestion of about 28 cm is not implausible. Certainly, $\mathfrak{P}^{46}$'s original vertical dimension must have been dependent on the height

of the pre-manufactured rolls from whence it was constructed.[149] Hurtado noted that, with few exceptions, the typical height of standard rolls is between 25-35 cm.[150] More nuanced is Johnson's analysis of papyrus rolls from the Ptolemaic and Roman periods, where he demonstrated that prior to the first century A.D., the height of Ptolemaic literary papyrus rolls was generally 25-26 cm, with a few examples measuring as high as 29 cm, while on the other hand, during the Roman period 25-33 cm became the more common height dimensions.[151] More specific still is Turner's Group 8, where he located $\mathfrak{P}^{46}$, putting the height range between 25-30 cm.[152] Hence, Kenyon and Sanders' suggestions fit well within this frame of measurement, and our method for determining the height dimension is justified in this regard. This will become more evident when we take into account the breadth of each page, where it can be demonstrated that the aesthetic physical feature (shape) of $\mathfrak{P}^{46}$ as a codex is tall *rectangular*.

In regard to breadth, the first two outermost extant sheets are the narrowest: sheet #1 (f08 & f97), still a conjoined sheet, preserves only 13.5 cm of its original size, whilst sheet #2 (f11 & f94), preserved in three fragmentary pieces, measures only 19.4 cm. Conversely, the broadest surviving sheets is sheet #8 (f17 & f88) at 31.1 cm.[153] Whilst sheet #43, the central sheet (f52 & f53), with some erosions on the fore-edges of f52, preserves 26.2 cm of its original size. In terms of individual leaves, the narrowest surviving is f94 (in two separate fragments) with a combined dimension of 5.8 cm. But

[149] Turner, *TEC*, 50, "… the height (of the constructed codex) cannot be greater than the height of the roll".

[150] Hurtado, *Earliest Christian Artifacts*, 164.

[151] Johnson, *Bookrolls and Scribes*, 141-43.

[152] Turner, *TEC*, 20. But cf. my proposed "re-classification" below.

[153] Sheet #10 and #11 are equally broad at 30.9 and 30.8 cm, respectively.

the smallest single fragment is f18 at 3.2 cm.[154] Conversely, the broadest individual leaf is f17, preserving 16 cm of its original size. Table 3-A7[155] (next page) shows the horizontal dimensions (breadth) for all the sheets (Column D) and individual leaves (Columns C and E).

These numbers, it must be emphasized, are the dimensions of the *extant* portions and should not be mistaken for the original dimensions. None of the leaves are completely preserved; the details in Table 3-A7 are dependent on the degree of preservation of each sheet/leaf, and allowances must be given for the eroded portions in approximating the original dimensions of each page.[156] Hence, in their original state, the breadth of each sheet is broader than what is recorded in the table.

[154] It is very likely that this extant fragment is a remnant of what used to be the actual joins of two κολληματα. The paring is straight and the fore-edges of the recto side resemble that of those with pasted joins.

[155] The figures in Table 3-A7 result from measuring perpendicularly, from the mid-portion of the page, the broadest portions of the opposite edges. This way, the degree of arbitrary measurement is reduced, especially in cases where there are evident overlapping shreds of papyrus in a conjugate bifolium, e.g., f39 and f66. Entries marked with "?" indicate considerable amount of erosion in those leaves; hence, the figure should not be taken as proximate to the actual original size. The "X" sign in columns C-E indicates that the sheet is missing.

[156] Aiming for precision presents methodological problems even to the experts. For instance, Turner, *TEC*, 23, in recognizing this difficulty, adopted a very tentative criterion, "It is quite unclear what allowances must be made for the wear and tear of time. The edges of pages of papyrus may have been broken off…The only way to proceed is to assume that when they preserve a straight edge the present dimensions of a page (leaf) are the original ones, and to see how far this assumption may require modification." To some extent, I succeeded in using this criterion, but not always.

TABLE 3-A7
BREADTH DIMENSION PER EXTANT PAGE

	FIRST-HALF LEAVES			SECOND-HALF LEAVES	
A	**B**	**C**	**D**	**E**	**F**
SHEET[157] NUMBER	**FOLIATION REFERENCE**	**SIZE IN CM**	**TOTAL SHEET SIZE (W)**	**SIZE IN CM**	**FOLIATION REFERENCE**
MS#1	F01	X	X	X	F104
MS#2	F02	X	X	X	F103
MS#3	F03	X	X	X	F102
MS#4	F04	X	X	X	F101
MS#5	F05	X	**X**	X	F100
MS#6	F06	X	**X**	X	F99
MS#7	F07	X	**X**	X	F98
#1	F08	9.3?	**13.5?**	4.2?	F97
MS#8	F09	X	X	X	F96
MS#9	F10	X	X	X	F95
#2	F11	13.6?	**19.4?**	3.5+2.3 = 5.8?	F94
#3	F12	13.6?	**27.2?**	13.6?	F93
#4	F13	14.7	**28.1?**	13.4?	F92
#5	F14[158]	14.9?	**29.8?**	14.9?	F91
#6	F15	15.2?	**30.0?**	14.8?	F90
#7	F16	15.3?	**30.3?**	15.0?	F89
#8	F17	16.0	**31.0**	15.0	F88
#9	F18	3.2(?)	**17.8?**	14.6?	F87
#10	F19	15.5	**30.9**	15.4	F86
#11	F20	15.6	**30.8**	15.2	F85
#12	F21	15.4	**30.6**	15.2	F84
#13	F22	15.2	**30.3**	15.1	F83
#14	F23	14.9	**30.2**	15.3	F82
#15	F24	14.9	**29.9**	15.0	F81
#16	F25	15.4	**29.9**	14.5	F80
#17	F26	15.2	**30.1**	14.9	F79
#18	F27	15.0[159]	**30.3**	15.3	F78
#19	F28	15.5	**30.5**	15.0	F77
#20	F29	14.7	**30.0**	15.3	F76
#21	F30	14.6	**29.6**	15.0	F75
#22	F31	14.8	**29.7**	14.9	F74
#23	F32	14.7	**29.7**	15.0	F73
#24	F33	14.8	**29.7**	14.9	F72
#25	F34	14.5	**29.5**	15.0	F71
#26	F35	14.7	**29.7**	15.0	F70
#27	F36	14.4	**29.5**	15.1	F69
#28	F37	14.3	**29.3**	15.0	F68
#29	F38	14.5	**29.4**	14.9	F67
#30	F39	14.3	**29.0**	14.7	F66
#31	F40	14.1	**28.7**	14.6	F65
#32	F41	14.2	**28.7**	14.5	F64
#33	F42	13.9	**28.2**	14.3	F63
#34	F43	13.9	**28.1**	14.2	F62
#35	F44	13.8	**28.0**	14.2	F61
#36	F45	13.7	**27.7**	14.0	F60
#37	F46	13.5	**27.4**	13.9	F59
#38	F47	13.7	**27.2**	13.5	F58
#39	F48	13.8	**27.5**	13.7	F57
#40	F49	13.7	**27.4**	13.7	F56
#41	F50	13.8	**27.4**	13.6	F55
#42	F51	13.3	**26.5**	13.2	F54
#43	F52	13.1	**26.2**	13.1	F53

[157] I added herein the supposed missing pages, accordingly prefacing them with the *siglum* "MS#_" (i.e., Missing Sheet Number X). Extant sheets are preceded by the "#" sign only.

[158] In Kenyon's facsimile there is an overlap of about 0.2 cm between the images of f14 and f91. The small "T-like" hole at the mid-lower portion in the folding area has been used as the reference instead, owing to the fact that this sheet is still a conjoint pair.

[159] F27 can be measured at 15.2 cm. However, since there is a vertical breakage of 0.2 cm in the page, the figure here reflects the deducted dimension accordingly.

Looking closely at the proposed width of $\mathfrak{P}^{46}$, Kenyon and Sanders disagree yet again. Kenyon reckoned the original width at approximately 16.5 cm, whilst Sanders suggested a lower figure at 15.2 cm. The complexity multiplies when we realize that these are not the only proposed sizes: Turner registered his own calculations at 13.5-15.2 cm; Junack et al. and Jaroš suggested 16 cm; and Metzger 16.2 cm.[160] Again, the discrepancy must have been due to sampling differences, and the most satisfactory way, it seems to me, to resolve the question is to scrutinize every extant conjugate pair. Looking at Table 3-A7, it may be argued immediately that, assuming that the various proposed figures refer to the outermost leaf of the complete codex,[161] suggestions smaller than 15.6 cm may be abandoned, since the broadest extant page we have presently is at 15.6 cm. But here a caveat on methodological deficiency must be registered in regard to giving the dimension of only one leaf and then assuming the opposite conjugate leaf is of the same size.[162] This is problematic if the intent is to ultimately conjecture the original sheet size of a codex manuscript, for it is misleading to assume that an equal dimension can always similarly result from the opposite page of a bifolium[163]—the *degree of erosion* and the *point of separation* are distinctly unique for

[160] Junack et al., *DNTAP*[2.1], XLI, "*Die Blätter mit einem ursprünglichen Format von maximal 27 x 16 cm hatten im vorderen Bereich 26 bis 29 Zeilen mit durchschnittlich 28 Buchstaben pro Zeile, im hinteren 29 bis 32 Zeilen mit bis zu 40 Buchstaben…*"; Jaroš, *DNT*, 1094, "*… eines paginierten Codex mit einem rekonstruierten Format von 27 mal 16 cm…*"; and Metzger, *Manuscripts of the Greek Bible*, 64. Note that none of these proponents provided calculation details on how they arrived at their figures.

[161] Although noting that Turner's proposal seems to refer to the size range from the middle pages to the outermost leaves.

[162] So are Kenyon, Sanders, Turner, Junack et al., and Jaroš.

[163] Skeat, "The Length of Standard Papyrus," 68, stated that, "($\mathfrak{P}^{46}$) 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, whilst the narrowest leaves (in the middle) are about 13 cm, giving an average of 15.1 cm." This estimate, however, is methodologically suspicious on two grounds: first, Skeat did not mention how he came up with the figure 17.2 cm, considering that the broadest extant leaf (f17) is only 16.0 cm; and second, Skeat seems to assume that the size of one extant leaf is always equal with its conjoint pair, which is not true in the case of $\mathfrak{P}^{46}$, as shown in Table 3-A37.

each sheet, making this untenable.[164] The way to go, I reiterate, is to investigate the circumstance of each leaf in close proximity with its conjugate pair.

What is clear from Table 3-A7 is the decreasing dimension of each sheet, with due cognizance of the degree of erosion that the extant outermost leaves presently have. This has two implications. First, it confirms that trimming was indeed enforced by the codex-manufacturer upon our manuscript. Second, this information enables us to design some calculation criteria on how to approximate the dimensions of the original (now lost) outermost sheets vis-à-vis the extant pages. As to the first, it is necessary to take cognizance of this point because not all single-quire manuscripts have been trimmed, as revealed by Turner.[165] A relevant query along this line is chronological in nature, i.e., Did the paring of the protruding fore-edges take place before or after the texts were inscribed? As this question has direct consequences for $\mathfrak{P}^{46}$'s margin profile, we shall relegate its resolution to that section, and expound more on the second point.

As per Table 3-A7, the two (now separated) leaves making up the central sheet (sheet #43) have a combined dimension of 26.2 cm. This cannot have been its original size as there are evident slight erosions on the fore-edges of f52. But the degree of erosion must have not been significantly large in view of the average outer margins within its immediate environment (f43-f62), which we can put between 2.5-2.8 cm. If we take the conservative average at 2.5 cm and add the other details (i.e., 2.5 [outer margin] + 10.0 [text area] + 1.2 [inner margin] = 13.7), then we can reasonably estimate the original width of the central sheet as about 26.8 cm (f52=13.7 + f53=13.1). This conservative estimate is consistent with the diminishing dimensions of the inner

[164] Sheet #8 (f17 & f88), for instance, bears this point out. What remains of f17 is measured at 16 cm (the broadest extant leaf), but its conjugate leaf is only at 15.1 cm, hence, a total dimension of 31.1 cm only.

[165] Turner, *TEC*, 23.

pages.[166] But this also means that the point of separation between the two leaves is not equal, with the left-hand page slightly getting more space.

Admittedly, the situation is more complex as regard the original outermost sheet (MS#1). But, definitely, we should not stop with a negative note. In fact, methodologically, the details we have extracted thus far from the relics of $\mathfrak{P}^{46}$ can be used as the starting point for possible reconstruction. As Table 3-A7 shows, sheet #8 (f17 & f88) is the broadest sheet presently at 31.0 cm.[167] Noting, however, that this sheet is already 15 or 16 sheets away from the original outermost sheet, we therefore can reverse the calculation process by looking at the trimming average of the extant sheets and look for appropriate inferences thereafter. This method is sustainable since the extant pages unequivocally display a somewhat steady diminishing pattern, from the outermost to the central sheets, despite the erosions that each page has suffered through time. This diminishing pattern is due to the codicological fact that $\mathfrak{P}^{46}$ is a single-quire manuscript.

If the approximated central sheet is 26.8 cm and the broadest surviving sheet is 31.0 cm, we can infer that the average trimming from the central sheet up until that point is 4.2 cm.[168] But this figure refers specifically to the codex lying *open*. It is the average of the *closed* codex that we should use, which would be half of this or around 2.1 cm. Taking 2.1 cm as the trimming average in a stack of 36 folded sheets (i.e., from sheets #8 to #43), we can thus safely assume that in every six folded sheets the trimming

[166] It would seem that this figure disturbs the pattern in the case of f51, with 13.3 cm. But it should not be so since there are equally evident erosions on the fore-edges of f51, denoting that its original dimension must have also been bigger than 13.3 cm.

[167] Assuming Turner's "straight edges" criterion, I am inclined to believe that f17 and f88, whilst there are evident erosions on both leaves especially at the bottom area, preserve the original size of this conjugate sheet.

[168] It is interesting to note that at one point Turner, *TEC*, 23, argued, "H. Ibscher has claimed that in P. Ch. B. II (P46) the variation is *as much as* 5 cm, though I cannot find an example of more than 3 cm." (Emphasis added). Given the figure we derived, Turner's assessment might need revision, even if he was referring to the codex when closed.

average is about 0.35 cm or about 0.18 cm in every three sheets. Applying this average backward, from sheet #8 down to the original outermost sheet (MS#1) we can roughly estimate the trimming average for 15-16 sheets (i.e., MS#1-#7) as about 1.0 cm. If we add to this the dimension of sheet #8 at 31.0 cm, then we can safely approximate the proportionate breadth of MS#1 as around 32.0 cm or a little more than 12 ¾ inches. This suggests that the total trimming from central sheet to the outermost sheet is about 3.1 cm (i.e., 2.1 [sheets #8-#43] + 1.0 [MS#1-#7]).

Assuming that our rough calculations are correct, we can thus say that the approximate original outermost sheet dimension of $\mathfrak{P}^{46}$ was 32 (B) x 28 (H) cm or about 12 ¾ x 11 inches, whilst the central sheet was 26.8 (B) x 28 (H) cm or about 10 ½ x 11 inches.[169] Viewed against Turner's dimensions-based manuscript groupings, we may need then to re-classify $\mathfrak{P}^{46}$ not with Group 8, but with Group 6 where the characteristic dimension is *circa* 16 (B) x 28 cm (H), agreeing with P. Berol. 13415 (a papyrus codex containing Christian prayers), and broader than $\mathfrak{P}^{39}$ ([16] x 25.6 cm) and $\mathfrak{P}^{19}$ ([16] x 23

[169] This newly acquired information is not without methodological implications, especially in the area of reconstructing the texts for lacunose portions. For instance, T.C. Skeat, "Did Paul write to the 'Bishops and Deacons' at Philippi? A Note on Philippians 1:1," *NovT* 37 (1995): 12-15; repr. in *CBW-Skeat*, 258-61, has argued for the likelihood that either the phrase εν επισκοπος και διακονος or παση τη μνεια υμων παντοτε εν had been omitted in $\mathfrak{P}^{46}$, calculating (mainly from the extant average number of letters per line) that the eroded portion at the bottom of f86^{r} could only accommodate five lines with average 33 letters/line (or a total of 144 letters). As it now stands, f86^{r} is around 21.5 cm, reckoning that there are erosions on all the edges. But with the newly obtained height dimension information, we can now review the evidence and take a second look at Skeat's proposal.

From the upper notional line of I^{01} to the lower portion of the last extant line (i.e., the 2nd line of Phil [I^{24}], specifically the lower curve of the extant *epsilon*) is about 18.1 cm. We can safely presume that the upper margin was originally 3.0 cm, giving a total of 21.1 cm, leaving us with 6.9 cm more (i.e., 28 cm – 21.1 cm= 6.9 cm). Skeat believes that the present lacuna accounts only for five missing lines (including the 2nd line of Phil). However, looking at the extant lines on the same page shows that on the average five lines account only for 3.5 cm. Furthermore, since Skeat included the 2nd line of Phil in these 5 missing lines, we must deduct one line, with a difference of 2.7 cm, leaving about 4.2 cm as the possible lower margin. However, this lower margin would be odd-one-out in this side of the codex, considering that the number of lines per page in this section has also significantly increased from 27-32 lines/page. A lower margin of 3.0-3.5 cm is more likely. The economical inference therefore is that six (6), not five (5), lines have been originally contained in the now eroded portion, which means that there was enough room for all the 173 letters of the NA-UBS text, including both εν επισκοπος και διακονος and παση τη μνεια υμων παντοτε εν.

cm).[170] This also makes $\mathfrak{P}^{46}$ the fourth biggest of the NT papyri after $\mathfrak{P}^{69}$ ([33] cm), $\mathfrak{P}^{74}$ (31 cm), and $\mathfrak{P}^{81}$ ([29]), and the sixth broadest, along with $\mathfrak{P}^{39}$ and $\mathfrak{P}^{19}$.

With these dimension details accordingly laid out, we are now perhaps a little closer to resolving the question of content in the missing pages. But in the meantime we still need to look at $\mathfrak{P}^{46}$'s general format—its margins, column, text area, and others.

IV. COLUMNS, TEXT-AREAS, AND MARGINS

Visual elements found scattered on the pages of $\mathfrak{P}^{46}$ include παραγραφοι, στιχοι, τιτλοι, τιτλος-highlighting lines, parsimonious punctuation marks (high and medial stops, apostrophes, *ancora*), accent marks, breathing marks, reading marks, corrections, and space-intervals. However, on any given page, it is the general format that stands out: page numeration, columniation, text-area and margination.

A. Columniation and Margination

There is one column in every extant page. There is no reason to doubt that the missing pages were also single-column. In fact, the age from which $\mathfrak{P}^{46}$ emerged is generally characterised by this format,[171] especially in those papyri that can be clearly classified as "Christian".[172] Preference for this format appears to be due for the most part to the observation that, as Turner explains, a single-column format in papyrus codices affords both utilitarian and aesthetic satisfaction.[173]

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[170] See Turner, *TEC*, 18.

[171] Turner, *TEC*, 35. See also Hurtado, *Earliest Christian Artifacts*, 166, who made the comment, "Overwhelmingly, early papyrus codices, non-Christian and Christian, have their texts in single-column format, and so in a codex of typical page size the lines of the text will be noticeably longer than the usual column width of a text in a roll, especially a roll prepared with an eye for elegance and sumptuous effect".

[172] For exceptions, see Hurtado, *Earliest Christian* Artifacts, 166-68. It is, however, another matter to argue that "Christians" popularized the single-quire format, as Turner, *TEC*, 87, correctly cautioned.

[173] Turner, *TEC*, 86-87, "Such a way of writing economizes in material, and this utilitarian motive may be one reason for its adoption. But there is probably also an aesthetic reason. Given that in a book of codex form each page is an easily apprehended unit of space when contrasted with the continuously extending space of the roll, the single column of writing inside that space… offers an aesthetically satisfying appearance."

Kenyon did not give any information about $\mathfrak{P}^{46}$'s columniation. Interestingly, Sanders described the columniation in relation to its scribe's professionalism, thus,

> Each page was written to produce a block of writing that would leave approximately the same margins. Yet the scribe was so well trained in his method that the variation in number of lines and in placing was generally slight.[174]

Sander's observation yet again positively speaks about our scribe's professional credentials, and indeed this is clearly corroborated by the way he negotiated every column in each extant page despite the material defects already present prior his actual copying.

$\mathfrak{P}^{46}$ has tall rectangular columns, the height of which in each page must have varied depending on the number of lines to a page. Like Sanders,[175] I neither found any vestigial rulings on the papyrus nor prickings (as in $\mathfrak{P}^{66}$)[176] to keep the column (texts) more or less falling within the text-area. In keeping with the general copying convention of the time, our scribe consistently copied the first letter of each line as if following an imaginary (vertical) line denoting the left-side text margin area. Hence, there are no indentations in $\mathfrak{P}^{46}$, no *ekthesis* nor *eisthesis*.[177] The only deviation involves pages with τιτλοι and *subscriptio*, which are centre-justified.[178] Furthermore, whilst there are intermittent cases of very slightly bigger initial letters of certain lines, yet there are no instances of fully enlarged and decorated initial letters typical in later

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[174] Sanders, *TCPC*, 5-6.

[175] Sanders, *TCPC*, 5.

[176] See Turner, *GMAW*^{2}, 108. For other possible "writing guides" used in the papyrus that are no longer detectable, see Turner, *GMAW*^{2}, 4-5. For a discussion about "vertical rows of dots" before particular lines on some of the Oxyrhynchus papyri, see Johnson, *Bookrolls and Scribes*, 93-99; see also Guglielmo Cavallo and Herwig Maehler, eds., *Hellenistic Bookhands* (Berlin: de Gruyter, 2008), 19.

[177] Two lone anomalies are the corrections in f37^r-l^{22} and f54^r-l^{07}. The former involves the protrusion of ⲁ before the line (definitely from another hand, due to the uncharacteristic stroke of the upper *ductus* and the ascender of the oblique stroke is a bit upright [the main hand is not]) while the latter involves the addition of ⲧⲁ (by the first hand) outside the imaginary left-side text margin area.

[178] F21^r (ⲡⲣⲟⲥ ⲉⲃⲣⲁⲓⲟⲩⲥ); f38^v (ⲡⲣⲟⲥ ⲕⲟⲣⲓⲛⲑⲓⲟⲩⲥ ⲁ); f61^r (ⲡⲣⲟⲥ ⲕⲟⲣⲓⲛⲑⲓⲟⲩⲥ ⲃ); f75^r (ⲡⲣⲟⲥ ⲉⲫⲉⲥⲓⲟⲩⲥ); f81^r (ⲡⲣⲟⲥ ⲅⲁⲗⲁⲧⲁⲥ); f86^r (ⲡⲣⲟⲥ ⲫⲓⲗⲓⲡⲡⲏⲥⲓⲟ[ⲩⲥ]); f90^r (ⲡⲣⲟⲥ ⲕⲟⲗⲁⲥⲥⲁⲉⲓⲥ); f94^r (ⲡⲣⲟⲥ [ⲑⲉⲥⲥⲁⲗⲟⲛⲓⲕ]ⲉⲓⲥ [ⲁ]); and f74^v ([ⲡ]ⲣⲟⲥ ⲕⲟⲣⲓⲛⲑⲓⲟⲩⲥ).

(parchment) manuscripts used to mark off paragraph units or columnar transitions.[179] Structure signals are visually coded in the space-intervals, rather than with sophisticated *sigla* more prevalent in the later parchment manuscripts.[180]

The right-side margin is in comparison a bit more uneven, although at times there seem to be infrequent attempts to finish evenly. Sometimes the scribe appears bent on saving space, as if attempting to complete a word, or at least the conventional word breaks (e.g., f50^{r}).[181] Sometimes strokes of the character at line-ends are used as fillers, particularly the medial horizontal stroke of the **ϵ**[182] or the bottom serif of the **ⲁ**,[183] or even the crossbar of a *nomen sacrum*,[184] or the overline of abbreviated final **ⲛ** at line-ends are elongated, to make the lines look justified, as in f35^{v}-ll$^{01-09}$ (see Fig. 3-1.9).

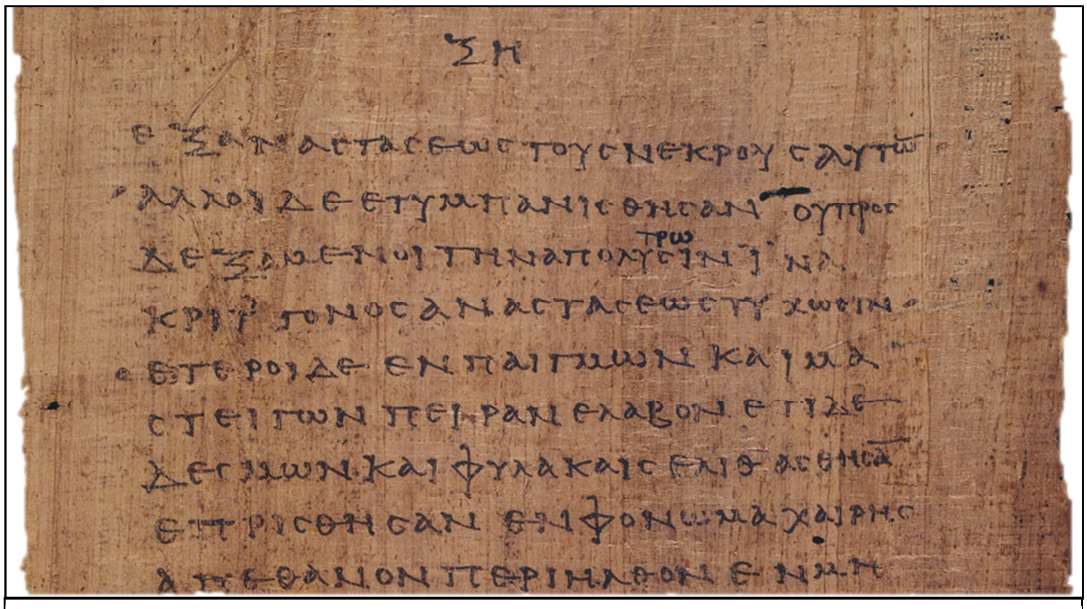


Figure 3-1.9 F35^{v}-ll$^{01-09}$ showing elongations of some characters at line-ends.

[179] On these points, see Bernhard Bischoff, *Latin Palaeography: Antiquity and the Middle Ages* (English edition; trans. Dáibhí Ó Cróinín and David Ganz; Cambridge: CUP, 1990), 27.

[180] More details on this point in Section Four, pp. 165-203.

[181] This is not the sole characteristic of 𝔓^{46}. Most of the NT papyri with Egyptian provenance also exhibit this irregularity. This may have to do with its Egyptian origin as compared with the more vertically precise right-hand edge, characteristic of the later Roman papyri; on this, see Tait, "Guidelines and Borders in Demotic Papyri," 66.

[182] For instance, f60^{r}-ll$^{01, 06, 10}$.

[183] For instance, f60^{v}-ll$^{04, 05, 20}$.

[184] For instance, f56^{r}-l^{18}.

On one occasion involving a correction by another hand (f38^{v}-l^{12}), the upper stroke of the final lunate sigma was lengthened not only to line-fill but more for safeguarding purposes, ensuring that nothing more would be added to the inserted correction. But there are cases of genuine line-fillers, usually indicating textual variations, as in f57^{r}-l^{18} (see Fig. 3-1.10).

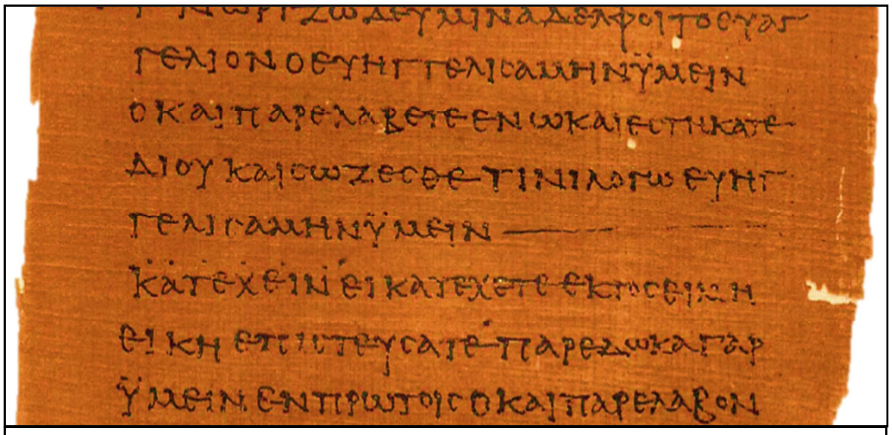


Figure 3-1.10 F57^{r}-l^{18}, showing the use of a line-filler in what could have been a problem in the *exemplar*.

Conversely, when a line is already aesthetically getting longer the last few letters are then copied in conspicuously smaller sizes (between 0.1-0.2 cm), apparently to keep the margination.

There is also an obvious attempt to keep the first lines of opposite pages in an opening parallel to each other.[185] Using mainly his experienced eyes, the scribe does the trick by aligning the first letter of the right-hand page's first line with that of the last letter of the left-hand page's first line, which under normal circumstances is just about 2-3 cm apart. But there is no clear attempt to keep this parallelism on the back and front pages of the same leaf;[186] the first line of the front page is almost always higher

[185] One deviation to this general pattern is in the opening f16^{r} and f17^{v}, where the first line of f17^{v} is slightly higher, but only because the first line of f16^{v} was sloping down, the scribe having followed the downward direction of the horizontal fibres. This seems to have happened also in f51^{r} and f52^{v}.

[186] Also noted by Sanders, *TCPC*, 5. In the following leaves, the first lines of the left-hand side of the front pages are slightly higher than the first lines of the back pages: f16, f21, f22, f28, f29, f32, f35, f43,

than that of the back page.[187] Furthermore, in these instances the number of lines is also comparatively higher on the front page than on the back page. At times this is caused by the irregular fibre direction, where the earlier parts of the first line of the front page started a bit higher than the first line of the back page.[188]

In the central opening, with a combined dimension of 26.2 cm, f52^{r} has an average text area of 9.9 cm (W) and 8.5 cm (W) for f53^{r}. This indicates that text areas, even in a corresponding opening, are not equally made, but most likely decided by the scribe's eye.[189]

Accordingly, when we compare the text areas of the inner sheets vis-à-vis the most extensive surviving outer sheets, we again feel the effect of the diminishing feature of our codex: as the pages become narrower in the central sheets, so are the writing areas.[190] As such, this provides circumstantial clues as to two temporal questions: 1) When was the trimming effected? and 2) When was the text written? Because the width of the writing areas diminished proportionately as the pages also diminished in dimensions, from the outer to the inner sheets, it is very likely that our codex was not written on until *after* it was bound, equally indicating that the trimming was effected *prior* to the copying event. And as we have stated above, this, in turn, would have made it an extreme necessity on the part of the scribe to calculate, with some expectations of precision, the number of sheets needed for a manuscript vis-à-vis the text intended to be inscribed, before it was bound and written on.

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f44, f46, f52, f53, f63-f65, f72, f76, f79, f82, f84, f86, f89, f91, and f92. F54 is the first instance where the first line of the front page seems to have been consciously placed one line higher than the back page. Furthermore, henceforth until f58, the first lines of the front pages are already higher by a half-line, corresponding to the first in-between space of the back pages.

[187] The exceptions to this involve f68-f69 and f87-f88, where the first lines of the back pages of f68 and f87 are one line higher than the front pages, but which was then reversed in f69 and f88.

[188] For instance, f15 and f19, f51.

[189] Note, however, that this particular example is not indicative that the right-side page *always* receives smaller text-area. On the contrary, the reverse is true, a point I shall develop in the next sub-section.

[190] On how the text areas went on each page of our codex, see Appendix D.

B. The Lines

Like most of the NT papyri, there are no distinctions in $\mathfrak{P}^{46}$ in the way prose and poetic entries are set on the lines, as we see happening more noticeably in the early uncial manuscripts, e.g., Sinaiticus, Alexandrinus, etc.[191] OT quotations, especially those with poetic nature, are not set distinctly on the lines, although many are marked with conspicuous space-gaps before and after. Accordingly, due to the fact that the bottom portion of our codex has substantially eroded (accounting for 1-6 lines), there is uncertainty whether there has been any overflow line/s, as in Sinaiticus.[192]

The number of lines to a page is not constant.[193] The first quarter (f08^{v}-f26^{r}) has 25-28 lines; 25-30 lines in the second quarter (f27^{v}-f52^{r}); 26-30 in the third quarter (f53^{r}-f75^{v}); and 28-32 in the last quarter (f76^{r}-f97^{v}).[194] We can deduce from this that the height of the written area varies considerably from page to page when compared by quarter (especially q.1 vis-à-vis q.4). However, this must have presented very little of an aesthetic obstacle to our scribe since the "increase" in the number of lines did not happen suddenly but *gradually*, and it is also likely that in any given opening the difference in the number of lines was easily camouflaged, in one way or another.

To a large extent, the length of the first line generally dictates the length of the succeeding lines on any given page.[195] In fact, in all the leaves with comparatively well-preserved text areas, I found six cases only where the first line is the shortest on the

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[191] Jongkind, *Scribal Habits of Codex Sinaiticus*, 36-37; Metzger, *Manuscripts of the Greek Bible*, 86-87.

[192] Jongkind, *Scribal Habits of Codex Sinaiticus*, 36-37.

[193] In view of this, reconstruction plays a vital methodological role in the determination of the number of lines per page. More details on this reconstruction in Section Five "The Enigma of the Missing Pages". See also Appendix E for an analytical summary of number of lines per page.

[194] With this quarterly description, one easily gets the impression that there has been a conscious *programmatic* increase in the way the scribe copied texts to a page, from the first quarter to the last. In fact, some have attempted to connect this impression with the "canonical" debate of the Pastorals, and therefore must be appropriately explored, deserving a separate discussion (see pp. 204-35).

[195] In the following, figures have been derived from Appendix D.

page[196] or just as equal as the shortest,[197] otherwise all the rest point to this general trend. Furthermore, against these six cases, in 24 instances the first lines are the longest, fourteen in the first-half of the codex[198] and ten in the second-half.[199] Of these, 21 fall on the right-hand pages,[200] and three on the left-hand.[201] In 75 openings, where the text areas are reasonably well-preserved,[202] I found 11 instances only where the first lines of the left-hand pages are slightly longer than that of the right-hand pages,[203] and three instances where both sides have equal length.[204] In all the rest, the first lines on the right-hand pages are longer than on the left-hand pages. These data suggest that our scribe copied slightly longer lines when writing away from the middle binding, with the copying hand getting more leeway[205]—which further suggests that our scribe was right-handed.

In pages where the first line[206] of a new book is fully preserved,[207] there seems to be a tendency to write it a bit shorter than the page's first line, but not necessarily

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[196] $F24^{r}$ (9.8 cm), $f78^{v}$ (9.9 cm), $f70^{v}$ (10.2 cm), $f85^{v}$ (11.0), $f88^{v}$ (10.3 cm). Note, however, that in $f78^{v}$, its shorter length is suspicious as this involves a textual variation—the genitive reading (του χρυ) of $\mathfrak{P}^{46}$ is a unique reading against the nominative ([ο] χρς), witnessed by the rest of the manuscript tradition. Hence, the suspicious space (about 1.5 cm) after last word (χρυ) in the line might have indirectly resulted from this—a potential case of a looming but unconsummated correction event.

[197] $F33^{rls}$, where l^{01} and l^{03} are of equal length at 9.9 cm.

[198] $F19^{v}$ (12.1); $f25^{v}$ (12.5); $f25^{r}$ (12.0); $f29^{v}$ (11.4); $f32^{v}$ (10.7); $f33^{v}$ (11.0); $f34^{v}$ (10.6); $f35^{v}$ (11.1); $f36^{v}$ (10.8); $f37^{v}$ (10.6); $f38^{r}$ (10.9); $f42^{v}$ (10.0); $f46^{v}$ (10.4); and $f50^{v}$ (10.4).

[199] $F89^{r}$ (12.4); $f89^{r}$ (12.4); $f74^{r}$ (12.0); $f72^{r}$ (11.3); $f71^{v}$ (10.8); $f71^{r}$ (11.5); $f66^{r}$ (11.4); $f65^{r}$ (11.1); $f63^{r}$ (11.1); and $f60^{r}$ (10.8).

[200] $F19^{vrs}$; $f25^{vrs}$; $f29^{vrs}$; $f32^{vrs}$; $f33^{vrs}$; $f34^{vrs}$; $f35^{vrs}$; $f36^{vrs}$; $f37^{vrs}$; $f42^{vrs}$; $f46^{vrs}$; $f50^{vrs}$; $f60^{rrs}$; $f63^{rrs}$; $f65^{rrs}$; $f66^{rrs}$; $f71^{rrs}$; $f72^{rrs}$; $f74^{rrs}$; and $f89^{rrs}$.

[201] $F25^{rls}$, $f38^{rls}$, and $f71^{vls}$.

[202] Presently, there are 85 openings. However, I have excluded ten openings either due to lacunae ($f08^{r}$ & $f11^{v}$ and $f94^{v}$ & $f97^{r}$) or due to their fragmentary nature ($f11^{r}$ and $f12^{v}$; $f12^{r}$ & $f13^{v}$; $f13^{r}$ & $f14^{v}$; $f17^{r}$ & $f18^{v}$; $f18^{r}$ & $f19^{v}$; $f91^{v}$ & $f92^{r}$; $f92^{v}$ and $f93^{r}$; and, $f93^{v}$ & $f94^{r}$).

[203] $F28^{rls}$ (11.5 cm) > $f29^{vrs}$ (11.4 cm); $f38^{rls}$ (10.9 cm) > $f39^{vrs}$ (10.3 cm); $f48^{rls}$ (10.2 cm) > $f49^{vrs}$ (09.3 cm); $f51^{rls}$ (09.7 cm) > $f52^{vrs}$ (09.3 cm); $f52^{rls}$ (10.0 cm) > $f53^{rrs}$ (08.7 cm); $f53^{vls}$ (09.8 cm) > $f54^{rrs}$ (09.5 cm); $f55^{vls}$ (09.5 cm) > $f56^{rrs}$ (09.4 cm); $f56^{vls}$ (10.5 cm) > $f57^{rrs}$ (09.5 cm); $f61^{vls}$ (10.3 cm) > $f62^{rrs}$ (10.2 cm); $f77^{vls}$ (11.3 cm) > $f78^{rrs}$ (11.2 cm); and, $f86^{vls}$ (12.2 cm) > $f87^{rrs}$ (11.4 cm).

[204] $F25^{rls}$ (12 cm) = $f26^{vrs}$ (12 cm); $f73^{rrs}$ (11.1 cm) = $f72^{vls}$ (11.1 cm); $f75^{rrs}$ (11 cm) = $f74^{vls}$ (11 cm).

[205] Without providing details, this observation was also suggested by Sanders, *TCPC*, 6, "… in general even-numbered pages have longer lines than odd-numbered, that is, verso than recto in the first half of the manuscript, and recto than verso in the second part. It seems that the scribe knew the difficulty of reading those line ends that come too near the binding center in a single quire manuscript".

[206] In the following discussion of the "first lines", figures are derived from Appendices F-1 to F-3.

the shortest in the page.[208] In pages starting with a new book, the first line takes the function of setting the length for the whole page, but is neither the longest nor the shortest.[209] Also, although the majority of the first lines do not have word-breaks at line-ends (110 cases [or 64%]), there seems to be no deliberate attempt to avoid wordbreaks, as there are 58 cases out of the 172 extant (about 34%).[210] This seems to give the impression that the length of the line was a primary concern for our scribe more than keeping word-units intact to avoid run-overs. However, it must be added immediately that there are comparatively few instances where our scribe started a page's first line with an unfinished word-unit from the last line of the previous page (18 of 172 [or only about 10%]).[211] More interesting still is the revelation when we make an analysis of the combined word-breaks in both the line-beginnings and line-ends of the first lines. In the 58 pages where the line-ending of the first line has word-break and in the 18 pages where the first line continues a word from the previous page, agreement is found only in eight pages out of 68 total combined pages[212] (or 11.8%).[213] On the other hand, out of 155 combined total pages[214] without word-breaks

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[207] Τιτλοι for Rom and 2Thess are not extant. On the other hand, whilst the τιτλοι for Phil (f86^{r}), Col (f90^{r}), and 1Thess (f94^{r}) have survived, their first lines are not fully intact.

[208] Hence,

Reference	Page 1st Line	Book 1st Line	Longest Line	Shortest Line
F21^{v} Heb	10.0	9.8	11.8	9.6
F38^{v} 1 Cor	10.4	10.3	11.4	9.7
F81^{r} Gal	12.5	12.0	12.8	11.5

[209] Hence,

Reference	Page 1st Line	Book 1st Line	Longest Line	Shortest Line
F61^{r} 2 Cor	10.2	10.2	10.5	9.5
F75^{r} Eph	11.0	11.0	11.4	10.3

[210] The four unaccounted pages (2%) involve those that, although extant, are lacunose for the part being examined, hence, excluded: F08^{v}, f08^{r}, f94^{r}, and f94^{v}.

[211] Out of 172 extant pages, the first lines of 12 pages (7%) are lacunose in the parts being examined, leaving 160 for analysis. Out of this 160, the first lines of 142 pages (83%) begin with a complete word.

[212] This involves analysing 18 pages with breaks at line-beginnings and 58 at line-ends. From their sum (i.e., 76) is deducted the 8 pages where they agree, hence, a total of 68 pages.

[213] For this information, see the summary totals in Appendix F-2, esp. columns E and F.

at line-beginning and line-end, the agreement is at 63% (or 97 pages). These two figures indicate that our scribe seldom ends the first line with a word running over the second line when the line-beginning continues a word from the previous page. This observation is corroborated by the fact that in 76 extant left-hand pages,[215] marking various openings, our scribe begins the first line with a broken word in only eight instances (10.5%) against 68 (89.5%), suggesting that our scribe is keen, although not fully successful, at ensuring that the last line of an opening would end with a complete word! This meticulous detail is definitely an aid to the reader!

The length of a line does not always translate to a higher number of characters. In fact, in many cases longer lines are due also to the calligraphic nature of particular characters (specifically the squarish letters occupying more space, i.e., 𝛥, 𝛯, 𝛷 and 𝛴), or to the presence of space-gaps (either in between words or at mid-words), or to the conscious avoidance of defects in the papyrus (as already mentioned above). For instance, in $f84^{r}$, l^{10} has a total of 40 characters, but when compared with l^{08}, with only 36, l^{10} is shorter by 0.6 cm. Furthermore, in the case of $f85^{r}$, l^{07} (with 42 characters) measures 12.7 cm, which is as long as l^{10} with only 37 characters.[216] Interestingly, the longest fully extant line I found is l^{04} of $f17^{v}$, measuring 13.2 cm, but containing 34 characters only.[217] On the same page, l^{07} and l^{08} have 39 and 38 characters, respectively, both measuring less than 13 cm. This point acquires special significance when we assess the suggested "character increase" in the latter half of

[214] This involves analysing 142 pages without breaks at line-beginnings and 110 at line-ends. From their sum (i.e., 252) is deducted the 97 pages where they agree, hence, a total of 155 pages.

[215] The total extant left-hand pages is 86, however, ten are excluded since they are lacunose in the portion being examined. On this and the following figures, see Appendix F-3.

[216] So is $f83^{r}$-l^{03} (42) vs -l^{16} (36).

[217] The line read: φοβος<_>τω αγαθω<_>εργω<_>αλλα τω κακω<_>θελεις<_> δε (34), which shows the presence of squarish characters (φ, β, δ) and slight space gaps (represented by <_>). But most noteworthy is the elongation of the middle stroke of the line-end *epsilon*, which is about 0.8 cm.

the codex, whether this is real or merely a perception, influenced by the aesthetic requirements of the characters in our manuscript.[218]

CONCLUSION

$\mathfrak{P}^{46}$ is not only a manuscript containing the texts of the Letters of Paul; it is first and foremost a manuscript with material components. The foregoing discussions are a preview of how the physical features/characteristics of the materials used in constructing this important papyrus codex manuscript have moulded the writing of the texts that it contains. What this section attempted to achieve is to show that in our search for "scribal habits" we need to look beyond the obvious. Whilst we must not underestimate the importance of the inscribed text, *how* the scribe who produced this manuscript negotiated each and every page of his codex gives equally potential pointers to his habits, proclivities, and practices. On every page, attention to these physical details makes the "activities" of our scribe more pronounced than ever, vividly revealing the material challenges he faced and the efforts he exerted to meet them for his text to be transmitted. But this is just the beginning... the search for *who* our scribe was in relation to *how* he "created" his manuscript and *what* he inscribed onto it continues in the ensuing pages.

[218] Sanders, *TCPC*, 6, in fact, suggested "In the first part of the manuscript even-numbered pages average well above thirty letters to the line, while the lines of odd-numbered pages seldom reach thirty letters. Toward the end of the manuscript the length of line increases on the average and sometimes reaches forty letters to the line in the broader columns." However, this is not entirely accurate, for there are already lines on the first part of the codex numbering to 40's (e.g., $f14^{v}$-$ll^{01, 02}$ [42 and 40]; possibly $f18^{v}$-l^{06} [43?]; -l^{20} [45?]; and $f18^{r}$-l^{20} [40?]). The figure will even rise if we take into account the lines ranging 36-39 characters. Needless to say, only an actual counting of every extant line can (in)validate suggestions to this effect.

SECTION TWO
Κολλήσεις IN $\mathfrak{P}^{46}$: SCRIBAL NAIVETÉ OR SOPHISTICATION?

INTRODUCTION

Despite the accolade of being the *locus classicus* on ancient papyrus manufacturing process,[1] Pliny the Elder's *Naturalis Historia* xiii 74-82 leaves a number of gaps in our knowledge as to the actual process of *joining* dried papyrus sheets to form a roll.[2] In paragraph 77 Pliny informed his readers that:

> Paper of whatever grade is fabricated on a board moistened with water from the Nile: the muddy liquid serves as the bonding force. First there is spread flat on the board a layer consisting of strips of papyrus running vertically, as long as possible, with their ends squared off. After that a cross layer completes the construction. *Then it is pressed in presses, and the sheets thus formed are dried in the sun and joined one to another*, (working) in declining order of excellence to the poorest. There are never more than twenty sheets in a roll.[3]

Apart from mentioning that the processed papyrus sheets (κολλήματα) are *joined one to another* (*inter se iunguntur*), Pliny left us with no information as to how these sheets are

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[1] Kenyon, *Palaeography of the Greek Papyri*, 15; Lewis, *Papyrus in Classical Antiquity*, 34.

[2] Despite the appearance of exhaustiveness and lucidity, modern students of ancient papyrus have found a number of information gaps in Pliny's accounts. An oft repeated hiatus in Pliny's report is the lack of explicit mention about the "height" of the papyrus sheets; on this, see Lewis, *Papyrus in Classical Antiquity*, 56; I.H.M. Hendriks, "Pliny, Naturalis Historia XIII, 74-82 and the Manufacture of Papyrus," *ZPE* 37 (1980): 121-36, p. 130; Adam Bülow-Jacobsen, "'Magna in latitudine earum differentia' (Pliny, NH XIII, 78)," *ZPE* 60 (1985): 273-74. For a recent review of the different proposals, see William A. Johnson, "Pliny the Elder and Standardized Roll Heights in the Manufacture of Papyrus," *CP* 88/1 (1993): 46-50.

Another debatable area is Pliny's statement that papyrus strips were *moistened with water from the Nile* which served as the gluing substance. Against Pliny's view, F.N. Hepper and T. Reynolds, "Papyrus and the Adhesive Properties of its Cell Sap in relation to Paper-making," *JEA* 53 (1967): 156-57, through a chemical experiment, argued that the papyrus plant itself has innate substance that can act as a gluing chemical when juiced out during the manufacturing process and not the water from the Nile. See also, Lewis, *Papyrus in Classical Antiquity*, 47-49. For a recent review of the debate, see Andrew Dimarogonas, "Pliny the Elder on the Making of Papyrus Paper," *CQ* 45/2 (1995): 588-90.

[3] Pliny the Elder, *Naturalis Historia* xiii, 77, "*texitur omnis madente tabula Nili aqua: turbidus liquor vim glutinis praebet, in rectum primo supina tabulae schida adlinitur longitudine papyri quae potuit esse resegminibus utrimque amputatis, traversa postea crates peragit. premitur ergo prelis, et siccantur sole plagulae atque inter se iunguntur, proximarum semper bonitatis deminutione ad deterrimas. numquam plures scapo quam vicenae*." English translation taken from Lewis, *Papyrus in Classical Antiquity*, 37. (Emphasis added).

actually joined together in relation to the following questions: In what direction did the κολλήματα face at the time they were joined? Which side of the κολλήματα face downward when joined, vertical or horizontal? Is there any rule in pasting the κολλήματα together, and what is the rationale for such a rule? Which edge of what sheet is pasted to what? And a whole gamut of other codicological questions that Pliny was unable to provide. It is not my intention here to provide definitive answers, but to present a view from the perspective of a particular manuscript. Specifically, this section deals with the pasting direction of extant κολλήσεις (joins) in $\mathfrak{P}^{46}$ and how it relates in establishing the profile of its scribe, in relation to his pre-copying working ethics.

I. PURPOSE AND METHODOLOGY IN LOCATING THE κολλήσεις IN $\mathfrak{P}^{46}$

Working from Kenyon's facsimile, E.G. Turner, whose work on ancient codicology remains a standard in the field, reported that he only observed "occasional" κολλήσεις from the codex of $\mathfrak{P}^{46}$, and suggested that the size of each κολλήμα is 30 cm (breadth) by 27 cm (height).[4] However, a thorough investigation suggests that Turner's claim is imprecise. Furthermore, how frequent or infrequent these "occasional" κολλήσεις have been is known only to Turner, for he never indicated their actual number and locations in $\mathfrak{P}^{46}$. But he cannot be faulted for that, since neither Kenyon nor Sanders had any actual discussion on the κολλήσεις in $\mathfrak{P}^{46}$ in their respective monographs.[5] Nonetheless, Turner's suggestion about the presence of the κολλήσεις and size of the κολλήματα needs to be revisited, if only to validate his proposed measurement, in light of the actual κολλήσεις in $\mathfrak{P}^{46}$. Furthermore, and more importantly, a discussion on the κολλήσεις of $\mathfrak{P}^{46}$ will ultimately have some bearings on the copying profile of our scribe.

[4] Turner, *TEC*, 49.

[5] Kenyon's lone allusion to $\mathfrak{P}^{46}$'s κολλήματα is when he hinted that some of its space-intervals are partly due to κολλήσεις (CBBP-1936, xiv), although he did not mention any actual example.

Admittedly, it is not easy to identify the κολλήσεις from Kenyon's facsimile for obvious reasons; therefore autopsy of the actual manuscript is indispensable.[6] But I hasten to add that even when investigating the actual manuscript one must be willing to simply admit that it is extremely difficult (almost impossible) at times to detect the κολλήσις in a particular sheet because detailed attention was given by the manufacturer to the joining process. Nonetheless, in locating the κολλήσεις in $\mathfrak{P}^{46}$, the main criterion has been to detect the *discontinuity of the horizontal fibres* from one κολλήμα to the adjoining κολλήμα.[7] Fortunately, using this criterion most of the *bifolia* easily gave up their secret, as it were, by spotting the points of dissonance between the horizontal strands of both κολλήματα[8] (e.g., Fig. 3-2.1-Upper), but other sheets required more time and perseverance to decipher. Occasionally, *sudden changes in colour feature* of the fibre strands of the adjoined κολλήματα can reinforce the detection of a κολλήσις (e.g., Fig. 3-2.1-Middle), though this alone cannot form a solid independent criterion. In the example, the colour difference between the κολλήματα is quite obvious, one sheet being darker than the other; one only needs to look for the beginning of the point of colour difference to locate the κολλήσις. Furthermore, at times *damage in the papyrus* can have a positive contribution, at least insofar as our present purpose is concerned, for another physical factor that occasionally helped in determining the κολλήσις locations is the *breakages of the uppermost κολλήμα* in the joins exposing the protruding edges of the right horizontal fibres[9] (e.g., Fig. 3-2.1-Lower). In a number of instances, a combination of two or three of these is present in a particular folio, which makes the task easier.

[6] Turner, *TEC*, 44, has stressed this necessity.
[7] On this main criterion, see Turner, *TEC*, esp. 43-53.
[8] For detailed analysis of κολλήσις locations, see Appendices C-1 and C-2.
[9] Also pointed out by Bülow-Jacobsen, "Writing Materials in the Ancient World," 21.

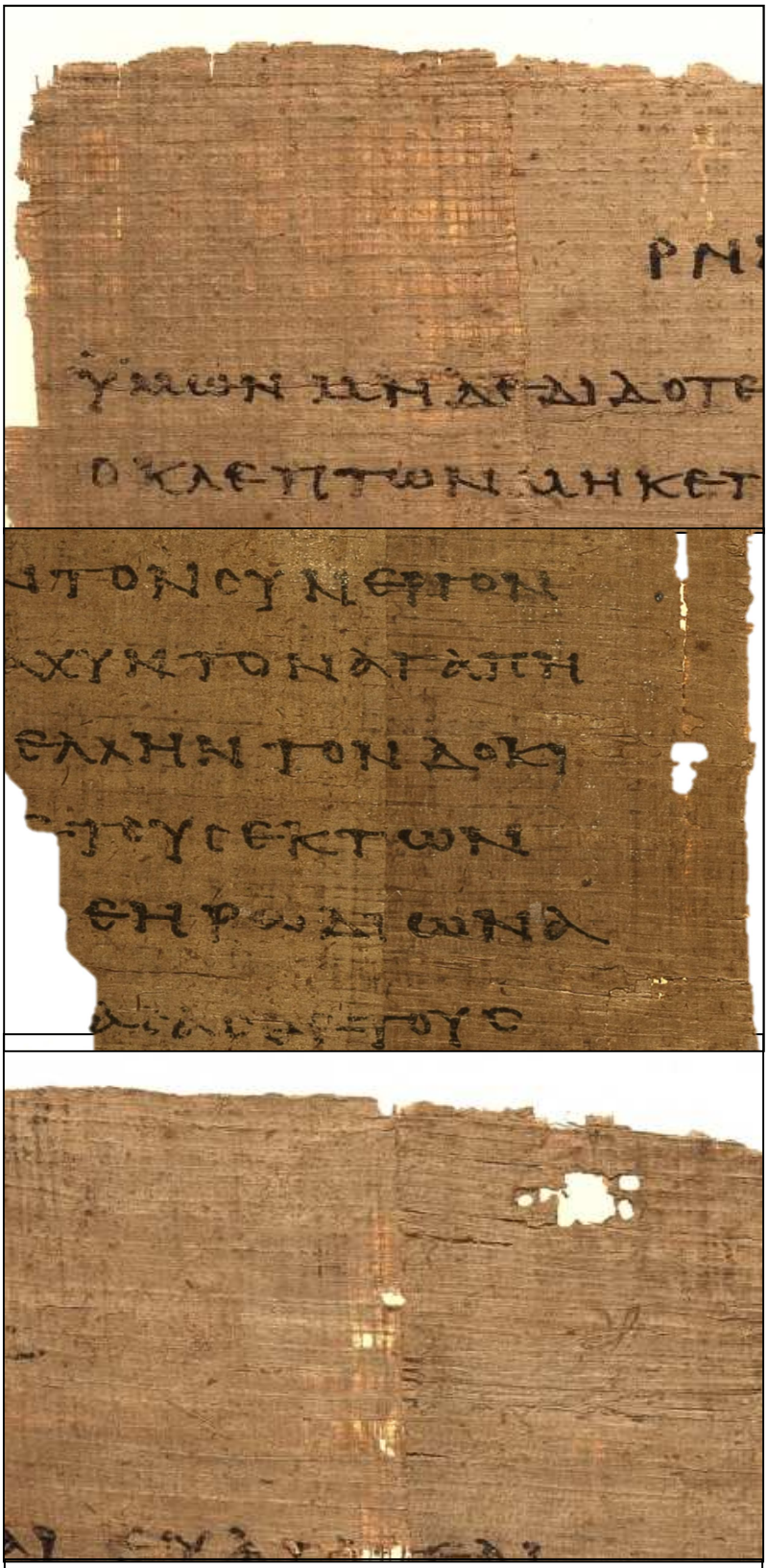


Figure 3-2.1
Upper: Discontinuity of the horizontal fibres (f79^{r})
Middle: Sudden difference in colour (f20^{r})
Lower: Damage on the right sheet (f19^{r})

II. LEFT-OVER-RIGHT: *THE* PASTING RULE!?

Having said the foregoing, we are now ready to describe the actual physical circumstance of $\mathfrak{P}^{46}$ in relation to its joins. Let me begin with the obvious: there are κολλήσεις in $\mathfrak{P}^{46}$. This is not surprising as this adhered to the convention of the day, as mentioned by Pliny the Elder, which is perhaps the closest we can get to the papyrus manufacturing procedure in antiquity. Nonetheless, Pliny's seemingly meticulous account evinces no direct evidence as to how each κολλήμα was actually joined to the other, specifically the point that has to do with pasting direction, that is, "Which κολλήμα is on top at the time of pasting, the right or the left?" Hence, modern students of papyri have attempted at various periods to come up with suggestions to bridge this information gap.[10] But as we shall see in a while, some of these suggestions need to be reconsidered.

Many hold the view that in pasting the κολλήματα together to form a roll, the left sheet was pasted *over* the right in any given join.[11] The *rationale* for such direction, it is claimed, is to minimise the impediment to a pen moving from left to right, therefore affording ease of writing,[12] which in effect has to do with "quality" of writing. (Needless to say, this proposal assumes that a scribe is always writing or copying texts from left to right). In order to further reduce the obstruction of the junction, it has been suggested also that a layer from one of the two-layered sheets was meticulously stripped away so

[10] For instance, Turner, *Greek Papyri*, 5, revealed, "If two sheets are pasted together, one must inevitably be slightly higher than the other. The ancient manufacturer contrived his joins on the inside of the roll to make a series of easy steps down; the scribe's pen, travelling from left to right, would, as it were, travel downhill." See also, Bülow-Jacobsen, "Writing Materials in the Ancient World," 19.

[11] Lewis, *Papyrus in Classical Antiquity*, 51; Gamble, *Books and Readers*, 45; Johnson, "The Ancient Book," 257.

[12] Turner, *Greek Papyri*, 5 (also quoted by Lewis, *Papyrus in Classical Antiquity*, 51, n24), characterised the connection of the *writing* direction with the *pasting* direction in this fashion, "… the scribe's pen, travelling from left to right, would, as it were, travel downhill." He further intimated, "The pasting of a roll is done in such a way that the new sheets takes up under the old, and *the scribe can write across the join without any resistance to the pen*," (*TEC*, 47). Emphasis added.

that the join itself comprised only of three layers instead of four, and was therefore less prone to damage.[13] The joins were managed with care such that the pasting under normal conditions is difficult to detect on the back of the roll (i.e., the side with the vertical fibres).[14] James Robinson, drawing examples from the Nag Hammadi papyri, painted a vivid description of this pasting process:

> The term *kollema*, meaning that which is pasted, derives from the custom of pasting side by side such individual *kollemata* into a roll. This is done by overlapping a few centimeters[15] of the right end of the *kollema* on the left over the left end of the *kollema* on the right, with the result that as one writes from left to right the writing instrument will move downward at the seam, rather than bumping into a sudden rise in the writing surface.[16]

More recently, Adam Bülow-Jacobsen took this suggestion further and as it were made this the standing pasting rule. He explained, "The sheets of the roll were pasted together in such a way that the left sheet was *always* over the right one in any given join."[17] Immediately, one is confronted with a problem with the emphasised adverb, because in the context of Bülow-Jacobsen's statement he did not provide actual examples where we can validate such a claim. The truth of the matter, which to my mind is not given attention in the literature, is that this supposed rule was formulated primarily on the basis of the orientation of the written text. For one cannot, with utmost certainty, judge that the κολλήματα were pasted in such and such direction apart from being physically onsite at the time of the actual manufacturing of the papyrus roll. This may sound a voice of scepticism but this criticism becomes imperative because if this was ever *the* rule, and received with wide acceptance in antiquity, this then does not apply to $\mathfrak{P}^{46}$, since an onsite autopsy clearly reveals that, taking the orientation of the written text as a

[13] Johnson, "Ancient Book," 257, quoting Turner, "Recto and Verso", 20; and R.A. Coles, M. Manfredi, P.J. Sijpesteijn, and A.S. Brown, *The Harris Papyri* (Vol. 2; The Netherlands: Terra, 1985), 115.

[14] Turner, *Greek Papyri*, 5.

[15] Turner, *Greek Papyri*, 5, suggested that the overlap is between one to two centimetres.

[16] Robinson, "Future of Papyrus Codicology," 23.

[17] Bülow-Jacobsen, "Writing Materials in the Ancient World," 19. (Emphasis added).

cue to its pasting direction, it is the right κολλήμα (with the vertical fibre facing down) that is *on top* of the left; hence, it is a case of *right-over-left*. This unimpeachable fact directly casts doubt against the supposed pasting rule. What implications does this fact present in view of the supposed "rule"? More importantly, are there derivable connexion between this minute papyrological detail and the scribe of our codex?

III. THE κολλήσεις PROFILE OF $\mathfrak{P}^{46}$

Based on the extant *bifolia* sheets, Kenyon proposed that $\mathfrak{P}^{46}$ was originally a codex of 52 sheets (or 208 pages).[18] However, of this number only 43 now survive in which I have identified 56 locations where κολλήσεις are evident or extant, and at least 7 more cases of possible κολλήσεις.[19] Although a κολλήσις affects both sides of the codex, all these identified κολλήσεις are most observable on the side with the horizontal fibres, conforming to the normal convention of pasting papyrus sheets together.[20]

Obviously, not all the surviving sheets have easily detectable κολλήσεις.[21] Whilst it is a remote possibility, this does not mean, however, that there were originally no joins ever in those sheets. In fact, some calculations might help resolve this question. In some cases, it is very likely that the absence of detectable κολλήσις in some *bifolia* may have been due to erosions on the margin areas of the manuscript which left no traces of joins. Turner has already underscored this point and it needs no repetition here.[22] At any rate, in the case of $\mathfrak{P}^{46}$, this is corroborated by the fact

[18] But see pp.204-35, esp. 228-34 for our alternative proposal as to $\mathfrak{P}^{46}$'s original number of pages.

[19] See previous discussion in Section One, esp. pp. 79-85. See also Appendix C-2.

[20] Turner, *TEC*, 44, 47, "In a well pasted roll, also, the joins between the sheets will show only on the recto… The join... is so cleverly made that it can be detected only on the side of the papyrus in a roll in which the papyrus fibers run in a horizontal direction."

[21] I did not find any trace of κολλήσις in the following sheets: F08 & f97; f11 & f94; and f17 & f88. In the following, I can only vouch for the "possibility" of a κολλήσις: F12^{r}, f18^{r}, f24^{r}, f34^{r}, f55^{r}, f91^{r}, and f92^{r}.

[22] Turner, *TEC*, 48, rightly pointed out, "Whereabouts in the sheets of this codex could the *kollēseis* occur? … They could be allowed at one side or other of the central fold; in the middle of either page; or toward the outside edge of either page. If they were in this last named position it is quite

that some of the extant κολλήσεις are almost on the fringe/verge of the outer margins (e.g., f24^{r}, f45^{r}, and f57^{r}) or sometimes at the inner margin near the folding area (e.g., f12^{r} and f36^{r}). Note also that some of the sheets without κολλήσεις are very fragmentary or their margin areas have already eroded.[23]

Another factor that may account for the absence of joins in some sheets is the point that $\mathfrak{P}^{46}$ is a codex of a single-quire, which, by the nature of its physical make-up, necessitates aesthetically the trimming of protruding fore-edges of the middle sheets when closed. In the trimming process, some of the joins might have been cut away. This becomes very plausible when one considers that $\mathfrak{P}^{46}$ is a bulky single-quire of 50+ sheets, and as we have argued in the previous section, the total trimming size from the central sheet to the outermost sheet is about 3.1 cm—a length enough to cut out a κολλήσις together with the presumed overlap of 2.0-2.5 cm.

One would have expected that if the papyrus sheets used in $\mathfrak{P}^{46}$ came from a roll, all κολλήματα would have been roughly of the same size. That is not the case, however.[24] The κολλήσεις are situated on different sides of the sheets (left, right, mid sections), suggesting different length sizes of the original κολλήματα used by the roll manufacturer,

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possible that with the passage of time the papyrus edge might have broken away and carried the *kollēsis* with it so as to render the *kollēsis* no longer observable. For similar reason small fragments of a papyrus codex may have lost a *kollēsis*".

[23] That is, f08 & f97; f11 & f94; f18 & f87.

[24] In fact, in many recovered papyri the sizes of the sheets also vary. For instance, Italo Gallo, *Greek and Latin Palaeography* (trans. Maria Rosaria Falivene and Jennifer R. March; Classical Handbook I; London: Institute of Classical Studies, University of London, 1986), 101, n5, mentioned of the Bodleian Hawara papyrus of the *Iliad* as having κολλήματα which measure 26, 24, 25, 18 and 27 cm.

Kenyon, *Palaeography*, 16-17, also reported the sizes of the κολλήματα in the following manuscripts, thus: "The finest literary papyrus in existence, the British Museum Odyssey (Pap. cclxxi) has *κολληματα* of just over 9 inches in width; while in the Bacchylides papyrus, which is likewise a handsome roll, they vary between 8 and 9 inches. In the Herodas MS., which is small in height and unostentatiously written, they are only 6 inches in width. The papyrus of Hyperides *in Philippidem* and Demosthenes' Third Epistle, which is only 9 ¼ inches in height, has *κολληματα* 7 ½ inches wide; while in a tax-register (Brit. Mus. Pap. cclxviii), which reaches the extraordinary height of 15 ½ inches, they are only 5 inches wide. The papyrus of the 'Αθηναιων Πολιτεια, which was originally intended merely for a farm-bailiff's accounts, has *κολληματα* of 5 to 5 ½ inches in width; and this is a very common size for non-literary documents."

which we have demonstrated in the preceding section of this chapter.[25] In fact, to Turner's list of papyrus codices with varying κολλήματα dimensions[26] should now be added the evidence of 𝔓[46]. Accordingly, thirteen sheets with κολλήσεις have only one join;[27] but most sheets have two,[28] which further emphasises the uneven length of the κολλήματα that were used. In most sheets with two κολλήσεις, the average interval is about 20 cm (see Fig. 3-2.2), but some can be as long as 30 cm. These figures exclude the overlap, which is probably roughly about 2.0-2.5 cm. Some intervals are conspicuously closer to each other though.[29] The conjoint leaf f19 and f86 is a case in point, the interval of which is only about 10.3 cm.

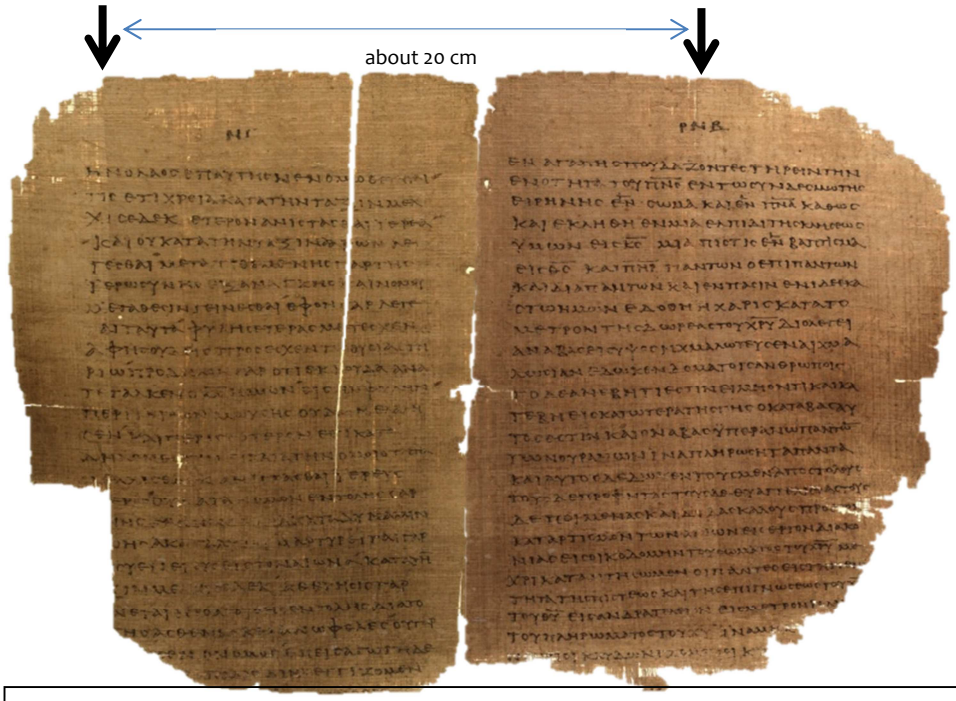


Figure 3-2.2 F27^{v} & f78^{r} showing the κολλήσεις on both pages.

[25] See pages 86-90.

[26] Turner, *TEC*, 47-48.

[27] F20^{rls} & f85^{rrs}; f21^{rls} & f84^{rrs}; f23^{rls} & f82^{rrs}; f25^{rls} & f80^{rrs}; f30^{rls} & f75^{rrs}; f31^{rls} & f74^{rrs}; f36^{rls} & f69^{rrs}; f39^{rls} & f66^{rrs}; f42^{rls} & f63^{rrs}; f44^{rls} & f61^{rrs}; f47^{rls} & f58^{rrs}; f49^{rls} & f56^{rrs}; and f51^{rls} & f54^{rrs}.

[28] There are 21 sheets with two κολλήσεις: F15^{rls} & f90^{rrs}; f16^{rls} & f89^{rrs}; f19^{rls} & f86^{rrs}; f22^{rls} & f83^{rrs}; f24^{rls} & f81^{rrs}; f26^{rls} & f79^{rrs}; f27^{rls} & f78^{rrs}; f28^{rls} & f77^{rrs}; f29^{rls} & f76^{rrs}; f32^{rls} & f73^{rrs}; f33^{rls} & f72^{rrs}; f34^{rls} & f71^{rrs}; f35^{rls} & f70^{rrs}; f37^{rls} & f68^{rrs}; f38^{rls} & f67^{rrs}; f40^{rls} & f65^{rrs}; f41^{rls} & f64^{rrs}; f43^{rls} & f62^{rrs}; f45^{rls} & f60^{rrs}; f46^{rls} & f59^{rrs}; and f48^{rls} & f57^{rrs}.

[29] See related discussion in pp. 79-85.

Some papyrologists have made connections between the frequency of joins in a manuscript and the quality of the papyrus. In particular, Johnson proposed that the frequency of κολλήσεις in a manuscript speaks of the poorer quality of the sheets used in the manufacture of the roll.[30] It is beyond the immediate scope of our present discourse to expedite this proposal. Be that as it may, one may deduce from scattered evidence that this unevenness in $\mathfrak{P}^{46}$ is suggestive of something about the working ethics of our scribe, who may have taken the various locations of the κολλήσεις in his codex, as inconsequential to his copying task, especially as one looks at the texture of the characters that were written across the joins. This point will be further propounded subsequently.

IV. $\mathfrak{P}^{46}$: EXCEPTION TO THE RULE?

In contrast with the supposed pasting rule, all the identified joins show that the edges of the right sheets (the edge of sheets where the gluing substance was applied) are consistently on top of the left. This pattern is confirmed by the following corroborating physical factors. First, in cases where there are ink residue breakages, the breakage happens more on the left κολλήμα than on the right, at the point of joins,[31] (although the reverse is true in some instances).[32] These breakages happen in two ways: *partial* and *severe* disintegration of ink elements. A partial breakage is exemplified by $f19^{r}$-ll^{17-19} (Fig. 3-2.3-Left, next page). In l^{17}, the second vertical stroke of **Η** in ευδοκησεν was written on the κολλήσις overlapping both edges of the κολλήματα. In this instance, the lower portion of the vertical stroke that was written on the left κολλήμα has already partially

[30] Johnson, "Pliny the Elder," 48, commented, "The width of sheets is specified simply because the frequency of joins has an obvious impact upon the quality of the surface. The more joins, the more imperfections to the surface. Thus the higher grades have wider sheets and fewer joins, and the lower grades have narrower sheets and more joins." See also his *Bookrolls and Scribes,* 88.

[31] My appreciation to Hugh Houghton for this point.

[32] For instance, the ν of των in $f91^{r}$-l^{08}.

disintegrated but the portion on the right remains unbroken.[33] An example of a severe breakage is f20^{r}-ll$^{23-24}$ (Fig. 3-2.3-Right). In l^{23}, the second vertical stroke of the ττ was written over the κολλήσις spanning both κολλήματα, but the ink elements on the left κολλήμα have severely disintegrated already, whilst the ink elements on the right are still obvious. In l^{24}, at the point of the joins, very little of the ink elements written on the left κολλήμα remains for the ϵ but the contrary is true for the right. This kind of ink disintegration characterises many of the instances where texts were written on joins. This is a minute detail, but the logical inference is that in a right-over-left pasting direction (where the writing direction is from left to right), ink-elements on the upper right κολλήμα, at the point of the join, seem to preserve better than those in the lower κολλήμα. Conversely, the reverse might be expected in a left-over-right direction.[34]

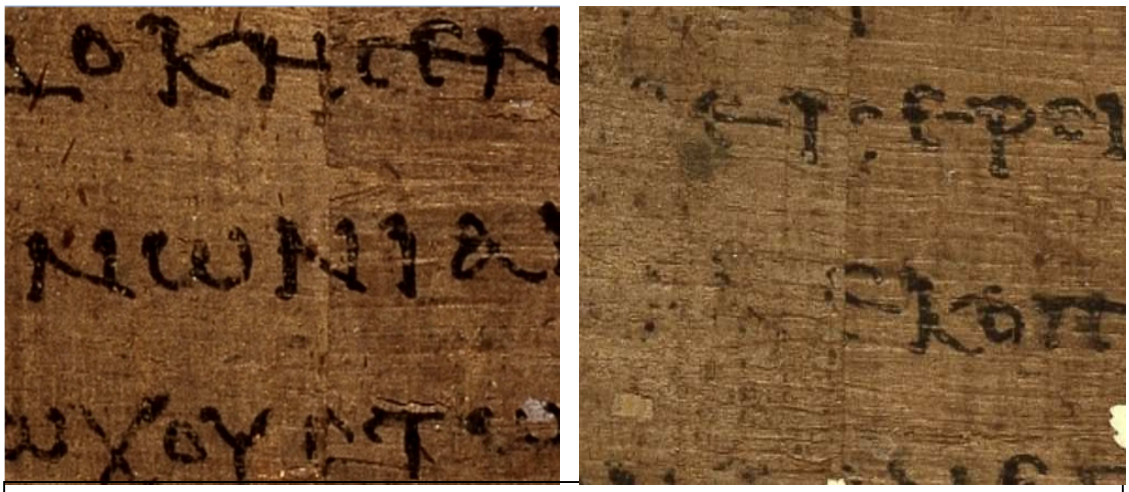


Figure 3-2.3
LEFT: F19^{r}-ll$^{17-19}$ showing the *partial* ink breakages on the edge of the left sheet portion.
RIGHT: F20^{r}-ll$^{23-24}$ showing *severe* ink disintegration on the edge of the left sheet.

[33] The same is true with the second ν of κοινωνιαν in l^{18} and the ς of πτωχους in l^{19}.

[34] The caveat in both these scenarios is that we can only be confident of this inference as it applies to our codex's *present state*; the ink settlement during the *actual* writing might have been different, on the assumption that the papyrus was more pliable at that point than it is today.

The second physical factor is more telling than the first. In some sheets the edges of the horizontal strands of the right κολλήμα show instances of partial peeling off already, giving away the actual overlap direction. In cases like this, the discontinuity of the horizontal strands of the left κολλήμα becomes very obvious even to the naked eye. F73^{r} perfectly demonstrates this point (Fig. 3-2.4).[35] The κολλήσις is located on the mid-left section of the page, near the page numeration (ρμβ), the upper portion of which displays partial peeling off, exposing the point of overlap. In addition, the detaching also shows the length of the under lap of the left κολλήμα, which in this case is about 2.4 cm. In this instance, it does not require an expert eye to decide with utmost certainty the actual direction of pasting; the evidence is so graphic to speak for itself.



Figure 3-2.4
Top portion of f73^{r} showing the partially peeled off edge of the right κολλήμα.

The foregoing points are already sufficient to dispute the *left-over-right* "rule" as far as $\mathfrak{P}^{46}$ is concerned. If such is the case, how might we appreciate this codicological phenomenon in our codex? A few possibilities may be entertained.

[35] Some other examples include f19^{r}, f21^{r}, f22^{r}, and f71^{r}.

One possibility is to take the evidence at face value, that is, when the roll manufacturer was pasting each κολλήμα, the pasting movement was from left to right, with the right κολλήμα as the newer sheets and so on. The process then is the reverse of what Robinson described for the Nag Hammadi papyri, which means that in the case of $\mathfrak{P}^{46}$ the left end (more precisely the back side of the left end) of the right sheet was glued and pasted onto the right end of the left sheet. This direction, in any case, supports the prime importance given to the side with the horizontal fibres, which under normal circumstances receives the writing in a roll. Note as well that when the papyrus is rolled, the inner side (i.e., the protected side) is the one with the horizontal fibres, and if the left hand is the one used for unrolling, the right-over-left direction then makes more sense, helping keep the joins intact as it goes along with the (un-)rolling. If this possibility is sanctioned, what we have therefore with $\mathfrak{P}^{46}$ is an example of an exception to the pasting rule! This is the simplest explanation. But are there any other possibilities?

Another is to move the discussion away from the material *make-up* of $\mathfrak{P}^{46}$ to the material *user*, that is, the ultimate culprit in this instance is not the *pasting direction* per se but the *user* of the codex-construction materials, in this case the scribe of $\mathfrak{P}^{46}$. However, to assume this point of view is to presuppose concomitantly the inefficiency and inexperience of this particular scribe: a neophyte scribe or apprentice of sort, who mistakenly positioned, assuming for argument's sake that the pasting norm was indeed *left-over-right*, the pre-manufactured codex, i.e., using the codex inversely (turned through 180˚), when he started copying the texts of his *exemplar* onto it. Naturally, this does not augur well for our scribe. To use the codex invertedly seriously betrays the very profession the scribe of $\mathfrak{P}^{46}$ represents, for this is one of the elementary routines that ancient scribes go through as they copy their text onto their writing material. Following

this train of thought, the case of the *right-over-left* pattern therefore in $\mathfrak{P}^{46}$ is not a case of exception to the rule, but a simple case of outright scribal ignorance; a very serious reflection on the copying ability of our scribe. Indeed, this argument can certainly account for the inversed position of the codex and therefore perpetuates the validity of the *left-over-right* pasting rule. However, it would be premature to declare this as a closed case, for this phenomenon can still be alternatively appreciated.

This suggestion puts the burden onto the scribe. But some considerations can be marshalled to contradict this. First, on palaeographical grounds, the calligraphy of $\mathfrak{P}^{46}$, as is already well known, betrays a hand "with some pretensions to style and elegance".[36] None would contest that the over-all style of $\mathfrak{P}^{46}$ is one of the most beautiful scripts among the extant biblical papyri, a hallmark of a manuscript produced in a controlled environment, perhaps in a *scriptorium* or a sort of a scribal school. In short, the scribe of $\mathfrak{P}^{46}$ was an experienced scribe, and earned his keep by producing manuscripts.[37] To insinuate that the scribe of $\mathfrak{P}^{46}$ used the codex inversely is to impute incompetence upon our scribe, bereft of any decisive proof. It is true that scribes, even the most experienced ones, were capable of blundering at anything at any point. But to err at the most basic task is more of an exemption than the norm. If indeed the direction of the writing is intrinsic with the direction of pasting, and therefore belongs to the most fundamental requirements of the scribal trade, committing this blunder would suggest scribal naiveté—something that goes against the marked professionalism of the hand inscribed in $\mathfrak{P}^{46}$.

[36] Kenyon, *CBBPIII-1936*, xiii.

[37] That our scribe was a professional copyist is the careful judgment of Zuntz, *TEDCP*, 12-19, 252-62. This is indeed corroborated by internal evidences, especially in the use of literary devices prevalent in the industry and the way the text was inscribed onto the codex. But perhaps the most decisive evidence in favour of this judgment is the presence of στιχοι in $\mathfrak{P}^{46}$.

Second, any naiveté of the scribe of $\mathfrak{P}^{46}$ is decisively contradicted on grounds of recognised scribal practices. On a textual level, this scribe uses copying conventions characteristic of a seasoned copyist. Kenyon and Sanders' monographs have already sufficiently demonstrated this and need not be repeated here, except to underscore the point that this scribe manifested awareness and self-correction. In fact, many of the corrections in this manuscript are *inter scribendum*.[38] It would be unwise to limit this consciousness to the textual level (i.e., correcting textual errors only) and exclude other aspects of the scribe's over-all copying task, especially those that relate to his pre-copying tasks, such as calculating the length of papyrus he needs, the amount and the mixture of ink required for his job, and many others, but most importantly for our purpose checking the suitability of the papyrus material for his writing purposes. The trade simply requires this protocol, and it would be absurd to think that there is no prior examination of the papyrus surface before the actual writing.[39] If indeed the direction of writing is fundamental to the pasting direction, there would have been a very slim chance for this scribe to err in positioning his codex, for this is safeguarded in that pre-copying process. And if indeed the *right-over-left* pattern was a basic "error", the scribe could have easily and immediately rectified it. But is there any clear internal indication that the scribe considered this pattern an "error"? Was it "irregular" to use papyrus sheets with the right edge of the κολλήμα overlapping the left?

The second question may be answered by way of citing other examples of manuscripts exhibiting the *right-over-left* pattern. Definitely, $\mathfrak{P}^{46}$ is not the only

[38] On the corrections in $\mathfrak{P}^{46}$ and how they might also point to the scribal habits of the different hands, see discussion in pp. 290-322, esp. 301-19.

[39] A case may be made for this in the example of the Manichaean codices and most of the Nag Hammadi codices wherein the sheets used either have only one or no κολλήσις at all, indicating the exceptional importance (if not a matter of pride) imputed by the end-users upon these codices, i.e., *de luxe* editions; on this, see Robinson, "Future of Papyrus Codicology," 43; also, Turner, *TEC*, 45, 50. Accordingly, this presupposes a pre-copying examination of the writing material.

manuscript that exhibits this pattern. Another manuscript with the same trait, although inconsistently, is the Hamburg Greek *Acta Pauli*, a multi-quire codex of *quaternion* format originally containing 64 pages, of which only 27 now survive. Although I have no way of confirming it first-hand, it is reported that this codex "has the primitive trait at the κολλήσεις that sometimes the left κολλήμα overlaps the right but at other times the right overlaps the left."[40] To this manuscript, Robinson also added that all the Nag Hammadi codices, except for three, similarly reflect this interchange in the κολλήσις directions.[41]

As to the first question, whether the scribe of $\mathfrak{P}^{46}$ considered the pattern a fundamental error, the most plausible reply, I believe, must have been a negative one. A simple exercise will demonstrate the point. Take two or more sheets of paper and arranged them in a sequence for pasting, the upper sides marked with "H" (for the side with horizontal fibres) and "V" (for vertical) on the opposite side. In addition, on the side with the "H" mark write the word "up" on the upper portion of the sheet and "bottom" at the bottom portion. Then while standing, facing the side marked "bottom", apply a gluing substance to the right edge of the left sheet and paste onto the left edge of the right sheet to form a roll. Obviously, the result would be a *left-over-right* pasting pattern (Fig. 3-2.5A, next page). This exercise seems to lend support to the supposed "pasting rule". However, this is a deceptive tack for it assumes a singular pasting context, that is, the κολλήτης (the paster)[42] always stood on the side with the imaginary "bottom" section. That is not always the case, however, since a similar procedure can have different result depending on where the *κολλήτης* actually stood during the pasting

[40] I gathered this information from Robinson, "Future of Papyrus Codicology," 42, who was commenting on the observations made by H. Ibscher about this manuscript.

[41] Robinson, "Future of Papyrus Codicology," 42. The three exemptions are Codices III, IV, and VI; Codices X and XIII do not have extant κολλήσεις.

[42] E.M. Thompson, *Introduction to Greek and Latin Palaeography*, 24.

session. If he stood on the side with the imaginary "up", the resulting arrangement will be a *right-over-left* (Fig. 3-2.5B)! The exercise is wee bit crude but is effective to prove the point. It only goes to show that during the actual pasting session, text writing (and therefore quality of writing) have nothing to do at all with the direction of pasting, because the desired direction can be easily remedied by way of positioning the roll (or the codex as the case maybe) to the desired direction of the κολλήσις. Therefore, pasting direction *per se* is not strictly the concern of the *κολλήτης*/manufacturer, but is the rightful domain of the user (i.e., the scribe). The direction flow of the text is not *consequent* to the pasting direction at the time of its actual roll production but a *subsequent* component mediated by the requirement of the user.

Figure 3-2.5A

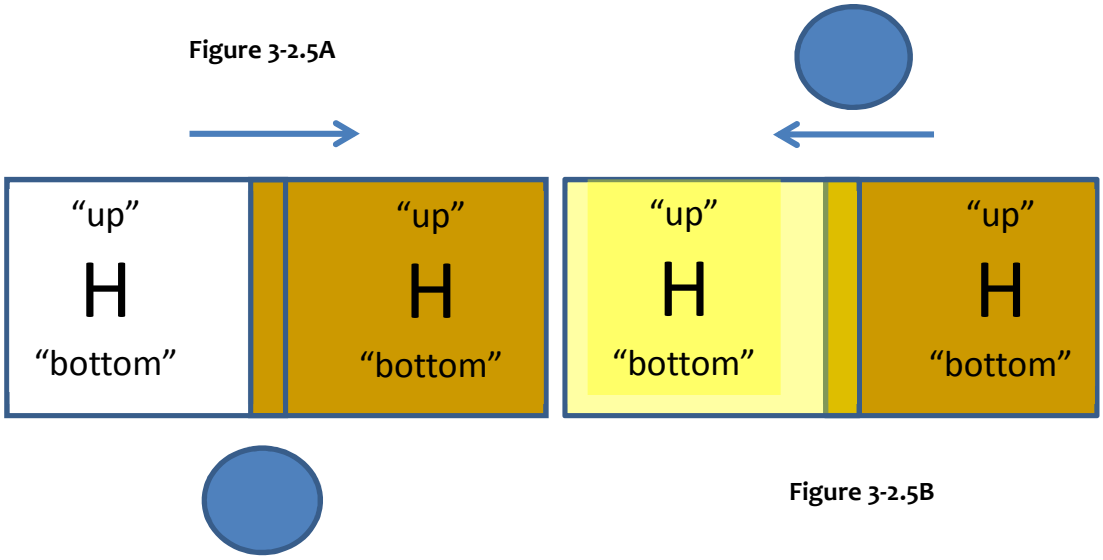


Figure 3-2.5B

The more justifiable suggestion, therefore, is to explore this circumstance from the perspective of *product quality*, that is, the κολλήσεις were so carefully and skilfully executed, satisfying the requirements of the art of pasting, so that to the scribe's naked eye he was working on seamless material.[43] All internal evidence points to the fact that utmost care was imposed by the roll manufacturer upon his material during the pasting

[43] In fact, I detected some of the markings only after using a magnifier during my autopsy visits.

process, so that, as a result, writing on them did not present any apparent difficulty at all.[44] This is corroborated by the observation that despite the codex's *right-over-left* direction of the joins the scribe had *actually written across* the joins, just as any other experienced scribe would, without leaving any evidence of trying to avoid them, except in a few inevitable cases as we have noted in the previous section. Furthermore, a look at the text will show that the train of copying generally flows naturally, without the joins showing any resistance to the pen, as though the scribe was writing on a continuous sheet.

CONCLUSION

The *right-over-left* pattern of the κολλήσεις in $\mathfrak{P}^{46}$ does not reflect our scribe's ineptness, but on the contrary is indicative, as we have attempted to demonstrate here, of his experience in utilising the material unperturbed even by the pasting direction. Using an "inversed" codex and yet still executing his task almost perfectly shows nothing but the scribe's skilful mastery of his craft, insofar as using the material he was familiar with throughout his life. This then poses a direct question as to the proposed rationale for assuming that pasting direction is "*always* left sheet over the right" because of the flow of writing. In view of the example of $\mathfrak{P}^{46}$ (and other manuscripts with similar circumstance), papyrologists may of necessity now need to review the earlier supposition that pasting direction is intrinsically connected with the quality of writing.

[44] On this, see Skeat, "Early Christian Book Production," 35; Gallo, *Greek and Latin Palaeography*, 8; Lewis, *Papyrus in Classical Antiquity*, 51, n24.

SECTION THREE
SCRIPTS AND STYLE:
A PALAEOGRAPHICAL ANALYSIS OF $\mathfrak{P}^{46}$

INTRODUCTION

Manuscript dating belongs to the preliminary stages of text-editing. Undeniably, the date ascribed to a manuscript affects how it is valuated.[1] Nonetheless, dogmatism has no place in manuscript dating,[2] since exactitude is always impossible.[3] Palaeographers are the first to acknowledge that palaeography in itself is not an exact science,[4] and therefore, some vestiges of methodological circularity may be detected at one point of the process or another.[5] Furthermore, and in particular, there are intrinsic problems in determining

[1] This is especially true in the context of historical and text-critical studies, as noted by Stuart R. Pickering, "The Dating of the Chester Beatty-Michigan Codex of the Pauline Epistles ($\mathfrak{P}^{46}$)," in *Ancient History in a Modern University, Volume II: Early Christianity, Late Antiquity and Beyond* (eds. T.W. Hillard et al; Grand Rapids, MI: Eerdmans, 1998), 216-27, p. 217, "Our view of the historical and text-critical contributions of the papyri is fundamentally influenced by the datings assumed for them".

[2] On this, see Nongbri, "The Use and Abuse of $\mathfrak{P}^{52}$," 23-48. See also Bagnall, *Early Christian Books in Egypt*, who proposed that there is a very slim chance for many surviving papyrus manuscripts to be dated to the second century; but cf. Hurtado, Review of Bagnall, *Early Christian Books in Egypt*; and Don Barker, "The Dating of New Testament Papyri," *NTS* 57 (2011): 571-82, pp. 581-82.

[3] Gallo, *Greek and Latin Papyrology*, 82-83, noted two possible ways of manuscript dating, first is the consideration of archaeological data relating to the circumstances of the manuscript in question (e.g., colophons, etc.), and if this fails, then one turns to palaeographical analyses of scripts and handwriting. See also, Bruce Metzger, "Recently Published Greek Papyri of the New Testament," *BA* 10/2 (May 1947): 25-44, p. 30; and Guglielmo Cavallo, "Greek and Latin Writing in the Papyri," *OHP*, 101-48, p. 102.

[4] For instance, Kenyon, *CBBPIntro*, 13, recognised that "(t)he dating of literary hands is by no means so securely established as the dating of documentary hands… Also,… experience gives a certain capacity for estimating age, which rests not so much on particular forms of letters… as on a general sense of style. Nevertheless,… those who have most experience are generally the least anxious to dogmatize, and are ready to admit that a fragment of objective evidence must be preferred to any amount of subjective estimates." More recently, Nongbri, "Use and Abuse of $\mathfrak{P}^{52}$," 46, echoed the same concern: "What emerges from this survey is nothing surprising to papyrologists: *palaeography is not the most effective method for dating texts*, particularly those written in a literary hand" (emphasis added). See also Barker, "Dating of NT Papyri," 571-73.

[5] Nongbri, "Use and Abuse," 46, n49, is correct to argue that some form of circularity operates when one argues for a *precise* date, simply by palaeographical comparison with dated manuscripts.

precisely the date of $\mathfrak{P}^{46}$, owing to the fact that $\mathfrak{P}^{46}$ (together with the other papyri in the cache) was not "discovered" in a pristine archaeological site, but in some clandestine corridors of commerce-orientated antiquities dealers in downtown Cairo and London.[6] It would have been a totally different situation had this been unearthed from the actual site, so that the geographical environment itself would lend evidence as to its date and other related forensic issues. That being said, let me state at the outset that I offer no new evidence nor am I convinced by the arguments of some parties to the debate to invalidate the first editors' dating to the third century, but I similarly echo the warning about the need for methodological caution when dealing with undated papyri, like $\mathfrak{P}^{46}$. However, this does not deter us from setting the on-going debate in a historical perspective.

I. DATING $\mathfrak{P}^{46}$

A. The Traditional Dating of $\mathfrak{P}^{46}$

To my knowledge, no piece of $\mathfrak{P}^{46}$ has ever been subjected to carbon dating;[7] hence, proposals have largely been on palaeographical grounds. In his two editions, Kenyon consistently dated $\mathfrak{P}^{46}$ to the *first half* of the third century,[8] on two main grounds: its *single-quire* format and the *cursive στιχοι*.[9] In the context of the whole Beatty acquisition,

[6] For a glimpse of how backdoor trading was involved in Mr Beatty's acquisition of the biblical papyri and other ancient artefacts, see Charles Horton, "'It was all a great adventure': Alfred Chester Beatty & the Formation of his Library," *History Ireland* 8/2 (2000): 37-42, pp. 40-41.

[7] Of course, carbon dating $\mathfrak{P}^{46}$ will only yield a reasonable date for its materials, i.e., the papyrus strips and ink compositions, and not necessarily the text it reflects.

[8] Kenyon, *CBBPIII*, ix; Idem, *CBBPIII-1936*, xiv. See also, Idem, Review of Henry Sanders, *A Third Century Papyrus Codex of St Paul*, *AJP* 57/1 (1936): 91-95, pp. 92-93; and Idem, *Recent Developments in the Textual Criticism*, 51-61. Interestingly, in his 1958 revision of *Our Bible and the Ancient Manuscripts* (rev. A.W. Adams; London: Eyre & Spottiswoode, 1958), Kenyon described $\mathfrak{P}^{46}$ as "written apparently about the beginning of the third century", a slightly nuanced description but still in the region of the third century. Viewed against Kenyon's consistent assertions of a third century date for $\mathfrak{P}^{46}$, the statement of Young Kyu Kim, "Palaeographical Dating of $\mathfrak{P}^{46}$ to the Later First Century," *Bib* 69 (1988): 248-57, p. 248, that Kenyon "abandoned his former dating" seems a confusing claim, if not mistaken.

[9] Kenyon, *CBBPIII-1934*, ix; Idem, *CBBPIII-1936*, xiv-xv. However, Kim, "Palaeographical Dating of $\mathfrak{P}^{46}$," 248, is not totally incorrect in saying that Kenyon's arguments for dating are devoid of palaeographical hard data; indeed Kenyon did not mention any *comparanda* outside the Beatty papyri. See also, Sanders, *TCPC*, 15.

Kenyon argued that "calligraphically the finest is also the earliest", then ranking 𝔓[46] as the second earliest,[10] he described the overall feature of its letters as "rather large, free, and somewhat stylish hands, well-spaced out, and elegant in type though not severely regular."[11] Whilst Kenyon made mention of the *general* style of the letters, it does not escape notice that his assertion lacks detailed discussion as to the *individual* letters of 𝔓[46].[12]

When Michigan published its own transcription, Kenyon's dating was put in question. However, although Sanders disagreed with Kenyon's dating, he still discerned a date within the third century. Based on the collective dating of the whole find in a supposed Coptic graveyard,[13] Sanders believed the *second half* of the third century is the more probable timeframe.[14] But such a line of thought is methodologically problematic; argumentative rather than evidential, much less palaeographical.[15] In fact, it is extremely difficult to assign dates to manuscripts based on the cumulative age of the whole find;[16] the state of material survival varies from individual manuscript to another.[17] Clearly, just as the question of content was not definitively concluded by the Michigan leaves, the question of dating—which by this time had already spawned other

[10] Kenyon, *CBBPIntro*, 13-14. He also gave the Isaiah and Jeremiah fragments the same ranking.

[11] Kenyon, *CBBPIntro*, 13-14.

[12] In fact, all that Kenyon has for the individual letters is a seven-sentence discussion involving 5 letters only (*CBBPIII-1936*, xiii).

[13] Sanders' dating methodology is fraught with subjectivity in that he argued, "… the more complete manuscripts should be younger than the fragmentary" (*TCPC*, 14). Such methodology did not go unchallenged. For instance, E.C. Colwell, Review of Henry Sanders, A *Third Century Papyrus Codex of the Epistles of Paul*, *CP* 32/4 (Oct 1937): 385-87, p. 386, described such methodology as resting on "very general considerations". More recently, Philip Comfort, *Encountering the Manuscripts: An Introduction to New Testament Palaeography and Textual Criticism* (B & H Academic, 2005), 134, expressed somewhat similar criticism: "Sanders dated the manuscript… almost exclusively based on archaeological surmising, and to the exclusion of any detailed palaeography... his method of dating is suspect and extremely hypothetical".

[14] Sanders, *TCPC*, 13-15.

[15] In fact, Sanders, *TCPC*, 16, argued also that the infrequent presence of the *nomen sacrum* $\overline{\chi\rho}$ militates against an early third century date, arguing that $\overline{\chi\rho\varsigma}$ is the older form.

[16] The reason for this is very obvious—each manuscript has their own distinct socio-historical production narrative, even if they were unearthed with a group of manuscripts when discovered.

[17] We do not need to look far out; the Chester Beatty papyri themselves are the best examples.

views[18]—was similarly left open-ended. For instance, Ulrich Wilcken, whom Kenyon acknowledged as the "first living papyrologist" at the time of its publication, assigned $\mathfrak{P}^{46}$ "*aber mit einem Ansatz um 200*",[19] a date readily supported by Lietzmann, among others.[20] On another tack (admittedly not on palaeographical grounds), Hoskier, throwing support behind Kenyon's dating, appealed to the close affinity of its text with that of other ancient translations to justify an early date.[21] On the other hand, Turner assigned a third century A.D. date (mainly) on the basis of its material dimension.[22] Despite various dating proposals even at the early stages of its publication, what became obvious, however, is that in the wider literature Kenyon's opinion was almost always solely appealed to.[23]

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[18] For instance, Hans Lietzmann, "Zur Würdigung des Chester-Beatty-Papyrus der Paulusbriefe," *Preussischen Akademie der Wissenschaften. Philosophisch-historische Klasse. Abhandlungen* XXV (1934): 774-82, p. 774, accepted a date about A.D. 200.

[19] Ulrich Wilcken, "The Chester Beatty Biblical Papyrus," *APF* 11 (1935): 112-14, p. 113, "*Ja, die Unzialschrift könnte mir schon in II. Jahrh. vorstellen, doch weist die kursive Zeile mit der Stichenzahlüng vielleicht doch schon auf das III. Jahrh. hin, aber mit einem Ansatz um 200 ware wohl auch diese vereinbar. Jedenfalls macht mir Taf. II einen ältern Eindruck als Taf. I.*" For a moment, this dating caused a stir worldwide when the Associated Press circulated in November 1934 that the "World's Oldest Bible", referring to the Chester Beatty II, was already in the National Library in Vienna; as it turned out, only one page of it was ever lent to the Library for a second opinion on its date; on this, see Edgar Goodspeed, "The World's Oldest Bible," *JBL* 54/2 (Jun 1935): 126.

[20] For instance, McCown, "Codex and Roll in the NT," 230. This dating also has the support of the Münster Institute; see Junack et al, *DNTAP*$^{2.1}$, XLIV-XLV; Wachtel and Witte, *DNTAP*$^{2.2}$, XLV. Accordingly, without citing reasons, C.H. Roberts, "The Christian Book and the Papyri," *JTS* 50 (July/Oct 1959): 155-68, p. 156, placed $\mathfrak{P}^{46}$ in his 3rd-century list, ambiguously agreeing with either Kenyon or Sanders.

[21] Hoskier, "A Study of the Chester-Beatty Codex of the Pauline Epistles," 149, "The revised date suggested for the papyrus is circa A.D. 200. If we are startled by this early attribution, we have only to examine the text, in order to rest assured that we are in the presence of something which is contemporaneous with, or which may have preceded the compilation of, the Sahidic version; thus, the circumstantial evidence is definite, for this is generally attributed to a period *circa* A.D. 190. To get behind the Sahidic is indeed a feat; for this liberates us from much reflex action on the Greek texts in Egypt, and leaves us in contact with the Sahidic base only, and with Old-Latin and Old-Syriac versions, which could have influenced our papyrus. It is a most interesting proposition."

[22] Turner, *TEC*, 20, 148; but cf. p. 91 where he seems open to the possibility of a second century dating. These provisional dates of course must be understood in the context of Turner's overall project thrust--that is, to determine manuscript age grouping based on codicological morphology.

[23] For instance, Beare, "Text of the Epistle to the Hebrews in $\mathfrak{P}^{46}$," 379; Jack Finegan, *Encountering New Testament Manuscripts* (London: SPCK, 1974), 72, 77, 181; Metzger, *Manuscripts of the Greek Bible*, 64; Vaganay and Amphoux, *Introduction to NT Textual Criticism*, 12; among others.

B. Breaking through the Third Century Barrier?

Half a century after the complete *editio princeps* was published, Young Kyu Kim made a bold attempt to re-date $\mathfrak{P}^{46}$, claiming he was "able to isolate decisive criteria for establishing the date of ($\mathfrak{P}^{46}$)".[24] Advancing a number of (scattered) criteria,[25] he assigned it to a "time before the reign of Emperor Domitian"[26] or before 81 A.D.—the earliest dating ever assigned to $\mathfrak{P}^{46}$.[27] Kim heavily utilized palaeographical language in arguing for his case. Whilst his audacious attempt is laudable, it remains to be seen whether he had chosen the best method to bear out his argument. For instance, Pickering, whilst commending Kim's meticulous attempt to place $\mathfrak{P}^{46}$ in an earlier time-frame by comparing individual letter formations with other early dated and datable papyri, has argued strongly that the main weakness of Kim's article is methodological—preference for *individual letter formation* (i.e., script) over the *general style* in which individual letters

[24] Kim, "Palaeographical Dating of $\mathfrak{P}^{46}$," 249.

[25] Kim's manner of presenting his arguments lacks organization, as already noted by Pickering, "Dating of the Chester Beatty-Michigan Codex," 216; cf. Comfort and Barrett, *Text of the Earliest*, 204-05; and Comfort, *Encountering the Manuscripts*, 135. But basically Kim's major arguments are as follow:

a. Literary and documentary papyri comparable (exact and similar *comparanda*) to the style of $\mathfrak{P}^{46}$ have been dated between the first century BC to the early second century A.D. (pp. 249-57);
b. The corrector who added ΚΑ (=ΚΑΙ) in f28^{r}-l^{11} provides "the" *terminus ad quem* (p. 249) and should be dated early—between second century BC to early second A.D.;
c. The script of the ligatures in $\mathfrak{P}^{46}$ is very rare after the first century A.D. (p. 249, n6-7);
d. $\mathfrak{P}^{46}$ is bilinear and its tendency to *keep the upper notional line* is very rare after the first century A.D. (p. 249, n6);
e. $\mathfrak{P}^{46}$'s *knobbed alpha* is found only in early papyri (p. 252);
f. The finials at the feet of the letters are seen in manuscripts dated from the last quarter of the third century BC to the third quarter of the first century A.D. (p. 252);
g. $\mathfrak{P}^{46}$ is uninfluenced by the *blob-ornamental* or the *decorated style with rake-formed serif* (p. 252);
h. ΕΓ- form (before compounds with Β, Δ and Λ) is earlier than the ΕΚ- form (pp. 254-56).

[26] Kim, "Palaeographical Dating of $\mathfrak{P}^{46}$," 254.

[27] Such very early dating has a number of implications for NT studies, some of which have been noted already by Daniel Wallace, Review of Young Kyu Kim, *Palaeographical Dating of $\mathfrak{P}^{46}$ to the Later First Century*, *BSac* 146 (1989): 451-52. See also, Pickering, "Dating the Chester Beatty-Michigan Codex," 226-27, where he described the implications of an early dating as a "two-edged sword".

were written.[28] Kim's arguments are also ambiguous as to whether a point is made on its *style* or on its *script*—the two are conflated. He also selectively underscored $\mathfrak{P}^{46}$'s "striking effort to keep to the upper line", implicitly suggesting that $\mathfrak{P}^{46}$ is "bilinear"—a rather incomplete revelation, as we shall elaborate later. Ultimately, Pickering concluded that Kim's proposal is "somehow illusory".[29] In the same way, Junack et al concluded that Kim's *comparanda* are "*ohne... restlos (zu) überzeugen*".[30] Furthermore, Griffin, who critiqued Kim's criteria point-by-point,[31] concluded "(A) first century date for this papyrus seems highly unlikely".[32] For all its worth, Kim's proposal evoked practically no support at all, and those who responded basically concurred with the widely supported view that $\mathfrak{P}^{46}$ belongs to the third century or closely before it.[33]

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[28] Pickering, "Dating of the Chester Beatty-Michigan Codex," 221, argued, "Certainly individual letter shapes need to be examined in isolation to detect similarities across centuries and across styles; but the styles in which they are embedded remain the controlling factor for overall dating of a hand. Many features combine in a complex way to make up the characteristic handwriting of a scribe. This explains why the general impression of a hand—the overall recognition of its interconnected features—is informative for palaeographic analysis". See also Turner's warning on this regard, *GMAW*[2], 20.

[29] Pickering, "Dating of the Chester Beatty-Michigan Codex," 222.

[30] Junack et al., *DNTAP*[2.1], XLIV, "*Eine Datierung mit einer starken Tendenz zur 2. Hälfte des 1.(!) Jahrhunderts meint jetzt Y.K. Kim vertreten zu können, ohne daß die herangezogenen Schriftbeispiele restlos überzeugen können, vielmehr fehlen in* $\mathfrak{P}^{46}$ *ihre typischen dekorativen Elemente*".

[31] Bruce Griffin, "The Palaeographical Dating of $\mathfrak{P}^{46}$" (paper presented at the SBL Annual Meeting—NT Textual Criticism Section, 1997), argued:

a. Ligatures in $\mathfrak{P}^{46}$ do not necessarily point to the first century, but to any period when the scribe had a "lapse in professionalism" (p. 2);
b. *Bilinearity* is common in the first century A.D. but began to break down in the second century, and by the third century it was common to find hands that kept to the upper notional line but not the lower, and it is this third century characteristic that is seen in $\mathfrak{P}^{46}$ rather than bilinearity (p. 3);
c. The corrector who added ϗ (=ϗι), with the separated *kappa*, cannot be conclusive as it consists of only two letters, and therefore a consistency of formation cannot be confidently established (pp. 3-4);
d. The use of ϵγ- form rather than the ϵϗ- form before compounds with ϐ, ϡ, and ϰ, persisted until the third century, as evidenced by $\mathfrak{P}^{13}$, and therefore does not in any way lend evidence to an early dating for $\mathfrak{P}^{46}$ (p. 4);
e. Whilst Kim may have been correct to note that the decorated (*Zierstil*) style was evident in the first century, it is an inconclusive evidence for an early dating for $\mathfrak{P}^{46}$, as that continued well into the third century (p. 5).

[32] Griffin, "Palaeographical Dating of $\mathfrak{P}^{46}$," 7.

[33] Metzger, *Text of the New Testament*[3], 265-66; Griffin, "Palaeographical Dating of $\mathfrak{P}^{46}$"; Pickering, "Dating of the Chester Beatty-Michigan Codex," 216-27. To this may be added the indirect

Kim, however, was not the first and last attempt—Philip Comfort also re-dated $\mathfrak{P}^{46}$ to an earlier historical milieu. In many ways, Kim and Comfort shared similar methodology. But in stark contrast with Kim, Comfort (and Barrett) dated $\mathfrak{P}^{46}$ to the *middle of the second century*, and before ca.175 at the latest,[34] arguing that putting $\mathfrak{P}^{46}$ in this timeframe "allows time for the formation of the Pauline corpus to have occurred and for an archetypal collection to have been produced and to circulate in Egypt".[35]

More recently, Min Seok Jang, in his 2010 dissertation, argued that $\mathfrak{P}^{46}$ should be dated between "A.D. 75-200, with even a third century date not being out of the question".[36] Jang claimed that earlier studies on the date of $\mathfrak{P}^{46}$—particularly Kim, Metzger, Griffin, Pickering, and Comfort—all have one common weakness: "they did not deal with sufficient comparable manuscripts",[37] and disputed their conclusions as "overstated or expressed with too much certainty".[38] Hence, in his thesis he marshalled a total of 310 literary and documentary papyri as his *comparanda* "to

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but nonetheless relevant argument of Royse, *SH-M*, 201, when he noted cases of conflations in $\mathfrak{P}^{46}$ suggesting that "a first century date would appear extremely implausible". Wallace, Review of Young Kyu Kim, 452, who at first glance seems to support Kim's proposal is in fact very cautious about the proposal, "Nevertheless evangelical students should be cautioned from uncritically embracing Kim's thesis just because it comports with their theology. A wait and see position should be adopted until the verdict of other palaeographers is reached".

[34] Comfort and Barrett, *Text of the Earliest*, 205-06; see also the re-affirmation of this dating and the expansion of Comfort's arguments in his *Encountering the Manuscripts*, 134-39. In Comfort and Barrett, *Texts of the Earliest*, 206, Comfort only gave the date "perhaps the middle of the second century", but in his *Encountering the Manuscripts*, 138, he added the maximum range as "at the latest, ca. 175". In a nutshell, Comfort argued that the following papyri are more similar to $\mathfrak{P}^{46}$ in style than those mentioned by Kim: P. Oxy 8, P. Oxy. 841, P. Oxy. 1622, P. Oxy. 2337, P. Oxy. 3721, P. Ryl. III 550, and P. Berol. 9810. Additionally, Comfort, *Encountering the Manuscripts*, 136, argued that the nine *nomina sacra* in $\mathfrak{P}^{46}$, in contrast with the assertion of Skeat, is still "fluid" than "fully developed", therefore, "*could* indicate an even earlier dating." (Emphasis his).

[35] Comfort and Barrett, *Text of the Earliest*, 206.

[36] Min Seok Jang, "A Reconsideration of the Date of Papyrus 46," PhD Dissertation, New Orleans Baptist Theological Seminary, 2010, abstract page; see also pp. 145, 147. Note, however, that in p. 145 Jang mentioned "the probable date of P^{46} is between A.D. 75 and 175 (125±50 yrs)." I am grateful to Dr William Warren for giving me access to a copy of Jang's thesis.

[37] Jang, "Reconsideration of the Date of Papyrus 46," 4.

[38] Jang, "Reconsideration of the Date of Papyrus 46," 146.

determine the most probable date" of $\mathfrak{P}^{46}$.[39] Despite a bigger statistical base, however, Jang equally failed to bring together a convincing case, both at methodological and analytical levels, on the following grounds:

1. Like Kim, Jang also dealt only with individual letter formations than $\mathfrak{P}^{46}$'s general *style.*
2. Jang made too many assertions, but too little palaeographical treatment proportionate to his claims. There is also very little engagement with relevant literature.[40]
3. Jang's proposed chronological timeframe lacks consistency.[41]
4. Jang tends to misrepresent the views of those he analysed.[42]
5. Pages 82-83 present a methodological problem, whereby Jang separately treated the four NT papyri in his set of *comparanda*, arguing that the proposed dates for $\mathfrak{P}^{9}$ and $\mathfrak{P}^{98}$ are outside his control variables' timeframe. But this reasoning seems flawed—if only $\mathfrak{P}^{9}$ and $\mathfrak{P}^{98}$ are outside his control variable, why then exclude the other two NT papyri ($\mathfrak{P}^{18}$ and $\mathfrak{P}^{78}$)?

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[39] Jang, "Reconsideration of the Date of Papyrus 46," abstract page.

[40] For instance, whilst he cited Cavallo's works, he nonetheless missed the most pertinent work of Cavallo where he specifically dated $\mathfrak{P}^{46}$ to the last third of the second century, on grounds of $\mathfrak{P}^{46}$'s general palaeographical style; see Guglielmo Cavallo, "Γραμματα Αλεξανδρινα," *Jahrbuch der Österreichischen Byzantinistik* 24 (1975): 23-54, pp. 34-35. It seems that Jang only cited Cavallo's article on the dating of $\mathfrak{P}^{46}$ (among others) via Pickering's article, see p. 45, n85;

[41] For instance, in the abstract page, he stated, "Based on the results of the research, the probable date of P^{46} is A.D. 75-200, with even a third century date not being out of the question." But a few pages later (p. 4), he would assert, "The hypothesis is that the probable date of P^{46} is not the third century but the second half of the first century (the late first century) or the first half of the second century. In other words, P^{46} was written probably between A.D. 75 and 175 (125±50yrs)". But another timeframe again in p. 147, "… the researcher concluded that the most probable date of P^{46} is between A.D. 75 and 200."

[42] Few examples of this tendency may be cited:

- Griffin did not argue *for* bilinearity of first century papyri as such, but Kim did; in the same way that Griffin did not argue *for* the separated *kappa*, but Kim did (p. 142);
- Also in p. 142, Jang noted that Pickering used the "Alexandrian Majuscule" style to date $\mathfrak{P}^{46}$; actually, it was Cavallo who placed $\mathfrak{P}^{46}$ within this style (and date), which Jang missed to consult;
- In p. 148, Jang claimed that Holmes' opinion is that "$\mathfrak{P}^{46}$ predates Marcion's canon"; but looking at Holmes' article shows that Holmes was not talking about $\mathfrak{P}^{46}$ as a whole but only about the "agreement in errors" among $\mathfrak{P}^{46}$, D, F, G, and Marcion (Holmes, "Text of $\mathfrak{P}^{46}$," 205);
- Citing Royse on the eight cases of Western-Alexandrian conflations that are "singular readings", Jang (p. 148) seems to subtly insinuate that a full study of this textual phenomenon in $\mathfrak{P}^{46}$ would give a dating favourable to his proposal. However, careful reading shows that Royse in fact was arguing against an earlier dating, hence, "Such readings suggest that $\mathfrak{P}^{46}$ is late enough that both these traditions had already arisen… and thus a first century date would appear extremely implausible" (Royse, *SH-M*, 201);
- Jang (p. 28, n34) noted that Zuntz "suggested that $\mathfrak{P}^{46}$ had been written earlier than A.D. 200", but Zuntz's statement is in fact stated differently "($\mathfrak{P}^{46}$) is unlikely to have been written much later than A.D. 200" (Zuntz, *TEDCP*, 11);
- Jang (p. 147) claimed, "If the date proposed in the conclusion is accepted as a probable date, $\mathfrak{P}^{46}$ should be considered along with $\mathfrak{P}^{52}$ as *one of the earlies*t New Testament papyri." (Emphasis added). This is uncalled for, because even at its traditional date (i.e., first half of the third century), $\mathfrak{P}^{46}$ is *already acknowledged* as one of the earliest NT papyri—in fact, the earliest and most extensive witness to the letters of St Paul!

6. In probing the "most probable date" for $\mathfrak{P}^{46}$, Jang used 15 "Characteristics of Letters in $\mathfrak{P}^{46}$" (pp. 10-14) as his control variables. However, some of the variable entries in the list, apart from being selective, are descriptively questionable.[43]
7. Finally, Jang's comparison formula is also problematic, if not flawed, rendering his conclusion a suspect.[44]

Critiquing what they thought to be a tendency of "some New Testament scholars"[45] to appeal to "theological palaeography", Pasquale Orsini and Willy Clarysse[46] most recently proposed an equally "confident" dating for $\mathfrak{P}^{46}$: 200-225 A.D.[47] Whilst this proposed date seems in accord with the general dating "consensus", it confidently pegs the date-range to just 25 years.[48] Moreover, whilst they have placed $\mathfrak{P}^{46}$ in a particular class, i.e., advanced "bureaucratic and chancery", it is not fully clear how they arrived

[43] The following may be cited:

- Characteristic # 2 is described as "A decorative style has hooks at the *beginning* of the letters α, δ, η, ι, κ, λ, μ, ξ, υ, φ, χ and ψ." (Emphasis added). However, even the second vertical stroke of Η (see p. 11, fig. 1-3) was also included, which obviously is not the *beginning* stroke of a letter;
- Char. # 5: It is true that Ε has "long central stroke" (p. 12, fig. 1-6), but Jang failed to note that the medial horizontal shaft has two forms in $\mathfrak{P}^{46}$—the attached and the detached (see discussion below);
- Char. # 12: $\mathfrak{P}^{46}$ indeed has the two-stroke Υ (p. 13, fig. 1-13), but the three-stroke Υ as well;
- Char. # 14 asserts that "letters keep to the upper line" (p. 14, fig. 1-15). However, this is not entirely true for $\mathfrak{P}^{46}$, since the letters Α, Β, Δ, Ζ, Λ, Ξ, Χ, Φ, and Ψ almost always violate the upper notional line (see detailed discussion below);
- Char. # 15, dealing with letter size is lumped into one range 0.3-0.6 cm (0.1-0.25 inch). However, the more reasonable criterion would have been to separate the naturally taller letters (Β Ξ Φ Ψ) from those with more consistently "normal" sizes. Note further that in the latter pages of $\mathfrak{P}^{46}$ the size of some of the letters tended to become smaller than the first half of the codex.

[44] His treatment of his *comparanda* of four NT papyri is a case in point. Having tallied the agreements *and* disagreements, he concluded "The third century is less likely for the date of P^{46}. Moreover, in spite of their limited number, the comparison of the four NT documentary papyri also favours a date of $\mathfrak{P}^{46}$ in the second century rather than in the third century" (p. 92). However, when one analyses the formula used for his Table 8 ("Percentage of the comparison of the New Testament documentary papyri"), one would immediately notice that he only tallied the "agreements" but not the "disagreements". Furthermore, his formula for the lone second century papyrus (i.e., $\mathfrak{P}^{98}$) is different from the three other papyri ($\mathfrak{P}^{9}$, $\mathfrak{P}^{18}$, and $\mathfrak{P}^{78}$), i.e., "total ÷ 1 x 100 = % agreement" for the former whereas "total ÷ 3 x 100 = % agreement" for the latter. Although he presented the latter three papyri with a composite sum (p. 91), he nonetheless divided them when he calculated the percentage, assuming that there are "45 comparison cases" when in fact there are only 15 same comparison cases! A more viable formula is to tally the three separately and not as a composite sum.

[45] But note the caution given by Larry Hurtado <http://larryhurtado.wordpress.com/2013/03/08/the-dating-of-nt-manuscripts-an-important-recent-analysis/> [accessed 25 April 2013], paragraph 7.

[46] Pasquale Orsini and Willy Clarysse, "Early New Testament Manuscripts and their Dates: A Critique of Theological Palaeography," *Ephemerides Theologicae Lovanienses* 88/4 (2012): 443-74, esp. pp. 462, 470.

[47] Orsini and Clarysse, "Early New Testament Manuscripts and their Dates," pp. 462, 470.

[48] Cf. Griffin, "Palaeographical Dating of $\mathfrak{P}^{46}$," 10.

at this conclusion,[49] especially that their only explicitly mentioned *comparandum* is the fragmentary $\mathfrak{P}^{87}$![50] If Kim and Jang failed because they concentrated only with *individual* letters, Orsini and Clarysse seem to have committed the same methodological flaw, discussing only the *general style* of $\mathfrak{P}^{46}$. In fact, they did not mention any letter formation of $\mathfrak{P}^{46}$ (or any of the manuscripts they [re-]dated) in their article. Hence, although the overall tone of their article is generally sound, their (very brief) discussion of the date of $\mathfrak{P}^{46}$ must be considered cautiously as well.

C. The Presupposition of this Thesis

When compared with other NT (or even with non-NT) papyri, dating $\mathfrak{P}^{46}$ will automatically present its intrinsic peculiarities, principally because of the extensive state of its preservation, i.e., 86 leaves. As such, there is no single criterion or characteristic feature that will satisfy the question of dating of $\mathfrak{P}^{46}$. Any attempt to (re-)date $\mathfrak{P}^{46}$ solely on the basis of letter formation is bound to encounter problems of consistency, as the individual letters of $\mathfrak{P}^{46}$ exhibit more than one form throughout the codex, which we do not normally witness in highly fragmentary papyri, e.g., $\mathfrak{P}^{52}$. A more viable methodology (which we cannot extensively undertake presently given our space limitation and different thrust) is one that takes into account *both* the style *and* the scripts of $\mathfrak{P}^{46}$, *together with* other observable features in the manuscript that will help reveal the utmost suitable chronological milieu from which $\mathfrak{P}^{46}$ most likely have

[49] Orsini and Clarysse, "Early New Testament Manuscripts and their Dates," 462, simply mentioned, "$\mathfrak{P}^{46}$ and $\mathfrak{P}^{87}$ can be inserted in the same graphic pattern as these two manuscripts, though they are formal, with a lower *ductus* and a more elegant letter formation. Their script shows a more advanced stage in the development of a bookish use of bureaucratic and chancery scripts: Cavallo indeed attributed $\mathfrak{P}^{46}$ to the 'Alexandrian stylistic class'. We, therefore, assign $\mathfrak{P}^{46}$ and $\mathfrak{P}^{87}$ to the early third century, excluding dates in the first or the first half of the second century."

[50] Note further that whilst they classed $\mathfrak{P}^{46}$ and $\mathfrak{P}^{87}$ together, their dating for the latter is surprisingly a 50-year range, i.e., 200-250 A.D.

emerged.[51] Needless to say, in assessing the date of $\mathfrak{P}^{46}$, various palaeographical,[52] codicological,[53] and textual-paratextual[54] factors must be seriously taken into account.

TABLE 3-C1
A Continuum of Proposed Dates for $\mathfrak{P}^{46}$

DATE RANGE	PROPONENTS
50-100	Kim
75-200	Jang
100-150[55]	Gagos
150-175	Comfort
150-300(?)[56]	Pickering
150-250[57]	Barker
175-225	Wilcken, Hoskier, Lietzmann, Zuntz, Aland (INTF), Cavallo, Metzger, Griffin, Royse
200-225	Orsini and Clarysse
200-300	Turner, Roberts
200-250	Kenyon
250-300	Sanders

[51] Here I take counsel from Turner, *GMAW*[2], 20: "A palaeographer familiar with the material will refuse assent to a precise year date to a manuscript simply by comparison with other texts and by no other criterion."

[52] Palaeographically, the "graphic stream" approach thus far seems to offer the most integrative method in assessing style and individual letter formation; on this method, see Cavallo, 'Greek and Latin Writing in the Papyri,' 101-48. Specific to $\mathfrak{P}^{46}$, Barker, "Dating of NT Papyri," 580, has emphasised four characteristics that need to be kept in mind in comparing $\mathfrak{P}^{46}$ with other papyri: 1) angularity of ⲗ, ⲫ, and ⲩ, 2) observance of the upper notional line, 3) decorative styles (angular tail endings) on the descenders, and, 4) cursive formation of ⲁ, ⲙ, and ⲉ. Furthermore, analyses of the various other hands in $\mathfrak{P}^{46}$ (i.e., page numerations, corrections, stichometrical notes, reading marks) will definitely provide additional palaeographical clues, perhaps as synchronic or diachronic data, as to the timeframe of $\mathfrak{P}^{46}$.

[53] In the same token, we must look, with same amount of importance, at some of the codicological details of $\mathfrak{P}^{46}$ such as gathering (quiring) make-up (as used by Kenyon), columniation, as well as morphological comparison exemplified in Turner's study of the typology of early codices. To this maybe added, with caution, Sander's proposal to look at $\mathfrak{P}^{46}$ in the bigger context of the whole Chester Beatty biblical papyri (a point also noted by Pickering).

[54] The pertinent text-critical issues that can potentially provide additional evidence for dating $\mathfrak{P}^{46}$ include 1) the presence of peculiar readings in $\mathfrak{P}^{46}$, foremost of which is Rom 16.25-27, 2) textual affinities with other ancient translations (so Hoskier), 3) "texttype" conflations (so Royse), 4) the compilation of the *corpus Paulinum* (so Zuntz, Comfort, and Holmes [indirectly]), and 5) the presence or absence of contracted forms within the complex system of *nomina sacra* (so Skeat); and perhaps the orthography of σιλβανος (so Zuntz, *TEDCP*, 259-62) may be additionally reviewed again.

[55] Gagos' dating is known only through Griffin's paper, p. 10, where he intimated that Gagos' dating of $\mathfrak{P}^{46}$ is "early to mid-second century". We have no way of independently confirming this, though.

[56] Pickering's dating proposal is a bit indirect, "… if the style is properly traced from the mid-second century onwards, we are bound to keep the Beatty text in fairly close chronological connection with its third and fourth-century relatives" (p. 223).

[57] Barker, "Dating of NT Papyri," 581, "Given our limited knowledge of scribal practices and that there are some corresponding style similarities that appear earlier, perhaps a tentative dating range of A.D. 150-250 should be assigned to P^{46}."

The various proposals for the dating of $\mathfrak{P}^{46}$ are reflected in Table 3-C1. From our vantage point, I still see no definitively convincing evidence to abandon altogether the third century benchmark for $\mathfrak{P}^{46}$.[58] Accordingly, in light of the collective palaeographical analyses as well as the other contributing factors mentioned above (and the related discussion below), I am disposed to take a date-range for $\mathfrak{P}^{46}$ from the latter part of the second half of the second century to an early part of the first half of third century,[59] for even within this timeframe, $\mathfrak{P}^{46}$ remains the *earliest* and *most extensive* manuscript witnessing to the *corpus Paulinum*,[60] if not the whole NT[61]—a timeframe potentially rich to provide us with further data in sketching salient points of the sociology of ancient book production and its accompanying scribal culture. Hence, the following section is an attempt to describe the individual letters—including morphological peculiarities evident from the same letters—that now fill the pages of $\mathfrak{P}^{46}$, with emphasis on how our scribe might have worked with his pen as he negotiated each page of his codex.

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[58] As Table 3-C1 clearly reveals, the "traditional" dating at third century still cannot be effectively ruled out since it still enjoys the support of most scholars. In fact, despite proposing an early dating, Jang, "Reconsideration of the Date of Papyrus 46," abstract page, himself is not prepared to totally abrogate the third century timeframe.

I must also add here a paratextual feature—still unexplored in the discussion of $\mathfrak{P}^{46}$'s date—that is, the use of apostrophe to separate two consecutive nasals, which according to Turner, *GMAW*[2], 13, n3, is not normally written in documents till the third century A.D.; see also, W. Cronert, *Memoria graeca Herculanensis* (Lipsiae: B.G. Teubner, 1903), 18. See related discussion in pp. 173, 197.

[59] Utilizing the Guglielmo Cavallo's "graphic stream" model, Barker, "Dating of NT Papyri," 581, recently proposed the range 150-250, a more cautious attempt at dating $\mathfrak{P}^{46}$, almost akin to what Griffin, "Palaeographical Dating of P-46," attempted to convey (proposing the range 175-225).

[60] It is supposed that the distinction of being the earliest manuscript containing a Pauline letter should go to $\mathfrak{P}^{32}$ (or P.Ryl. I 5), containing Titus 1.11-15, 2.3-8, housed at the John Rylands Library, dated by C.H. Roberts, *Manuscript, Society and Belief in Early Christian Egypt: The Schweich Lectures of the British Academy 1977* (London: OUP for the British Academy, 1979), 13, to the 2nd century. This may be true, but the continuing question on the authorship of the Pastorals will keep this suggestion tentative.

[61] $\mathfrak{P}^{52}$ presently is the widely acknowledged oldest surviving NT manuscript. C.H. Roberts, *An Unpublished Fragment of the Fourth Gospel in the John Rylands Library* (Manchester: Manchester University Press, 1935), dated it to the "early part of the second century", enjoying the support of many scholars. However, Nongbri, "Use and Abuse of $\mathfrak{P}^{52}$," 23-48, alternatively advanced that the most likely dating for $\mathfrak{P}^{52}$ is the "early part of the third century"—or in the same region as $\mathfrak{P}^{46}$! If Nongbri's argument holds water, then $\mathfrak{P}^{46}$ might as well be the earliest surviving NT manuscript. It remains to be seen though how other palaeographers will evaluate Nongbri's thesis. In any case, it would be an interesting development to watch.

II. THE MAIN HAND

A. Unitary Composition; Diverse Features

Although he considered the script of $\mathfrak{P}^{46}$ second only, in terms of beauty, to the P. Beatty VI (Num and Deut), Kenyon positively described the script of $\mathfrak{P}^{46}$, thus,

> It is far more calligraphic in character, a rather large, free, and flowing hand with some pretensions to style and elegance. It is upright and square in formation, and well spaced out between the letters and between the lines… In general it may be said that the letters are rather early in style and of good Roman formation.[62]

It is this general description that dominates the literature.[63] However, advances in palaeographical studies since 1936 require more than just a general depiction of the character formations in $\mathfrak{P}^{46}$—a more detailed look at each letter in the context of its overall style is necessary, especially if these details are used in crafting dating proposals.[64] Fortunately, all the letters of the Greek alphabet are adequately represented in the main text of $\mathfrak{P}^{46}$; hence, a fuller palaeographic description is a *desideratum.* The appropriate question to begin with is: "How many scribe/s cooperated to produce the text of $\mathfrak{P}^{46}$?"

There are definitely other hands in $\mathfrak{P}^{46}$. F38^{r} (see Fig. 3-3.1 [next page]) provides a graphic overview of various textual elements at work throughout the pages of $\mathfrak{P}^{46}$, betraying various hands other than our scribe.[65] These are easily distinguished from the main hand since their formations and writing styles are distinctly recognizable.[66] These features are underscored at appropriate junctures but suffice it to say in the meantime

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[62] Kenyon, *CBBPIII-1936*, xiii. Similarly, Sanders, *TCPC*, 12, noted, "The writing is of the book hand type and the letters are carefully formed and well spaced."

[63] For instance, Metzger, *Manuscripts of the Greek Bible*, 64; Aland, "Significance," 115.

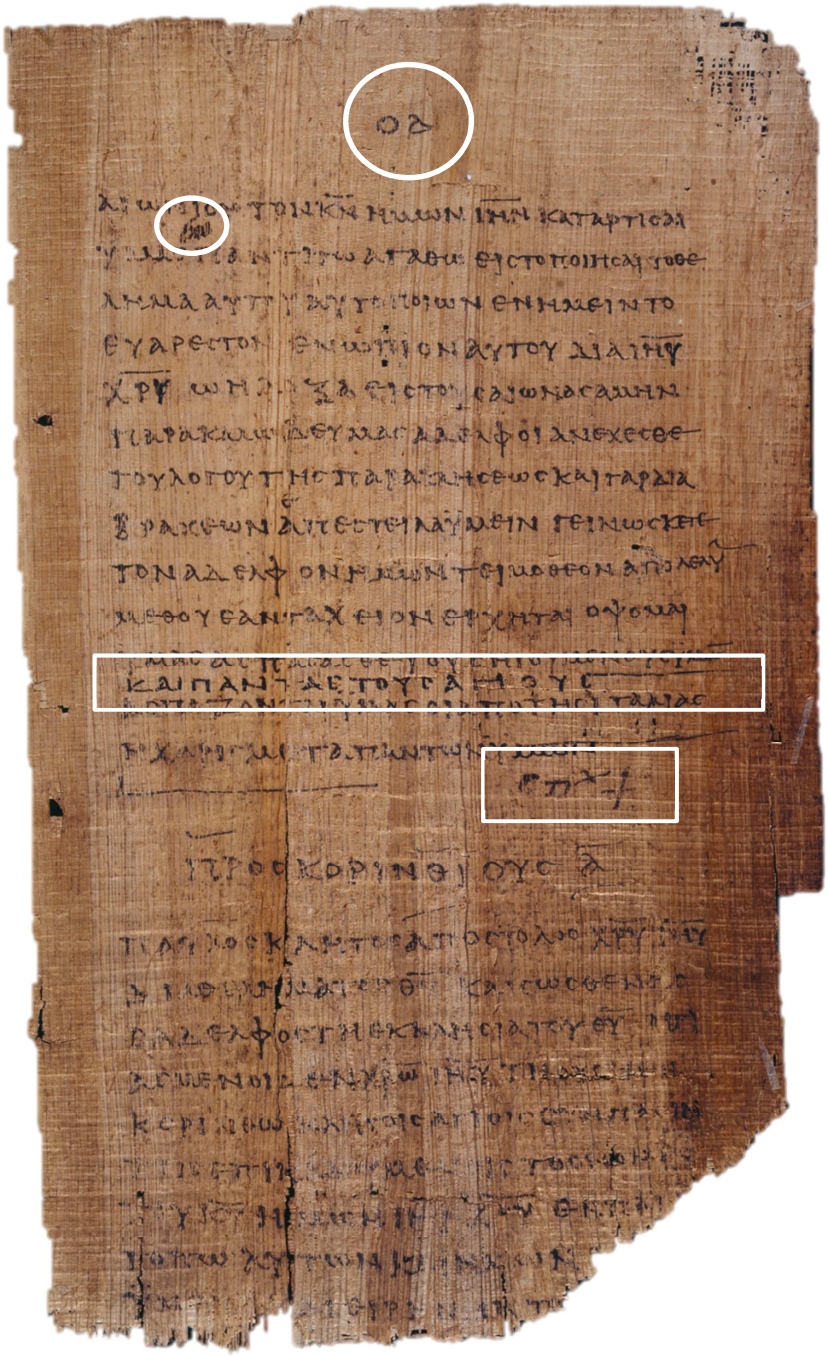
[64] Whilst Comfort and Barrett, *Text of the Earliest*, 204-06 criticised Kim's palaeographical method in re-dating $\mathfrak{P}^{46}$, they nonetheless equally lack sufficient palaeographical discussion.

[65] Page numeration (ΟΔ), *stichos* note ($\overline{CTT^{\chi}}$ $\overline{\gamma}$), corrections (l^{02} [ϵ$\overline{N}$], l$^{04\text{-}05}$ [$\overline{IHC}$/$\overline{\gamma}$ || $\overline{XPC}$/$\overline{\gamma}$], l^{08} [ⲁ$^{\epsilon}$ⲡⲉⲥⲧⲉⲓⲗⲁ], l^{12} [ⲕⲁⲓ ⲡⲁⲛⲧⲁⲥ ⲧⲟⲩⲥ ⲁⲅⲓⲟⲩⲥ], and l^{14} [$\overline{\gamma MWN}$]). To these must be added the reading marks (no example on this page). The page numerations and *stichos* notations (and some corrections) appear to be contemporary with the main hand, but the reading marks must be of a (not so) later date, when our codex was already used for (public/liturgical) reading.

[66] Note that getting familiar with the individual letter formation is methodologically significant in the classification of corrector/s, as will become evident in Chapter Four-Section Two, pp. 290-322.

that there are indeed other hands in $\mathfrak{P}^{46}$ if we will take it in its totality (i.e., its transmitted form). However, for our present palaeographical purposes, the question is narrowed down only to the main hand.

Figure 3-3.1
F38^{v} showing different hands at work on the page



Since its discovery, none has questioned the single-hand inscription of the text of $\mathfrak{P}^{46}$. It is not out of place, however, to raise at least a passing observation that there appear to be some instances of apparent script change (admittedly, not too drastic) from one page to another, especially when one compares the letters in the earlier pages with those of the latter ones, where the number of characters per line significantly increased. Does this indicate another scribe/s in operation? Very unlikely. In an extensive manuscript, the slightest change in angle, in terms of handling the pen, inevitably would result in different kinds of stroke even within the same codex, by the same scribe.[67] As David Parker rightly noted for Codex Sinaiticus, "Moreover, one may expect some degree of unevenness across hundreds of pages even in the most expert of scribes, so that two pages from opposite ends of a manuscript may look disconcertingly as though they were by different scribes, even though an examination page by page shows no change in *style*."[68] On top of this, it may also be argued that ink colour,[69] despite intermittent changes in density (thickness) due to re-inking (or sharpening of the pen), throughout our codex is very uniform, pointing to but a single hand. Hence, there is no sufficient ground to assume multi-scribal inscription for $\mathfrak{P}^{46}$. That being said, we now proceed with a palaeographical description of $\mathfrak{P}^{46}$.[70]

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[67] The departure point here is when the scripts are radically different, as can be seen in P. Oxy. xviii 2192 [<http://163.1.169.40/gsdl/collect/POxy/index/assoc/HASH0148/7f28c43c.dir/POxy.v0018.n2192.a.01.hires.jpg> (accessed 23 Jan 2012)], showing three different hands (see Turner, *GMAW*^{2}, 114).

[68] D.C. Parker, *Codex Sinaiticus: The Story of the World's Oldest Bible* (London/Massachusetts: British Library/Hendrickson, 2010), 51. (Emphasis added).

[69] Of the ink colour, Sanders, *TCPC*, 12, stated, "The ink is dark brown and has faded little. There has been little rubbing of the surface, so that almost every letter of the preserved portion is still legible."

[70] In view of the points already raised against analysis that pre-eminently privileges individual letter formations, the following analysis is presented rather *characteristically* than *alphabetically*, discussing each letter more in relation to the identified features characteristic of its general style than individual letter formation.

We must begin with an admission that the writing style of $\mathfrak{P}^{46}$ does not mechanically fall into one single well-defined palaeographical typology but reflects diverse features characteristic of various graphic styles,[71] as will become evident later.[72] In marked contrast with $\mathfrak{P}^{45}$'s right-leaning style or $\mathfrak{P}^{47}$'s rather thick letter-formations, the letters of $\mathfrak{P}^{46}$'s main hand are generally written in an upright block uncial script[73] and regularly-spaced (sans defects in the material), with few of the letters slightly influenced by cursive formations.[74] Kenyon is not incorrect in saying that the letters of $\mathfrak{P}^{46}$, when compared with the other Chester Beatty papyri, are "calligraphically more beautiful, with pretensions to style and elegance"[75]. Such a description undeniably speaks volumes about the marked professionalism of our scribe—one with obvious concern for the aesthetic look of his manuscript.[76]

[71] For various "graphic streams", see Cavallo, "Greek and Latin Writing," esp. 101-36. Similarly, the earlier categories of Turner's "style of handwriting", *GMAW*^{2}, 20-23, remains indispensable.

[72] This fact is not unique to $\mathfrak{P}^{46}$ but a palaeographical "truth". As Turner, *GMAW*^{2}, 20, rightly noted, "… several styles of writing were simultaneously in use. Contemporary with each other, they cross-fertilize and hybridize easily."

[73] Apart from Kenyon, the uprightness of the letters has been duly noted also by Kim, "Palaeographical Dating of $\mathfrak{P}^{46}$," 253; Pickering, "Dating of the Chester Beatty-Michigan Codex," 225; and Barker, "Dating of NT Papyri," 579. Accordingly, Junack et al, *DNTAP*$^{2.1}$, XLIV, describes its scripts as "*aufrechte, elegante und flüssige Unziale professionallen*". Furthermore, suggesting elsewhere that $\mathfrak{P}^{46}$ belongs to the Alexandrian majuscule group, Cavallo, "Greek and Latin Writing," 120, placed this graphic stream under the general heading "Imperial Peak", a general characteristic of which is the tendency to keep an upright axis.

[74] Especially the letters ⲁ, ⲉ and ⲙ, as also noted by Barker, "Dating of NT Papyri," 580.

[75] So is Metzger, *Manuscripts of the Greek Bible*, 64.

[76] A related observation along this line is the fact that throughout the extant pages there is one instance only where our scribe apparently made use of sponge to effect correction (f74^{r}-l^{02}). Furthermore, I have found no instance where our scribe had written over erroneous characters (except for a few which I call "unconsummated errors"; on this, see pp. 241-42, 312, and 317). On the contrary, he neatly effected *in scribendo* corrections by way of a right-to-left oblique line on the character(s) and/or expunging dots above the letter(s) at issue. This detail, too, paints a portrait of a scribe particularly concerned about the aesthetic effect of corrections, unlike some other NT papyri of comparable age, e.g., $\mathfrak{P}^{66}$ which notoriously exhibits enormous examples of *in scribendo* corrections through water and sponge or scrapping to the point of damaging some of the papyrus strips (on $\mathfrak{P}^{66}$ corrections, see Gordon Fee, "The Corrections of Papyrus Bodmer II," *NovT* 7 [1965]: 247-57).

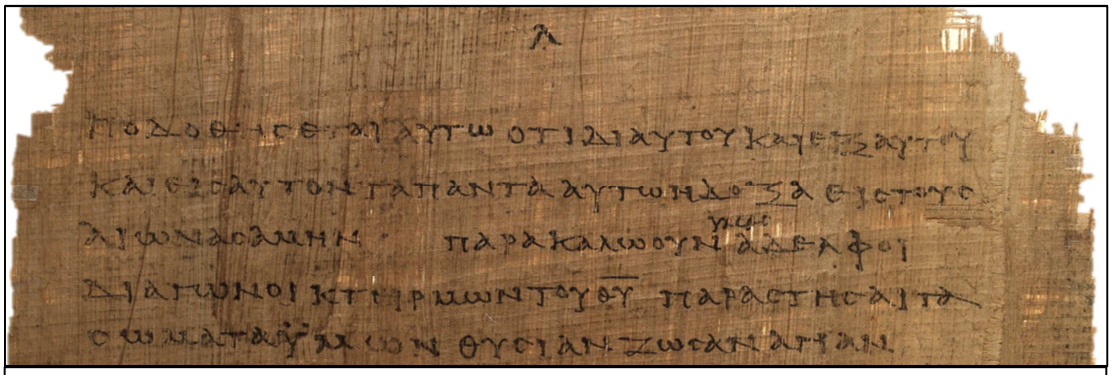


Figure 3-3.2 F16^{v}-II$^{01-05}$: Capacious interlinear spaces compared to the linear characters

As shown in Fig. 3-3.2, interlinear spaces are regularly bigger (usually at 0.4-0.5 cm) than the height of the characters on the lines (usually at 0.3-0.4 cm), excluding the naturally taller and/or broader letters such as **ϐ**, **ϫ**, **ϰ**, **ϗ**, **ϰ**, **ϒ**, and **ϕ**. At times **ο** is even written between 0.1-0.3 cm, without being reduced to a mere dot (Fig. 3-3.3). Notably, the first letters of each line are normally written slightly bigger than the rest.

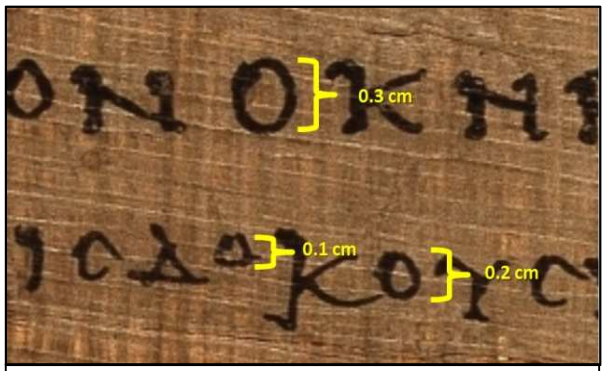


Figure 3-3.3 F82^{r}-II$^{06-07}$: Various sizes of **ο**

B. Ligatures

Occasionally, and in contrast with Sanders' categorical denial,[77] ligatures appear on the lines of $\mathfrak{P}^{46}$, mostly connected by letters with horizontal bars (**ϵ**, **ϑ**, **τ**) or by the finials of

[77] Sanders, *TCPC*, 13, stated, "Smaller letters, sometimes a little crowded, occur at line ends, but there are no ligatures". However, Kim was correct to note Sanders' categorical error in denying the presence of ligatures in $\mathfrak{P}^{46}$ (Kim, "Palaeographical Dating of $\mathfrak{P}^{46}$," 249). Nonetheless, it must be noted that if Sanders was guilty of categorically denying the presence of ligatures in $\mathfrak{P}^{46}$, Kim is guilty, too, of categorically using the presence of ligatures to peg its date to a particular (earlier) time-era. On the contrary, Griffin, "Palaeographical Dating of P-46," 2, argued that ligatures are indicative more of a "lapse of professionalism on the part of the scribe" than of a time-period of manufacture.

vertical and oblique strokes of a letter (especially, ⲁ, ⲗ, ⲙ, and ⲡ).[78] Except for a single case (f38^{r}-l^{09} [Fig. 3-3.4]), there are no other cases of ligatured abbreviation.[79]

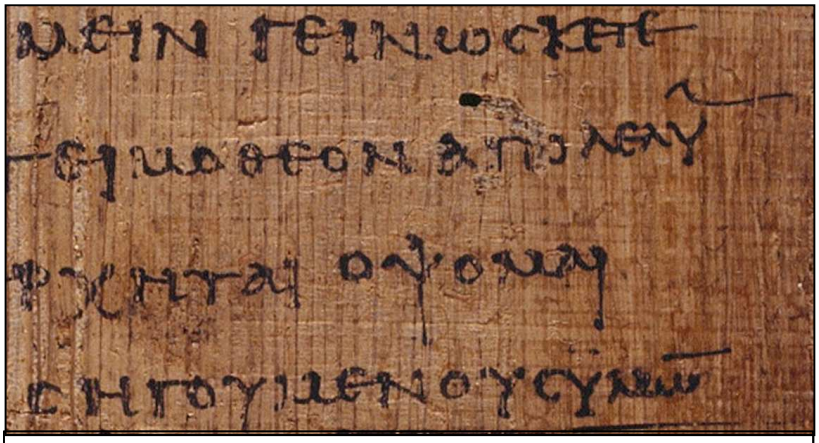


Figure 3-3.4 F38^{v}-ll$^{08-11}$: Some ligatures and the lone abbreviation

Two letters are formed by a single stroke only (ⲓ and ⲟ[80]); nine with two (ⲁ, ⲅ, ⲑ, ⲗ, ⲣ, ⲥ, ⲧ, ⲭ, and ⲱ[81]); twelve with three (ⲃ, ⲇ, ⲉ, ⲍ, ⲏ, ⲕ, ⲙ, ⲛ, ⲡ, ⲩ, ⲫ, and ⲯ); and one with four (continuous) strokes (ⲝ).

C. Bilinearity?

There is an apparent tendency to keep the upper notional line, for instance, with the smaller *omicrons* and *omegas* apparently "hanging" onto the upper notional line.[82] But it should not be automatically deduced from this that $\mathfrak{P}^{46}$ is strictly bilinear.[83]

[78] It is, of course, not a "genuine" ligature when compared with truly ligatured manuscripts. Cavallo's *pseudoligature* ("Greek and Latin Writing in the Papyri," 120) might be a more appropriate description, referring to a characteristic feature of the "Alexandrian majuscule" graphic style.

[79] But I must add that $\mathfrak{P}^{46}$ has some other conventions of abbreviation, namely, the cases of line-end ⲛ (represented by a crossbar placed above the preceding vowel) and *nomina sacra* contractions.

[80] The ⲟ is usually written in a single stroke due to its comparatively small size.

[81] There are two cases where ⲱ were written differently, both involving the line-end ⲛ abbreviation, forming three strokes; see f48^{v}-ll$^{16-17}$; cf. f53^{r}-l^{15}; and f66^{v}-l^{24}.

[82] Barker, "Dating of NT Papyri," 579, also noted, "There is some emphasis on keeping to an upper notional line, but not always, by writing letters such as the omega and omicron in a smaller script and placing them closer to the upper line and by 'hanging' the upsilon and sometimes the beta down from the upper line."

[83] See also Griffin, "Palaeographical Dating of P-46," 3; Pickering, "Dating of the Chester Beatty-Michigan Codex," 225; Barker, "Dating of NT Papyri," 580.

Although Kim was correct in his observation that there is a "striking effort to keep (the letters) to the upper line", he nonetheless failed to mention that the scribe was equally less constrained to keep the bottom line, allowing some letters, with vertical or long oblique strokes,[84] to recurrently break the lower line. In fact broad letters (**ϐ**, **ϗ**, **ϩ**, and **χ**) usually violate the lower line. Interestingly, despite this elongation, our scribe avoided connectivity with letters of the following line[85] (unlike, for instance, the *Hawara Homer* [P. Oxy. xviii 2161],[86] where the *ascenders* and *descenders* of the vertical strokes of the letters **φ** and **ψ** touched portions of both the preceding and following lines [Fig. 3-3.5]).

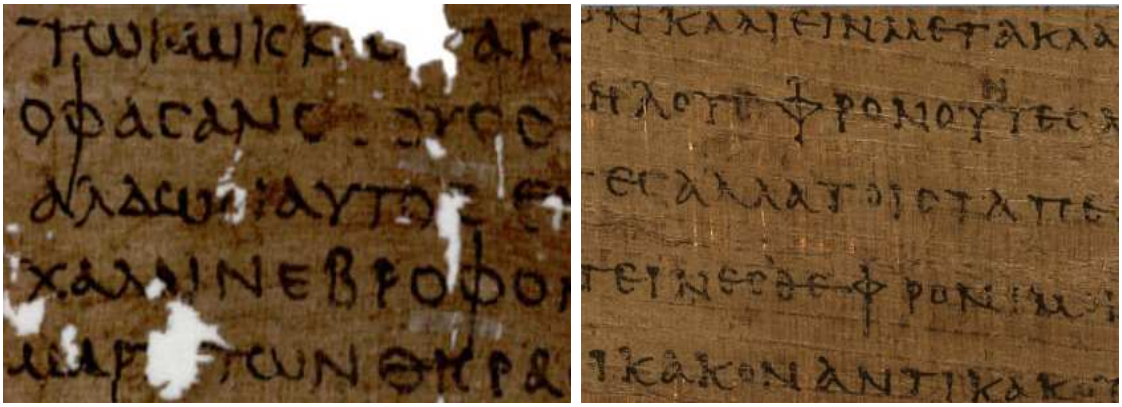


Figure 3-3.5 **Left:** P. Oxy. xviii 2161, column 2, lines 15-19 (second century A.D.)
Right: $\mathfrak{P}^{46}$, f16^{r}-II$^{08-12}$

When at line-beginnings and at line-ends, the descender of the **ρ** almost always violates the lower boundary. Additionally, in regard to the upper notional line, the hooked upper terminations of the oblique strokes of **ϧ**, **ϫ**, and **ϫ** also frequently violate

[84] For instance, **ι** is often prolonged below the lower line, as well as the vertical strokes of the letters **ρ**, **φ**, **ψ**, and **γ**. In addition, the oblique stroke of hooked **ϧ** also infrequently violates the bottom notional line.

[85] The lone anomaly throughout the extant pages is f53^{v}-I^{02}, where the descender of the line-end **ρ** touched the oblique stroke of the **ϧ** directly below it.

[86] Plate available in Turner, *GMAW*^{2}, 55; and Cavallo, "Greek and Latin Writing in the Papyri," 128.

the upper boundary (see Fig. 3-3.6), and rarely ι[87] at line-ends.[88] Cumulatively, therefore, it is more fitting to describe 𝔓[46] as "roughly bilinear"[89] than "strictly bilinear".[90]

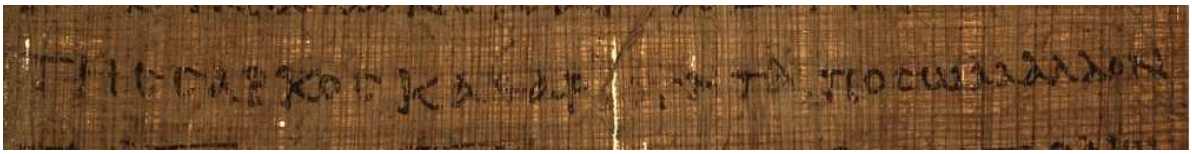


Figure 3-3.6 F30[v]-l[14]: Upper terminations of the *alphas* and *lambdas* violating the upper notional line

D. Angularity

Some letters are distinctly angular in formation (particularly ϐ, ⲇ, ⲍ, ϒ, ϒ and Φ).[91] The central strokes of Φ are written with a broad angular diamondish shape, begun and closed at the top;[92] the vertical stroke is normally between 0.6-0.7 cm in height, always violating both the notional parallels, resembling a characteristic of what Turner calls "Formal round".[93] To some extent, ϒ is written pretty much like the Φ, with its vertical strokes also violating the notional boundaries. Apart from being angular, our scribe appears not bothered by the two forms (in terms of length) of the ϒ throughout the codex:[94] the first keeps its size in accord with other letters, i.e., keeping the parallel notional lines, with a shorter finial at its descender tending to the

[87] Here we must note again the premature assessment of Sanders, *TCPC*, 13, when he stated, "Iota is often prolonged below the line, never above." In contrast, there are indeed cases at mid-lines (e.g., f20[r]-l[01] [ϵϒⲗοΓιⲁϹ]; f82[r]-l[14] [ΤΙ]; f88[r]-l[13] [ϊϹοΥϒⲭοΝ]) and at line-ends where it violates the upper or both notional parallel lines. When at line-ends, ι violates the lower notional line most often than observing it; rarely, it violates both the notional parallels (e.g., f15[r]-l[18], f49[r]-l[10], f55[r]-l[01], f60[r]-l[05], f63[r]-l[06], f85[r]-l[06]).

[88] On the violations of the notional parallels by these letters, see Turner, *GMAW*[2], 3; also, for Φ and ϒ, see Cavallo and Maehler, *Hellenistic Bookhands*, 7.

[89] On this designation, see Turner, *GMAW*[2], 3.

[90] The significance of such distinction comes to fore when we consider the discussion regarding the dating of 𝔓[46], where Kim, among other things, argued for a very early date for 𝔓[46] on the basis of its bilinearity.

[91] Barker, "Dating of NT Papyri," 579. Note also the angularity of the rough breathing marks, which are formed in what Sanders, *TCPC*, 19, described as "half an H", above a few monosyllabic words (see our discussion in Section Four, esp. 175-76).

[92] Griffin, "Palaeographical Dating of P-46," 6.

[93] Turner, *GMAW*[2], 21.

[94] Also noted by Kim, "Palaeographical Dating of 𝔓[46]," 250, n6.

right; whilst the second form has a prolonged descender that violates the lower notional line,[95] with a finial flourishing to the left (Fig. 3-3.7).[96]

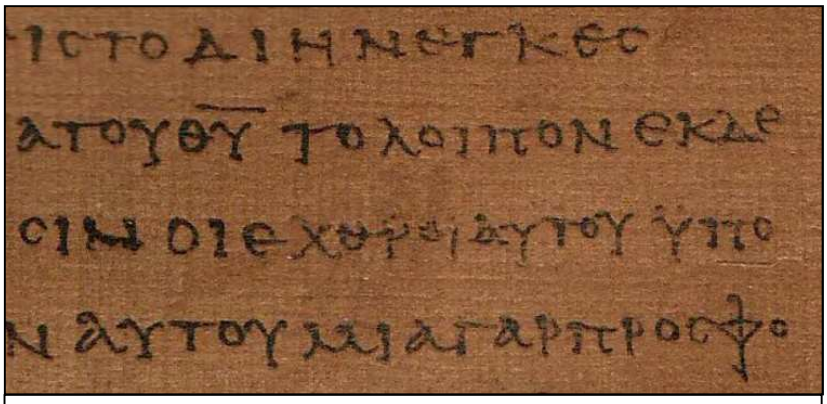


Figure 3-3.7 F31^{v}-II$^{11-14}$: The two forms of ϒ on same lines.

B is broad and regularly violates the lower line, usually measuring between 0.4-0.6 cm in height, on a normal line (i.e., characters written between 0.3-0.4 cm [Fig. 3-3.8]). Whilst **Λ** is equally broad it nonetheless rarely violates the imaginary lower line, but it may occupy a space proportionate to two-to-three characters, especially with our scribe's tendency to put very slight spaces before and after **Λ**. Notably, the upper termination of the oblique stroke frequently has slight hook toward the left or sometimes a roundel.[97] Also, as first pointed out by Jang,[98] **Λ** (but more common in **Ζ** [e.g., f51^{r}-l^{05}]) often has a "loop" at the angular strokes (Fig. 3-3.8).

[95] Contra Kenyon, *CBBPIII-1936*, xiii, who described the ϒ as "rather deeply indented, but *not* prolonged downwards". (Emphasis added).

[96] This second (prolonged) form of ϒ resembles the characteristic of what Turner, *GMAW*^{2}, 21, (quoting Cavallo) calls as "biblical majuscule"; see also, Cavallo, "Greek and Latin Writing in the Papyri," 129. The upper termination also frequently displays a slight downward hook.

[97] Noted by both Metzger, *Manuscripts of the Greek Bible*, 64; and Kim, "Palaeographical Dating of $\mathfrak{P}^{46}$," 250.

[98] Jang, "Reconsideration of the Date of Papyrus 46," 12.

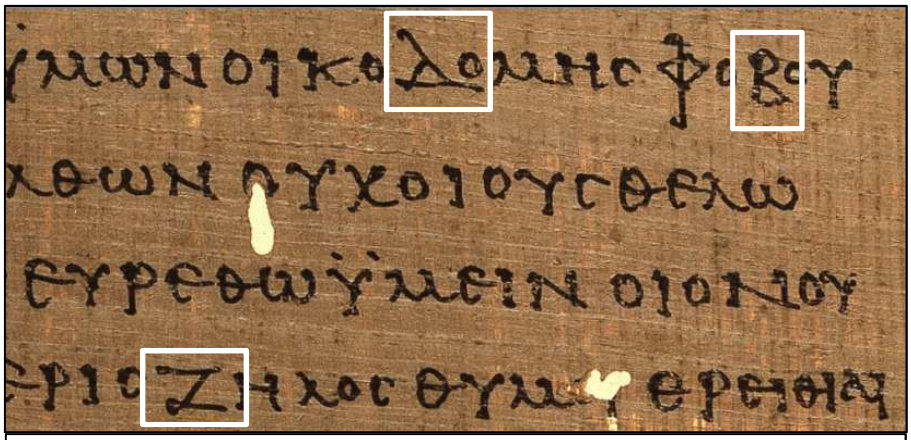


Figure 3-3.8 F74^{r}-II$^{04–07}$: Hook and Loop in ⲁ and ⲍ

E. Serifs and Hooks

The characters are noticeably "serified", located at the top and base of most letters, resembling characteristics of a sub-type under Turner's "Formal round" or Schubart's *Zierstil.*[99] Many of the serifs are formed at the feet of vertical strokes with a short horizontal line tending to the left[100] (ⲏ, ⲓ, ⲕ, ⲡ, ⲛ, ⲣ, ⲧ, ⲩ, ⲯ, ⲫ; rarely for ⲅ; but the second vertical strokes of ⲡ and ⲏ tend to the right [Fig. 3-3.9]).



Figure 3-3.9 Collage of letters with serifs at the foot of the verticals



[99] Turner, *GMAW*^{2}, 21.

[100] On the use of serifs in earlier Hellenistic manuscripts--which might have indirectly influenced the scripting tradition of our scribe, see Cavallo and Maehler, *Hellenistic Bookhands*, 10.

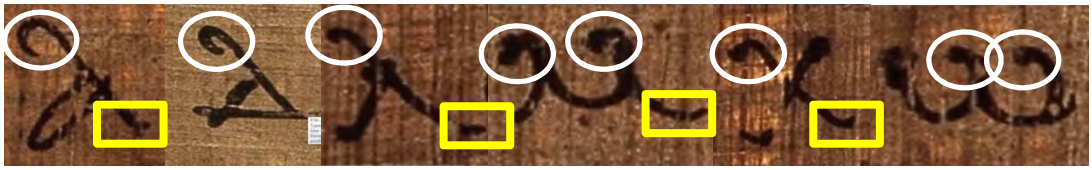


Figure 3-3.10 Collage of characters with upper hooks and with lower finials.

The upper terminations of the oblique strokes of ⲁ, ⲇ, ⲗ, ⲙ, ⲭ, and ⲱ have (but not always) small curves rounded toward the left, sometimes forming a small hook[101] (Fig. 3-3.10). Conversely, the lower terminations of ⲁ, ⲗ, and ⲭ (infrequently the final oblique stroke of ⲙ) irregularly have curves to the right (Fig. 3-3.10), with finials sometimes touching portions of the proceeding letter. From time to time, the supralinear bars of *nomina sacra* also have curves on both ends, but at times only the left end flourishing upward; so also the right end of the bottom horizontal stroke of ⲥ, curves down to the left.

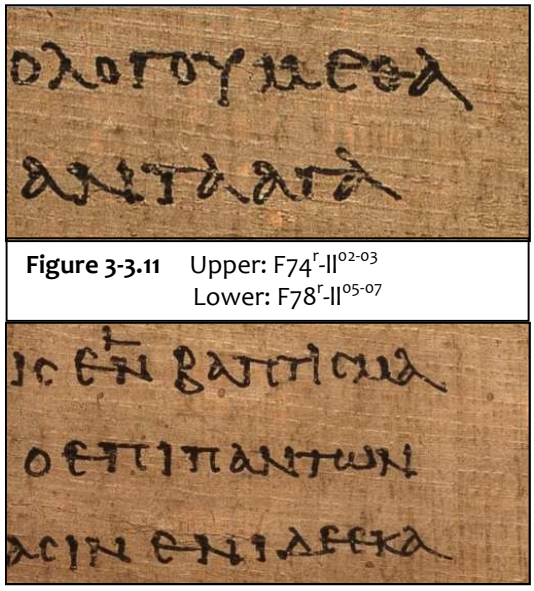


Figure 3-3.11 Upper: F74^{r}-II$^{02-03}$
Lower: F78^{r}-II$^{05-07}$

The ⲁ, formed in two-strokes, tends to have a "hook" at the upper termination of the oblique stroke;[102] sometimes the hook becomes a loop.[103] But the upper termination is not always "hooked" nor its foot serified. Instances are equally numerous where it is plainly an oblique stroke. But whether hooked or not, when located at line-ends, the lower termination is almost always prolonged (Fig. 3-3.11)—sometimes lavishly prolonged as if functioning

[101] Rarely (but notably), the tip of the hooked oblique touches the circular stroke. It should be noted, however, that it is equally frequent for the upper termination of the oblique stroke of ⲁ to be plainly simple (i.e., without the hooks or roundels) with a curved circular stroke—similar in many ways to that of P. Marmarica (end of 2nd or early 3rd century A.D.—plate available in C.H. Roberts, *Greek Literary Hands: 350 B.C.—A.D. 400* [Oxford: Clarendon, 1956], 19). ⲉ, ⲥ, and ⲧ have some resemblances also.

[102] The ⲁ in ⲁⲩⲧⲏ of f77^{r}-I^{20} looks a bit awkward, looking more like a ⲗ, because the hook was well extended above the upper notional line. This happens as well to ⲇ, e.g., f74^{r}-II$^{02, 04}$.

[103] For instance, f74^{r}-I^{16}.

as a line-filler (e.g., f15^{r}-l^{14} [hooked] and f16^{v}-l^{04} [unhooked]). The curved stroke is semi-ovalish, sloping to the right; but very rarely overshoots the oblique stroke (e.g., f73^{v}-l^{14} [ⲦⲀϒⲦⲎⲚ]).

F. Curves and Horizontals

When compared to ⲟ, ⲉ is rather ovalish than round. The head-stroke is usually longer than the lower stroke, but never longer than the medial stroke. The head-stroke's right tip also never touches the medial shaft.[104] In most cases, the left end of the medial shaft is attached to the central part of the first downward curve stroke (not on the cusp), but a detached form very rarely occurs also (e.g., third ⲉ in f13^{r}-l^{05} [ⲈⲔⲀⲖⲈⲤⲈⲚ], -l^{09} [ⲈⲤⲦⲀⲒ] and f69^{v}-l^{18} [ⲠⲢⲞⲈⲖⲐⲰⲤⲒⲚ], l^{19} [ⲠⲢⲞⲈⲠⲎⲄ=]),[105] often with the right end of the bar touching the following letter, almost forming a two-letter ligature (Fig. 3-3.12). Both forms may occur on the same page and on the same line. The medial shaft is often extravagantly extended as if doing a line-filling function,[106] and when at line-ends as if to keep the imaginary right text margin vertically straight (with a very low success rate though) (Fig. 3-3.12). Accordingly, the oblique stroke of ⲁ (e.g., f71^{v}) at line-ends is also prolonged at times, presumably with the same line-filling function (Fig. 3-3.12). Additionally, when at line-ends, ⲟ and ⲱ are usually written in smaller sizes, without any drastic change in strokes.[107]

[104] In this regard, the upper curve of ⲉ does not embrace the characteristic of the Alexandrian majuscule" as described by Cavallo, "Greek and Latin Writing in the Papyri," 120.

[105] *Contra* Griffin, "Palaeographical Dating of P-46," 6.

[106] First noted by Sanders, *TCPC*, 13.

[107] Accordingly, ⲟ and ⲱ, frequently written in smaller sizes (except when they begin the line), seem always committed to the upper notional line—a tendency observable in other second and third century papyri, as listed by Barker, "Dating of NT Papyri," 580.

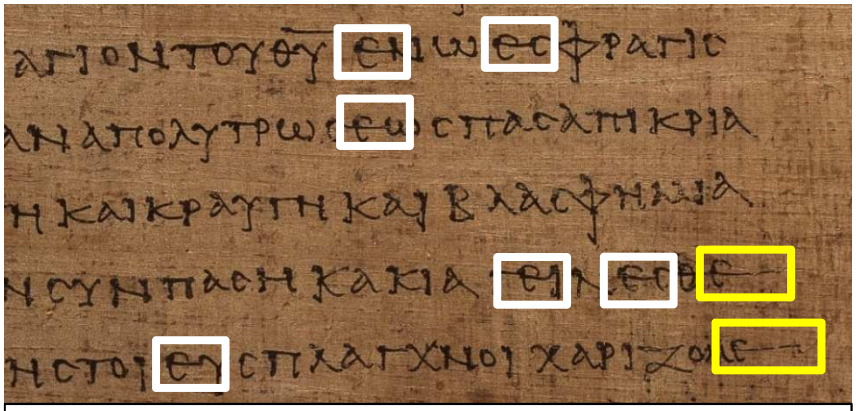


Figure 3-3.12 F79^{r}-II$^{08\text{-}12}$ showing ϵ's medial stroke touching other letters and elongated at line-ends

The head-stroke of τ (so is π and γ) usually "hangs" onto the upper line and its vertical stroke also keeps the lower notional line. However, there are occasional instances where the upper portion of the vertical stroke leans to the right (without affecting the angle of the head-stroke), as well as less frequent instances of finials at the lower termination tending left. Many are the instances where the right end of the head-stroke touches the proceeding letter, specifically at the cusp portions of ο and ϵ (and less frequently the upper portion of the first vertical stroke of η), sometimes triggering a series of two-to-three interconnected letters (as if coalescing into a graphic unity), notably (but not always) in words with the τ-ο-υ sequence (Fig. 3-3.13). This must have been due to the speed at which these letters were formed.

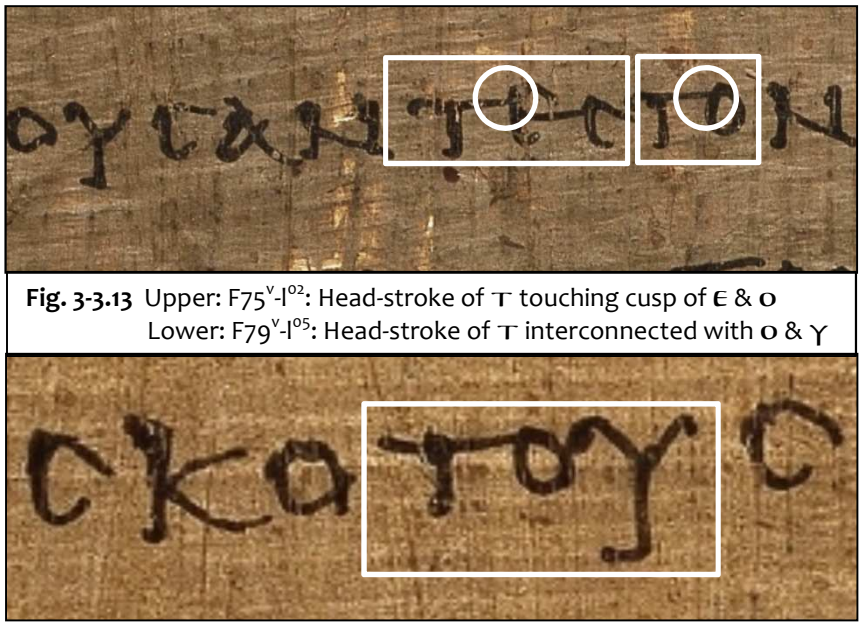


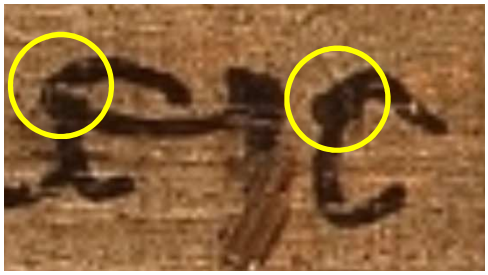
Fig. 3-3.13 Upper: F75^{v}-I^{02}: Head-stroke of τ touching cusp of ϵ & ο
Lower: F79^{v}-I^{05}: Head-stroke of τ interconnected with ο & υ

ⲙ has one form only and is inclined to breadth. The first stroke is rather small and fluctuating towards the cursive. Apart from the noticeable "looped" heads, the two oblique middle strokes combine to form a dish shape.[108] The foot of the second oblique has finials flourishing to the right, at times touching a portion of a proceeding roundish letter (especially when followed by ⲱ; and infrequently ⲟ).

There is resemblance in the formation of the circular stroke of the roundish ⲟ and the ovalish ⲑ, both commencing and closed at the upper left side; except for the size and the medial horizontal stroke in ⲑ the morphological difference is sometimes almost indistinguishable.[109] Similarly, the first curved strokes of ⲥ and ⲉ also commence at the upper left side, almost at the same angle with ⲟ and ⲑ (Fig. 3-3.14). The head-stroke of ⲥ is a bit flattish, somewhat bending downward,[110] and is always longer than the lower curve.



Fig. 3-3.14 Formations of ⲑ, ⲟ, ⲉ, and ⲥ.



[108] Also observed by Barker, "Dating of NT Papyri," 579.

[109] Although it must be immediately added that our scribe never reduces the size of ⲑ as it usually does with ⲟ.

[110] Noted both by Kenyon, *CBBPIII-1936*, xiii; and Sanders, *TCPC*, 12.

ϩ and ⲛ are broad letters and are morphologically very stable, with marked commitment to bilinearity. To distinguish the ϩ from ⲛ, both made of three separate strokes and both left ends of the oblique strokes persistently attached to the uppermost tip of first vertical stroke, the right end of the oblique stroke of ϩ is attached to the mid-portion of the second vertical stroke, whilst that of the ⲛ to the bottommost part (Fig. 3-3.15). It is also remarkable that occasionally the bottommost part of the second vertical stroke of ϩ flourishes with a finial, as if attempting to connect with the proceeding letter (see Fig. 3-3.15). Of the ⲛ, Kenyon noted, "letter ν at the end of a line is occasionally represented by a stroke above the preceding vowel." But the statement requires qualification—this only happens when the ⲛ is the last character in a *complete* word.[111]

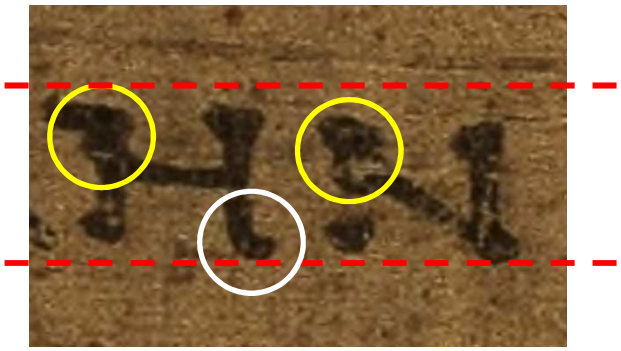


Figure 3-3.15 Formation, bilinearity, and flourishing of ϩ and ⲛ.

CONCLUSION: THE SCRIBE AND HIS PEN

Which "style" then should be assigned to 𝔓[46]? An objective reply must be an ambiguous one (or at least tentative), for reasons we have already mentioned above. The main difficulty lies in the extensive state of its preservation—because it is so extensive, various morphologies for a particular letter can be marshalled throughout

[111] Note, however, that there is inconsistency in the application of this convention in 𝔓[46].

the extant leaves. This fact alone immediately highlights the lack of consensus as to the dating of $\mathfrak{P}^{46}$, for the multiple features derivable from the extant pages make the choosing of *comparanda* a very difficult undertaking. However, the foregoing morphological description shows that by and large the scribe of $\mathfrak{P}^{46}$ is generally consistent in the sequential execution of strokes and characteristic features that formed the individual letters. The letters are upright without any discernible slant, suggesting that the pen may have been handled at 45 degrees. Individual letters are generally written calligraphically beautiful. Accordingly, *in scribendo* corrections also betray a scribe who is careful not to make his manuscript look "dirty", as we have already attempted to show in the previous sections. Overall, $\mathfrak{P}^{46}$'s palaeographical features depict a scribe who, despite some minor (if not negligible) formal discrepancies, is well practiced in his craft, achieving superb legibility[112]—which may suggest the end for which this manuscript was purported; a point we shall try to further explore in the next section.

[112] Barbara Aland's description of its scribe's calligraphic taste is very apt, "*Ganz im Unterschied zum Kopisten von $\mathfrak{P}^{45}$, der den Text seiner Vorlage rasch und eigenständig erfasste, ist dieser Schreiber offensichtlich mehr auf die kalligraphische Schönheit seiner Abschrift konzentriert… Was die Kalligraphie anlangt, so überragt $\mathfrak{P}^{46}$ alle anderen frühen Codices aus dem Fayyum*" ("Sind Schreiber früher Neutestamentamenlicher Handschriften Interpreten des Textes?" in *Transmission and Reception: New Testament Text-Critical and Exegetical Studies* [TS Third Series, Vol. 4; eds. J.W. Childers and D.C. Parker; Piscataway, NJ: Gorgias, 2006], 119-22, p. 121).

SECTION FOUR
PLAYING BY THE RULES?
VISUAL FEATURES, STRUCTURE SIGNALS, SENSE PAUSES, AND THE COPYING HABITS OF OUR SCRIBE

INTRODUCTION

Describing the way characters were inscribed on $\mathfrak{P}^{46}$, Sanders noted keenly,

> There are few cases of punctuation by a single dot in high position… Double dots like a colon occur once… On the other hand, there are very many slight spaces left in the text, often where they serve admirably for punctuation. Some of these spaces are sufficiently large so that one may be sure that the scribe intended them to mark the ends of paragraphs. Others are so narrow that they may be explained as *accidental*.[1]

A year later, Kenyon picked up Sander's observation and despite his rather less optimistic assessment of these "slight spaces", his conclusion interestingly opened up a new dimension relevant for scribal studies: "(The space-intervals) suggest at any rate some perception by the scribe of the sense of what he was writing".[2]

Space-intervals (and other features in the manuscript for that matter), as an established ancient scribal feature, have implications for the wider historical questions about early Christianity in general, and the early Christian book production enterprise in particular.[3] I hasten to add that space-intervals do not only primarily serve the purposes of the copyists; to a larger extent they reflect a concern for the intended end-users of the

[1] Sanders, *TCPC*, 16-17. (Emphasis added).

[2] Kenyon, *CBBPIII-1936*, xiv, offers his own analysis of these spaces, thus, "Pauses in sense are occasionally indicated by slight space-intervals between words. Sanders indicated a large number of such intervals, but most of them have, I think, no significance. Some are due to flaws in the papyrus (as at junctions of *κολλημματα*), some to the scribe's habit of leaving a slight space after an abbreviation, some seen to be purely accidental or hardly perceptible."

[3] In this regard, Hurtado's *Earliest Christian Artifacts*, esp. 155-90, is particularly instructive.

manuscript being manufactured, for they serve as *readers' aids*.[4] A few questions of interest immediately come to fore: Did the scribe of $\mathfrak{P}^{46}$ really have a sense of what he was copying, as Kenyon had suggested? If so, how are we going to account for mid-word space-intervals? Are space-intervals products of unfortunate accidence, and therefore *nonsensical*? Are they also reflected in other manuscripts or are they peculiar to $\mathfrak{P}^{46}$?

I. METHODOLOGICAL CONSIDERATIONS

Space-intervals, as a mode of punctuation, perform various tasks. E.G. Turner mentioned the following: 1) to separate a *lemma* (a quoted passage to be explained) from a comment, 2) to close a period, 3) in conjunction with a *paragraphus*, and 4) to indicate a change of speaker in dramatic texts.[5] We can trace some of these functions in $\mathfrak{P}^{46}$.

Sanders identified over a thousand space-intervals in $\mathfrak{P}^{46}$.[6] Conversely, Kenyon somewhat downplayed their significance and attributed some of them to the flaws of the material, *nomina sacra*, and simple copying accidence.[7] Hence, his 1936 transcription reflects fewer space-intervals than are actually in the manuscript,[8] choosing only those that, in his judgment, are "plainly intentional and denote a pause in the sense".[9] This immediately raises methodological problems, foremost of which is the potential

[4] Turner, *GMAW*[2], 8, whilst noting that systematized use of punctuation is of late invention, affirmed that there are ways in which scribes conveyed modes of punctuating, to help users or readers of the manuscripts being produced, specifically mentioning two features: deliberate space-intervals and the prolonging of the strokes in ε, α, and ς—these two features, interestingly, are both present in $\mathfrak{P}^{46}$.

[5] Turner, *GMAW*[2], 8.

[6] Sanders, *TCPC*, 17.

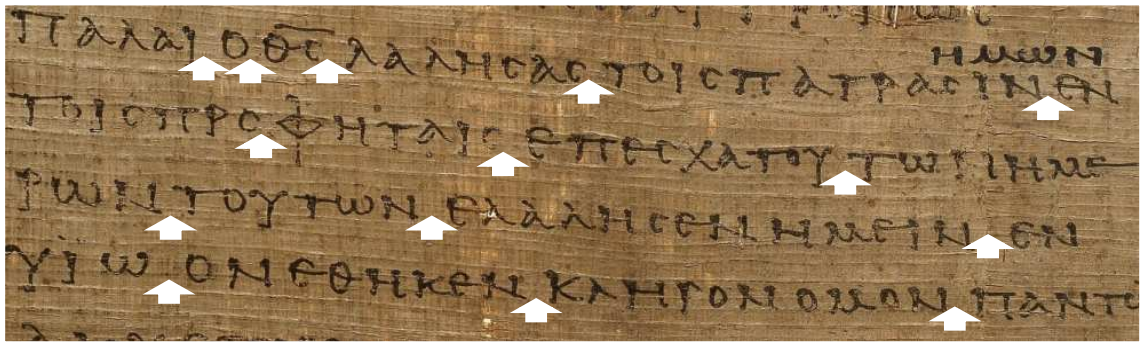
[7] Kenyon, *CBBPIII-1936*, xiv.

[8] This is not distinctive to Kenyon; other earlier editors were also liable to this. The pursuit for the traditional goal of textual criticism may have somehow influenced this "editorial tendency". Stanley Porter, "Pericope Markers in Some Early Greek New Testament Manuscripts," in *Lay-out Markers in Biblical Manuscripts and Ugaritic Inscriptions* (eds. M. Korpel and J.M. Oesch; Pericope 5; The Netherlands: Royal Van Gorcum, 2005), 161-76, pp. 161-62, keenly observed that "… editors are so concerned to establish the text for the purpose of collation with other texts that they often pass by distinctive features of particular manuscripts. In other words, there is a greater concern for the text itself, almost in an abstract sense, than there is for the particularities of individual manuscripts, with all of their differences in handwriting, size, and accompanying palaeographical features."

[9] Kenyon, *CBBPIII-1936*, xiv.

subjectivity of such criterion.[10] Needless to say, we need more hard data than just a sweeping description of this phenomenon. In fact, space-intervals in $\mathfrak{P}^{46}$ come at various measurements, and occur at various sense levels, and in view of this, there is a high degree of arbitrariness involved if one formulates sense classifications based *solely* on the measurement of space-intervals.[11] A less arbitrary methodology is indeed a *desideratum.*[12]

Figure 3-4.1 F21^{r}-II$^{02-05}$, showing the presence of space-intervals at various locations in the lines, and the degree of breadth variation for each space-gap.



The project, nonetheless, is not totally impossible, as some patterns can be singled-out where space-breaks may be located (Fig. 3-4.1). As a method, all space-

[10] Peter Head, "Significant Spaces in $\mathfrak{P}^{46}$," <http://evangelicaltextualcriticism.blogspot.com/2009/08/significant-spaces-in-p46.html> (accessed July 23, 2011), put this to fore when he inquired, "… I noticed that (Kenyon's) edited text marks out certain spaces… Looking at the facsimile, however, it becomes apparent that the perception of the editor as to what is significant for punctuation has been at work here, since there are other spaces, equal or larger, that are not signalled… It suggests that correspondence with what Kenyon thought was 'a pause in the sense' was one critical factor in identifying the significant spaces, which suggests that this is not a particularly objective measure. Of course, there is wisdom to be gained from constant and thoughtful exposure to manuscripts and Kenyon's opinions are obviously thoughtful and experienced. But if one wanted to study $\mathfrak{P}^{46}$ in order to understand the 'perception by the scribe of the sense of what he was writing', one would need to do a lot of careful thinking (and even perhaps some measuring)."

[11] This has been the methodology adopted by Kenyon, *CBBPIII-1936*, xiv, "I have thought it best to indicate them only when they are plainly intentional and denote a pause in the sense. Only an examination of the facsimile will show exactly what the facts are. They suggest at any rate some perception by the scribe of the sense of what he was writing."

[12] A further difficulty in isolating the genuine space-intervals from the purely *accidental* cases (if any) lies in the fact that our codex in its actual size is relatively small (32 cm [B] x 28 cm [H]), making measurements quite a daunting task—and here again the issue of potential subjectivity on the part of the researcher must be consciously recognized.

intervals will be documented at the first instance,[13] without prejudice as to whether they genuinely have to do with "pauses in the sense" or not,[14] then they will be examined whether there are certain classifiable "structure signals" (grammatical [e.g., pauses indicating punctuation requirements at the levels of phrase, clause, and sentence, or even at pericopal level], aesthetics [e.g., OT quotations, *nomina sacra*], etc.) that can be detected throughout the extant portions of manuscript where these space-intervals occur, or whether these space-gaps can be explained by factors other than sense-signals, especially if the space-intervals transpired at mid-word or in places they are least expected. If these space-intervals are structure signals, the *exemplar* or a scribal tradition is in view, but if these are not, then we are witnessing yet again another aspect of our scribe's copying habits being revealed. To isolate these copying habits is the methodological aim of this section. To do this, I shall limit my analysis to the text of Hebrews in $\mathfrak{P}^{46}$ for to the following empirical reasons: 1) the text of Hebrews is comparatively more complete than other epistles with reading marks (i.e., Rom, 1Cor, and Phil), and 2) the reading marks herein are more constant throughout than in others.[15]

But let us first explore the more obvious visual features present in $\mathfrak{P}^{46}$, and evaluate how they fared against the wider tradition of ancient book production vis-à-vis actual scribal use in other ancient manuscripts.

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[13] In preparing Appendix H, I was greatly helped by the accompanying Windows modules that are capable of magnifying digital images of $\mathfrak{P}^{46}$ many times over, making detection of slight space-intervals a lot manageable.

[14] Jongkind, *Scribal Habits of Codex Sinaiticus*, 95-96, proposed, as a methodology in studying paragraph structures in Codex Sinaiticus, that a paragraph is strictly demarcated by the following variables: "start of a new line, an extruded first letter, a space of at least several letters on a line, or a *paragraphus* above the first full line of the paragraph, space of at least several letters on a line, or a *paragraphus* above the first full line of the paragraph". However, this rigid methodology hardly works for $\mathfrak{P}^{46}$—the difference in literary circumstance between the two manuscripts is simply enormous.

[15] Nonetheless, for illustration purposes I will include at strategic junctures supplementary examples from the other epistles, especially those pertaining to physical matters, i.e., κολλήματα and papyrus strands.

II. PUNCTUATIONS AND OTHER VISUAL FEATURES IN $\mathfrak{P}^{46}$ AS "READER'S AIDS"[16]

David Trobisch, describing generally the paratextual features of $\mathfrak{P}^{46}$, stated, "Following the conventions of book publishing in antiquity, the text is written in capital letters and *without* spaces between the words. *Structuring signals like paragraphs, punctuation marks, or chapter headings are missing.*"[17] Obviously, Trobisch's remark reflects the view that subsidiary palaeographical matters, i.e., punctuation, accentuation, and the use of breathing marks, were rarely employed amongst ancient papyri.[18] But is this a fair assessment?[19] Or is this observation simply influenced by previous assessments (i.e., Kenyon and Sanders) about the sense-pauses in $\mathfrak{P}^{46}$? Whilst Trobisch's description is not totally untrue, there is imprecision in his assessment.

Contra Trobisch, there are in fact sporadic examples of structure signals scattered throughout $\mathfrak{P}^{46}$, although they are not as plentiful and systematically deployed as in later manuscripts (e.g., in the mould of Codex Sinaiticus). But this dearth of visual structural details, I think, simply points to the relatively early age of our papyrus. In fact, most of the earlier NT manuscripts are less elaborate and parsimonious in their use of punctuations, but it would be misleading to construe this parsimony as a lack of concern for clarity, especially in aid of the readers.[20] Turner's comment that the absence of actual

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[16] On the concept of scribal features as "aids to readers", see Roberts, *Manuscript, Society, and Belief*, 21-22; Metzger, *Text of the New Testament*, 21-31; Gamble, *Books and Readers*, 74; and Hurtado, *Earliest Christian Artifacts*, 177-85.

[17] David Trobisch, "Structural Markers in New Testament Manuscripts, with special attention to observations in Codex Boernerianus (G 012) and Papyrus 46 of the Letters of Paul," in *Lay-out Markers in Biblical Manuscripts and Ugaritic Tablets*, 177-190, p. 179. (Emphases added).

[18] On this, see, Kenyon, *Palaeography of Greek Papyri*, 25-26; and Thompson, *Introduction to Greek and Latin Palaeography*, 58-59.

[19] Actually, there is inconsistency in Trobisch's statement since he also remarked on the same article that "Some, like scribal errors and corrections, only affect the wording, but others like ornamentation at the end of a book or lines added to highlight titles, function as structural markers" (p. 181). Both the end-of-a-letter line ornamentations and title highlighters are present in $\mathfrak{P}^{46}$.

[20] On this point, see, Johnson, "The Ancient Book," 261-2. Aland, "Significance of the Beatty Papyri," 109, noted that in contrast with the literary documents, the presence of reading aids in the

punctuation marks had been compensated by other features derivable from the manuscripts themselves (particularly the presence of space-intervals and the elongated strokes of some portions of particular letters) is sufficient to convince us that manuscript readers had visual access to structure signals from *within* the manuscripts themselves. In fact, in the case of $\mathfrak{P}^{46}$, both Kenyon and Sanders have already made general remarks about these structure signals.[21] Hence, we only need to provide the specific details here and add those that they failed to note down for one reason or another.

Like many other earlier manuscripts, the text of $\mathfrak{P}^{46}$ was written in a single-column, *scriptio continua* format. Our scribe did not use periods or single dots in low position (.), comma (,), and question marks (;)—the more graphic punctuations usually present in later parchment manuscripts. There are no indented letters (*eistheses*), or protruding letters (*ektheses*),[22] although this latter device was already present in the Magdalen papyrus fragments of Matthew ($\mathfrak{P}^{64}$), dated to about 200 A.D.[23] There are

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earliest Christian papyri—such as accents, breathing marks, punctuations, marks to indicate foreign words, etc.—reflect more their function in the community, designed for reading in both worship services or private devotions.

[21] Kenyon, *CBBPIII-1936*, xiv, observed, "The letter ν at the end of a line is occasionally represented by a stroke above the preceding vowel. Line-filling marks are not used. Initial ι and υ are generally marked by a diaeresis, but with many exceptions. A square rough breathing occurs occasionally. There is very small amount of punctuation with a high dot by the original scribe, and in Romans, Hebrews, and the later chapters of 1 Corinthians readings marks (a rather thick, short, oblique stroke or dot) have been added by another hand, perhaps that which has inserted the page-numeration."

Sanders, *TCPC*, 16, added, "There are few cases of punctuation by a single dot in high position. I have counted less than a dozen that are certain in the Michigan portion. The number of doubtful cases is also small. In the Beatty portion a similar punctuation is mentioned as rare. On this point photographs are unreliable. Double dots like a colon occur once, near the end of Romans, after XVI, 27 and before XVI, 1. This may be interpreted as setting off the last chapter of Romans as a separate letter. I have no indications of a similar punctuation elsewhere in the manuscript."

[22] As mentioned in p. 110, n177, there are only two cases of line-beginning protrusions in $\mathfrak{P}^{46}$: $f37^{r}$-l^{22} and in $f54^{r}$-l^{07}.

[23] Porter, "Pericope Markers in NT Manuscripts," 164-70, also noted that the following papyri already employed *ekthesis*: $\mathfrak{P}^{4\ \&\ 67}$, $\mathfrak{P}^{15\ \&\ 16}$, $\mathfrak{P}^{66}$, $\mathfrak{P}^{71}$, $\mathfrak{P}^{75}$, $\mathfrak{P}^{77\ \&\ 103}$, $\mathfrak{P}^{88}$, and $\mathfrak{P}^{90}$. On its presence in $\mathfrak{P}^{64}$ and similar correspondence with codices Vaticanus, Alexandrinus, and Bezae in terms of starting a new section, C.H. Roberts, "An Early Papyrus of the First Gospel," *HTR* 46/4 (1953): 233-37, p. 234, suggested "This system of division can now be carried back a couple of centuries if our dating of the papyrus is correct." [For reviews of the date of $\mathfrak{P}^{64}$ (also $\mathfrak{P}^{4\ \&\ 67}$) in light of the earlier date proposed by C.P. Thiede, see David Parker, "Was Matthew Written before 50 CE? The Magdalen Papyrus of Matthew," *Expository*

however a few instances of *colons* or single dots in high position (·),[24] corresponding to breaks in sense at the clause levels. These high dots are marked with space-intervals also (either by one or two-letter length). Furthermore, there are two instances where a *dicolon* was used, and both are equally marked with space-intervals. Whilst the use of the *dicolon* can be traced back to as early as the 4th century BC, and in literary papyri functions to divide texts, to mark changes of speaker (in both dramatic texts and Platonic dialogues), and as a strong stop,[25] the instances in $\mathfrak{P}^{46}$ portray two different functions. The *dicolon* in Heb 11.5 seems to signal the following OT quotation, but the one at the end of the doxology in Romans 15.33 surely calls attention (especially with the presence of a three-letter space-gap) to the conspicuous relocation of the doxology (Fig. 3-4.2).[26]

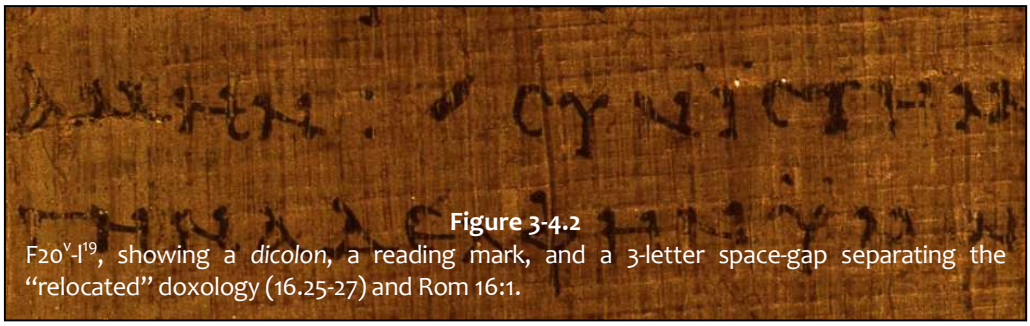


Figure 3-4.2
F20^{v}-I^{19}, showing a *dicolon*, a reading mark, and a 3-letter space-gap separating the "relocated" doxology (16.25-27) and Rom 16:1.

Another textual variation indicator in $\mathfrak{P}^{46}$ is the *ancora* (↑) [Fig. 3-4.3], which also gives away the scribe's level of astuteness whilst copying his *exemplar*. There are

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Times 107 (1995): 40-43; repr. in *Manuscripts, Texts, Theology*, 55-63; P.M. Head, "The Date of the Magdalene Papyrus of Matthew (P. Magd. Gk. 17 = $\mathfrak{P}^{64}$): A Response to C.P. Thiede," *Tyndale Bulletin* 46 (1995): 251–85].

[24] Rom 10.16; 11.36 (after αμην of the doxology of Chap 11); Heb 1.7 and 1.9 (both within OT quotations); 8.12; and 12.19.

[25] On these functions, see Turner, *GMAW*^{2}, 9.

[26] On the role of the *dicolon* in the "relocated" doxology and their wider implications to the production of the Letter to the Romans, see the differing opinions of Sanders, *TCPC*, 16-17, 35, and Kenyon, *CBBP-1936*, xviii. Interestingly, whilst Gamble, *Textual History of the Letter to the Romans*, 33, equally suggested that the doxology is set off from chapter 16, nonetheless, he only mentioned the "diagonal slash" (i.e., reading mark) and not the *dicolon*, which is the more important variation signal in this instance. See related discussion in pp. 263-65.

three passages marked with the *ancora*,[27] all in Hebrews, and all written upright, *within* the text area (not in the margin area as would be expected in the literary and non-literary papyri).[28] In all these, clusters of words were accidentally lost due to *homoioteleuton*: Heb 8.8;[29] 12.6;[30] and 9.14.[31] Therefore, the manuscript reader is alerted as to the loss of text—which was surprisingly left uncorrected by the scribe who inscribed this mark![32]

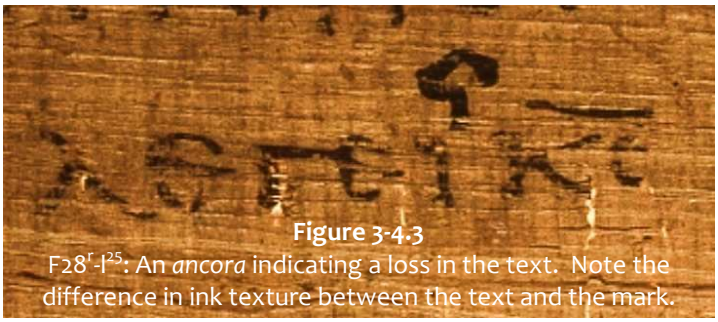


Figure 3-4.3
F28^{r}-I^{25}: An *ancora* indicating a loss in the text. Note the difference in ink texture between the text and the mark.

Apostrophes in $\mathfrak{P}^{46}$, some of which look like a grave accent and others like a smooth breathing mark, perform different functions. As an *elision marker*,[33] the scribe used it with the elided αλλα,[34] ουδε,[35] ουχι,[36] τουτο,[37] κατα,[38] and ποτε[39] (Fig. 3-4.4a,

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[27] Both Kenyon and Sanders printed in their main texts the *ancora* in Heb 12.6. However, they both failed to print (or misread) the other two occurrences of *ancora* in Heb 8.8 (misread as ς by both) and 9.14 (missed by Sanders, and noted only in the apparatus by Kenyon).

[28] See Turner, *GMAW*^{2}, 16.

[29] Its presence here signals the loss of λεγει ιδου ημερα ερχονται. Note, however, that the mark is not from the first hand due to difference its ink density; so is Kenyon, *CBBPIII-1934*, 34; and *DNTAP*$^{2.2}$, 292.

[30] This is from another hand as well, and signals the loss of παιδευει μαστιγοι δε παντα $\overline{υν}$ ον παραδεχεται εις παιδειαν υπομενετε ως υιοις υμειν προσφερεται ο $\overline{θς}$ τις γαρ υιος ον ου.

[31] The loss of $\overline{υω}$ καθαριει την συνειδησιν ημων απο νεκρων εργων εις το λατρευειν is signalled by the *ancora*. Kenyon, *CBBPIII-1936*, 36, suggested a second hand who added the *ancora* here; whilst Sanders, *TCPC*, 72, proposed a third hand. *DNTAP*$^{2.2}$, 302, whilst noting the *ancora*, made no suggestion.

[32] For the likelihood that the corrector who inscribed these *ancoras* did not restore the missing texts, see our discussion in Chapter Four, pp. 295, n23, 309.

[33] For the function of the apostrophe in elision contexts, see Turner, *GMAW*^{2}, 8.

[34] 1Cor 2.5 αλλ` εν; 1Cor 10.23 αλλ` ου (2x); Gal 4.23 αλλ`||ο; Gal 4.29 αλ` ωσπερ; Eph 2.19 αλ` εστε. The placement of the apostrophe in Heb 3.16 is quite interesting as it was placed in between the two *lambdas* of the elided form of the conjunction αλλα. It is possible, owing to the very slight space between these two consonants, that the scribe only originally copied the *alpha* and the *lambda*, hence the slight space, but then also added the second lambda eventually.

[35] Heb 9.18 ουθ` η; and 1Cor 14.21 ουδ`||ως.

next page). It also functions as an *accent* (alternatively called "hook"), specifically between consecutive (liquid) consonants[40] in the root κριττον[41] (Heb 3.16; 7.7; 8.6 [2x]; 11.35 κριττονος; 11.40 κριτ=||τον), and ηλαττωσας (Heb 2.7 [Fig. 3-4.4b]).[42] Finally, there are also three instances of an apostrophe appearing at the end of a proper name, perhaps due to their indeclinable endings (Rom 10.19 ισραηλ᾽ ουκ εγνω; Phil 3.5 ϊσραηλ᾽ φυλης;[43] and Gal 3.8 αβρααμ᾽ οτι[44] [Fig. 3-4.4c]).

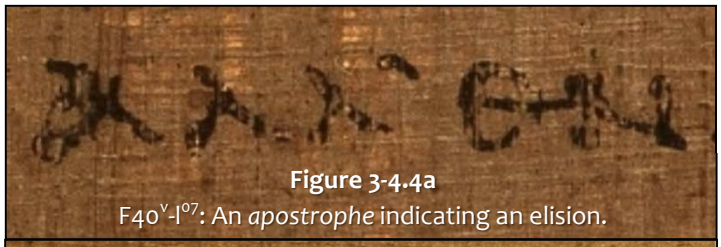


Figure 3-4.4a
F40^{v}-l^{07}: An *apostrophe* indicating an elision.

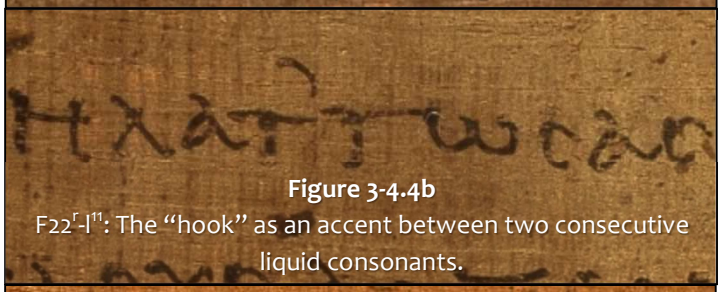


Figure 3-4.4b
F22^{r}-l^{11}: The "hook" as an accent between two consecutive liquid consonants.

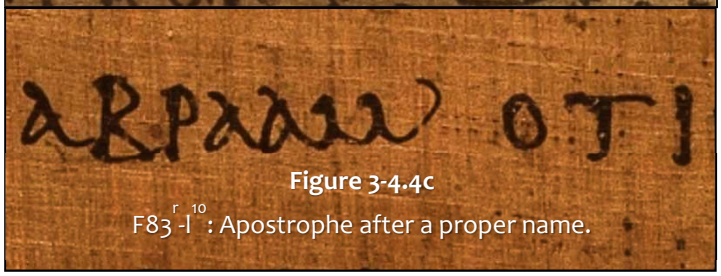


Figure 3-4.4c
F83^{r}-l^{10}: Apostrophe after a proper name.

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[36] 1Cor 7.28 ουχ᾽ ημαρτεν. For an observation of this particle's aspiration from a linguistic perspective, see Eleanor Dickey, "The Greek and Latin Languages in the Papyri," in *OHP*, 149-69, p. 153.

[37] Rom 10.7; Heb 11.16 and 13.15 τουτ᾽ εστιν.

[38] Gal 1.13 καθ᾽ υπερβολην.

[39] Gal 2.6 ποτ᾽ ησαν.

[40] However, ηττημα (1Cor 6.7) and ελαττονε- (2Cor 8.15; Heb 7.7) are unmarked.

[41] Although κριττον in Heb 1.4; 7.22; 9.23; 10.34; 11.16; 12.24; and 1Cor 7.9 are unmarked.

[42] Note also the case in Heb 3.16 αλ᾽λ ου παντες. Interestingly, Turner, *GMAW*^{2}, 11, 19, 108, believed that the separating apostrophe between double consonants within a word is suggestive of a manuscript's production date later than A.D. 200; also Johnson, "Ancient Book," 262; but cf. David G. Martinez, "The Papyri and Early Christianity," in *OHP*, 590-622, pp. 599, 613, n.49.

[43] Turner, *GMAW*^{2}, 108, also noted the presence of this word-end apostrophe with other words but equally found it difficult to account for its function in this context.

[44] Thompson, *Greek and Latin Palaeography*, 62, noted that αβρααμ receives an apostrophe in some manuscripts since it is a "name not having a Greek termination".

Sanders found only one instance of an *accented* word (Heb 6.16 πέρας). But to this we can now add four more: Eph 5.9 αληθειά (Fig. 3-4.5); Heb 10.16 αύτη;[45] Heb 9.24 προσώπου$^{/\omega}$;[46] and 1Cor 10.15 ώς.[47] All these have acute accents.

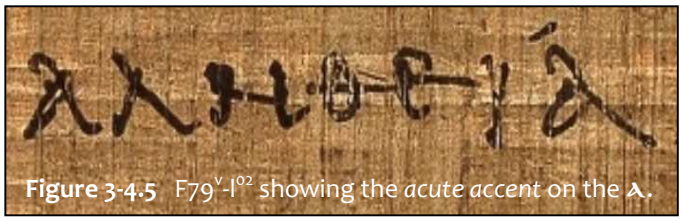


Figure 3-4.5 F79^{v}-I^{02} showing the *acute accent* on the ⲁ.

Turner mentions two major functions of the *diaeresis*: 1) "organically", it separates vowels in a cluster that do not belong together, and 2) "inorganically", to mark off an initial vowel or a final vowel.[48] In our manuscript, *diaeresis* by and large has an *inorganic* function, for initial ι and υ[49] are generally marked with a *diaeresis*, although with many exceptions. Medial *iota* with a diaeresis occurs also in thirty cases, mostly dealing with the word υϊος,[50] and twice with υϊοθεσιας;[51] and thrice with χοϊκος.[52] This may be due to the fact that these are vowels forming a diphthong. But we have also found non-

[45] This particular instance can also be interpreted as an unusually formed rough breathing mark, as in the case of ώς of 1Cor 10.15. *DNTAP*[2.1] suggested a reading mark but that is very unlikely as the ink density is very similar with that of the text.

[46] The first hand copied προσωπου at first, but *in scribendo* wrote *omega* above the *omicron* and *upsilon* and also added what appears to be an acute accent on the first *omega*, to read προσώπω.

[47] Surprisingly, whilst the ink mark is quite obvious (both in the actual manuscript and the plate), this has not been noted yet in any of the transcriptions of 𝔓^{46}. It is likely, though uncertain, that in its present context, the accent functions more of a rough breathing marker.

[48] Turner, *GMAW*[2], 10; cf. Thompson, *Greek and Latin Palaeography*, 63. See also, Bell and Skeat, *Fragment of an Unknown Gospel*, 4-5, where they distinguished the "original" and "extended" usages of the diaeresis over certain vowels.

[49] 41 cases in Rom; 55 in Heb; 121 in 1Cor; 116 in 2Cor; 36 in Eph; 28 in Gal; 33 in Phil; and 29 in Col. None is recorded from 1Thess because of its highly fragmentary state.

[50] But note that in Rom 9.26, involving this word, the diaeresis seems to be on top of the initial *upsilon* rather than the *iota*—an example also noted by Johnson, "Ancient Book," 262.

[51] Rom 8.15 and Eph 1.5.

[52] 1Cor 15.47, 48, 49. Intriguingly, αχαια in 2Cor 9.2 was copied as αχαϊνα with the diaeresis on the medial *iota*. But the word is a line-end word and runs through the following line with the *iota* as the first letter (i.e., αχα‖ϊνα). As such, it may have been that the scribe misconstrued it to be the conjunction ινα which usually receives a diaeresis; cf. the mid-line αχαϊα in Rom 15.26 which is marked.

diphthong instances--four times with proper names;[53] six times with the root -ιστημι prefixed with combining prepositions;[54] once with συνϊδησεσιν (2Cor 5.11); and once with ανϊεντες (Eph 6.9). On the other hand, there is a single instance only of medial *upsilon* with *diaeresis* (Heb 2.8 ανϋποτακτον). Words beginning with υμ- or υπ-[55] mostly received *diaeresis*, due perhaps to the high occurrence frequency of words beginning with these combinations.[56] Note, however, that although the beginning combination υσ- occurs 16 times, only one has a *diaeresis* (Phil 2.30 ϋστερημα), and whilst the combination υψ- has 9 extant cases none is marked. Intriguingly, the lone occurrence of the combination υε- is marked (Heb 6.7 ϋετον), and the combination υδ-, which has 3 extant instances, are marked in two cases (Heb 10.22 ϋδατι and Eph 5.26 ϋδατος; Heb 9.19 is the anomaly). Another interesting case is that of Gal 2.14 where a single word had two diaereses (i.e., ϊουδαϊζειν).[57] Cumulatively, such irregularities in application seem to point more to the caprices of the scribe of $\mathfrak{P}^{46}$ than its *exemplar*.

There are no examples of *iota* adscripts or subscripts in $\mathfrak{P}^{46}$. However, *rough breathing marks*, mostly in the form of half an H ῾, appear at various places in the manuscript (see Fig. 3-4.6, next page). Sanders noted twelve instances;[58] I have identified over a dozen more however. All these occur with monosyllabic words--as rightly observed by Colwell:[59] once with a definite article,[60] once with a conjunction,[61]

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[53] ησαϊας (Rom 9.29), αχαϊα (Rom 15.26), γαϊον (1Cor 1.14), and αχαϊκου (1Cor 16.17).

[54] προϊσταμε᾽ν[ος] (Rom 12.8), συνϊστημι (Rom 16.1); συνϊσταν (2Cor 3.1); συνϊστανομεν (2Cor 5.12); συνϊσταντων (2Cor 10.12); συνϊστασθαι (2Cor 12.11); συνϊστανω (Gal 2.18).

[55] Note however that the elided υφ, of which we have 5 extant cases (1Cor 4.3; 2Cor 3.3; 8.19; 8.20; and 12.11), receives the diaeresis in only one instance (2Cor 3.3).

[56] The following initial combinations do not appear in $\mathfrak{P}^{46}$: υα-, υγ-, υθ-, υκ-, υλ-, υν-, υξ-, υο-, υρ-, υτ-, and υω-.

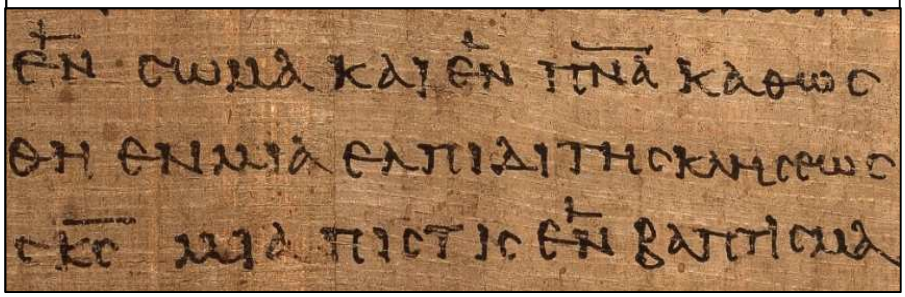
[57] It may be argued that ϊουδαϊζειν is a case of a diphthong at mid-word. However, this does not seem to be the case as in Gal 1.14 ($f81^{v}$-l^{10}) only the initial *iota* in ϊουδαισμω has the diaeresis (the medial *iota* of the same word in Gal 1.13 is a correction, thus, not helpful for comparison).

[58] Sanders, *TCPC*, 19; see also, Colwell, Review of Henry Sanders, 386.

[59] Colwell, Review of Sanders, 386.

seventeen times with the numeral one,[62] and six times with relative pronouns.[63] Whilst the use of this device is not strictly constant throughout the manuscript, its presence in the instances mentioned might have been intended by the scribe to discriminate them from similar words that should be read with smooth breathing,[64] i.e., the numeral ἑν vs. the preposition εν; ἑις vs. preposition εις; relative ὁυ vs. the negator ου, etc.

Figure 3-4.6 F78^{r}–II$^{03, 05}$: The "half H" rough breathing marks.



Abbreviations are not utilised in 𝔓^{46} merely for space-saving purposes; their functionality rests on the kind of abbreviation the scribe made. Abbreviations were used for nine *nomina sacra*—words usually treated with special theological implications—not only by way of contraction but also by putting a supralinear bar on the contracted letters.[65] Hence, it provides a ready signal to the reader that the contracted letters in focus have special function and should not be read verbatim. Apart from the *nomina sacra* other abbreviations in 𝔓^{46} include a sole occurrence of

[60] Rom 11.18 αλ ἡ ριζα σε.

[61] 1Cor 10.15 ὡς φρονιμοις.

[62] Rom 12.5 ἑν σωμα εσμεν εῃ [χω̄]; 1Cor 6.16 ἑν σωμα; 10.17 ἑν σωμα; 11.5 ἑν γαρ εστιν; 12.13 ἑν πνα̅; 12.14, 26 ἑν μελος; Eph 2.14 τα αμφοτερα ἑν; 2.15 ἑνα κοινον; 4.4 (2x) ἑν σωμα και ἑν πνα̅; 4.5 ἑν βαπτισμα; Phil 2.2 ἑν φρονουν||τες; 1Cor 4.6 ἱνα μη εἱς υπερ του ενος; 10.17 εἱς αρτος; 1Cor 9.24 εἱς δε λαμβανει; and Gal 3.20 ο δε θ̅ς̅ ἑις εστιν.

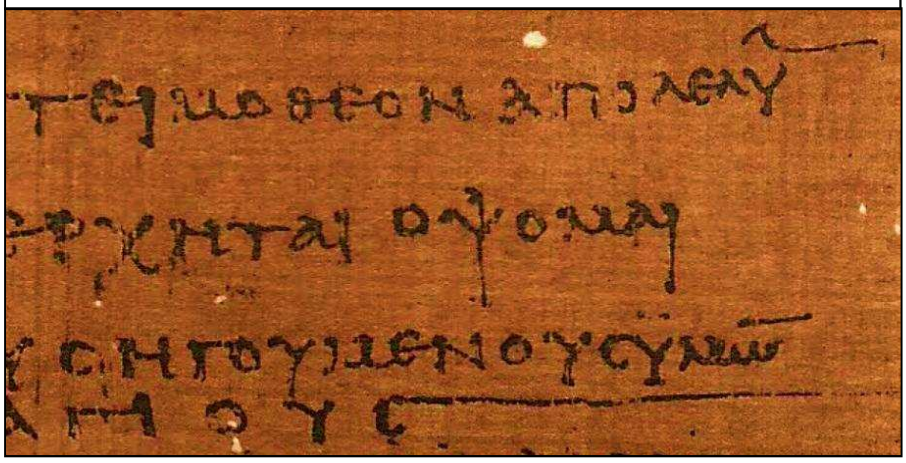
[63] Rom 14.5 ὁς δε||[κρι]ῃ̣ει; Heb 12.14 ὁυ χωρις; 2Cor 10.13 ὁύ εμε||ρισεν; Eph 3.27 ὁυ εγενηθην; Col 2.12 εν ὡ και συνηγερθητε; Col 2.17 ἁ εστιν σκεια.

[64] For this reason, Colwell, Review of Sanders, 386, suggested that 𝔓^{46}'s use of the rough breathing mark is "true to the general usage of his time".

[65] For fuller discussion of the *nomina sacra* in 𝔓^{46}, see pp. 323-66, esp. 330-34.

και-compendium[66] and sporadic cases of line-end final ν. Interestingly, there is also a lone instance where a long line-end word was abbreviated by our scribe, at the end of Hebrews (Fig. 3-4.7). This implies that our scribe was also familiar with the convention of abbreviation.

Figure 3-4.7 F38^{v}-I^{09}: The abbreviated form of απολελυμενων. Note also the abbreviated final ν in I^{11} and a line-filler in I^{12}.



There are also παραγραφοι in 𝔓^{46}, filling portions of a line,[67] or the whole of it.[68] Three are forked παραγραφοι (f74^{v} [2Cor], f81^{r} [Eph], f90^{r} [Phil]) whilst the rest are straightforward horizontal lines. Whilst in general this feature aesthetically functions to facilitate ease of reading, it was typically used to mark-off major breaks in the text amongst the (non-)literary manuscripts.[69] Specifically, however, in 𝔓^{46}, παραγραφος was used by the scribe to strictly mark the transition from one epistle to

[66] Strictly speaking, the lone occurrence of a και compendium (ϗ) is not from the first hand but belongs to an unidentifiable corrector, who inserted it in f28^{v}-I^{11} (Heb 7.25).

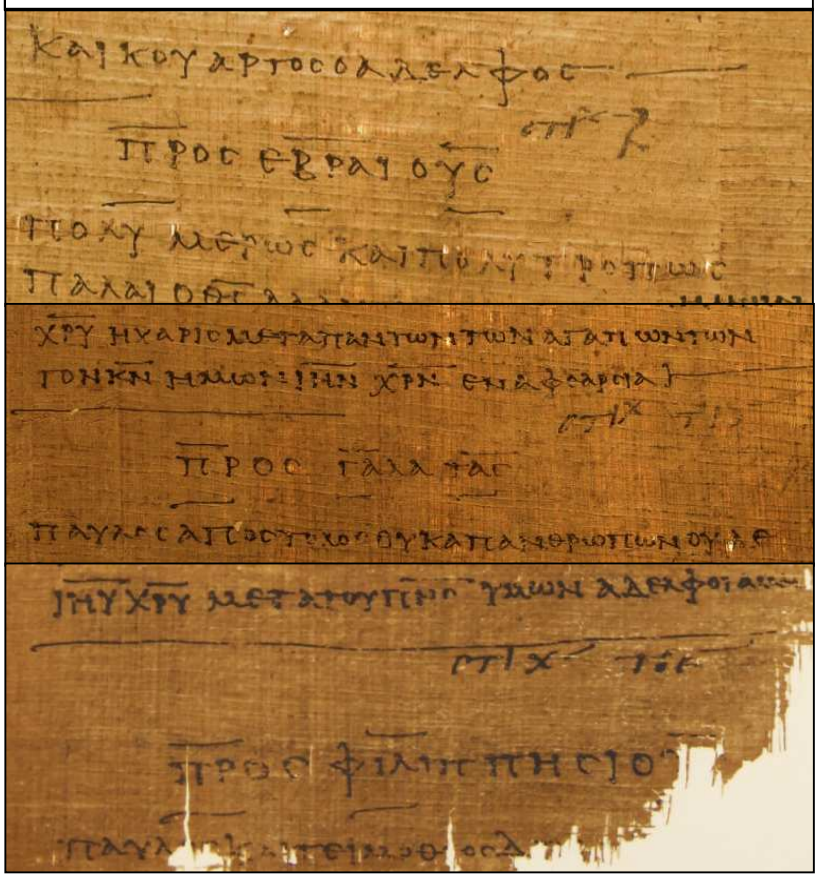
[67] Thus, f21^{r} (Rom and Heb); f38^{v} (Heb and 1Cor); f74^{v} (with the *subscriptio* of 2Cor); f81^{r} (Eph and Gal); and f94^{r} (Col and 1Thess).

[68] Thus, f86^{r} (for Gal and Phil) and f90^{r} (for Phil and Col).

[69] Thompson, *Greek and Latin Palaeography*, 58. See also, Turner, GMAW^{2}, 8, 12-13; and Francesca Schironi, *ΤΟ ΜΕΓΑ ΒΙΒΛΙΟΝ: Book-Ends, End-Titles, and Coronides in Papyri with Hexametric Poetry* (Durham, NC: ASP, 2010), 10, 16-20. On how the *paragraphus* might have functioned and aided reader-speaker in a public address in antiquities, see, W.A. Johnson, "The Function of the Paragraphus in Greek Prose Texts," *ZPE* 100 (1994): 65-68; Idem, "The Ancient Book," 261.

another,[70] as all the seven extant παραγραφοι (with the exception of the *subscriptio* of 2Cor)[71] are placed immediately between the last line of an epistle and the τιτλος line of the ensuing letter (Fig. 3-4.8).

Figure 3-4.8 F21^{r}, f81^{r}, and f86^{r} showing παραγραφοι of various lengths.



As with the παραγραφος, our scribe also used *line fillers* in a number of instances, but most commonly to close extra spaces of the last line of an epistle before the παραγραφος line. This is true for f21^{r} (Rom), f38^{v} (Heb), f74^{v} (2Cor), f81^{r} (Eph), and f90^{r}

[70] Very few extant NT papyri with book transition pages have survived. But those extant seem to share the same παραγραφος function as that of 𝔓^{46}. For instance, 𝔓^{74} (f68^{v}) and 𝔓^{75} (f42^{v}) all have forked paragraphoi at the end of the last line of a certain book/epistle.

[71] The bottom portion of f60^{v}, which may have contained the *subscriptio* of 1Cor, is broken already.

(Phil). The lone anomaly, however, is f57^{r}-l^{18} (1Cor 15.2 [Fig. 3-4.9]) where the device indicates scribal doubt as to the text in the *exemplar*.[72]

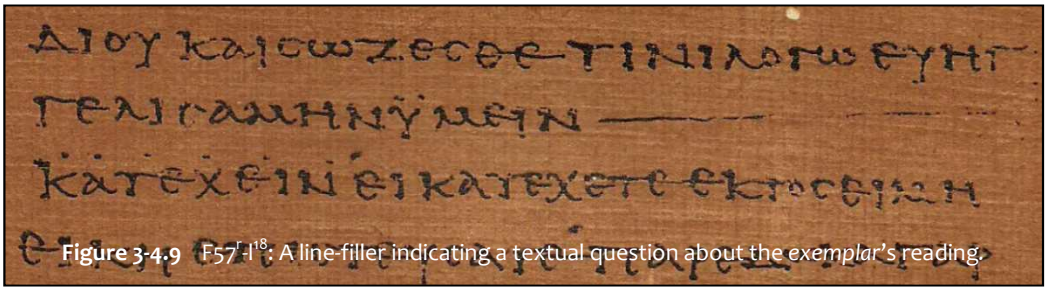


Figure 3-4.9 F57^{r}-l^{18}: A line-filler indicating a textual question about the *exemplar*'s reading.

Finally, each epistle is aesthetically introduced with an ornamented τιτλος. Book titles for the epistles, except for Rom, are fully (Heb, 1Cor, 2Cor, Eph, Gal, and Col) or partly extant (Phil and 1Thess), generally set centre-justified with three[73] horizontal strokes[74] above and below certain letters[75] (Fig. 3-4.10).[76] They are written in black ink also unmistakably similar to that of the main text,[77] indicating that these book titles are part of the main hand's original work and not added later.[78] More importantly, the presence of these τιτλοι also indicates some level of consciousness about the kind of recognition, already arrived at during this stage of NT textual

[72] On this, see Zuntz, *TEDCP*, 254-55.

[73] But note that Eph only has two whilst 2Cor has four. Whilst 1Cor has three, the second horizontal pairs are not on the first letter of book but on the mid-portion. Note further that 1Thess most likely does not have the horizontal lines.

[74] The horizontal strokes must have been written immediately *after* the whole title had been completely written, with upper stroke first then the lower.

[75] It seems that the general pattern is to mark the first letter of the preposition, i.e., ΠΡΟC, then the first and last letters of the book name or the book sequence as in the Corinthian letters (i.e., Α and Β); exception to this is Eph with only the first letter of ΠΡΟC and the last letter of the book name.

[76] At the end of 2Cor, the *subscriptio* has survived ([Π]ΡΟC ΚΟΡΙΝΘΙΟΥC Β) set similarly with that of its τιτλος, except that the Β was placed underneath. It also has the ornamental horizontal lines. Because the Β has survived, it may be presumed that 1Cor originally had a *subscriptio*, too, but has now eroded.

[77] It should be noted that as the book trade developed, τιτλοι were soon more elaborately presented as well, including writing them with a more colourful ink and in bigger sizes; see, Metzger, *Manuscripts of the Greek Bible*, 17; see also, Jongkind, *Scribal Habits of Codex Sinaiticus*, 37. For a list of coloured titles in the illuminated manuscripts, including biblical manuscripts, see Paul Binski and Patrick Zutshi, Western Illuminated Manuscripts: A Catalogue of the Collection in Cambridge University Library; with the collaboration of Stella Panayotova (Cambridge: CUP, 2011).

[78] On top of this, there is also no palaeographical reason to conclude otherwise.

transmission (at least in this region of Egypt), attached to each letter in regard to their perceived original intended recipients,[79] for there are no observable indications that these were affixed with an iota of doubt.

Pages with τιτλος (i.e., transition pages) are optically unmistakable as they also include other equally conspicuous visual elements: παραγραφος, στιχος notation, page numeration, and the text. Compared with other later manuscripts, titles in $\mathfrak{P}^{46}$ are very short and straightforward, and perhaps may also be an additional pointer to the early age of our codex. For instance, the first extant title ⲡⲣⲟⲥ ⲉⲃⲣⲁⲓⲟⲩⲥ (*cum* ℵABIKΨ 0142 0150 0151) has 6 other variant forms in the manuscript tradition, the longest of which is read by 0243 (10th century): εγραφη απο Ιταλιας δια Τιμοθεου η προς Εβραιους επιστολη εκτεθεισα ως εν πινακι.[80]

In terms of dimension, most of the titles are written between 0.3-0.4 cm,[81] much like that of the main text, at least 1.0 cm above the first line of the book and 1.0-2.0 cm from the left text margin.[82]

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[79] This point takes special significance when viewed in the context of the debate as to the recipient of Ephesians. Whilst it is true that the text does not have εν εφεσω in 1.1 (*cum* ℵ* B* 6 424c 1739) and whilst Marcion has "*ad Laodicenses*", it is equally true that our scribe (and most likely in his *exemplar*, too) recognized that this letter had a specific original recipient: ⲡⲣⲟⲥ ⲉⲫⲉⲥⲓⲟⲩⲥ—another point deserving an independent research of its own, if only to satisfy the question of the original audience of Ephesians, from the standpoint of the earliest manuscript witness.

[80] *DNTAP*[2.2], 243, also recorded the following titles: Παυλου επιστολη προς Εβραιους (P, 9th century); Παυλου αποστολου επιστολη προς Εβραιους (0278, 9th century); του αγιου και πανευφημου αποστολου Παυλου επιστολη προς Εβραιους (L, 8th century); and του αυτου θεωδωρι του ερμηνεια εις την προς Εβραιους επιστολην (075, 10th century). Hence, it may be inferred from this list that the further away the manuscript is (to the actual event), the longer the title becomes.

[81] In this regard, the basis of measurement is the first letter of the preposition, on the assumption that this is the visual guide of the scribe in terms of setting the whole title. For a complete measurement profile of all the titles, see Appendix G.

[82] Interestingly, whilst the length of the Galatians' title is not the shortest, the distance of its τιτλος from the left text margin area is 2.6 cm.

The longest τιτλος, in terms of length, is that of Phil at 8.6 cm, which happens to have the most number of extant characters as well at 16;[83] whilst the shortest is that of Eph at 4.9 cm, with 12 characters.

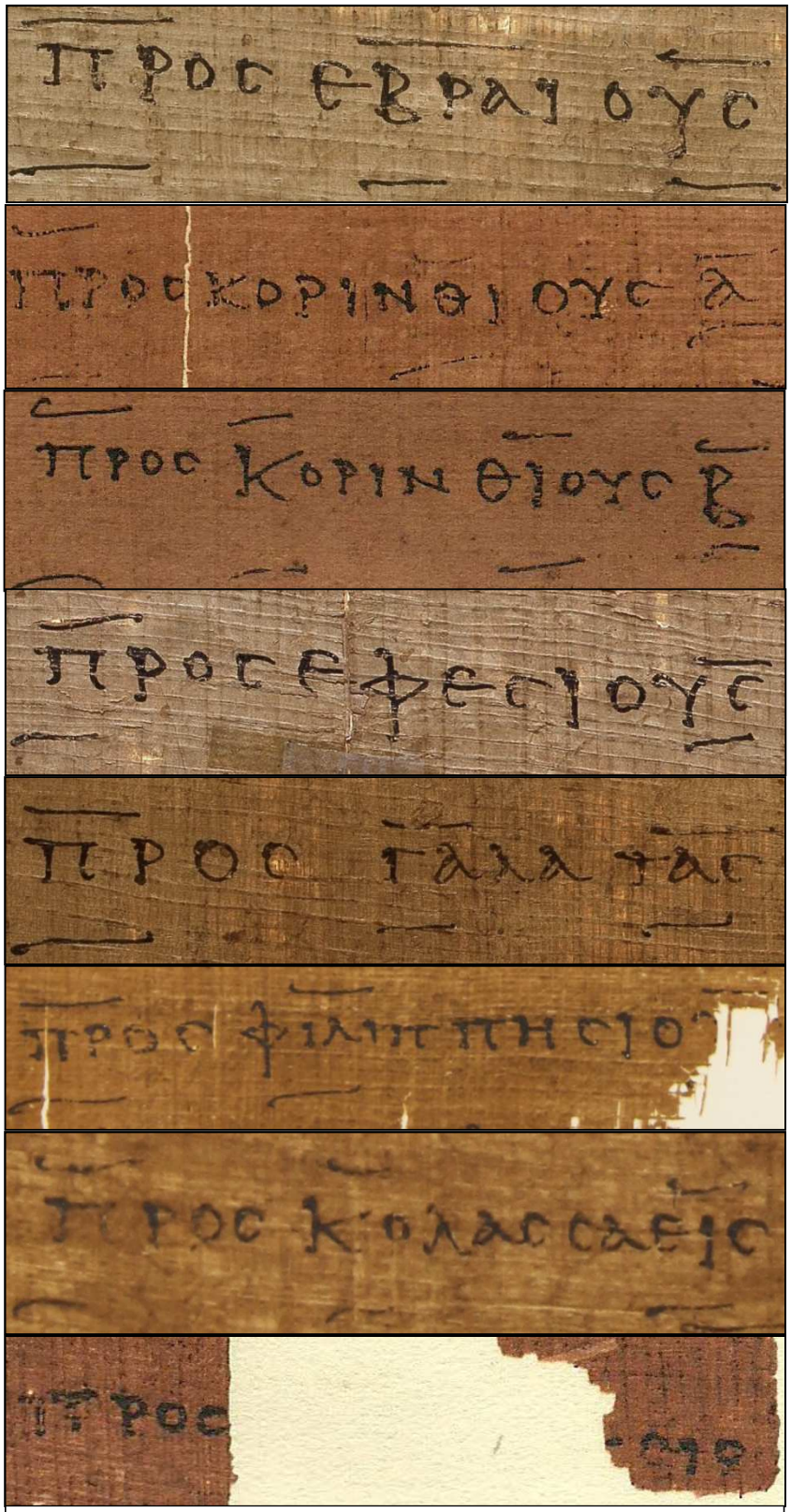


Figure 3-4.10 Collage of all extant book titles

[83] 1Thess has the most number at 19, but only 8 are extant.

Cumulatively, these *visual* features[84] are for the benefits of the readers, helping facilitate easier comprehension processing of what the text is all about. Some of them accentuate the existing textual tradition to which our scribe subscribed; others betray the conventions of the scribal trade; still others show the scribe's own facility in using them.

III. SPACE-INTERVALS AS "STRUCTURE SIGNALS" FOR READERS

We now turn to the use of space-intervals in $\mathfrak{P}^{46}$—a feature that is strictly non-textual but equally functioned to aid readers visually. Three derivable functions of space-intervals are examined in this sub-section: 1) as aesthetic signals, 2) as punctuation signals, and 3) as grammatical signals.[85]

A. As Aesthetic Signals

1. For Nomina Sacra

The most consistent application of space-intervals in $\mathfrak{P}^{46}$ concerns instances with *nomina sacra*, of which 128 cases are extant in Hebrews alone. Both Sanders and Kenyon have correctly recognized this phenomenon. They, however, noted only the space-intervals *after* the divine contractions,[86] ignoring equally frequent space-gaps *before* them. As a matter of fact, space-intervals, enough for one or more letters, occur *before* (almost always) and *after* (always) a *nomen sacrum* (Fig. 3-4.11, next page). For the former, the article preceding a *nomen sacrum* (unless a line-end case) is almost always marked with space-intervals, perhaps due in part to the apparent tendency of

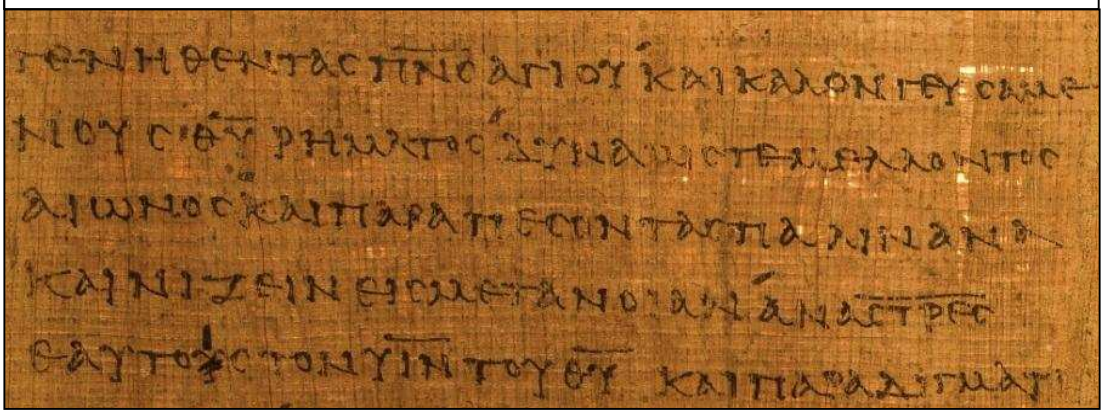
[84] Since my aim in this section is to profile the habits of our scribe insofar as his use of visual features is concerned, I have deliberately excluded other visual features, such as the page numeration and *στιχοι*, as they are not the original properties of our scribe.

[85] Analyses and figures herein are derived from a marked-up transcription of the text of Hebrews containing all the identifiable space-gaps and where they transpire; see Appendix H.

[86] Sanders, *TCPC*, 17, "An abbreviation regularly obtained a small space *after* it, whether there was a sense pause or not"; Kenyon, *CBBPIII-1936*, xiv, "Prof. Sanders has indicated a large number of such intervals, but most of them have, I think, no significance.… (Some are due) to the scribe's habit of leaving a slight space *after* an abbreviation…" (Emphases added).

our scribe to put a space before articular phrases (see related note below). Accordingly, however, even when a *nomen sacrum* is anarthrous[87] the space-gap is still distinguishable most of the time.

Figure 3-4.11 F26^{v}: small and big space-intervals in I$^{01, 02, 04, 05}$, before and after *nomina sacra.*



For post-*nomen sacrum* space-gaps, the only exception is at line-ends. This might have been primarily occasioned by the writing of the crossbar after the contraction has been written down—the gap seems to be a natural consequence once the scribe lifted his pen to draw the superscript line, which most of the times also extends to the still vacant space, before the following word or group of words is written. Consequently, the resulting space-gaps might have functioned as a sort of visually reinforcing the crossbar in setting off the contracted word, facilitating easier comprehension of the coded abbreviation, and therefore ease of reading considering that $\mathfrak{P}^{46}$ is a *scriptio continua* manuscript.[88]

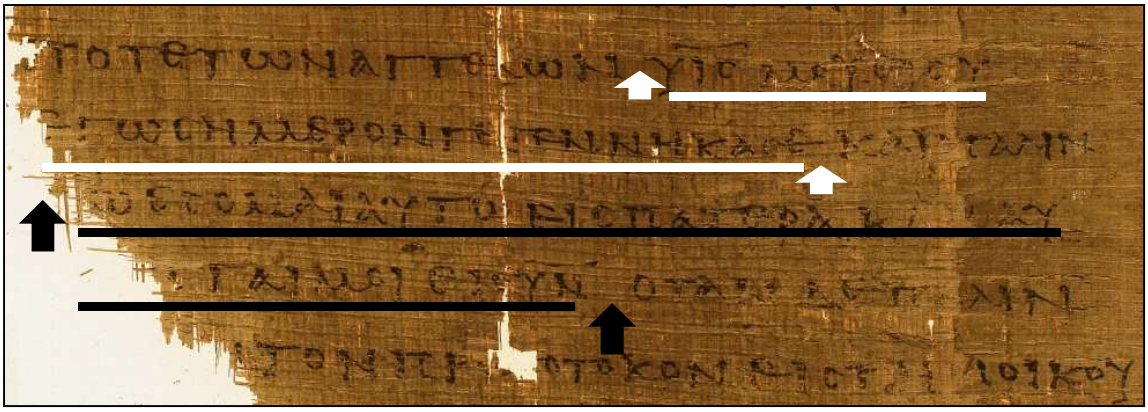
[87] Anarthrous *nomina sacra*, with preceding space-intervals, include Heb 1.6, 10, 14; 2.6, 9; 3.1, 6; 4.12; 6.1, 4, 5, 20; 7.21; 8.10, 11; 12.23, 24; 13.6, 21.

[88] On the possible function of the crossbar in *nomina sacra*, see A.H.R.E. Paap, *Nomina Sacra in the Greek Papyri of the First Five Centuries A.D.: The Sources and Some Deductions* (Leiden: Brill, 1959), 124. See also, Hurtado, *Earliest Christian Artifacts*, 112-117.

2. *For Old Testament quotations*

OT quotations in UBS-GNT[89] are easily distinguishable even by non-expert users, for they are indented and beautifully set off in boldface. Such a format is decidedly useful to readers. Conversely, this is not the case for our earliest surviving manuscripts, including $\mathfrak{P}^{46}$. As previously mentioned there are no traces of indentations in $\mathfrak{P}^{46}$, nor does it start with a new line to signal OT quotations, as in our modern printed Greek texts—perhaps codices Sinaiticus and Vaticanus come closer to that. Despite this, however, there are remarkably distinguishable space-intervals in $\mathfrak{P}^{46}$ in most cases where OT quotations begin and end (Fig. 3-4.12).

Figure 3-4.12 F21^{r}: Two OT quotations (first marked with white and second marked with black). Note the space-intervals, both at the beginnings and endings, in ll$^{19\text{-}20}$ and ll$^{20\text{-}21}$.



Of the around 50 OT quotations in UBS-GNT, only 40 quote beginnings are extant in $\mathfrak{P}^{46}$. Of this number, one is a line-end scenario (Heb 1.5, f21^{r}-l^{20}), another involves a *homoioteleuton* (8.8, f28^{r}-l^{25}), and apart from five[90] that are unmarked, all the rest have a one-letter or two space-intervals before the first word of the quotation. Furthermore, at

[89] OT quotes in NA are also indented but set-off in italicised format (instead of boldface).

[90] The absence of space-intervals in 1.10 (f22^{v}-l^{07}) and 2.13 (f23^{v}-l^{04}) might have been due to the proximity of the other quotations before and after these particular quotes. On the other hand, I can offer no possible explanation for the absences in 10.5 (f31^{v}-l^{20}), 37 (f32^{r}-l^{17}), and 11.21 (f34^{v}-l^{09}).

least seven instances are also prefaced with reading marks.[91] On the other hand, only 37 quote endings now survive in $\mathfrak{P}^{46}$. Of these, five are line-end cases,[92] and apart from 6.14 ($f26^{r}$-l^{04}),[93] all the rest are marked with space-intervals. Furthermore, eighteen of these also have reading marks, including all the line-end cases.[94]

Obviously, there is no exact one-to-one correspondence between all space-intervals at quote beginnings and quote endings and the reading marks—perhaps that should be expected of $\mathfrak{P}^{46}$, as the scribe (reader) who added the reading marks at a later time apparently has used the device selectively, for his own purposes. The point, nonetheless, can still be made that this limited correspondence is not a coincidence, but rather a systemic evidence of a *prevailing tradition* that was meant to help read sacred manuscripts more meaningfully in public (liturgical) contexts.

The presence of space-intervals around the immediate regions of a *nomen sacrum* and OT quotation cannot be hastily attributed to our scribe's peculiarity, or a product of his own artistic invention. In fact, both these phenomena are widely shared amongst ancient manuscripts, even by those earlier than $\mathfrak{P}^{46}$ itself. It is very likely that this is already reflected in the *exemplar*, which our scribe willingly reflected in his codex. As such, these features are a tenacious vestige of a wide-spread scribal convention than a particular scribe's copying tendencies.

B. As Punctuation Signals

In the absence of systematic use of punctuations, space-intervals may usurp the function of a sense-division marker at various levels. Whilst this has already been

[91] Heb 2.6 ($f22^{r}$-l^{09}); 3.15 ($f24^{v}$-l^{04}); 4.3 ($f24^{v}$-l^{23}), 4.7 ($f24^{r}$-l^{08}); 6.13, 14 ($f26^{r}$-$l^{01, 02}$); and 12.5 ($f35^{r}$-l^{13}).

[92] Heb 1.12 ($f22^{v}$-l^{13}); 3.11 ($f23^{r}$-l^{22}); 11.5 ($f33^{v}$-l^{08}), 21 ($f34^{v}$-l^{10}); and 12.26 ($f36^{r}$-l^{18}).

[93] It is possible that the scribe no longer placed a space-interval here for two reasons: first, σε was not part of the quote, but was the last word in this sentence, and second, the following sentence starts with the conjunction και. Note also that a reading mark is placed after the word σε.

[94] Heb 2.8, 13; 3.15; 4.5, 7; 5.6; 7.1, 17, 21; 10.9, 17, 30, 38; 11.5 (2x), 18, 21; and 12.26.

essentially confirmed in the studies on the other major NT manuscripts,[95] we have yet to see similar in-depth analysis for $\mathfrak{P}^{46}$. Hence, probing on how space-intervals in $\mathfrak{P}^{46}$ might have functioned as punctuation signals now deserves to be pursued. Our method here is quite straightforward, which only requires profiling the degree of (dis)agreements of $\mathfrak{P}^{46}$ vis-à-vis other papyri with the text of Hebrews, as well as the reading marks integral with $\mathfrak{P}^{46}$ itself, in order to probe the implications of such correlation.[96] To further check the correlation,[97] we will also profile $\mathfrak{P}^{46}$'s (dis)agreement against the common texts of NA[28] and UBS[4], on the assumption that these two Greek text editions are equally cognizant of the Greek structuring practices derivable from the manuscript tradition.[98]

1. *The Reading Marks in $\mathfrak{P}^{46}$*

Strictly speaking, the reading marks in $\mathfrak{P}^{46}$ are not the original property of the first hand.[99] However, its collation here against the space-intervals of the text of $\mathfrak{P}^{46}$ may be justified on grounds that these marks give us a documented historical window as to how the original lay-out of $\mathfrak{P}^{46}$ was immediately construed by its intended users,[100] for reading marks were an aid for public reading.[101] Reading marks abound in Hebrews

[95] For instance, Henry Sanders, *The New Testament Manuscripts in the Freer Collection, Part I: The Washington Manuscript of the Four Gospels* (New York: Macmillan, 1912), 12-4; Roberts, "An Early Papyrus of the First Gospel," 234; Parker, *Codex Bezae*, 31-34, 73-96. Also, see Hurtado, *Earliest Christian Artifacts*, 180-82, for other studies on this account.

[96] In investigating the degree of agreement-disagreement of $\mathfrak{P}^{46}$ vis-à-vis these papyri, I have relied on the cumulative data provided by *DNTAP*[2.2], Comfort and Barrett, and Karl Jaroš, *Das Neue Testament nach den ältesten griechischen Handschriften*, adjusting them as necessary.

[97] This is necessary since, apart from $\mathfrak{P}^{46}$, surviving papyri with Hebrews are not only few but also very fragmentary: 3rd century ($\mathfrak{P}^{114}$), 3rd-to-4th ($\mathfrak{P}^{12}$, $\mathfrak{P}^{13}$), 4th ($\mathfrak{P}^{17}$, $\mathfrak{P}^{89}$, $\mathfrak{P}^{126}$), and 6th-to-7th ($\mathfrak{P}^{79}$, $\mathfrak{P}^{116}$).

[98] On this, see relevant explanations in NA[26], 44*, and NA[27], 46*.

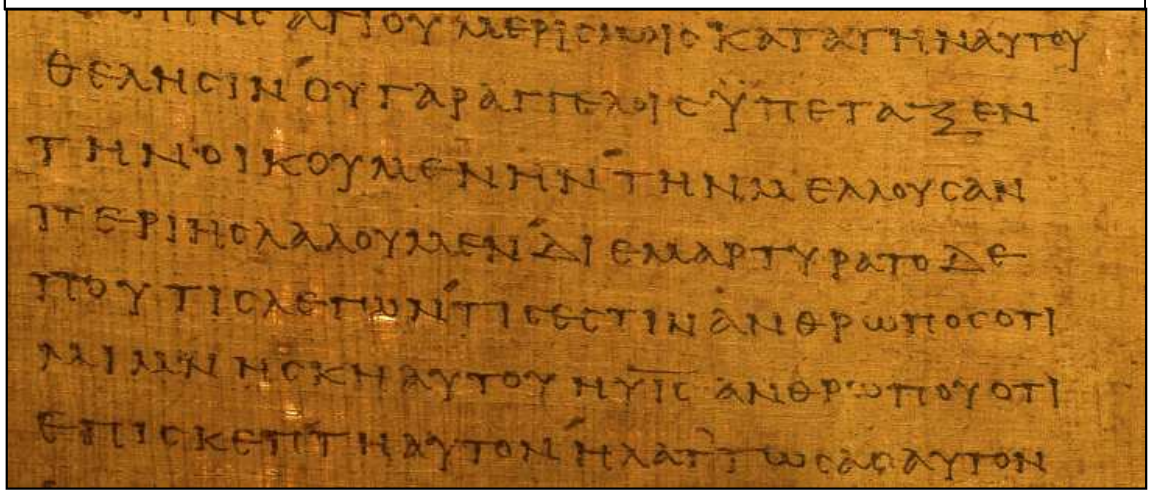
[99] See Kenyon, *CBBPIII-1936*, xiv; and Sanders, *TCPC*, 17.

[100] On the possible use of reading marks in the context of public reading or dramatic delivery in antiquities, see Turner, *GMAW*[2], 144.

[101] On the conduct of public reading in Christian antiquities, see Gamble, *Books and Readers*, 203-31.

(see Fig. 3-4.13), although there are intermittent occurrences in Romans, 1 Corinthians and Philippians also.

Figure 3-4.13 F22^{r} showing series of reading marks at various points. Note also the slight (some very slight) space-intervals where these reading marks are situated.



Kenyon recorded 350 reading marks in the 34 folios that the text of Hebrews was copied onto, whilst Sanders (in its still fragmentary state) noted 212 cases.[102] *DNTAP*$^{2.2}$ documented 369 instances, whilst Comfort and Barrett have 356. However, I have recorded a total of 392 reading marks in Hebrews,[103] the breakdown of which as per location in the lines are as follow: 343 at *mid*-lines; 18 *before* line-beginnings,[104] and 31 at *line-ends*.[105]

[102] Heb 8:9-9:9 (f29^{v}-f29^{r}) and 9:26-13:25 (f31^{v}-f38^{v}) were yet to be publicised by the Beatty camp at the time Sanders published his edition.

[103] Breakdown for each edition is as follows:

COMPARATIVE CHART OF READING MARKS COUNT IN HEBREWS														
	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**TOTALS**
KENYON	1	26	24	20	25	45	43	16	34	37	42	25	12	350
SANDERS	1	28	24	20	25	44	43	11?	16?	0?	0?	0?	0?	212?
DNTAP$^{2.2}$	1	27	24	19	26	45	44	18	37	41	48	27	12	369
COMFORT-BARRETT	1	26	25	21	25	43	42	16	37	38	44	26	12	356
EBOJO	2	28	26	23	26	49	48	18	38	46	49	26	13	392

[104] Heb 3.10; 4.16; 6.4, 17; 7.7, 11 (2x), 23; 9.18, 24; 10.12; 11.6, 10, 22, 35, 36; 12.3, 27.

[105] Heb 3.4, 5, 10, 17, 18; 6.2, 16, 17, 18 (2x); 7.2, 6, 11 (2x), 14; 8.3; 9.6, 7, 12, 24; 10.3, 25; 11.5, 20, 21, 35 (2x); 12.2, 13, 26; and 13.24.

2. Space-intervals agreeing with Reading Marks in $\mathfrak{P}^{46}$ and the NA-UBS Punctuations

272 of these 392 (or 69%) agree with punctuations in NA and UBS, marking clause, sentence, and paragraph units. The 31 line-end cases all mark various sense-levels: 10 sentence, 15 clause, and 6 phrase levels. Even in these end-line cases, the agreement with the punctuation placements in NA and UBS is very high at 81% (25 out of 31). This degree of remarkable agreement can only be possible if viewed from the perspective of an existing sense-division tradition that has been transmitted through generations of manuscript production, which the editors of NA-UBS have recognized and eventually integrated in their editions. But do we have any corroborating evidence pointing to this ancient "existing tradition"?

More remarkable than the agreement of the reading marks with the punctuation placements in NA and UBS is the fact that 336 out of the 392 reading marks (or 86%) agree with the space-intervals in $\mathfrak{P}^{46}$. The agreement percentage may even increase to 94% (367 out of 392) if we include the line-end cases. This notable agreement clearly confirms the presence of a prevalent tradition of sense-divisions amongst ancient manuscripts and how their ancient users construed them, even before the era of more visually elaborate manuscript production came to fruition.

3. Space-intervals agreeing with other Papyri

Such a proposal is further reinforced if we consider the degrees of agreement when we compare the space-intervals in $\mathfrak{P}^{46}$ vis-à-vis other papyri. Unfortunately, apart from $\mathfrak{P}^{46}$, almost all of the eight other papyri with Hebrews are in a very sorry physical state. In fact, not much information is available from $\mathfrak{P}^{12}$ owing to its nature as an amulet

manuscript, containing only the first verse of Hebrews.[106] The same is true for $\mathfrak{P}^{114}$ (with only 10 lines of a few letters each [1-6 characters invariably], containing 1.7-12), $\mathfrak{P}^{116}$ (containing very disjointed portions of 2.9-11 and 3.3-6),[107] and $\mathfrak{P}^{126}$ (containing fragmented portions of 13.12-13 and 13.19-20).[108]

Fortunately, the more extensive of these fragmentary papyri can provide ample information to bear out our argument. For instance, in portions where $\mathfrak{P}^{46}$ and $\mathfrak{P}^{13}$ are both extant, there are 122 occurrences of a *dicolon* or high dots in $\mathfrak{P}^{13}$, where NA and UBS also invariably indicated sense-pauses at various levels (paragraph, sentence, clause, and phrases). Notably, the agreement in the presence of space-intervals in $\mathfrak{P}^{46}$, the *dicolon* and high dots in $\mathfrak{P}^{13}$, and punctuations in NA and UBS is at 63% (77 instances);[109] agreement in the absence of space-intervals is at 6% (7 cases), or a combined total of 69% (i.e., agreement in the presence + agreement in the absence of space-intervals). These agreements range from sentence, clause, and phrase levels. Interestingly, within these mutual agreements, there are 13 instances where only $\mathfrak{P}^{46}$ and $\mathfrak{P}^{13}$ distinctively agree together.[110] Disagreements, on the other hand, stand at 31% (39 cases), the majority of which are cases of the absence of *dicolon* or high dots in $\mathfrak{P}^{13}$ whereas $\mathfrak{P}^{46}$ has space-intervals. There are also two instances of a "singular witness" in $\mathfrak{P}^{13}$ (3.4 and 4.12) that are neither in $\mathfrak{P}^{46}$ nor NA-UBS.

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[106] For its earliest transcription, see Bernard Grenfell and Arthur Hunt, *Amherst Papyri* (Part I; London: Henry Frowde/Oxford University Press Warehouse, 1900), 31; see also, *DNTAP*$^{2.2}$, 243. For image and transcription, see Jaroš, *Das Neue Testament*, 4389-91.

[107] For image and transcription, see Jaroš, *Das Neue Testament*, 4374-79.

[108] See Clivaz, "A New NT Papyrus: $\mathfrak{P}^{116}$," 158-62.

[109] Sanders, *TCPC*, 18, noted only 34 cases of agreement, due to the still fragmentary state of Hebrews at the time he compared the $\mathfrak{P}^{46}$ with $\mathfrak{P}^{13}$.

[110] Heb 3.2 ($f23^{v}$-l^{25}); 3.5 ($f23^{r}$-l^{06}); 3.19 ($f24^{v}$-l^{14}); 4.10 ($f24^{r}$-l^{14}); 10.34a ($f32^{r}$-l^{08}); 10.34b ($f32^{r}$-l^{10}); 10.37 ($f32^{r}$-l^{17}); 10.39a ($f32^{r}$-l^{21}); 11.10 ($f33^{r}$-l^{04}); 12.1 ($f35^{v}$-l^{23}); 12.3 ($f35^{r}$-l^{07}); 12.5 ($f35^{r}$-l^{14}); and 12.8 ($f35^{r}$-l^{18}).

$\mathfrak{P}^{17}$ preserves only 21 very fragmentary lines corresponding to 9.12-19. In portions where both $\mathfrak{P}^{46}$ and $\mathfrak{P}^{17}$ are extant, $\mathfrak{P}^{46}$ agrees with $\mathfrak{P}^{17}$ in two cases (both at sentence level) and disagrees in one (clause level).[111] On the other hand, in portions where both $\mathfrak{P}^{46}$ and $\mathfrak{P}^{89}$ are extant, no agreement in presence has been noted. But this is quite misleading as $\mathfrak{P}^{89}$ is very fragmentary, and there is a possibility that it did not utilize any punctuation marks throughout.[112] Finally, $\mathfrak{P}^{79}$ preserves 10.10-12 (recto), 28-30 (verso),[113] and there is a very high percentage of agreement in portions where $\mathfrak{P}^{46}$ and $\mathfrak{P}^{79}$ are both preserved--it agrees[114] with $\mathfrak{P}^{46}$ five times, with a possibility of one more.[115]

All these (dis)agreements in $\mathfrak{P}^{46}$ vis-à-vis the reading marks and markings in other papyri with Hebrews cannot simply be dismissed as nothing more than coincidences; the agreements are glaring and attest strongly to the observation that there were indeed general patterns, whilst diversities also exist, on how sense-divisions were marked by ancient scribes and construed by their immediate reading patrons. This also indicates that Greek sentence structure has not changed radically over the years.

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[111] Agreements: Heb 9.12 and 9.15; disagreement: 9.16.

[112] Palaeographical details are few for this manuscript; see Rosario Pintaudi, "N.T. Ad Hebraeos VI, 7-9; 15-17," *ZPE* 42 (1981): 42-44.

[113] See Kurt Treu, "Neue neutestamentliche Fragmente der Berliner Papyrussammlung," *APF* 18 (1966): 37-48.

[114] Here I have documented the dual reading mark (˝) in $\mathfrak{P}^{79}$ instead of *dicola* or high dots (as in $\mathfrak{P}^{13}$), vis-à-vis the space-intervals in $\mathfrak{P}^{46}$.

[115] Heb 10.10, 11, 28, 29 (2x). In 10.29, some parts of ενυβρισᾳς have already eroded and the dual reading marks may have been lost with the eroded portion.

C. As Grammatical Markers

Space-intervals in $\mathfrak{P}^{46}$ also functioned as grammatical markers, as can be evidenced by the patterns of grammatical groupings deducible by looking at where particularly these gaps occur, both at the micro- and macro-grammatical levels.

1. Phrase and Clause Levels

Before prepositional phrases

There are certain marked prepositional phrases that are more likely to receive slight space-intervals than others. Topping the list are those preceded by the preposition εις which are almost always spaced, if they occur at the mid-lines or even if they are the last word of the line;[116] but compound words prefaced with εισ- are very seldom spaced. Other prepositional phrases that are commonly spaced include those preceded by κατα, προς, προ, εν, and απο. These prepositions seem to have functioned as "cue words" insofar as space-intervals are concerned.

Before conjunctive and articular phrases

The conjunction και, when at mid-line or as the last word in the line, almost always receive a slight space-interval when it forms a phrase (or a short clause); when it begins the line it is immediately followed by a slight space-interval before the next word is written. On the other hand, γαρ and δε are slightly spaced either before or after they are written. Furthermore, there is also a marked tendency by our scribe to put space-intervals, of varying measurements, before phrases preceded by an article.

[116] In Hebrews alone, the phrase εις τον αιωνα always go together without a space in between words.

2. *Paragraph/pericopal level*

Based on their lay-outs, NA[28] identified 47 paragraph units (PU's) for the book of Hebrews, whilst UBS[4] identified 58; they exactly agree on 36 instances.[117] Except in six instances of eroded portions,[118] all these identified paragraph units are extant in 𝔓[46] (although four are line-end cases);[119] therefore, a reasonable comparison could be made.

Notably, the majority of these identified PU's have corresponding reading marks (RdMrks) in 𝔓[46]. In fact, sans the six cases of erosions and other unverifiable cases,[120] the PU-RdMrk (i.e., paragraph unit-reading mark) correspondence is at 73% (or 35[121] out of 48 verifiable instances).[122] This high degree of agreement indicates that whoever added these reading marks must have been using either a model manuscript with existing mark-ups for reading or he was following a tradition of structure-division already in wide circulation for public reading of manuscripts.

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[117] PU's where NA[28] and UBS[4] agree: Heb 2.1-4, 5-9, 10-18; 4.14-16; 5.1-4, 5-10, 11-14; 6.9-12, 13-20; 7.1-3, 4-10, 11-19, 20-25, 26-28; 8.1-6, 7-13; 9.11-14, 15-22, 23-28; 10.11-18, 19-25, 26-31, 32-39; 11.8-12, 13-16, 17-22, 23-31; 12.1-3, 4-11, 18-24, 25-29; 13.1-6, 18-19, 20-21, 22-23, and 24-25.

The following table shows where NA and UBS disagree:

NA[28]	UBS[4]
1.1-14	1.1-4, 5-14
3.1-19	3. 1-6, 7-11, 12-19
4.1-13	4.1-11, 12-13
6.1-3, 4-8	6.1-8
9.1-10	9.1-5, 6-10
10.1-10	10.1-4, 5-10
11.1-7, 32-40	11.1-2, 3, 4-7, 32-38, 39-40
12.12-17	12.12-13, 14-17
13.7-17	13.7-16, 17

[118] 4.13 ($f24^{r}$-l^{27}); 7.10 ($f27^{v}$-l^{25}), 19 ($f27^{r}$-l^{25}); 10.31 ($f32^{v}$-$l^{29?}$); 11.3 ($f32^{r}$-$l^{28?}$); and 13.19 ($f37^{r}$-l^{26}).

[119] Heb 3.11 ($f23^{r}$-l^{22}); 10.25 ($f32^{v}$-l^{12}); 12.13 (UBS only [$f36^{v}$-l^{06}]); 13.21 ($f38^{v}$-l^{05}), and 25 ($f38^{v}$-l^{13}).

[120] 3.11, 13.21, and 25 ($f38^{v}$-l^{13}) are line-end cases, whilst 9.14 involves a case of haplography.

[121] 2.4 ($f22^{r}$-l^{06}), 2.9 ($f22^{r}$-l^{22}), 18 ($f23^{v}$-l^{21}); 3.6 ($f23^{r}$-l^{10} [*cum* UBS]), 19 ($f24^{v}$-l^{15}); 4.11 ($f24^{r}$-l^{19} [*cum* UBS]), 16 ($f25^{v}$-l^{07}); 5.10 ($f25^{r}$-l^{05}), 14 ($f25^{r}$-l^{17}); 6.3 ($f25^{r}$-l^{25} [*cum* NA]), 8 ($f26^{v}$-l^{12}), 12 ($f26^{v}$-l^{24}), 20 ($f26^{r}$-l^{22}); 7.25 ($f28^{v}$-l^{14}), 28 ($f28^{r}$-l^{02}); 8.13 ($f29^{v}$-l^{21}); 9.5 ($f29^{r}$-l^{11} [*cum* NA]), 10 ($f30^{v}$-l^{03}), 22 ($f30^{r}$-l^{15}), 28 ($f31^{v}$-l^{08}); 10.4 ($f31^{v}$-l^{19} [*cum* UBS]), 10 ($f31^{r}$-l^{06}), 18 ($f31^{r}$-l^{24}), 25 ($f32^{v}$-l^{12}), 39 ($f32^{r}$-l^{22}); 11.7 ($f33^{v}$-l^{20}), 12 ($f33^{r}$-l^{13}), 16 ($f33^{r}$-l^{26}), 22 ($f34^{v}$-l^{14}), 31 ($f34^{r}$-l^{16}); 12.11 ($f36^{v}$-l^{02}), 13 ($f36^{v}$-l^{06} [*cum* UBS]), 17 ($f36^{v}$-l^{19}), 24 ($f36^{r}$-l^{10}); 13.6 ($f37^{v}$-l^{11}).

[122] The following are unmarked: 1.4 ($f21^{r}$-l^{18}), 14 ($f22^{v}$-l^{19}); 5.4 ($f25^{v}$-l^{17}); 7.3 ($f27^{v}$-l^{08}); 8.6 ($f28^{r}$-l^{23}); 11.2 ($f32^{r}$-l^{25}), 38 ($f35^{v}$-l^{15}), 40 ($f35^{v}$-l^{20}); 12.3 ($f35^{r}$-l^{09}), 29 ($f36^{r}$-l^{26}); 13.16 ($f37^{r}$-l^{14}), 17 ($f37^{r}$-l^{20}), 23 ($f38^{v}$-l^{05}). These thirteen translate to 27%.

But even more remarkable than the PU-RdMrk correspondence is the PU-SI (i.e., paragraph unit-space interval) correspondence. In fact, where they are extant and not line-end cases, space-intervals of varying measurements mark all the PU's identified in NA^{28} and UBS^{4}, or a 100% correspondence! This strongly indicates that the cumulative presence of space-intervals with the mutual concurrence of the reading marks and other papyri markings plus the agreement of NA^{28}-UBS^{4} paragraphing divisions reveals an existing scribal practice of sense-unit division that might have antedated our scribe, and perhaps even his *exemplar*! It may not be as elaborately visual as in the tradition of later parchment manuscripts—Hurtado's description of it as "emergent and developing"[123] fits well—but nonetheless they serve the purpose of structure markings.[124] I could not agree more with Sanders' suggestion that some of the space-intervals were intended by the scribe "to mark the ends of paragraphs",[125] as seen from the examples above. For this high degree of correspondence, we can only commend our scribe for faithfully reflecting, hence, perpetuating, this scribal tradition, which in turn gives us a more concrete glimpse at the interpretive milieu from which $\mathfrak{P}^{46}$ (and other MSS) emerged.[126]

But is there anything then about the space-intervals in $\mathfrak{P}^{46}$ that we can confidently attribute to the copying proclivities of its scribe? The answer seems to lie in instances where $\mathfrak{P}^{46}$ preserves space-breaks that are unattested by the manuscript tradition.

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[123] Hurtado, *Early Christian Artifacts*, 181.

[124] For a somewhat related study along this line with a similar conclusion, see Porter, "Pericope Markers in Some Early Greek New Testament Manuscripts".

[125] Sanders, *TCPC*, 17.

[126] Hurtado's suggestion is here very instructive, "On the other hand, it is certainly clear, and notable, that by about 200 some Christian scribes were registering sense-unit divisions in biblical texts by various scribal devices… (T)his means that the early manuscripts in which these devices were deployed are artifacts of early Christian exegesis of these texts, and probably also reflect something of how these texts were read liturgically, by about 200… Studies of the early Christian reception of these texts, the canonization process, early liturgical practices, and related matters should all take due notice of this evidence" (*Early Christian Artifacts*, 181; see also, Idem, "Sociology of Early Christian Reading," 58).

IV. SPACE-INTERVALS AS A WINDOW TO THE SCRIBAL WRITING HABITS OF $\mathfrak{P}^{46}$

A. Space-intervals at mid-words

Occasionally, we find space-intervals occurring at the middle of a word. Some of the mid-word space-breaks are due to the scribe's attempt *to avoid blemishes in the papyrus*. Kenyon already mentioned the papyrus joins of κολλήματα as "flaws in the papyrus" that resulted in "accidental" pauses.[127] Heb 2.4 ($f22^{r}$-I^{04}) may be illustrated as an example, where the gap between sigma and iota in the word δυναμεσιν must have been stimulated by the κολλήσις which our scribe consciously avoided (Fig. 3-4.14). Kenyon seems to have made this a major reason for space-intervals. However, this kind of break is very rare; I have found only five other cases in Hebrews.[128] The small number is perhaps due to the characteristic pliability of the papyrus during its codex production which hardly posed a writing problem to our scribe,[129] and whoever was directly responsible for its construction into a codex bears the mark of a skilled artisan.

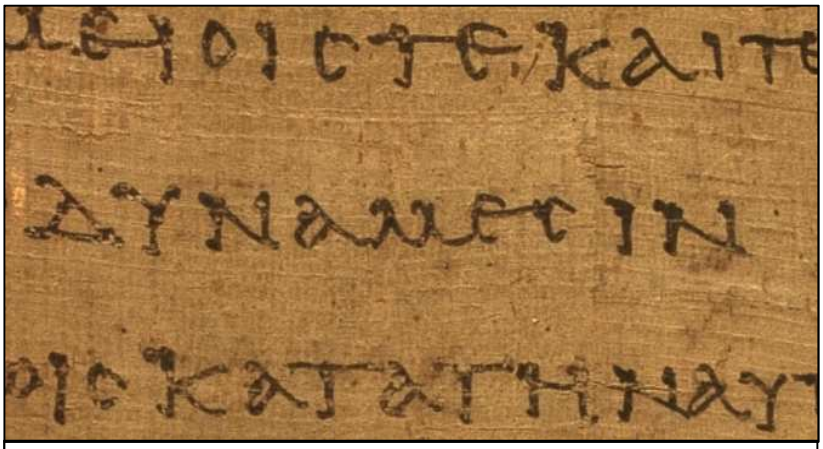


Figure 3-4.14 $F22^{r}$-I^{04}, showing a mid-word gap due to κολλήσις.

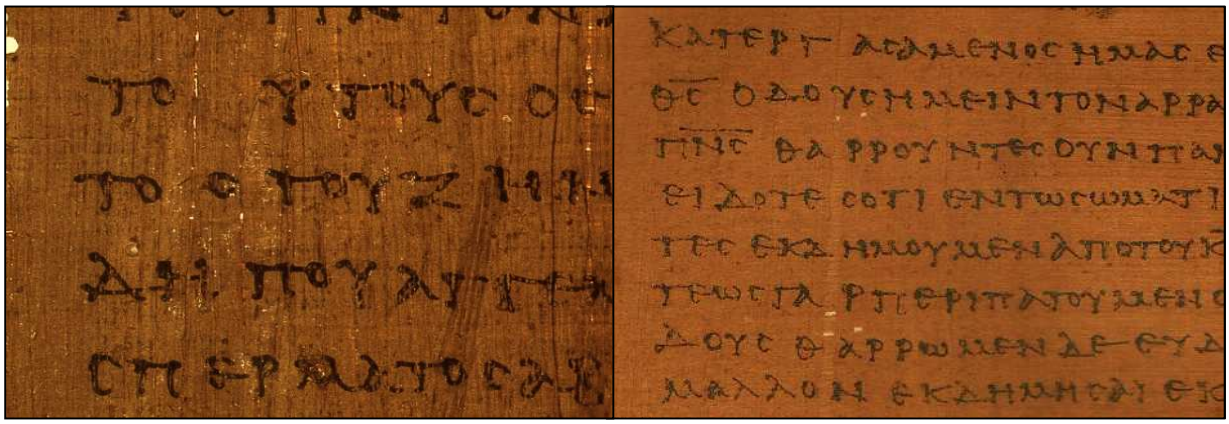
[127] Kenyon, *CBBPIII-1936*, xiv.

[128] The list includes $f21^{r}$-$II^{07, 15}$ (Heb 1.2, 4); $f22^{r}$-I^{04} (Heb 2.4); $f23^{r}$-I^{26} (Heb 3.13); and $f28^{r}$-$I^{03, 16}$ (8.1, 5). Outside of Hebrews, only the following are most conspicuous: Rom 15.27-28 ($f19^{r}$-II^{20-26}); 1Cor 3.16 ($f41^{r}$-II^{01-02}); 6.18-19 ($f44^{r}$-II^{14-17}); 12.24-4.1 ($f54^{r}$); and 15.21-22 ($f58^{r}$-II^{08-11}).

[129] Of the 43 surviving sheets, 56 κολλήσεις have been documented. As we have shown in pp. 79-86 and 119-36), in most cases, our scribe had taken these κολλήσεις as inconsequential to his copying task, and *de facto* wrote characters, including the divine abbreviations (*nomina sacra*), across them.

A more common material defect causing mid-word space-intervals is the *manufacturing problem related to the vertical papyrus strands*,[130] either very slightly broken off or improperly pasted, that must have accidentally happened *before* the scribe copied his text. One very good example is f23^{v}-ll$^{12-14}$, involving Heb 2.15-16 (Fig. 3-4.15 [Left]), where the overlapping joins of two vertical papyrus strands, at that point, may have accidentally broke off beforehand, prompting the scribe to wisely avoid it, although unfortunately creating noticeable space-gaps in the words in the proximate region of that page. Outside Hebrews, f65^{v}, containing 2Cor 5.5-13,[131] also provides a good illustration (Fig. 3-4.15 [Right]). This type does not mark grammatical sense units, but it points to our scribe's degree of attentiveness with regards to the physical minutiae of the material he was using. At least 29 other cases[132] of space-intervals are attributable to this sort, in the book of Hebrews.

Figure 3-4.15 Right: F23^{v}-l$^{12-15}$, space-gaps in between letters due to defective vertical strand. Left: F65^{v}-ll$^{01-08}$, a long vertical strand was stripped off that our scribe avoided.



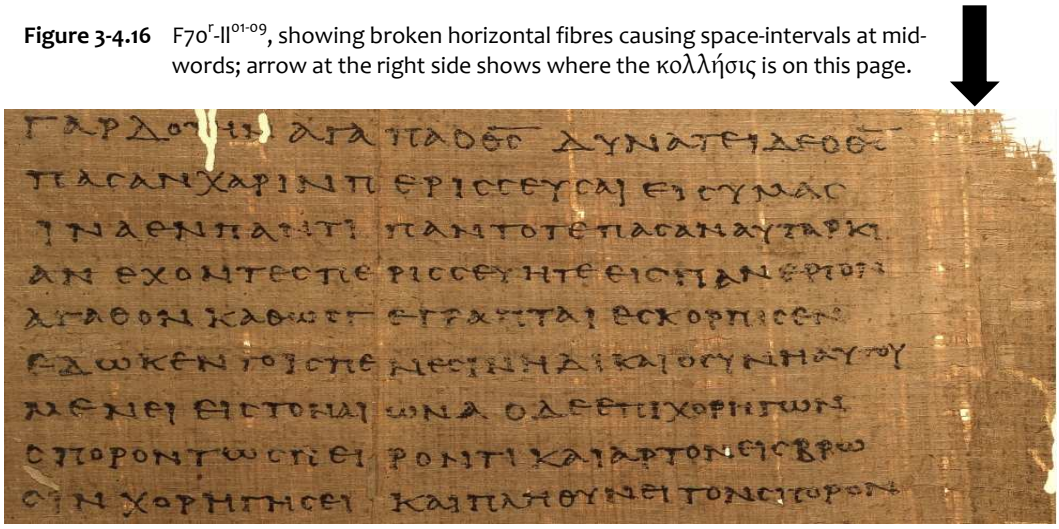
[130] See related discussion of material blemishes in pp. 60-68.

[131] To this maybe added f54^{r} (12.24-13.1) where the space-gaps at mid-words are comparatively very pronounced.

[132] Heb 1.8 βασι_λειας (f22^{v}-l^{03}), 9 κα_ι (f22^{v}-l^{04}); 2.1a ρ_ως (f22^{v}-l^{20}), 1b μ_ηποτε (f22^{v}-l^{21}), 2a λ_αληθεις (f22^{v}-l^{22}), 2b [πα=]||σ_α (f22^{v}-l^{23}), 2c ε_νδ̣ιḳον (f22^{v}-l^{24}), 2.3 ε_κφευξωμε[θα] (f22^{v}-l^{25}), 11a αι_τιαν (f23^{v}-l^{01}), 11b αυ_τους (f23^{v}-l^{02}); 5.2 π_ερικειται (f25^{v}-l^{12}), 4 υ_πο (f25^{v}-l^{16}); 6.5a γευ_σαμε=||νους (f26^{v}-l^{01}), 5b μελλο_ντος (f26^{v}-l^{02}), 6a πα_λιν (f26^{v}-l^{03}), 6b α_να=||καινιζειν (f26^{v}-l^{03}), 7 πι_ουσα (f26^{v}-l^{06}), 11 δει_κṿυσθ̣αι (f26^{v}-l^{19}); 7.2 βασιλευ_ς (f27^{v}-l^{04}), 3 ζω_ης (f27^{v}-l^{06}), 5a λαμβ_ανοντες (f27^{v}-l^{12}), 5b αδ_ελφους (f27^{v}-l^{14}), 5c ο_σφυος (f27^{v}-l^{15}), 7.27 αναγ_κην (f28^{v}-l^{19}); 9.11 σ_κηνης (f30^{v}-l^{05}); 11.7a κα_τεσκευασεν (f33^{v}-l^{16}), 7b τη_ριαν (f33^{v}-l^{17}), 7c τε_κρεινεν (f33^{v}-l^{18}), and 7d κα_τα (f33^{v}-l^{19}).

It is noteworthy that there is a singular instance, outside of Hebrews, where the presence of conspicuous space-intervals at mid-words in almost all the lines of the page was caused neither by the κολλήσις nor the vertical strand, but by broken *horizontal* strands. As Fig. 3-4.16 (involving 2Cor 9.7-10.1) shows, the upright breakage across horizontal fibres must have happened either at the time of the codex production or immediately after it, for our scribe had detected it already and judiciously avoided it, skipping at least 0.3-0.5 cm in between letters. Fortunately, this breakage did not cause any textual variation in the process, which again hints on our scribe's attention level.

Figure 3-4.16 F70^{r}-II$^{01-09}$, showing broken horizontal fibres causing space-intervals at mid-words; arrow at the right side shows where the κολλήσις is on this page.



But not all mid-word space-breaks are due to the physical defects of the material; some are simply because *the word at issue is comparatively longer*. Almost all of the longer words, those that comprised of four or more syllables, receive a slight space-interval either at the last or the second to the last syllable. The placement of the space-break may have to do with where the word is expected to be accented, or perhaps marking the point where the scribe lifted momentarily his pen and moved his hand a bit toward the right to continue writing the word. At any rate, this type of space-interval points more to our scribe's writing regimen than to grammatical sense-units.

Another writing habit of our scribe that caused mid-word space-intervals is the use of *apostrophe functioning as accent between consecutive liquid consonants*. Our particular example for this is the lemma κριττονος and its derivative forms, which whenever accented, always receive a very slight space-interval at the point of accenting, particularly between the consecutive consonants, i.e., κριτ̀_τονος.[133] (Accordingly, both the lone occurrences of accented ηλατ̀_τωσας [Heb 2.7] and αλ_λ [Heb 3.16] receive the same slight space-interval also).[134] On the other hand, none of the unaccented forms[135] shows any sign of slight spacing. Hence, in these accented instances, the space-interval does not function as a grammatical marker, but was necessitated by the accent stroke of the apostrophe when the scribe momentarily lifted his pen.

B. Space-intervals due to calligraphic requirements

Some of the slight space-intervals that occur before, in the middle, or after a word at any time are due to the *calligraphic nature of particular characters*. This is generally true for the broader letters **ⲇ**, **ⲭ**, **ⲝ**, **ⲫ**, and occasionally with **ⲃ**; a very slight space appears both before and after these letters. Also, whenever initial ι and υ are with diaeresis, slight space-gaps appear before and after these letters. In these instances, the space-gaps are occasioned more by the requirements of calligraphy than of sense. However, in instances where initial ι or υ with diaeresis clearly precedes clauses, the presence of space-intervals is probably due more to the requirement of sense, especially clauses starting with the result/purpose conjunction ἵνα.[136]

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[133] So is 7.7; 8.6a and b; and 11.35. In 11.40, the word is the last on the line and runs through the next line, yet a space between the consecutive τ's seems to have been intended by the scribe.

[134] Both the ηλαττωμενον (Heb 2.9) and ελαττοῳ (Heb 7.7) are unaccented, and are not marked with slight space-intervals between the two consecutive τ's.

[135] Heb 1.4; 7.22; 9.23; 10.34; 11.16; and 12.24; κριττονος in Heb 7.19 is not extant.

[136] It would be interesting to note that the two surviving occurrences of ἵνα in $\mathfrak{P}^{52}$ (c. 125 A.D.) may have similar circumstance.

C. Space-intervals indicating correction events

When our scribe spotted an error—either from his *exemplar* or his own doing—and *in scribendo* initiated corrective measures, space-intervals are used to call attention to these correction events, particularly those involving correction with expunging dots. In Hebrews, the first example of this type is in f23^{r}-l^{12} (3.7), involving the expunging of the first person possessive pronoun (μὸὺ) and correcting it to the third person αυτου. It is evident that in between these two words a 2-letter gap was deliberately placed by our scribe to signal the correction event (Fig. 3-4.17).

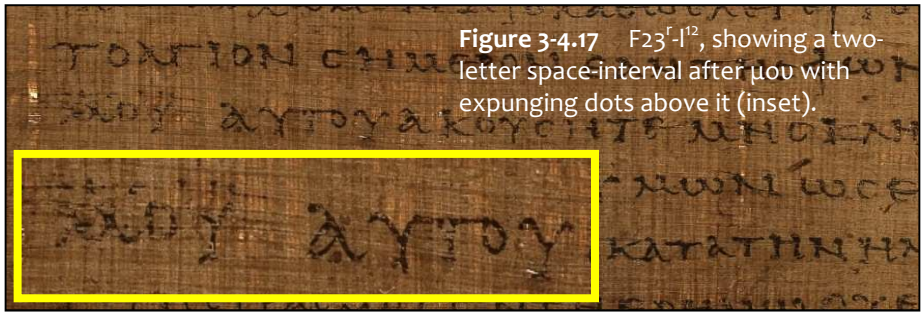


Figure 3-4.17 F23^{r}-l^{12}, showing a two-letter space-interval after μου with expunging dots above it (inset).

Other correction events of this sort include 10.22 (γάρ__μετα); 11.21 (αυ=<ιωσεφ>||τὸὺ__ευλογησεν); and possibly 7.1 (τὼ__αβρααμ). This is also true outside of Hebrews. As a matter of example, we may cite f47^{r}-l^{12} (Fig. 3-4.18), showing correction events involving single letters by expunging dots and slash marks as well as the space-intervals after it, i.e., ειδωλα$^{<o>}$__θυτα_εσθειειν.

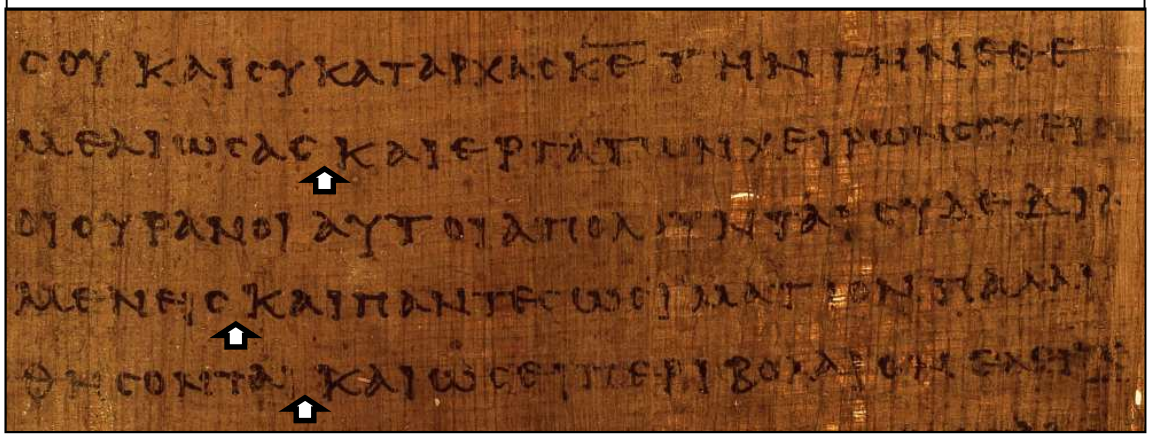
Figure 3-4.18
F47^{r}-l^{12}: Space-gap after an *in scribendo* correction event.



D. Space-intervals involving line-end words running through the following line

Another scenario where we detected a remarkably consistent employment of slight space-intervals involves cases where the final word of a line runs through the following (or line-end word breaks [Fig. 3-4.19]). I have documented 282 cases of this sort in Hebrews (some of these are longer words [see discussion above]). However, 26 of these 282 are not practically helpful in the analysis as the portions of the line following we are concerned with have already broken-off[137]—leaving us with 256.

Figure 3-4.19 F22^{v}-II$^{07-11}$ Slight space-intervals after the running-through-lines words



Of the 256 valid samples, I found only 18 instances (or 7%) where the continuing syllable/s of the broken word in the following line did not have a space interval after it. But the number can get even smaller if we consider possible valid reasons for its absence. In fact, in eleven cases, my impression is that the absence was caused by one of the following: 1) physical defect in the earlier part of the word,[138] 2) elision of familiar combination (i.e., involving εστιν),[139] 3) the attraction of final ς to

[137] Heb 1.5; 3.3 (2x), 13 (2x); 4.4, 13; 5.7; 6.13; 7.11, 20, 28; 8.7; 9.9 (2x), 25, 26; 10.7, 20 (2x); 11.26 (2x), 34 (2x); 13.2, 19.

[138] Heb 2.2 (κ[αι πα=]||σ_απαραβασις); 5.1 (προσ=||φερ_ηδωρα); and 11.7 (δι ης κα=||τε_κρεινεντον).

[139] Heb 2.14 (του=||τεστιν) and 9.5 (ου=||κεστιν).

initial τ and θ,[140] 4) proximity to the usually marked prepositional phrases,[141] or 5) a comparatively longer word.[142] This leaves us with only seven cases where the absence of the slight space-intervals is inexplicable.[143] Converted to percentage, the presence of space-intervals involving one or another of the reasons given then translates to 97%.

What this reveals about our scribe's writing habit is that the division of a line-end word and the writing of the remaining syllables onto the following line are done in rapid sequence, most likely without the scribe looking back at his *exemplar* for the next word or series of words to copy until he copied the last character.

Moreover, this data also tells us that our scribe copies the text of his *exemplar* by *word or series of words rather than by letters.*[144] This observation is cumulatively implied already by what has been indicated in the foregoing discussion. But that is not all. Another clue to this writing practice of our scribe may be discerned in the way the space-intervals recur and are situated in each line. Lest we be drowned with details,[145] I attempt to illustrate this point by means of using the first page of Hebrews ($f21^{r}$-$ll^{04\text{-}25}$), containing 22 extant lines—a good representative of the rest of the pages of the book (Fig. 3-4.20).

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[140] Heb 10.1a (αυ=||ταιςθυσιαις) and 10.1b (προσερχο=||μενουςτελειωσαι).

[141] With προσ- (Heb 11.40, κριτ=||τοντι_προσβλεψαμενοι) and with προ- (τρε̣[χω=]||μεντο_προκειμενον).

[142] Heb 13.7 (ανα=||θεωρου_ντεστην). In this example, the space-interval occurred earlier, at the fourth syllable, instead of the last. It is also possible that the final -ς was attracted to the initial τ of the immediately following word.

[143] Heb 2.9, 12; 4.1; 9.26, 10.16, 38; and 12.2.

[144] Although I have not utilised directly the methodology suggested, Colwell, "Method in Evaluating Scribal Habits," 116-17, equally found similar copying characteristic for the scribe of $\mathfrak{P}^{45}$, concluding that its scribe, as opposed to $\mathfrak{P}^{66}$ and $\mathfrak{P}^{75}$, copies phrases and clauses, whereas the two latter copy by letters and syllables.

[145] For a detailed collation of the whole of Hebrews, see Appendix H.

Figure 3-4.20 F21^{r}-II$^{04-25}$ <-> represents one-letter space-interval <--> represents two-letter space-interval	**SPACE INTERVAL RECURRENCE PER LINE**
προς εβραιους	
L04: πολυ<__>μερως <_> και πολυ<_>τροπως	3
L05: παλαι <_> ο <_> θ̅ς̅ <_> λαλησας <_> τοις πατρασιν $^{<\eta\mu\omega\nu>}$ <_> εν	5
L06: τοις προ<_>φηταις <_> επ εσχατου <_> των ημε=	3
L07: ρων <_> τουτων <_> ελαλησεν ημειν <__> εν	3
L08: υϊω <__> ον εθηκεν <_> κληρονομον <_> παντω̄	3
L09: δι ου <_> εποιησεν <_> τους αιωνας <__> ος ων	3
L10: απαυγασμα <_> της δο<_>ξ<_>ης <_> και χαρα=	4
L11: κτηρ <_> της υποστασεως <_> αυτου <__> φερων τε	3
L12: τα παντα <_> τω ρηματι της <_> δυναμεως	2
L13: δι αυτου <_> καθαρισμον <_> των <_> αμαρτιων	3
L14: ποιησαμενος <_> εκαθισεν <_> εν δεξια της	2
L15: μεγαλλω<_>συνης <_> εν υψηλοις <_> τοσου<_>των	4
L16: κριττων <_> γενομενος <_> αγγελων <_> οσ=	3
L17: ω <_> διαφορωτερον <_> παρ αυτους <_> κεκλη=	3
L18: ρονομη<_>κεν <_> ονομα <_> τινι <_> γαρ ειπεν	4
L19: ποτε των <_> αγγελων <__> υ̅ι̅ς̅ <_> μου ει συ <____>	3/4(?)
L20: εγω <_> σημερον <_> γεγεννηκα σε <_> και παλιν <_>	4?
L21: εγ]ω <_> εσομαι αυτω <_> εις πατερα <_> και αυ=	3
L22: τος ε]σται μοι <_> εις υ̅ν̅ <__> οταν <_> δε παλιν	3
L23: αγαγ]η <_> τον πρω[τ]οτοκον <_> εις την οικου	2
L24: μενην] <_> λεγει <_> κ[α]ι προσκυνησα<_>τωσαν	3
L25: αυτω παντε]ς <_> αγ[γ]ελοι <_> θ̅υ̅ <__> και προς μεν	3/4(?)

The space-interval recurrence pattern is thus: 2's (3), 3's (14), 4's (3), and 5's (1). The 2's represent three groups of word/s, 3's represent four groups, and so on. 3's is the average number of space-intervals per each line; the 4's and 5's are caused either by the presence of *nomina sacra* or space-gap within the longer words or a physical defect. On the other hand, the three instances of 2's are basically due to the presence of consecutive phrases in the line. But the more important detail we need to underscore here is the point that, unless the scribe was daunted by the material blemish or the presence of longer words, the space-intervals are most consistently situated at strategic junctures where sensible words or phrases are intelligibly formed. In fact, in this particular page, the number of characters per line ranges from 22-30. This shows that our scribe is capable of biting off at one time as much as five-ten (5-10) characters representing a word or 7-14 representing a phrase. Our scribe does not copy by the

number of letters but by words or group of words.[146] He generally does not leave space-intervals for the sake of putting one—he carefully selected each placement; some must have been already present in his *exemplar*, due to the requirements of their trade; but some definitely betrayed his own copying proclivities.

CONCLUSION

This section underscores the need to re-assess how we methodologically adjudicate a particular scribe's level of understanding of what he is copying. Whilst visual scribal features, more evident and systematic in later parchment manuscripts, are a good indicator of structure signals, nonetheless the presence of space-intervals in earliest surviving manuscripts, without or with frugal use of punctuation, are equally a good gauge of how scribes understood the grammatical and sense structures of their *exemplars*. Hence, there is a need to re-evaluate the premise that the absence of punctuations and other visual features in the earlier manuscripts necessarily means the absence of the scribe's sense of what he is copying—the example of $\mathfrak{P}^{46}$ make this point evident. But along this line, we also need to re-assess the *measurement-based* methodology on how to isolate "sense-pauses" in our manuscripts, particularly the earlier ones with *scriptio continua* format. For instance, what has been appreciated as "accidental" space-intervals are after all not accidents; they reflect more the insufficiency of our knowledge about how particular scribes made use of this literary device vis-à-vis the physical material available to them at the time of manuscript production.

Space-intervals in $\mathfrak{P}^{46}$ are not only indicative of *pauses in sense*, but to an equal extent are also indicators of temporary (split-second?) pauses in *copying activity*,

[146] A digression, but nonetheless a corroborating evidence to this copying habit is the fact that the 1,402 characters that were accidentally lost due to incremental omissions (large-block haplographies) all represent intelligible words—our scribe had not lost a syllable/s in a leap; he lost words, phrases, or clauses, but not syllables. On this type of variation, see pp. 266-81.

demarcating actual spots where our scribe momentarily lifted his pen, glanced back at his *exemplar* for the next word or group of words to copy (and perhaps verbalized them), returned to his manuscript, and continued his copying assignment. This copying flow is only disturbed intermittently by defects in the papyrus itself or by some other factors beyond his control (or at times punctuated also by re-inking or re-sharpening of the pen).

Our scribe never leaves a space-interval in the middle of a word unless out of necessity where his only option is to sensibly avoid the physical limitations of the material. That space-intervals are also evident (in most cases) before and after broad letters does not necessarily mean they are blunders; conversely, they unveil the marked professionalism of our scribe, fully cognizant of the stylistic requirements of a good and beautiful calligraphy. That he left space-intervals after a textual error, or what he perceived to be an error, and appropriately corrected them, also gives us an idea of the extent of his knowledge about his responsibilities as a scribe—he did not only view himself as a paid copyist but also as an *ad hoc* corrector endowed with the authority to effect textual changes when warranted. These are all marks of a scribe who knew his job, was committed to it, and played by its rules.

SECTION FIVE
THE ENIGMA OF THE MISSING PAGES

INTRODUCTION

Throughout the history of its research, many approached the question of content in the missing last pages of $\mathfrak{P}^{46}$ theologically (i.e., canonically).[1] Jeremy Duff, however, arguing from the standpoint of *scribal intention*, proposed recently that the missing last pages of $\mathfrak{P}^{46}$ included the Pastoral Epistles or at least was intended to be included by its scribe. Due to the strategic importance of $\mathfrak{P}^{46}$ in the canonical discussion of the Pastorals, this section engages Duff's proposal, reappraising the usual bases for the arguments that $\mathfrak{P}^{46}$ did or did not contain the Pastorals and arguing that an alternative appreciation of the palaeographical-codicological evidence of $\mathfrak{P}^{46}$ raises new issues and possibilities as to the question of its content. In particular, I will attempt to demonstrate how the scribal habits of the original hand puts the discussion of the elusive content of the last pages of $\mathfrak{P}^{46}$ in a new dimension.

I. THE MAKING OF THE "CONSENSUS"

In his 1934 edition, based on only 10 surviving sheets, Kenyon concluded,

> The final page of the MS. in its present state, which begins with I Thess. v.23, would have sufficed to finish that Epistle and begin 2 Thessalonians. The remainder of 2 Thessalonians would have occupied a little more than two leaves, leaving four leaves and the greater part of the fifth blank at the end. *That space would about suffice for I Timothy, but not for the rest of the Pastoral Epistles. It is, therefore, perhaps more probable that they were left blank.*[2]

[1] For instance, Spencer, Reviews of Kenyon and Sanders, 70, suggested, "Sufficient evidence is not yet at hand to determine the question of the final pages… The second place given to Hebrews… shows that this work was unquestioned and held authority next to Romans. Probably the Pastorals with Philemon, being personal epistles, had not won their way in the canon and hence were omitted."

[2] Kenyon, *CBBPIII-1934*, vi-vii. (Emphasis added).

As a basis for comparison, Kenyon used Alexander Souter's Oxford Text. Subsequently, in 1936, having acquired full access to what is now the 86 extant leaves of $\mathfrak{P}^{46}$, he reiterated his earlier proposal,

> The Beatty-Michigan MS. consists… of 86 leaves… Seven leaves are missing from the beginning, which implies the loss of an equal number at the end.[3]

Then he continued,

> The last page of the MS. in its present state could have held the subscription to 1 Thessalonians, the title of 2 Thessalonians, and about twenty lines of the text of that Epistle. *There are still seven leaves of the codex to account for, corresponding to the seven lost at the beginning*. Two of these would suffice for the remainder of 2 Thessalonians, leaving five as to which we have no evidence. This is not enough for the Pastoral Epistles… The last five leaves may have been left blank, or some additional leaves may have been attached at the end so as to take the Pastoral Epistles; but the evidence does not exist which would allow this problem to be solved.[4]

Although Kenyon's proposal did not fare unchallenged,[5] it has nonetheless held its place in scholarship since then, and is recurrently reflected in most of the standard NT introductions and commentaries,[6] and in the works of some of the more prominent NT textual critics of our time[7] (although French and German text-critical

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[3] Kenyon, *CBBP-1936*, viii. In the earlier edition, he described the sheet formation of our codex, thus, "The pages were numbered in the upper margin, and sufficient page numbers have fortunately been preserved to establish the original formation… since our fol. 2, which begins with Rom. viii. 15, bears the page numbers 20 and 21, a calculation of the amounts of text involved gives the following conspectus of its formation: *7 leaves (1 blank page, and pp. 1-13) containing Rom. I.1—V. 17; lost… seven leaves lost at the end, corresponding to the 7 lost at the beginning*" (p. vi). (Emphasis added).

[4] Kenyon, *CBBPIIISupp-1936*, x-xi. (Emphasis added).

[5] For instance, as early as 1935, Sanders already questioned Kenyon's proposal, and himself proposed that the missing pages might have contained 2 Thessalonians and an abbreviated form of the Pastorals (he suggested that Philemon was situated in the mid portion of the codex). However, this proposal did not win advocates primarily because the concept of "abbreviated" Pastorals cannot be independently confirmed, and it is unthinkable that no remnant vestige of such form survived, if it ever existed; on this, see Kenyon, Review of Sanders, 92. But most decisively, the eventual purchase of 46 more sheets by Mr Beatty categorically disproved Sander's suggested location for Philemon.

[6] C.K. Barrett, *The Pastoral Epistles in the NEB; with Introduction and Commentary* (Oxford: Clarendon, 1963), 2; M. Dibelius and H. Conzelmann, *Die Pastoralbriefe* (4th ed.; Tübingen: Mohr, 1966), 2; H. Köster, *Einführung in das Neue Testament* (Berlin: de Gruyter, 1980), 736; among others.

[7] Finegan, "Original Form of the Pauline Collection," 92-93; B.M. Metzger, "Recently Published Papyri of the NT," *Biblical Archaeologist* 10/2 (May 1947): 25-44, p. 36; Idem, *Manuscripts of the Greek Bible*, 64, "($\mathfrak{P}^{46}$) is a single-quire papyrus codex, originally with 104 leaves of which 86 survive today…, containing the Pauline Epistles (but not the Pastorals)… Seven leaves are lost from

scholarships seem to have taken a more cautious stance).[8] In fact, subsequent studies along this line have also shown that there must not have been enough space to accommodate the cumulative texts of 2 Thessalonians, the Pastorals and Philemon in the lacunae, as can be seen in the Table 3-E1.[9] This table succinctly illustrates that there are more characters than what the supposed 14 missing pages could carry, regardless of what comparison base is used. Eventually, Kenyon's formula apparently have become the critical consensus—a "consensus" Jeremy Duff challenged and described as "misleading", alternatively offering his own appreciation of the evidence of $\mathfrak{P}^{46}$.

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the beginning and seven from the ending of the codex. The seven leaves lost from the end probably contained 2 Thessalonians, but would have been insufficient for the Pastoral Epistles"; Idem, *The Text of the NT: Its Transmission, Corruption, and Restoration* (Oxford: OUP, 1993), 55-54; E.J. Epp, "The Papyrus Manuscripts of the NT," in *TNTCR*[1], 37; repr. in *PNTTC*, 411-36; Idem, "Textual Criticism in the Exegesis of the NT, with an Excursus on Canon," *Handbook to Exegesis of the NT* (NTTS 25; ed. S.E. Porter; Leiden: Brill, 1997), 76; repr. in *PNTTC*, 461-96; but note his caveats in view of Duff's proposal, in "Issues in the Interrelation of NT Textual Criticism and Canon," 609.

Interestingly, even in the most recent papyrological handbook, Kenyon's verdict is perpetuated; see David Martinez, "The Papyri and Early Christianity," in *OHP*, 590-622, p. 596, who echoed this view, "The codex is a single-quire type, and judging from what we know of that format, we may determine that the original… lacked all or part of 1 and 2 Timothy and Titus since the missing leaves at the end could have accommodated 2 Thessalonians but not the Pastorals."

[8] For instance, Aland and Aland, *Text of the New Testament*, 49, "The earliest manuscript of the Pauline letters, $\mathfrak{P}^{46}$, dating from about 200, includes (Hebrews)…; unfortunately the text breaks off at 1 Thessalonians, so that it is unknown whether 2 Thessalonians, Philemon, and the Pastoral letters were originally included." On the other hand, Léon Vaganay and Christian-Bernard Amphoux, *An Introduction to NT Textual Criticism* (2nd rev. and updated edition; rev. C.B. Amphoux and Jenny Heimerdinger; trans. Jenny Heimerdinger Cambridge: CUP, 1991), 12, was more straightforward, "$\mathfrak{P}^{46}$ contains the Pauline Epistles in a special order: Romans – Hebrews – 1 Corinthians – 2 Corinthians – Ephesians – Galatians – Philippians – Colossians – 1 Thessalonians… (the rest have disappeared)." Kirsopp Lake, Review of Frederic Kenyon, *Chester Beatty Biblical Papyri*, *JBL* 55/3 (Sept 1936): 244, also took a rather wait-and-see stance and appeared open to the possibility that only 1 Timothy was included in the missing pages.

[9] Others who provided estimates but are not included in this chart, for lack of details, include David Trobisch, *Paul's Letter Collection: Tracing the Origins* (Minneapolis: Fortress, 1994), 16, who suggested 23 pages, and Epp, "Issues in the Interrelation," 612, estimating some twenty four pages; among others.

TABLE 3-E1 COMPARATIVE CHART OF SPACE CALCULATIONS BY VARIOUS SCHOLARS			
	COMPARISON BASE	**FORMULA**	**RESULT**
FREDERIC KENYON-1934	Souter's Oxford (SO) Text	1 leaf = about 33 SO lines 2Thess = a little more than 2 leaves; 4 ½ leaves ≠ PE (Pastorals = 390 SO lines)	9 pages left blank at the end
FREDERIC KENYON-1936	Souter's Oxford (SO) Text	Used the character profile of the last preserved pages; 1Tim = 167 SO lines = 8 ¼ pages 2Tim = 124 = 6 Tit = 70 = 3 ½ Philm = 31 = 1 ½ **19 ¼ pages**	- 2Thess included, in 4 pages; - Pastorals and Philm not included, leaving 10 pages left blank.
HENRY SANDERS (1935)	Oxford 1880 edition	Used the character profile of the last preserved leaves; 1 page = 26 lines of Oxford text 2Thess = 99 lines = 3 pages 1Tim = 215 = 8 2Tim = 156 = 6 Titus = 90 = 2 2/3 Philm = 40 = 1 1/3 **21 pages**	- 3 pages for 2Thess; - 11 pages contained ***abbreviated*** forms of 1 & 2 Timothy, as all Pastorals require additional 6 pages; - Philm included in the middle portion of the codex.
JACK FINEGAN (1956)[10]	Westcott-Hort	Of the 14 pages, the last two pages were left blank; 13 pages = 325 lines of WH 2Thess = 101 lines Philm = 42	13 pages not enough for all the Pastorals, unless additional pages (***extra quire***) are glued on at the end.
ROBERT GRANT (1963)[11]	Base not mentioned	2Thess = 4 ¾ pages 1Tim = 8 ¼ Tim = 6 Tit = 3 ½ Philm = 1 ½ **24 pages**	The scribe miscalculated the number of pages he needed, as the available space is not enough for the texts of the Pastorals and Philm.

II. QUESTIONING THE "CONSENSUS"

Although it was not the first time Kenyon's proposal was challenged, Duff's 1998 article entitled "$\mathfrak{P}^{46}$ without the Pastorals: A Misleading Consensus?" appears to have put a brake on the almost wholesale acceptance[12] of Kenyon's formula. It would be productive therefore to review at this juncture his arguments for questioning the "consensus".

[10] Finegan, "Original Form of the Pauline Collection," 92-93.

[11] Grant, *Historical Introduction to the NT*, 209-10.

[12] Those who eventually accepted Duff's proposal or were open to this opinion include I.H. Marshall and P. Towner, *A Critical Commentary on the Pastoral Epistles* (Edinburgh: T & T Clark, 1999), 6-7; Lee Martin McDonald and Stanley Porter, *The Early Christianity and its Sacred Literature* (Peabody, MA: Hendrickson, 2000), 492; among others. Note also the subsequent caution voiced by Epp, "Issues in the Interrelation," 609; and Parker, *NT Manuscripts and their Text*, 253.

A. "Increased Input = Intention" Theory: The Duff Proposal

Duff's challenge was an attempt to address the problems posed by the evidence (or at least the interpretation of the evidence) of $\mathfrak{P}^{46}$ against the canonical status of the Pastorals. In a nutshell, Duff described the prevailing consensus as "(fitting) very badly the actual evidence".[13] He rejected Quinn's proposal that the Pastorals were excluded in this codex because $\mathfrak{P}^{46}$ was intended to be a collection of "letters to the churches".[14] Conversely, he asserted that "it is far more likely that $\mathfrak{P}^{46}$ originally did contain the Pastorals",[15] arguing that the "relatively steady increasing" number of characters in the latter pages (see his graph in Fig. 3-E2)[16] indicates that the scribe *intended* to include the Pastorals but unfortunately miscalculated the actual space requirements.[17] He then suggested two possible scenarios in which this conundrum could have been salvaged by the scribe. First, the scribe intended to include the Pastorals but realizing the insurmountable space constraints, despite his best efforts, eventually abandoned the quest and left the pages blank. Second, which Duff preferred, the scribe appended an extra quire of four leaves to accommodate the texts of Pastorals and Philemon. He took this quire addition as normal practice under certain conditions,[18] and pointed to its

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[13] Duff, "$\mathfrak{P}^{46}$ and the Pastorals," p. 581.

[14] Duff, "$\mathfrak{P}^{46}$ and the Pastorals," 582. Quinn, "$\mathfrak{P}^{46}$—The Pauline Canon," 379-85, advanced the hypothesis that the collection of books reflected in $\mathfrak{P}^{46}$ does not point to a *de facto* content of the Pauline canon but simply preserving a well-accepted practice of gathering books together constitutive of Paul's "letters to churches" as opposed to "letters to individuals". He then suggested that the Pastorals and Philemon were excluded in $\mathfrak{P}^{46}$ simply because they appropriately belong to the "letters to individuals".

[15] Duff, "$\mathfrak{P}^{46}$ and the Pastorals," 579.

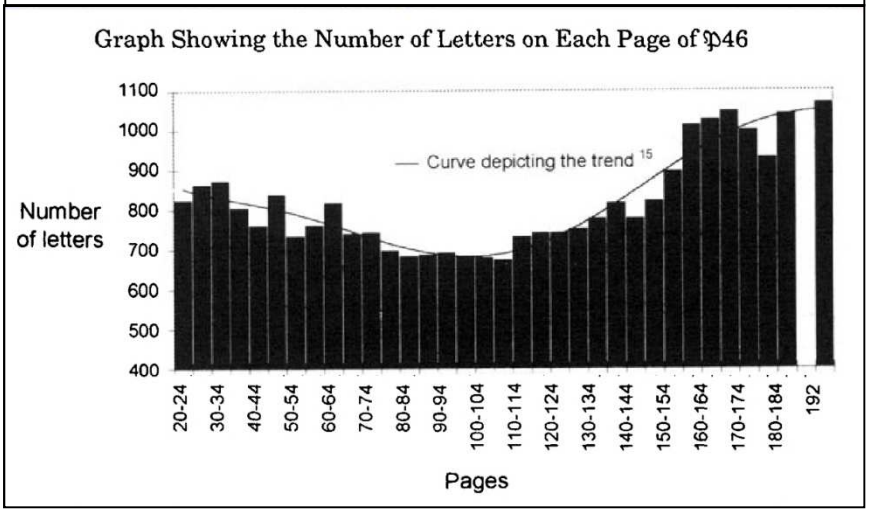
[16] Duff, "$\mathfrak{P}^{46}$ and the Pastorals," 581, approximated the number of characters in $\mathfrak{P}^{46}$ by explaining that "The number of letters in the epistles which are not present in the manuscript has been calculated from a modern critical text, adjusted to reflect the spelling used in $\mathfrak{P}^{46}$."

[17] Duff, "$\mathfrak{P}^{46}$ and the Pastorals," 585-86; see also, Marshall and Towner, *The Pastoral Epistles*, 6-7. Quite independent of Duff but also discerning of the increasing trend in $\mathfrak{P}^{46}$ (although not as detailed as Duff's), McDonald and Porter, *Early Christianity and its Sacred Literature*, 492, commented that "The lack of the Pastorals in $\mathfrak{P}^{46}$, while important to note, cannot prove that they were not in existence."

[18] Duff, "$\mathfrak{P}^{46}$ and the Pastorals," 588, argued that this is "quite consistent with what we know of scribal practices that in such a situation he would have simply added a couple of extra sheets in order to complete the codex".

circumstantial similarity with the Toura manuscript of Origen's *On the Passover*, and some Nag Hammadi codices. He concluded, "This hypothesis fits the evidence of the papyrus itself and our knowledge of scribal practices far better than assuming he purposely compressed his writing near the end in order to leave the last ten pages blank."[19] But is Duff's hypothesis really cognizant of the *scribal practices* of this papyrus?

Figure 3-E2 Jeremy Duff's Character Graph



B. Questioning Duff's Methodology: Alternative Options and Other Considerations

What may be immediately noted is that Duff's article presents no *new* evidence. What he presents is a possible alternative to *explain* the evidence. That there is an increasing trend in the number of characters in the second half of the codex is a point already underscored both by Sanders and Kenyon. That the scribe may have added more leaves at the end of the quire proper to accommodate the text of the Pastorals is a possibility already mentioned by Kenyon himself and Sanders as well,[20] and alluded to by other earlier scholars.[21]

[19] Duff, "$\mathfrak{P}^{46}$ and the Pastorals," 588-89. But Duff was not only content in describing this increasing trend; he also argued that $\mathfrak{P}^{46}$ does not disprove anything about the Pauline provenance of the Pastorals and that it has nothing to do with the formation of the Pauline corpus (pp. 589-90).

[20] Sanders, *TCPC*, 11, "If one wished to crowd into our manuscript these three Pastoral Epistles entire, it would be necessary to assume more crowding than the ones on the existing leaves and at least

That being said, we must admit that Duff's hypothesis is a clever attempt. However, despite his claim that his two hypotheses fit the evidence better than the "consensus", his proposal bristles with a number of problems.[22] First, *methodologically*, counting the characters based on the modern text adjusted to the spelling of $\mathfrak{P}^{46}$ is a very arbitrary criterion as $\mathfrak{P}^{46}$'s orthographic profile is to a large extent not only inconsistent but also unpredictable (e.g., *nomina sacra* and itacisms), and therefore makes a significant statistical difference in the case of the eroded portions, as will become evident later. Duff's proposal also necessarily presupposes that a thorough study of the orthographic profile of $\mathfrak{P}^{46}$ had been previously undertaken by him. Whilst we give him the benefit of doubt, his article gives no clear hint that he has indeed undertaken this study. In fact, we are not told explicitly what kinds of spelling adjustments on the NA[27] text have been made to conform to $\mathfrak{P}^{46}$. Furthermore, this theory also bears the burden of assuming that the scribe no longer committed "incremental errors" in the missing pages, which seems very unlikely given his known profile to omit accidentally series of letters or words due to *parablepsis*, an issue we shall further develop in the ensuing pages. Hence, Duff's statistical graph is suspect.[23] A preferable option is to count the extant characters and formulate the average actual count of the characters to a page, then draw appropriate inferences from there.[24]

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three extra leaves added. Those willing to discard the Epistle to Titus can find place in the manuscript for the two Epistles to Timothy by assuming all pages crowded to the utmost limit and one page added."

[21] For instance, Finegan, "Original Form," 93; Grant, *Historical Introduction*, 210; D. Guthrie, *The Pastoral Epistles-TNTC* (London: Tyndale, 1957), 13-14; Jeremias, *Briefe*, 4; J.N.D. Kelly, *A Commentary on the Pastoral Epistles* (London: A. & C. Black, 1963), 4; and Giversen, "The Pauline Epistles on Papyrus," 211; among others.

[22] See also Parker, *NT Manuscripts and Their Texts*, 253-54.

[23] See also pertinent questions on Duff's graph raised by Epp, "Issues in the Interrelation," 613-16.

[24] Parker, *NT Manuscript and Their Texts*, 253, rightly noted, "In order to establish Duff's claim that this greater number of letters results from the scribe's realising that he was running out of space, it would also be necessary to ensure that this was not a feature of other single-quire codices and unrelated to problems associated with fitting the text into the available space."

Second, *analogically*, Duff's comparison of $\mathfrak{P}^{46}$'s hypothetical extra quire with that of Origen's and the Nag Hammadi codices is a bit off the mark, because their text-historical circumstances are in no way similar.[25] In fact, even before one can assume additional appended leaves, one must establish beyond doubt first that the Pastorals were indeed the books that our scribe intended to put onto (the missing pages of) his codex—an undertaking that would be highly speculative in this instance due to the absence of surviving evidence from the material itself. In fact, Duff's claim that the Pastorals are the "natural first choice"[26] is not only speculative but also circular. It is also untrue that there is no "evidence to the contrary", as Epp has already identified codices containing *both* canonical and non-canonical books.[27]

Third, Duff's "increased input = intention" theory rests solely on the consideration of $\mathfrak{P}^{46}$'s *text*—it is a very *text*-focused argument. Whilst this is not unwarranted, it must be viewed as incomplete. In fact, the increasing trend *alone* does not categorically prove anything about the scribe's intention to fill in the missing pages with the Pastorals and Philemon—other paratextual features *in* the manuscript must equally be taken into account if this proposal is to be sustained. These features help in resolving the question whether there was really a *programmatic increase* in the latter part of the codex. Accordingly, Duff's "increased input = intention" theory must, by necessity, operate on the assumption that every bit of space in the manuscript is valuable, and therefore should not be wasted. As such, this can be done in two ways: a) *maximizing space use* by increasing

[25] Parker, *NT Manuscript and Their Texts*, 253, convincingly refuted Duff's use of these two manuscripts to draw an analogy for a hypothetical additional leaves at the end of $\mathfrak{P}^{46}$.

[26] Duff, "$\mathfrak{P}^{46}$ and the Pastorals," 585, "If we look for material that the scribe may have wanted to put in the codex but would not have quite fitted in, the natural first choice is the rest of the letters accepted by him as Pauline: in the absence of evidence to the contrary, this is likely to mean the thirteen canonical Pauline epistles and Hebrews."

[27] Epp, "Issues in the Interrelation," 617-18, cited $\mathfrak{P}^{72}$, codex Sinaiticus, and Codex Alexandrinus as some of the codices containing non-canonical materials also.

letter density (e.g., additional lines per page), and b) *gaining space* by minimizing space requirements for the *inscriptio* and *subscriptio*. Unfortunately, Duff dealt with the former only, presumably because of his text-focused methodology, to carry his whole argument. However, $\mathfrak{P}^{46}$'s *paratextual* features point in another direction. Features such as book titles, script size and spacing, and page-by-page line profile must be examined also in order to validate the proposal that the scribe was indeed systematically crowding his codex with texts and was scrounging every available space he could muster.

C. Codicological Givens

Furthermore, some codicological givens that have bearing on the discussion of space calculation in the missing pages of $\mathfrak{P}^{46}$ must also be taken into consideration:

a. Closely examining its extant pages, $\mathfrak{P}^{46}$ betrays its status as a single quire codex manuscript, and in this kind of quire formation the beginning and ending pages are wider than the middle pages (due to trimming, to avoid protrusions), and therefore are capable of carrying more text.[28] This is unequivocally true in the case of $\mathfrak{P}^{46}$;[29]
b. In a codex, right-hand pages can accommodate more characters than the left-hand; the folding in the middle can affect the writing hand when it is moving from the extreme left side of the left-hand page towards the middle, but moving away from the middle folding gives more flexibility for the writing hand to inscribe more characters. With some exceptions,[30] this is generally true with $\mathfrak{P}^{46}$;
c. With some exceptions, there is a clear general scribal attempt in the extant pages of our codex to keep within a range the number of characters to a page. As such, with some level of confidence (but equally cognizant of the requirements of individual script formation),[31] we can make informed calculations as to how many characters were put in by the original hand per line. This is very helpful when we try to reconstruct the missing pages and eroded portions of extant $\mathfrak{P}^{46}$ leaves.

III. THE PASTORALS AND THE SCRIBAL HABITS OF $\mathfrak{P}^{46}$

A. Testing Duff's Theory in light of the Paratextual Features of $\mathfrak{P}^{46}$

As noted above, Duff's argument largely hinges on the *textual* feature of $\mathfrak{P}^{46}$. He operated on the assumption that the increasing number of characters in the latter half of this

[28] Also pointed out by Epp, "Issues in the Interrelation," 611.

[29] See our detailed discussion of this codicological feature in pp. 87-109.

[30] In 29 instances, the left-hand pages have more characters than the right-hand: $F14^{rls}$-$f15^{vrs}$, $f18^{rls}$-$f19^{vrs}$, $f25^{rls}$-$f26^{vrs}$, $f28^{rls}$-$f29^{vrs}$, $f29^{rls}$-$f30^{vrs}$, $f31^{rls}$-$f32^{vrs}$, $f32^{rls}$-$f33^{vrs}$, $f33^{rls}$-$f34^{vrs}$, $f34^{rls}$-$f35^{vrs}$, $f36^{rls}$-$f37^{vrs}$, $f37^{rls}$-$f38^{vrs}$, $f38^{rls}$-$f39^{vrs}$, $f39^{rls}$-$f40^{vrs}$, $f41^{rls}$-$f42^{vrs}$, $f42^{rls}$-$f43^{vrs}$, $f44^{rls}$-$f45^{vrs}$, $f47^{rls}$-$f48^{vrs}$, $f48^{rls}$-$f49^{vrs}$, $f50^{rls}$-$f51^{vrs}$, $f51^{rls}$-$f52^{vrs}$, $f53^{vls}$-$f54^{rrs}$, $f54^{vls}$-$f55^{rrs}$, $f57^{vls}$-$f58^{rrs}$, $f59^{vls}$-$f60^{rrs}$, $f66^{vls}$-$f67^{rrs}$, $f67^{vls}$-$f68^{rrs}$, $f70^{vls}$-$f71^{rrs}$, $f76^{vls}$-$f77^{rrs}$, and $f85^{vls}$-$f86^{rrs}$.

[31] On this, see our discussion of line lengths in pp. 114-18.

codex points to the *intention* of the scribe to include the Pastorals and Philemon, i.e., having reached the middle-point of the codex the scribe realized that he would be running out of space if he continued his 26-28 lines/per page evident in the first half of the codex. A cursory look at Duff's chart seems to support this. But is this argument sustainable? Did the scribe really maximize the space available to him? A closer look at the *paratextual* features of $\mathfrak{P}^{46}$ seems to point to another direction, particularly the script and spacing measurements, book titles, and number of lines per page.

1. *Script Size and Spacing*

If one compares the number of characters in the second half of the codex against the first, one can definitely see an increase (although I would rather call it a *fluctuating increase*, especially if one looks at individual pages rather than averaging a certain number of pages at determined intervals). However, basic to the determination of increasing letter density is the use of its scripts and spacing. If our scribe really *intended* to squeeze in as many characters as possible onto the codex, as argued by Duff,[32] in order to arrest an impending problem of lack of space, then he should have consciously considered every available space important and utilised it with extreme care (i.e., space economy). However, this does not seem to be the case with $\mathfrak{P}^{46}$. Despite the second-half line increase, the scribe does not really seem to have been intent on maximizing his space. The sizes of the script and line spacing, in fact, appear to remain constant. A comparison between f08^{r} and f97^{v} (the second and penultimate extant leaves) bears out this point; the script sizes are constant between 0.3-0.35 cm and the interlinear spacing at 0.4-0.5 cm (Fig. 3-5.1, next page).[33]

[32] Duff, "$\mathfrak{P}^{46}$ and the Pastorals," 584.

[33] The constancy of the space in between lines was also observed by T.C. Skeat, "Did Paul write to the 'Bishops and Deacons' at Philippi," 258, n3.

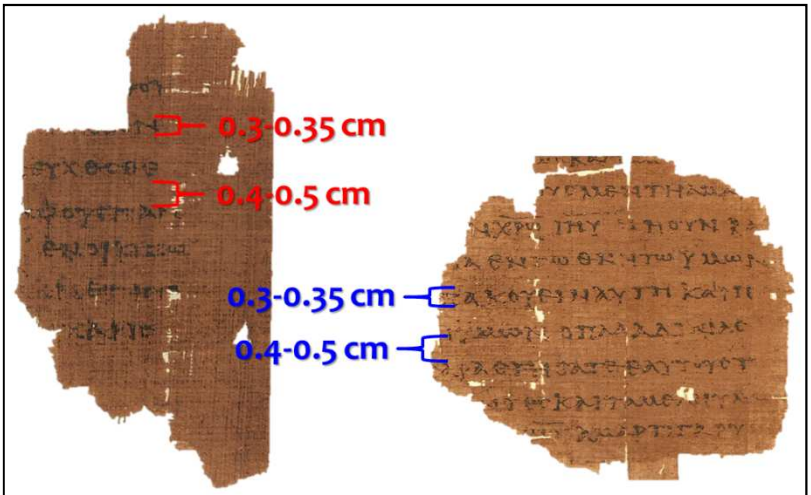


Figure 3-5.1
Increase in line numbers, constancy in script size and spacing

But could the scribe write in an even smaller size? The answer lies in the scattered evidence throughout the codex, where the smallest characters, especially *omicron* and *sigma*,[34] were written in 0.1 cm, and yet still readable and beautifully formed (see Fig. 3-5.2). Despite this capability, the scribe obviously did not opt to fully use this script size option available to him—a fact that does not point to space economy.

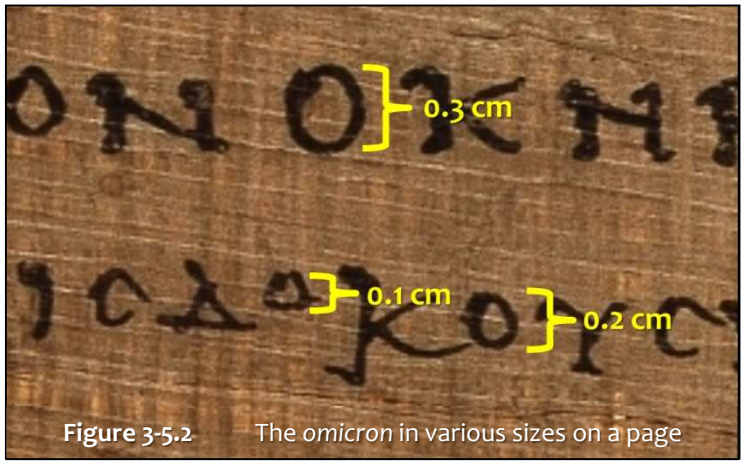


Figure 3-5.2 The *omicron* in various sizes on a page

2. *Tale from the Titles (Τιτλοι)*

The cumulative space computation for the transitional pages with τιτλος, as well as the lone extant *subscriptio* for 2Cor,[35] also has its own tale to tell. Table 3-E3,[36] showing the

[34] Palaeographically speaking, the *omicron* and *sigma* in $\mathfrak{P}^{46}$ could be easily written in smaller sizes since their formation is quite simple, and their cusps meet at upper left side.

[35] It might be possible also that in its original state, f60^{v} contained the subscription to 1Cor, since there is still sufficient space left on the page. The present page is about 22 cm but only 2 lines needed to

measurements for all the extant τιτλος and the 2Cor *subscriptio*, illustrates that the cumulative total amount of space covered by all these is about 29.6 cm, corresponding to 45 lines (or about 1 1/3 page). Accordingly, it seems that in our codex a book title, without losing its aesthetic transitional function, could be appended in just three lines (as in Heb, and perhaps 2Cor and Eph as well); beyond that number excess lines are already "wasted spaces". Hence, to append all the extant book titles the scribe would have only needed 24 lines. But since the increase has been noted at midpoint of the codex,[37] we must deduct the space covered in the first half, which is at 5.8 cm or 8 lines respectively. This means that by occupying more space for the τιτλοι and *subscriptio* than what is required, the scribe has lost 13 precious lines or more than 1/3 of a page!

TABLE 3-E3
BOOK TITLES AND SUBSCRIPTION MEASUREMENTS IN $\mathfrak{P}^{46}$

BOOK TITLES/ SUBSCRIPTION	MEASUREMENT (in cm)	CORRESPONDING # OF LINES IN CONTEXT	REMARKS
ROM			Not extant
HEB	2.5	3	
1COR	3.3	5	
2COR	3	4	From the Page #
2COR (Subs)	9.7	10	Subscription
EPH	2	3	From the Page #
GAL	2.6	4	
PHIL	4	6	
COL	2.9	5	
1THESS.	3.5	5	Fragmentary
TOTALS	**29.6**	**45**	

be reconstructed, which in context requires only about 1.2 cm. Assuming Kenyon's measurement that the page might have been 28 cm throughout the codex, then this page still has about 5 cm available.

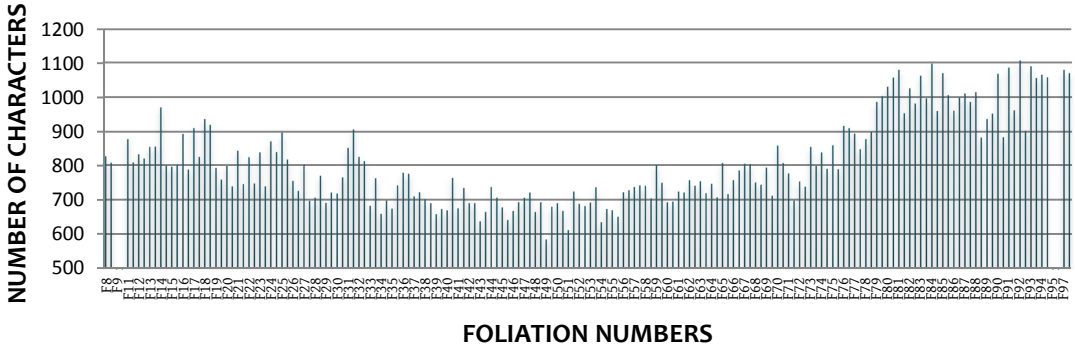
[36] See also Appendix I—Extant Book Titles and *Subscriptio* Measurement Profile, for a visual graph on how many line/s corresponds to a particular book title size.

[37] The midpoint is at 1Cor 11.4; therefore this only covers book titles for Heb and 1Cor (book title for Rom is not extant).

3. *Increasing(?) Number of Lines*[38]

Duff's "increase input = intention" theory largely rests its fate on what he calls "relatively steady"[39] increase in the number of lines to a page in the second half of our codex. He also rejects the view that this increase is codicologically explicable, i.e., outside pages are wider than the middle pages. But whilst the increase is not doubted, the way Duff factored this in is dubious. In fact, contrary to his claim, it works against his argument, for there is incontrovertible evidence that this increase is *not* steady (systematic), belying an intention or motive to put in more text in any way the scribe could. This non-programmatic increase has until now been unnoticed because the chart provided by Duff profiles only the number of characters in five-page intervals. However, the real picture is unveiled when we profile 𝔓[46]'s character input page-by-page (Table 3-E4).

TABLE 3-E4 PAGE-BY-PAGE CHARACTER PROFILE OF P[46]

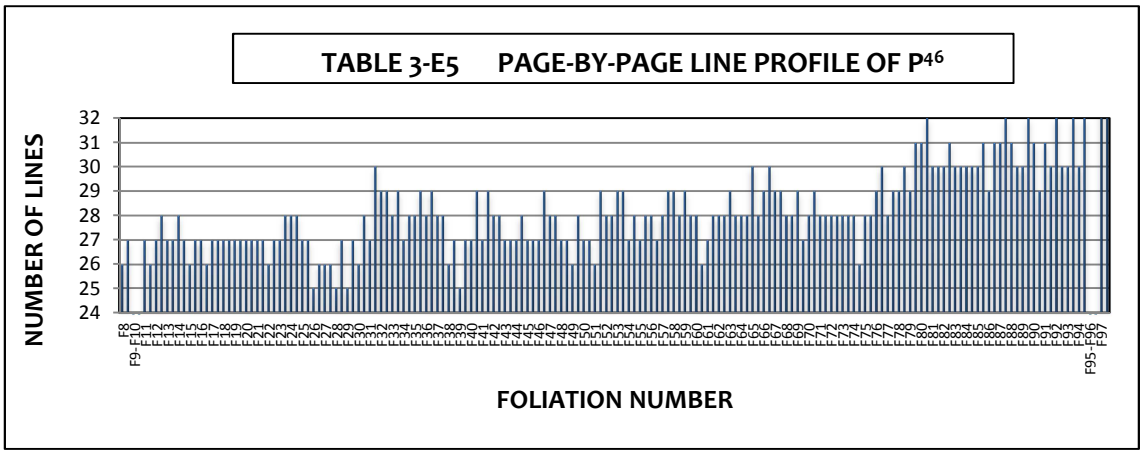


This chart says a lot to the contrary, clearly betraying a roller-coaster character of copying, not only in the latter part but *also* in the first part of the codex. Although the numbers are different, the similar fluctuating pattern is incontestable. The sudden increase (closing in to 1000 characters to a page) started in f79, but after

[38] The summary results presented in this section are derived from a more detailed analysis documented in Appendix J—Summary Chart of Character Count per Page.

[39] Duff, "𝔓[46] and the Pastorals," 584.

only a few pages slid down again to about 900 characters to a page, and then the increase-decrease has become erratic, almost pattern-less! This becomes even more illuminating when we look at the scribe's page-by-page line input (Table 3-E5).



In the first half of the codex, the scribe was putting in an average of 26-29[40] lines to a page. After the centrefold he started copying 27-29 to a page until f64^{r}, and then 28-32 for the rest of the extant pages. Statistically-speaking, this seems to indicate a scenario of systematic increase. Furthermore, since we have assumed that in this codex right-hand pages can carry more text than the left-hand pages, instances where this pattern is reversed would seem to corroborate further a scenario of an "increasing trend". In fact, this "reversed pattern" (i.e., left-hand > right-hand) seems to be the case in 37 openings.[41] But do these figures really indicate a "relatively steady increase"? Was the scribe really "steadily fitting more text on each page"?[42]

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[40] Exemptions to this include f26^{v}, f29^{v}, and f39^{v} where they all have 25 lines to a page.

[41] F12^{rls}-f13^{vrs}(28 > 27), f14^{rls}-f15^{vrs} (27 > 26), f21^{rls}-f22^{vrs} (27 > 26), f24^{rls}-f25^{vrs} (28 > 27), f25^{rls}-f26^{vrs} (27 > 25), f28^{rls}-f29^{vrs} (27 > 25), f29^{rls}-f30^{vrs} (27 > 26), f30^{rls}-f31^{vrs} (28 > 27), f31^{rls}-f32^{vrs} (30 > 29), f33^{rls}-f34^{vrs} (29 > 27), f35^{rls}-f36^{vrs} (29 > 28), f36^{rls}-f37^{vrs} (29 > 28), f37^{rls}-f38^{vrs} (28 > 26), f38^{rls}-f39^{vrs} (27 > 25), f40^{rls}-f41^{rrs} (29 > 27), f41^{rls}-f42^{vrs} (29 > 28), f42^{rls}-f43^{vrs} (28 > 27), f44^{rls}-f45^{vrs} (28 > 27), f46^{rls}-f47^{vrs} (29 > 28), f47^{rls}-f48^{vrs} (28 > 27), f48^{rls}-f49^{vrs} (27 > 26), f49^{rls}-f50^{vrs} (28 > 27), f50^{rls}-f51^{vrs} (27 > 26), f51^{rls}-f52^{vrs} (29 > 28), f53^{vls}-f54^{rrs} (29 > 27), f54^{vls}-f55^{rrs} (28 > 27), f66^{vls}-f67^{rrs} (30 > 29), f67^{vls}-f68^{rrs} (29 > 28), f70^{vls}-f71^{rrs} (29 > 28), f76^{vls}-f77^{rrs} (30 > 28), f78^{vls}-f79^{rrs} (30 > 29), f80^{vls}-f81^{rrs} (32 > 30), f82^{vls}-f83^{rrs} (31 > 30), f85^{rls}-f86^{vrs} (31 > 29), f87^{vls}-f88^{rrs} (32 > 31), and f89^{vls}-f90^{rrs} (32 > 31).

[42] Duff, "$\mathfrak{P}^{46}$ and the Pastorals," 585.

The answer must be a negative one. Conversely, these figures point more to our scribe's copying idiosyncrasies since more lines in the left-hand pages do not necessarily translate to higher character density. In fact, of these 37 instances only 12 came after the midpoint.[43] One would expect naturally that in these 37 instances more lines mean more characters, as is generally true in the first half of the codex for this "reversed pattern" scenario.[44] Yet this expectation is reversed in the second half. Not only is the number of reversed patterns smaller, this expectation is also frustrated by the fact that although in seven cases there are more lines in the left-hand pages yet the total character input is ironically way below than that of the right-hand![45] For instance, whilst $f78^{vls}$ has one line more than $f79^{rrs}$ (i.e., 30 > 29), yet $f79^{rrs}$ has 987 characters whereas 900 only for $f78^{vls}$![46] Conversely, even if the left-hand page has more lines than the right-hand,[47] the difference in character density is not always very substantial. For instance, even if $f76^{vls}$ has two lines more than $f77^{rrs}$ (with 28), yet the difference is only about 16 characters (911 > 895)! Or as in the case of $f54^{vls}$ and $f55^{rrs}$, where we have 28 > 27 lines respectively, the difference is only four characters (673 > 669)![48]

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[43] $F53^{vls}$-$f54^{rrs}$, $f54^{vls}$-$f55^{rrs}$, $f66^{vls}$-$f67^{rrs}$, $f67^{vls}$-$f68^{rrs}$, $f70^{vls}$-$f71^{rrs}$, $f76^{vls}$-$f77^{rrs}$, $f78^{vls}$-$f79^{rrs}$, $f80^{vls}$-$f81^{rrs}$, $f82^{vls}$-$f83^{rrs}$, $f85^{rls}$-$f86^{vrs}$, $f87^{vls}$-$f88^{rrs}$, and $f89^{vls}$-$f90^{rrs}$.

[44] That is, in 17 out of 25 instances of "reversed pattern" (i.e., left-hand > right-hand), the left-hand pages have more lines and more characters than the left-hand pages. The eight anomalies include $f12^{rls}$-$f13^{vrs}$, $f21^{rls}$-$f22^{vrs}$, $f24^{rls}$-$f25^{vrs}$, $f27^{rls}$-$f28^{vrs}$, $f30^{rls}$-$f31^{vrs}$, $f35^{rls}$-$f36^{vrs}$, $f46^{rls}$-$f47^{vrs}$, and $f49^{rls}$-$f50^{vrs}$.

[45] In $f66^{vls}$-$f67^{rrs}$, $f78^{vls}$-$f79^{rrs}$, $f80^{vls}$-$f81^{rrs}$, $f82^{vls}$-$f83^{rrs}$, $f87^{vls}$-$f88^{vrs}$, $f89^{vls}$-$f90^{rrs}$, and $f93^{vls}$-$f94^{rrs}$, where the left-hand pages have nine more lines, the sum total character input of the left-hand pages against the right-hand is 6,720 against 7,082, or a difference of 362 characters. Conversely, in the five cases ($f53^{vls}$-$f54^{rrs}$, $f54^{vls}$-$f55^{rrs}$, $f67^{vls}$-$f68^{rrs}$, $f70^{vls}$-$f71^{rrs}$, and $f76^{vls}$-$f77^{rrs}$) where the left-hand page has seven more lines and more characters, the difference is 205 only.

[46] This is also true for $f66^{vls}$ > $f67^{rrs}$ (785 < 805), $f82^{vls}$ > $f83^{rrs}$ (982 < 1063), $f87^{vls}$ > $f88^{rrs}$ (987 < 1015), and $f89^{vls}$ > $f90^{rrs}$ (953 < 1068), and possibly $f92^{vls}$ > $f93^{rrs}$ (903 < 1090).

[47] For instance, $f53^{vls}$ > $f54^{rrs}$ (736 > 635), $f67^{vls}$ > $f68^{rrs}$ (804 > 750), $f70^{vls}$ > $f71^{rrs}$ (806 > 776), and $f85^{vls}$ > $f86^{rrs}$ (1006 > 961).

[48] Cf. $f62^{vls}$-$f63^{rrs}$ where the right-hand page has one line more than the left-hand (18 < 29), yet the difference is only 13 characters (741 < 754).

In cases where both pages of an opening have equal number of lines, one might anticipate that the character density would be about the same, as in the case of f57^{vls}—f58^{rrs} (29 lines apiece) where the difference is only one letter (i.e., 742 > 741). This is not always true, however, and we can cite the example of f55^{vls}—f56^{rrs} where both pages have 28 lines, respectively. In this opening, f55^{vls} has about 651 characters whereas 722 for f56^{rrs}, or a difference of about 71 characters! Even more surprising is f72^{vls}—f73^{rrs} where the right-hand page has 118 characters more than the left-hand page, even though both have 28 lines apiece!

Contrary to Duff's claim, all these figures indicate that our scribe is not "steadily fitting more text on each page". Whilst the scribe is copying more lines he was not actually copying more characters systematically—putting a big question mark as to whether the scribe was really intent on maximizing the space available to him.

4. Variations affecting text length

Compared against the NA^{28}-UBS^{4} text, variations in $\mathfrak{P}^{46}$ could be classified as orthographic, nonsense, replacement, grammatical, addition, and omission variants. For our present purpose, I shall focus only on the last two types as well as variations involving *nomina sacra*, which all cumulatively affect text length.[49]

a. Variations Lengthening the Text

Throughout $\mathfrak{P}^{46}$ addition variants occur in 148 instances, involving 159 words. If we translate this into the number of characters, the net total of characters added is 517, the breakdown of which per book is shown in Table 3-E6. This covers only the extant portions, but it is not unreasonable to assume that addition variants also occurred in

[49] Actually, orthographic variations, particularly those that are *itacistic* in nature also affect text length. But we have excluded this category for the meantime since it reflects more the linguistic aspect of the text than the copying tendencies of our scribe.

the missing pages. Admittedly, the critical question is how to locate these additions in the missing pages, but it seems that the least arbitrary methodology is to look for the average projected number of additions in the missing pages. Hence, if there are 517 added characters in the extant pages, we can divide that by the number of extant pages (which is at 172 pages). This yields an average quotient of about three (3) letters added per page. If we then multiply this with the hypothetical number of missing pages (which is 14), we then have a projected total average of 42 added letters in the supposed 14 missing pages or about 1.2 lines!

TABLE 3-E6 TABLE OF ADDITIONS										
	ROM	HEB	1COR	2COR	EPH	GAL	PHIL	COL	1THESS	TOTALS
# OF INSTANCES										
	30	26	31	22	9	5	18	7	0	148
# OF WORDS										
	33	26	33	22	9	5	23	8	0	159
# OF LETTERS										
	104	73	115	62	23	14	104	22	0	517

b. Variations shortening the Text

The case for omissions is more pronounced than additions. These can be divided into two types: grammatical[50] and incremental[51] omissions. Throughout the extant pages of $\mathfrak{P}^{46}$, grammatical omissions can be located in 363 instances, involving 396 word units. Translated into character count, this corresponds to 1,176 letters lost (see Table 3-E7, next page).

[50] This involves (accidental) omissions of nouns, pronouns, verbs (*cum* participles and infinitives), adverbs, adjectives, particles, prepositions, and conjunctions, which do not significantly affect the sense (quality) of the text.

[51] That is, accidental omissions, either due to the scribe or to the scribe of his *exemplar*, dramatically affecting the sense (quality) and length (quantity) of text because of *parablepses*, i.e., due to *homoioteleuton* or *homoioarcton*; on these categories, see Robert Markham, "The Critical Apparatus: A Symposium on the Bible Societies' Greek New Testament," *TBT* 18 (1967): 3-11, pp. 6, 11. For more detailed discussions, see pp. 267-73.

TABLE 3-E7 TABLE OF GRAMMATICAL OMISSIONS										
	ROM	HEB	1COR	2COR	EPH	GAL	PHIL	COL	1THESS	TOTALS
# OF INSTANCES										
	59	75	87	56	23	30	18	14	1	363
# OF WORDS										
	67	81	93	59	26	35	19	15	1	396
# OF LETTERS										
	217	267	222	167	86	106	60	48	3	1,176

Incremental omissions speak volumes about our scribe's quality of copying. Hence, a few observations are in order. First, these omissions are clearly *accidental* in nature, resulting from *parablepses*, where the eyes of the scribe inadvertently leaped forward due to similar letter or group of letters, or similar letter formation. Furthermore, these copying accidents have transpired at various points in the codex, and therefore cannot be categorically attributed to an intention to shorten the text—the scribe simply blundered in copying his *exemplar*.[52] There are 70 instances of accidental incremental omissions. Most of the *parablepses* occurred in the longer books (Rom-2Cor), where at least 1,066 characters were accidentally lost, whilst a loss of at least 336 characters for the shorter epistles (Eph-Col; the fragmentary state of the 1Thess prohibits any investigation of this nature). Second, collating against the NA^{28}-UBS^{4} common text, our scribe lost 289 words or a net total of 1,402 characters (see Table 3-E8), which cumulatively can account for more than a page in $\mathfrak{P}^{46}$! But what is noteworthy for our present purpose is the fact that almost two thirds (or 848) of the 1,402 characters, or 41 out of the 70 extant cases, were committed after the scribe reached the mid-portion of the codex. Translated into ratio, it means that in the 172 extant pages, the scribe omitted an average of eight characters per page due to this type of omissions.

[52] Note, however, that we will argue later that some of these incremental omissions may have been present in the *exemplar* already, which our scribe simply copied; see pp. 272-73.

TABLE 3.E8 TABLE OF INCREMENTAL OMISSIONS										
	ROM	HEB	1COR	2COR	EPH	GAL	PHIL	COL	1THESS	TOTALS
# OF INSTANCES										
	9	12	14	12	10	5	5	3	0	70
# OF WORDS										
	13	58	63	81	26	15	14	19	0	289
# OF LETTERS										
	80	281	303	402	117	66	71	82	0	1402

If we now combine the grammatical and incremental omissions in $\mathfrak{P}^{46}$, we then have a total of 2,578 characters lost (Table 3-E9). Dividing this by the total number of extant pages (at 172), we then have an average quotient of 15 letters lost per page due to accidental omissions. And if we take this figure as the averaging indicator for the supposed missing pages (at 14 pages), the total projected number of lost characters in 14 pages will be 210—or corresponding to at least 7 lines! But whilst this is a loss in text, this actually means a space gain for our scribe!

TABLE 3-E9 TOTALS OF COMBINED OMISSIONS IN $\mathfrak{P}^{46}$										
	ROM	HEB	1COR	2COR	EPH	GAL	PHIL	COL	1THESS	TOTALS
Combined Totals of Omissions (Instances)										
	68	87	101	68	33	35	23	17	1	433
Combined Totals of Omitted Words										
	80	139	156	140	52	50	33	34	1	685
Combined Totals of Omitted Letters										
	297	548	525	569	203	172	131	130	3	2,578

c. Nomina Sacra Contractions

Another feature in the codex that shortens the text is the scribe's use of the *nomina sacra*. Needless to say, this system of contracting divine names, especially the ones whose forms have been consistently established in the extant pages, should be taken into account when we compute the amount of text in the missing pages. Out of the traditional 15 contractive forms for divine names, 9 have been used in the extant pages of $\mathfrak{P}^{46}$: θεος, χριστος, ιησους, κυριος, πνευμα, πατηρ, υιος, σταυρος, and

ανθρωπος.[53] Table 3-E10 shows that in the NA[28]-UBS[4] text comprising the books of 2 Thessalonians, 1 and 2 Timothy, Titus, and Philemon, there is an expected 242 occurrences of these 9 contractive forms.[54]

TABLE 3-E10 PROJECTED LIST OF *NS* IN THE MISSING PAGES						
	2THESS	1TIM	2TIM	TIT	PHILM	TOTALS
ΘΕΟC	18	23	13	14	2	70
ΧΡΙCΤΟC	10	14	13	4	8	49
ΙΗCΟΥC	13	12	13	4	6	48
ΚΥΡΙΟC	23	6	15	1	5	50
ΠΝΕΥΜΑ	3	2	3	2	1	11
ΠΑΤΗΡ	3	2	1	1	1	8
ΥΙΟC	1	0	0	0	0	1
CΤΑΥΡΟC	0	0	0	0	0	0
ΑΝΘΡΩΠΟC	1	2	1	1	0	5
SUM TOTAL						242

If, however, we attempt to determine how much abbreviating could have been done with these 242, we will get a more vivid picture. Combining all the occurrences of the *nomina sacra* in the various grammatical forms, and therefore in their actual contractive forms, the sum total of 1,324 characters could therefore be reduced to 649 in 𝔓[46]! (See Table 3-E11).

TABLE 3-E11 PROJECTED LIST OF *NOMINA SACRA* PER GRAMMATICAL FORM						
	NOM	GEN	DAT	ACCUS	VOC	TOTALS
ΘΕΟC	44 to 22	176 to 88	44 to 22	12 to 6	0	280 to 140
ΧΡΙCΤΟC	28 to 12	196 to 84	90 to 45	14 to 6	0	328 to 147
ΙΗCΟΥC	18 to 9	150 to 90	65 to 39	12 to 6	0	245 to 144
ΚΥΡΙΟC	102 to 34	150 to 90	30 to 12	12 to 4	0	294 to 140
ΠΝΕΥΜΑ	12 to 6	63 to 21	16 to 6	0	0	91 to 33
ΠΑΤΗΡ	5 to 3	30 to 15	5 to 3	6 to 3	0	46 to 24
ΥΙΟC	4 to 3	0	0	0	0	4 to 3
CΤΑΥΡΟC	0	0	0	0	0	0
ΑΝΘΡΩΠΟC	32 to 16	0	0	0	8 to 4	40 to 20
SUM TOTAL						1,328 to 651

[53] On how the *nomina sacra* system was used in 𝔓[46], see pp. 323-66. For the complete list of all the *nomina sacra* in 𝔓[46], arranged according to their grammatical forms and their locations, see Appendix P-1, pp. 829-54.

[54] For a list of these projected occurrences, see Appendix P-2, pp. 855-63.

5. Implications

What then are the implications of all these mathematical computations? First, in regard to the addition variants, the expected total average of 42 added letters for the supposed 14 pages is comparatively insignificant for this only translates to about 1.2 lines. On the other hand, omission variants and the *nomina sacra* significantly affect the quantity of text in the missing pages. This can be illustrated in the following equation:

2,591 (combined totals of grammatical and incremental omissions)
÷ 172 (number of extant pages)

= 15 (average number of letters lost per page)
x 14 (number of supposed missing pages)

= 210 (projected number of letters lost in 14 pages)
+ 651 (projected reduction due to *nomina sacra*)

= 861 letters off (but actually a gain of at least ¾ page!)
+ 14 pages (supposed number of missing pages)

= 14 ¾ pages available to the scribe!

What this equation implies is that after considering both the textual and paratextual features of this codex, there is theoretically more space than has been supposed in the earlier studies! In fact, character loss due to accidental omissions and *nomina sacra* is theoretically a space gain for the scribe!

Second, a prior question now needs to be raised in this regard—How much space is needed to accommodate the texts of 2 Thessalonians, the Pastorals, and Philemon? Stated differently, is the 14 ¾ pages enough to accommodate the cumulative texts of these epistles? Inherent to this question is a methodological one—How should the required space for these epistles be computed? Table 3-E12

reveals that the projected number of characters for these epistles is about 24,415.[55] With this figure in mind, how should we calculate the number of characters that can be fitted into each of these 14 ¾ pages? Obviously, the easiest way is to divide this number by 14 ¾ pages, yielding a result of 1,655 per page! But is this methodologically sustainable? I think not.

TABLE 3-E12 PROJECTED # OF CHARACTERS FOR 2THESS, THE PASTORALS, AND PHILEMON	
BOOK	**TOTAL # OF CHARACTERS**
2Thess 1.9-3.18	3,294
1Tim	8,857
2Tim	6,526
Tit	3,723
Philm	1,565
Titloi	450
TOTAL	24,415

Methodologically, the most logical thing to do in this instance is to average the actual performance rate of the scribe's copying activity in the extant pages closest to the missing portions. This has the advantage of coming up with a more realistic copying average in terms of actual text-input. Table 3-E13 (next page) shows the figures based on different page-variables.

[55] This figure is only an approximation, but nonetheless it is inclusive of possible variables that might affect text-length, e.g., *itacism* (for the most consistent ones), contracted *nomina sacra*, and book titles.

TABLE 3-E13 CHART SHOWING VARIOUS LEAF AVERAGES IN THE LAST EXTANT PAGES OF 𝔓[46]						
	22-page Average	16-page Average	10-page Average	6-page Average	4-page Average	2-page Average
F85[r]	1070					
F85[v]	1006					
F86[r]	961					
F86[v]	1000					
F87[r]	1011					
F87[v]	987					
F88[r]	1015	1015				
F88[v]	883	883				
F89[r]	937	937				
F89[v]	953	953				
F90[r]	1068	1068				
F90[v]	884	884				
F91[r]	1086	1086	1086			
F91[v]	962	962	962			
F92[r]	1107	1107	1107			
F92[v]	903	903	903			
F93[r]	1090	1090	1090	1090		
F93[v]	1056	1056	1056	1056		
F94[r]	1065	1065	1065	1065	1065	
F94[v]	1058	1058	1058	1058	1058	
F95[r]-F96	x	x	x	x	x	x
F97[r]	1079	1079	1079	1079	1079	1079
F97[v]	1070	1070	1070	1070	1070	1070
TOTALS	**22251**	**16216**	**10476**	**6418**	**4272**	**2149**
AVERAGE # OF CHARACTERS/PAGE	1012/p	1015/p	1050/p	1070/p	1068/p	1075/p

This chart makes evident that character increase in the last 26 pages is, as mentioned earlier, a fluctuating increase if we consider the actual total text-input per page. This also shows that Duff's proposed 1,050/page is based on the 10-page average, which I think is a reasonable proposal. We shall now use these various averages to see how many pages are required for the texts of the missing epistles.

TABLE 3-E14 GRIT SHOWING VARIOUS PAGE REQUIREMENTS							
BOOKS	**TOTAL # OF LETTERS REQUIRED**	**NUMBER OF PAGES REQUIRED (Totals ÷ [average] characters per page)**					
		1,012/p	**1,015/p**	**1,050/p**	**1,070/p**	**1,068/p**	**1,075/p**
2Thess	3,294	3.25	3.24	3.14	3.07	3.08	3.0
1Tim	8,857	8.75	8.73	8.43	8.27	8.29	8.23
2Tim	6,526	6.45	6.43	6.22	6.09	6.11	6.07
Titus	3,723	3.68	3.67	3.55	3.47	3.48	3.46
Philm	1,565	1.55	1.54	1.49	1.46	1.47	1.46
Book Titles	450	0.44	0.44	0.43	0.42	0.42	0.42
TOTALS	24,415	24.12	24.05	23.27	22.78	22.85	22.64

What Table 3-E14 clearly shows is that the total number of characters in the cumulative texts of the aforementioned epistles cannot possibly fit into the available amount of space (i.e., 14 ¾ pages), even if we consider the various scribal copying habits that can gain space for the scribe (i.e., text reduction due to *nomina sacra* and accidental omissions), and even if we take the highest text-input average at 1075/page. The space available is simply not enough—these combined texts need between 22-24 pages! This unambiguously confirms the opinion of earlier studies on the space requirement. And perhaps Kenyon's statement must now be considered the final word on the matter when he said, "The space required is... nearly twice as much as is available, and by no compression of writing is it conceivable that these four Epistles could have been included."[56]

That 2Thess was part of this codex can be inferred on the following grounds. First, the manuscript tradition does not seem to provide any evidence that the two letters to the Thessalonians circulated independently of each other. Second, the amount of available space in the missing pages can easily accommodate the projected number of characters for 2Thess. Third is the suggestive evidence from the τιτλος of 1Thess. Although not fully extant, one can still observe an ink residue (of the same colour) at the upper right portion close to the final **ϲ** (Fig. 3-5.3, lower image). It is possible that this has been the left tip of a decorative horizontal bar usually placed above and below certain characters in the τιτλος. However, the upper image seems to indicate, unlike the other extant τιτλοι, that this book title was not ornamented; the pattern in the extant τιτλοι has been to mark the first letter of the preposition with decorative bars (see p. 179, n75), which we do not see in this case. The only other possibility is that this ink-spot was part

[56] Kenyon, *CBBPIII-1936*, xi.

of the upper ductus of the oblique stroke of the sequence designator ⲁ. Either way, this implies that there was originally a character in this portion of the τιτλος, and it seems to me that the designator ⲁ is the most logical candidate. Cumulatively, these all suggest that there was ΠΡΟC ΘΕCCΑΛΟΝΙΚΕΙC Β in this codex.

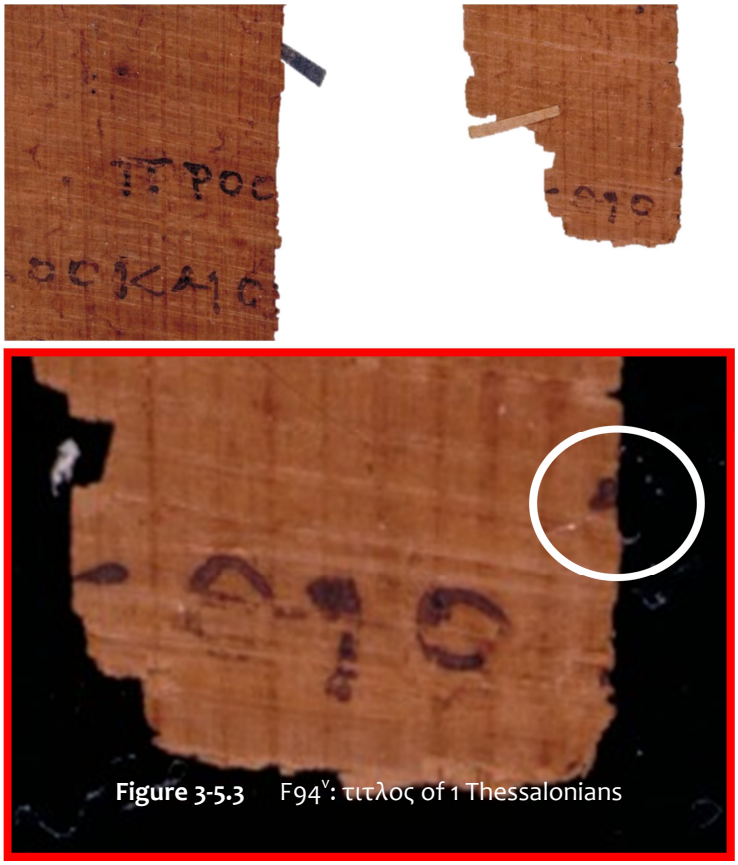


Figure 3-5.3 F94ᵛ: τιτλος of 1 Thessalonians

But have we asked all the pertinent questions already? Or have we failed to ask the most basic (but perhaps the most crucial) question of all: "How many pages are *really* missing based on the actual evidence?"

IV. NEW QUESTIONS ABOUT THE QUESTION OF SPACE REQUIREMENT

Did $\mathfrak{P}^{46}$ really lose seven sheets (=28 pages)? Since its initial publication in 1934 (and in the subsequent Sanders-1935 and Kenyon-1936 editions) the *only* evidence appealed to in support of the view that there are seven missing sheets is the extant pagination numbering.[57] Since $\mathfrak{P}^{46}$ is a single-quire codex, it is then deduced from this that there

[57] Kenyon, *CBBPIII-1936*, viii, "… since nearly all (the leaves) have preserved their original page-numeration, the composition of the volume is beyond doubt. Seven leaves are missing from the beginning, which implies the loss of equal number at the end"; Sanders, *TCPC*, 7, 10, "At the beginning of the manuscript Sir Frederic establishes a loss of seven leaves. The first page of the leaf was blank and unnumbered… As the Beatty leaves are joined in pairs an equal number of leaves were lost at the end of the manuscript… Seven leaves are lost after the last Beatty fragment, since seven are lost before the first

are seven missing leaves (= 14 pages) at the beginning and a corresponding seven leaves at the end; all discussions as to whether the missing text would fit onto the fourteen pages have been derived solely on this observation. But can we trust the extant page numerations to be truly reflective of the actual number of missing pages?[58] What does the evidence really tell us?

There are untold stories about this page numeration in $\mathfrak{P}^{46}$ that may prove critical in our quest for an answer. First, whilst we are told that there are seven missing sheets (=28 missing pages) based on the pagination numeration, we are nonetheless never told that the page numbers were appended onto the codex *after* the scribe finished copying his manuscript! Second, and more importantly, we are also not told that the original hand is *not* the one who appended the pagination numbers! Robert Grant seems to suggest that the original scribe himself was responsible for the page numbers.[59] But palaeographical evidence readily disproves this suggestion. A look at Fig. 3-5.4 shows that the one who appended the pagination numbers used a pen with a thicker nib and an ink with heavier black chemical composition than that

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one"; Grant, *Historical Introduction*, 209, "The outside leaves are lost, but since the pages were numbered by the scribe we know how many were there… Since seven leaves (fourteen pages) are missing at the beginning, an equal number must be missing at the end"; and Epp, "Issues in the Interrelation," 609-10, "Since most pages of this single quire codex are numbered, it is clear that seven leaves or fourteen pages from the beginning of the codex have not survived, and there is no dispute that Romans 1.1–5.17 would fit onto those pages and originally occupied them. This means, of course, that fourteen pages also are missing from the end." Duff, "$\mathfrak{P}^{46}$ and the Pastorals," 580, starting his calculations not with fo8^{v} but f11^{v} (the first page with extant pagination number) stated, "… the space available on these nineteen pages is very close to the amount of space the scribe is likely to have needed for the first seven and a half chapters of Romans: thus, the conjecture that these pages did originally contain the first part of Romans has never been questioned."

[58] The significance of this question comes to vogue when we consider that there are papyrological evidences where the pagination numbers did not start from the actual beginning pages. For instance, Bonner, *A Papyrus Codex of the Shepherd of Hermas*, 8-9, reported that the page numerations in this Hermas codex were added not until it was damaged! On the other hand, Turner, *TEC*, 75ff, reported that there are some codices with irregular pagination features, i.e., starting page numerations inside a codex.

[59] Grant, *Historical Introduction*, 209.

of the text (but similar with one of the correctors and perhaps the στιχος marker).[60] The styles of writing between the text and the page number are also evidently palaeographically different. These have implications for our study, for with these in mind we now can broach the view that the original hand was *not* guided by the pagination numbers when he was copying the text of his *exemplar* onto the codex![61]

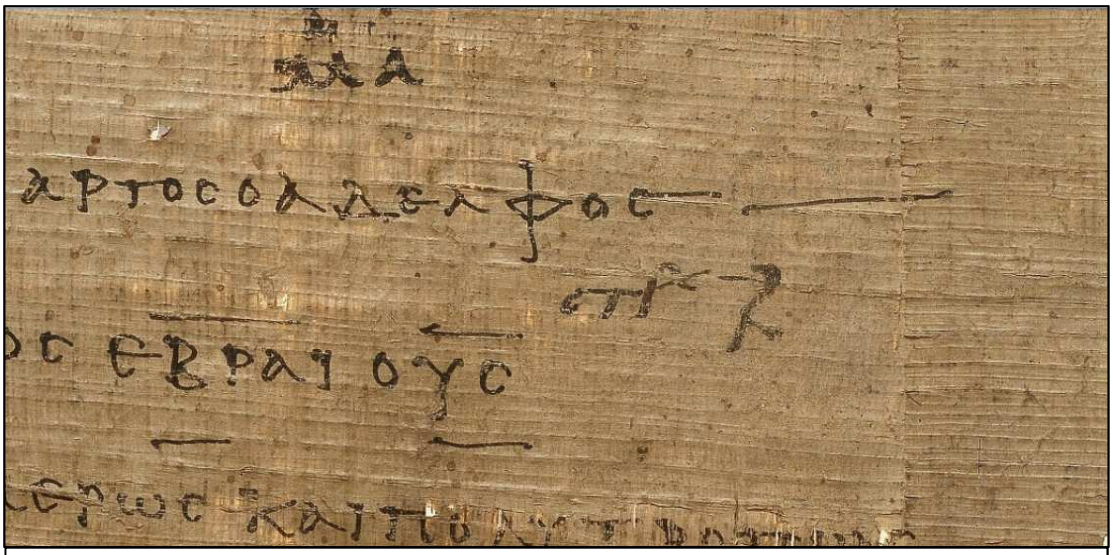


Figure 3-5.4 F21^{r}: Page numeration (with an ink smudge over the ϻ) and a στιχος note.

Furthermore, it is not altogether out of place to cast some doubts against Kenyon's suggestion that the unpaginated opening (f51^{r}-f52^{v}) was a result of an "opening mistake,"[62] especially if we note that this is the opening *immediately before* the centrefold. Was it really a mistake? Or could this have been a way of balancing off an even earlier blunder, between pages 1-19? I can only speculate, but what this

[60] Zuntz, *TEDCP*, 253, is not incorrect in proposing that this scribe "used a broad pen and very black ink,… (who) also added the page numbers and wrote the *stichoi* under each epistle; in other words, this is the hand of the *ex officio* corrector who, still in the scriptorium, applied the finishing touches to the work of the (original) scribe." See related discussion in pp. 290-322, esp. pp. 307-11.

[61] Here the point raised by Turner, *TEC*, 75, is very instructive: "… pagination, when it occurs, is often written in a hand different from that of the original scribe… It would seem, therefore, that it was not running pagination utilized by the scribe to keep his sheets in order, but was added subsequently."

[62] Kenyon, *CBBPIII-1936*, ix, "The pages were numbered throughout, and nearly all the page-numbers are preserved. *By a mistake of the scribe, however, two pages… escaped numeration, so that henceforth the page-numbers are lower by two than they should have been.*" (Emphases added).

implies is that the scribe who appended the page numerations was, by many indications, a sloppy scribe-corrector, as Zuntz rightly noted, "(This scribe) did his work *very carelessly*... (leaving) numberless slips uncorrected, skipped two pages in numbering..., and occasionally produced nonsense by his very corrections and his punctuation marks".[63]

Whilst locating pagination numbers is a standard codicological method in establishing the number and sequence of a manuscript's sheet formations,[64] it is not the only way; other paratextual features in the manuscript can help in its establishment (or rectification as the case may be). One alternative method that can reinforce this quest is to calculate how much of the missing text of Romans can fit onto a corresponding number of pages, based on the average number of characters to a page from the first extant pages. Surprisingly, no studies on the missing pages of $\mathfrak{P}^{46}$ have been conducted along this line of inquiry, presumably because Kenyon's proposal was simply accepted at face value. But in a situation where very little evidence is available, every proposed methodology is worth testing.

Using the UBS^{4}-NA^{28} common text as comparison base and adjusting it to the known copying habits of the scribe of $\mathfrak{P}^{46}$, the probable total number of missing characters before f08^{v} (i.e., Rom 1.1-5.17a) is about 10,437.[65] Admittedly, this figure pales in comparison with the projected figure for the texts of 2Thess, the Pastorals, and Philm at around 24,415 characters! But having a comparatively smaller amount of text in the opening pages of a codex seems to be a frequent scribal copying pattern in

[63] Zuntz, *TEDCP*, 253. (Emphasis added).

[64] Turner, *TEC*, 77.

[65] Breakdown: Book title (90), Chap One (2,750), Chap Two (2,190), Chap Three (2,003), Chap Four (1,956), and 5.1-17a (1,448). As with the projected number of characters for 2Thess, Pastorals, and Philm, this is also an approximate, but is cognizant of the scribal habits in the extant pages, i.e., the reduction due to *nomina sacra*, and expansion due to *itacism* (specifically for υμειν, ημειν, τειμη, θλειψις, and κρεινομαι).

the manuscript tradition, since the scribe might still be feeling his way into the manuscript. For purposes of methodological consistency, we will also employ here the different page-variables we applied for the missing pages at the end of the codex (Table 3-E15).

TABLE 3-E15 CHART SHOWING VARIOUS LEAF AVERAGES IN THE BEGINNING EXTANT PAGES OF $\mathfrak{P}^{46}$						
	22-page Average	**16-page Average**	**10-page Average**	**6-page Average**	**4-page Average**	**2-page Average**
F08^{v}	826	826	826	826	826	826
F08^{r}	807	807	807	807	807	807
F9^{v}-F10^{r}	x	x	x	x	x	x
F11^{v}	879	879	879	879	879	
F11^{r}	808	808	808	808	808	
F12^{v}	835	835	835	835		
F12^{r}	820	820	820	820		
F13^{v}	856	856	856			
F13^{r}	857	857	857			
F14^{v}	971	971	971			
F14^{r}	800	800	800			
F15^{v}	796	796	796			
F15^{r}	802	802	802			
F16^{v}	894	894				
F16^{r}	788	788				
F17^{v}	911	911				
F17^{r}	824	824				
F18^{v}	937					
F18^{r}	920					
F19^{v}	792					
F19^{r}	759					
F20^{v}	799					
F20^{r}	739					
TOTALS	**18,420**	**13,474**	**10,057**	**4,975**	**3,320**	**1,633**
AVERAGE # OF CHARACTERS/PAGE	837/P	842/P	1,005/P	829/P	830/P	816/P

If we put these averages against the projected amount of text amounting to the first missing chapters (and book title) of Romans (Table 3-E16) the result is quite a revelation--there was much space for all the 10,437 characters if we go by the proposal that there were indeed 14 missing pages! In fact, if we get the simple average for all these, the scribe would have only needed 11.5 pages! This figure transports the canonical discussion of the Pastorals with regard to the missing pages of $\mathfrak{P}^{46}$ to a totally new dimension and brings to light new questions! Needless to say,

the inevitable question that needs to be raised immediately is the issue of content--What was contained in the preceding 2.5 pages if indeed there were 14 missing pages? Or were there really 14 missing pages? Did the scribe who appended the page numeration first commit his page numbering error in these pages, which he then rectified in the unpaginated leaf?

TABLE 3-E16 GRIT SHOWING VARIOUS POSSIBLE PAGE REQUIREMENTS FOR THE MISSING TEXT OF ROMANS							
BOOKS	**TOTAL # OF LETTERS REQUIRED**						
		837/p	**842/p**	**1005/p**	**829/p**	**830/p**	**816/p**
Book Title	90	0.1	0.11	0.09	0.11	0.11	0.11
Chap One	2,750	3.28	3.27	2.74	3.32	3.31	3.37
Chap Two	2,190	2.61	2.6	2.18	2.64	2.64	2.68
Chap Three	2,003	2.39	2.38	1.99	2.42	2.41	2.45
Chap Four	1,956	2.34	2.32	1.95	2.36	2.36	2.39
5:1-16	1,448	1.73	1.72	1.44	1.75	1.74	1.77
TOTALS	10,437	12.45	12.39	10.39	12.6	12.57	12.77

There seem to be two possible scenarios to explain this. First, assuming the traditional 14-missing pages view, it is not improbable that the first leaf (hypothetical pages one and two [pp.1-2]) was blank and served as the outer protective covering for the codex. Hence, the scribe started writing onto the codex in the second half of page three (p.3). This view is not without prior proponents. However, its downside is that the corresponding last leaf of the codex must be presumed to have also been left blank, reducing further the space available for the contested texts of the aforementioned epistles.

The second possible scenario, which seems to me more likely given all the codicological-palaeographical variables at hand, is that the scribe, with a little bit of compressing, might have actually started writing his text on the hypothetical page

four (p.4), and the hypothetical page three (p.3) served as the outer covering,[66] which means that only six (6) sheets are missing,[67] or 12 pages at the beginning and 12 at the end also!

But if only 12 pages were missing at the end, and if the very last page also functioned as outer covering, corresponding to the very first page at the beginning (leaving only 11 pages for writing), what book/s then were included in these missing last pages after 2 Thessalonians? Did the scribe really intend to include the Pastorals in these pages? Or any of the books of the Pastorals? But if so, do we have surviving evidence that the books of the Pastorals circulated independently? Or could it be that other "Christian books" occupied these pages?[68] Or did the scribe terminate his copying task after completing 2Thess, then left the other pages blank, as Kenyon has maintained?[69]

CONCLUSION

We have shown in this section that a fresh methodological approach (i.e., codicological-palaeographical) to the problem of content of the last missing pages of $\mathfrak{P}^{46}$ can yield more concrete data in (re)interpreting the available evidence. Using this approach, we argued that the increasing number of characters in the second half of the codex does not prove anything about the intention of the scribe to include the Pastorals and Philemon—the

[66] This proposal is not without any precedence. For instance, Turner, *TEC*, 76, cited $\mathfrak{P}^{46}$ among the examples of papyri whose very first page was left blank either to serve as a wrapper, or to carry the title (of the collection?), or to be glued to the binding cover. Whilst assuming a 14-page lacuna, Kenyon, *CBBPIII-1936*, ix, was firm in his belief that the very first page was blank and left unnumbered. It is unfortunate that Duff, "$\mathfrak{P}^{46}$ and the Pastorals," 580, decided to dismiss the importance of the function the first page in settling the question of content, arguing that "The various possible functions of the verso side of the first leaf (page zero), such as a title page or a cover, are immaterial" to his arguments. Obviously, he had to do this to avoid the prospect of losing more space to the title page or cover.

[67] Although indirectly, Kirsopp Lake, Review of Kenyon, 244, seems to have suggested this by stating, consciously or unconsciously, "It is to be hoped that this publication will tempt fate to go one step further in its efforts to embarrass editors and provide us with some of the missing six folia from the end."

[68] This has been raised by Epp, "Issues in the Interrelation," 617-19, arguing in essence that the increasing trend in $\mathfrak{P}^{46}$ does not categorically prove the inclusion of the Pastorals, and raising the hypothetical scenario that other (non-canonical) books may also vie for the same coveted space.

[69] Kenyon, *CBBPIII-1936*, x-xi.

scribe was not economizing his space. Indeed, there certainly must have been a reason (or reasons) for the increase, but wisdom dictates to simply accept the fact that at present we cannot know for certain what the reason(s) is; we can only prove what it is not.

We also argued that contrary to the prevailing view, it seems more likely that only six sheets are missing; the first 12 pages contained the front cover page and the text of Rom 1.1-5.17a. The corresponding 12 pages at the back contained the text of 2Thess 1.9b-3.18 (occupying about four pages) and the last outer page served as back cover page. The remaining seven pages are not enough for the Pastorals and Philemon. If they have been left blank, it is not against the known scribal practice.

CHAPTER FOUR
PAPYRUS 46 AS A CHRISTIAN DOCUMENT: THE TEXTUAL FEATURES OF $\mathfrak{P}^{46}$

We now proceed to the textual properties of $\mathfrak{P}^{46}$, with a focus on the habits that can be discerned from the way our copyist inscribed his text onto his papyrus codex. The textual character of $\mathfrak{P}^{46}$ is now widely acknowledged to represent a kind of "free text", which means that it does not strictly follow a particular textual cluster. It is not in the immediate purview of this chapter to establish the intricate textual relationships of $\mathfrak{P}^{46}$ with other manuscripts. In fact, that deserves another focused study in itself. My intention, nonetheless, is to see how all the documented variations in $\mathfrak{P}^{46}$ may be used to extract his copying habits vis-à-vis the material that was available to him during his copying sessions. Special focus is made on variants that occur with recurring frequency as well as those that are peculiar to $\mathfrak{P}^{46}$.

SECTION ONE
FOIBLES AND FUMBLES:
SCRIBAL HABITS IN THE VARIATIONS OF $\mathfrak{P}^{46}$

INTRODUCTION

It seems almost automatic to depict the scribe of $\mathfrak{P}^{46}$ in an adverse light as against the positive quality of its *exemplar*. For instance, Barbara Aland described $\mathfrak{P}^{46}$ as aesthetically elegant but "(t)he quality of the copy is not comparable with the beautiful hand... $\mathfrak{P}^{46}$ represents a rough and inadequate copy of a good exemplar."[1] Or in Michael Holmes' assessment, "The manuscript itself... is by no means a good manuscript... Nonetheless, the *Vorlage* which lay before the scribe of $\mathfrak{P}^{46}$ preserved a text of perhaps unequalled quality; indeed, with surprising frequency it alone... among all extant witnesses preserves the true wording of the Pauline archetype".[2] Undoubtedly, the shaping of this attitude has been largely influenced by Zuntz's oft-quoted valuation of $\mathfrak{P}^{46}$:

> The excellent quality of the text represented by our oldest manuscript, $\mathfrak{P}^{46}$, stands out again. As so often before, we must here be careful to distinguish between the very poor work of the scribe who penned it and the basic text which he so poorly rendered. $\mathfrak{P}^{46}$

[1] Aland, "Significance of the Chester Beatty," 116-17. On another occasion, contrasting $\mathfrak{P}^{46}$ from $\mathfrak{P}^{66}$, she reiterated, "*Dieser früheste Pauluscodex, den wir besitzen, ist mit Versehen, Irrtümern, Sorglosigkeiten übersät, aber—und das ist zunächst das Entscheindende—der Text, der Handschrift zugrunde liegt, ist gut*" ("Sind Schreiber Früher Neutestamentlicher Handschriften Interpreten Des Textes?" in *Transmission and Reception*, p. 119).

[2] Holmes, "Earliest Commentary on Romans," 189. Of its scribe, Holmes' stated, "The manuscript itself, as Zuntz observes, 'is by no means a good manuscript,' even though it was penned by a professional scribe and corrected (somewhat haphazardly) by an expert: the mistakes, habits, and characteristics of its blundering and not always attentive scribe have been well documented". For the last part of the quote, Holmes cited Colwell's article on scribal habits (*cum* Royse's dissertation), but there seems to be a confusion there since Colwell never made any specific remark about the scribe of $\mathfrak{P}^{46}$ in his article. On the other hand, Kurek-Chomycz, "Is there an 'Anti-Priscan' Tendency in the Manuscripts?" 110, n9, relying on Royse's dissertation, advised caution in considering the witness of our codex, "$\mathfrak{P}^{46}$ is replete with errors and singular readings. Thus, despite its antiquity and the fact that the basic text it attests is of high quality…, it is prudent to treat this particular papyrus with caution."

> abounds with scribal blunders, omissions, and also additions. In some of them the scribe anticipated the errors of later copyists; in some other instances he shares an older error; but the vast majority are his own uncontested property. Once they have been discarded, there remains a text of outstanding (though not absolute) purity.[3]

In the context of scribal studies, however, a clear-cut distinction must be made, insofar as possible, between the text underlying the *exemplar*, the scribe who penned it, and our scribe who used that *exemplar*, i.e., comparison and contrast between the two scribes is indispensable. Despite major breakthroughs in Zuntz's (and Royse's) impressive analyses, the information gap between what was done by our scribe and what was already in his *exemplar* essentially remains a gargantuan challenge, both at methodological and practical levels.[4] Was the hand behind the *exemplar* of $\mathfrak{P}^{46}$ a better (more careful) copyist than our scribe? Did our scribe corrupt the *exemplar*'s "text of excellent quality" or did he in fact do a better job than the scribe of his *exemplar*? Did he copy his *exemplar* faithfully, both in terms of content and format? These are some of the questions we intend to probe in this section, in regard to our scribe's habits from the perspective of variations.[5]

Methodologically, instead of focusing on variations analysing only the rigid formal categories (i.e., omission, transposition, addition, replacement, etc.), I shall

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[3] Zuntz, *TEDCP*, 212-13. Most recently, in his study of the Galatians text from various manuscripts, Carlson, "Text of Galatians," 323, claimed that his analysis corroborates Zuntz's view that "P^{46} is a poor copy of an excellent exemplar" and that the text it reflects is "near the archetype" of the Pauline corpus.

[4] Zuntz and Royse have contributed greatly to the study of "errors" in $\mathfrak{P}^{46}$; the amount and the quality of material they have methodically assembled and cogently presented are a treasure. As such, I do not intend to repeat here what they have already masterfully done, except to underscore some aspects of the intersection of the textual transmission of $\mathfrak{P}^{46}$ and certain mechanics of ancient book production in light of the "errors" now fossilised in $\mathfrak{P}^{46}$, which may diverge from Zuntz's and Royse's analyses. Furthermore, due to the focused objective of this thesis—i.e., to profile the scribal habits of the main hand of $\mathfrak{P}^{46}$ and not to directly assess its textual character in relation to establishing the "original text" of the Pauline corpus—many of the discussions by Zuntz and Royse on the text of $\mathfrak{P}^{46}$ would be left untouched unless alternative views/explanations are more convincing.

By and large, it is less difficult to isolate scribal habits that are *material*-related than *text*-related. The latter is much more complicated than is usually perceived and the evidence from the text is much more indirect than one might wish. Conversely, the level of certainty that a generated activity is scribally created is higher when we are dealing with material-related and other paratextual features in the codex.

[5] In this regard, Hurtado, *Earliest Christian Artifacts*, 189, is instructive: "Even the mistakes of scribes give us hard data for estimating attitudes toward the texts copied, their own efforts and those of unknown others reflecting a concern for the wording of these texts and for careful transmission of them."

alternatively explore also *why* our scribe might have committed some of those variations at those particular junctures in the codex, and probe whether, by way of identifying recurring and significant patterns of error, they form his actual copying habits, whether "singularly" attested or with shared witnesses. Our thesis is that whilst there are variations caused by the very nature of the text being copied (i.e., the "sacred text" *in* the *exemplar* and the *exemplar* itself), there are also variations that can be clearly attributed to the dynamics of the scribal profession *as well as* to the nature of the production material used. The quintessential question needing an urgent answer is "When and in which part of the codex was our scribe most prone to deviate from his *exemplar*?" Answer(s) to this question will lead us to the "Why?" part of our inquiry. Ideally, in order to probe this, we must identify and categorise all the variations in $\mathfrak{P}^{46}$ against the rest of the manuscript tradition. However, the greatest methodological hurdle in achieving this is the sheer enormity of extant manuscripts with the Pauline Epistles vis-à-vis the time element available for this project to be completed. Hence, in order to control our data, achievable within the prescribed time, I have chosen to collate the text of $\mathfrak{P}^{46}$ against the common text of UBS^{4}-NA^{28}.[6] In making this methodological decision, I am not in any way *a priori* presupposing that these critical editions reflect the "original text" in their entirety, nor do I assume that our scribe's *exemplar* resembles the common text of UBS^{4}-NA^{28}; this choice is simply the most pragmatic way to achieve the present goals of this section.[7]

[6] I started this research project (and built my original database) using the common text of NA^{27}-UBS^{4}. However, I have now updated NA^{27} to NA^{28} since it has become available in late 2012. Although there are some textual differences between these two editions, particularly in the Catholic Epistles, their texts for the Pauline Epistles are basically the same; the new information in the apparatus of NA^{28} have been accordingly added though to my original data.

[7] Accordingly, in this section I am using the term textual "variant/s" (and "error/s") in $\mathfrak{P}^{46}$ relative to the readings of the main text of NA^{28}-UBS^{4}. I am aware, however, that in most cases it would be extremely difficult to decide with certainty whether a variant reading in $\mathfrak{P}^{46}$ (collated against NA^{28}-UBS^{4}) is genuinely a "deviation from" or a faithful "reflection of" the text of its *exemplar*. Hence, I speak of variant readings only as they deviate from NA^{28}-UBS^{4}.

I. WHAT THE NUMBERS SAY

A. Counting the "Errors": The Number of Variations in $\mathfrak{P}^{46}$

Collated against UBS^{4}-NA^{28}, we recorded a cumulative total of **1,939**[8] variations. But if we deduct the 535 *itacisms* and *nasals*—as they are not "errors" in the strict sense but more of linguistic idiosyncrasies—we then have a total of **1,404** indicative variations.[9] Given that this figure is derived from the 172 extant pages, this means that our scribe's average copying variation rate is 8.2 variations per page.[10] Expressed visually, a variation is committed every after 131 characters or four-five lines on the average.

The breakdown per book of the 1,939 is shown in the following:

TABLE 4-A1
NUMBER OF VARIATIONS PER BOOK

BOOK	With Itacisms and Nasals	Less Itacisms and Nasals
ROM	286	(286 - 78) = 208
HEB	424	(424 - 112) = 312
1COR	472	(472 - 158) = 314
2COR	290	(290 - 83) = 207
EPH	136	(136 - 33) = 103
GAL	143	(143 - 23) = 120
PHIL	110	(110 - 32) = 78
COL	74	(74 - 15) = 59
1THESS	4	(4- 1) = 3
TOTAL	**1,939**	**(1,939 - 535) = 1,404**

As Table 4-A1 reveals, 1Cor has the highest number of deviations from UBS^{4}-NA^{28}, followed by Heb. This seems logical as these are the two largest extant books in our codex; hence, opportunities for error-commission are higher. Rom and 2Cor come

[8] This and the ensuing figures are essentially derived from Appendix K–Table of Variations in $\mathfrak{P}^{46}$.

[9] Note however that the temporary putting aside of these orthographic variations is not due to the view that orthographic variants have no intrinsic value, or what Colwell, "Scribal Habits," 111, describes as "stumbling block to the reconstruction of the original text and the establishment of manuscript relationships". Whilst this may be true for those purposes, for purposes of scribal studies orthographic variations are in fact a rich resource for providing us with direct and hard data about our scribe's copying habits, which we shall attempt to articulate in the next sub-section.

[10] It is difficult to ascertain whether at this rate our scribe is a poor copyist or one of the best copyists we have for the earliest New Testament manuscripts, since there are no other ancient manuscripts, to my knowledge, whose variations have been collated against UBS-NA (or against other critical texts) in their entirety and not only their "singular readings".

very close to each other although Rom originally must have had more deviations considering that 1.1-5.17a is no longer extant. In the same vein, 1Thess reflects the lowest precisely because of its extremely fragmentary nature.

B. "The Error of Counting?" Other Methodological Possibilities

It would be misleading, however, if the immediate and only conclusion to be derived from these figures is that our scribe is a chronic blunderer, for it is yet to be established which of these variations are indeed from our scribe and which are from his *exemplar*. We have already argued that singularity in witness does not fully guarantee scribal creation,[11] and therefore we must seek other ways to take this inquiry further.[12] A potentially promising scheme is to appreciate these variations from various angles, inclusive of both the rigid formal categories and *also* how these variations are visually located on the page.

C. Formal Categories

In terms of formal categories, the breakdown for the 1,939 is shown in Table 4-A2:

TABLE 4-A2
TABLE OF VARIATIONS PER FORMAL CATEGORIES[13]

	Ortho/ NonSe	Comp	Repl	Trans	Gram	Om	Add	Confl	Uncon Err	Misc	TOTALS
ROM	117	6	26	18	21	67	30	0	1	0	**286**
HEB	199	11	31	14	46	85	26	3	4	5	**424**
1COR	232	9	27	26	34	102	31	2	6	3	**472**
2COR	142	1	20	13	23	66	22	0	3	0	**290**
EPH	67	3	13	3	7	34	9	0	0	0	**136**
GAL	57	5	16	4	17	36	5	3	0	0	**143**
PHIL	52	2	7	3	5	23	18	0	0	0	**110**
COL	35	2	5	1	7	17	7	0	0	0	**74**
1THESS	2	1	0	0	0	1	0	0	0	0	**4**
TOTALS	**903**	**40**	**145**	**82**	**160**	**431**	**148**	**8**	**14**	**8**	**1,939**

[11] See pp. 42-51. Of course, the exceptions here are those that are clear scribal blunders.

[12] Due to space constraints the treatment of variations in this section is not fully exhaustive. Nonetheless, the examples provided are deemed representative of the general patterns of error in $\mathfrak{P}^{46}$, both at textual and paratextual levels, from which more dedicated future studies could be based.

[13] Legend: Ortho/Nonse (=Orthographic and Nonsense); Comp (= Compound words); Repl (= Replacement [or Substitution]); Trans (=Transposition); Gram (=Grammatical); Om (=Omission); Add (=Addition); Confl (=Conflation); Uncon Err (=Unconsummated Error); and Misc (=Miscellaneous).

What Table 4-A2 reveals straightaway is that bulk of the 1,939 variations are orthographic/nonsense variants (46.6%),[14] followed by omissions (22.2%), grammatical variants (8.3%), additions (7.6%), and replacement (7.4%)—all these already account for about 92% of the cumulative total. The smaller entries include eight cases of miscellaneous variations (0.4%),[15] 11 cases of conflation (0.6%),[16] and 14 instances of "unconsummated errors" (0.6%).[17] The rest accounts for the 82 cases of transpositions (4.2%) and 40 cases of compound words (2.0%). A few observations are now in order.

As noted earlier, 535 of the 903 orthographic variants are itacisms and nasals and they can be easily discerned. It is more difficult to decide on variants involving vowel change that are both grammatically and contextually sensible, whether they should be categorized as a grammatical alteration or an orthographic variation, especially those involving **ο-ω/ω-ο**, **η-υ/υ-η**, and **ε-αι/αι-ε** interchanges. Since they can be easily aurally confused in the copying context, I have categorised them under the rubric of orthographic variants. There are 73 cases of these interchanges. The remaining 295 are divided between pure orthographics (unusual elided forms, additions/subtractions of one or more letters, etc.) and nonsense (in form or in context).

The number of omission variants is remarkably high; this agrees with the earlier observations by Royse (for the whole codex using "singulars" only) and Silva (for

[14] I have purposely lumped together these two since the divide between them in $\mathfrak{P}^{46}$ is really not clearly significant, i.e., most of the nonsense variants are also due to their orthographic formations and almost all the "pure" orthographics do not make sense as well, either in form or in context. The only two distinctions to be made, which we shall discuss shortly, are those which I termed "orthographic=grammatical" (Orthog=Gram) variants as well as those that are itacistic and nasals.

[15] Heb 13.23c involves the lone first hand abbreviation in the whole codex (απολελῡ [=απολελυμενον]). Three involve expansions: Heb 9.22c (αιμαεκχυσιας to αιματος εκχυσιας); 1Cor 7.8 (καγω to και εγω); and 1Cor 10.6 (κακεινοι to και εκεινοις). Finally, four involve wrongful corrections (or perhaps corrections toward another *exemplar*?): Heb 1.1 (τοις πατρασιν <ημων>); 10.25b (την ἐπι||συναγωγην); and 13.5a (αρκουμενο>ι/ς<); and 1Cor 8.7 (εσθιουσιν).

[16] Heb 3.6b; 10.2a, 38a; 11.15b; 1Cor 7.40; 8.7b; 13.12; Gal 1.11c; 2.8b; 4.9; and Col 3.5.

[17] Rom 10.13b; Heb 2.7a; 9.12c; 11.7c; 13.4; 1Cor 3.2b, 21; 4.5b; 5.5; 11.3a; 15.24a; 2Cor 7.10, 13b; and 10.4a.

Galatians using all kinds of variations).[18] Although this is statistically unimpeachable, the question of whether all these are from our scribe is not resolved by the figure—it only highlights the exceptionally high number of omissions *in* $\mathfrak{P}^{46}$.[19]

Additions are comparatively not as many as omissions, but nonetheless equally significant for isolating scribal habits, especially dittographies involving more than one word. In fact, the cumulative total of added characters for the 148 cases is 517 (=159 words) spread across our codex, most of which are in 1Cor (115), Rom (104), and (surprisingly) Phil (104).[20] The significance of this for scribal studies is treated below.

Regarding the 40 cases of compound words, the general tendency is toward compound-to-simple (25 cases),[21] with four instances in the other direction (simple-to-compound).[22] We also recorded four cases of compound-to-compound,[23] a case of corrected simple-to-compound,[24] a complex compound-to-simple compound,[25] a

[18] Royse, *SH-M*, 270-98; see also, Ibid, "Early Texts of Paul (and Hebrews)," 183; Silva, "Text of Galatians," 19.

[19] This observation becomes more apparent when we deal with the omissions involving more than one word, which is explored in the next sub-section.

[20] Breakdown of 148 per book: Rom 30 (=104 characters); Heb 26 (=73); 1Cor 31 (=115); 2Cor 22 (=62); Eph 9 (=23); Gal 5 (=14); Phil 18 (=104); Col 7 (=22); and 1Thess 0 (=0).

[21] Rom 8.17c (συμπασχομεν το πασχομεν); 11.18b (κατακαυχασαι το καυχασαι);

Heb 1.6 (εισαγαγη το αγαγη); 6.11b (ενδεικνυσθαι το δεικνυσθαι); 7.27b (εφαπαξ το απαξ); 8.10 (επιγραψω το γραψω); 11.14b (επιζητουσιν το ζητουσιν); 12.4d (ανταγωνιζομενοι το αγωνιζομενοι); 12.25a (εξεφυγον το εφυγον);

1Cor 7.13a (συνευδοκει το ευδοκει); 14.23a (συνελθη το ελθη); 14.37a (επιγινωσκετω το γεινωσκετω); 15.48b (επουρανιος το ουρανιος); 15.48d (επουρανιοι το ουρανιοι);

2Cor 12.16b (κατεβαρησα το εβαρησα);

Eph 2.2b (ενεργουντος το εργουν||τος); 2.5a (παραπτωμασιν το σωμασιν); 6.10 (ενδυναμουσθε το δυ||ναμουσθε);

Gal 1.17a (ανηλθον το ηλθον), 18 (υπεμεινα το εμεινα); 2.13b (συναπηχθη το απηχθη); 5.5 (απεκδεχομεθα το εκδεχομεθα);

Phil 1.7d (συγκοινωνους το κοινωνους);

Col 3.16a (ενοικειτω το οικειτω); and 3.24b (απολημψεσθε το λημψεσθε).

[22] Heb 10.29b (αξιωθησεται το καταξιωθησεται); 1Cor 10.9a (επειρασαν το εξεπειρασαν); 15.7 (ειτα το επειτα); and Phil 2.3c (ηγουμενοι το προηγουμενοι).

The analysis of Royse, *SH-M*, 326, although treating only the "singulars", agrees with this observation.

[23] Rom 9.27a (υπολειμμα το καταλιμμα); Heb 10.26d (απολειπεται το καταλειπεται); 1Cor 16.5 (διερχομαι το παρερχομαι); and Gal 6.2 (αναπληρωσετε το αποπληρωσετε).

[24] Heb 3.11b (καιεται corrected to <κατα>καιεται).

[25] Heb 10.25a (εγκαταλειποντες το καταλειποντες).

replacement from copula-to-compound,[26] and an instance with an orthographic-grammatical question.[27]

In sum, these formal categories are very helpful in providing a general description of the variations that are *in* $\mathfrak{P}^{46}$, from which an assessment of its over-all textual character can be made. However, they do not decisively elicit information about our scribe's *actual* copying habits. But is there any other way(s) to appreciate these variations apart from their traditional formal categories?

D. Visual Locations of "Errors"

One way we can take our investigation further is by looking at these variations from the standpoint of their visual location in the codex, as this provides us with hard data in regard to whether a side, fibre orientation, or line locations are crucial elements in ascertaining where our scribe was most prone to commit "errors".[28] In fact, this highlights the visual component of the ancient copying trade. Excluding the nasal and itacistic variations, the "side" distribution of indicative variations per book is as follows:

TABLE 4-A3
VARIATION DISTRIBUTION TABLE PER SIDE

	LEFT-HAND	RIGHT-HAND	TOTALS
ROM	87	121	208
HEB	169	143	312
1COR	156	158	314[29]
2COR	105	102	207
EPH	55	48	103
GAL	66	54	120
PHIL	38	40	78
COL	27	32	59
2THESS	2	1	3
TOTALS	**705**	**699**	**1,404**

[26] 1Cor 7.5b (ητε το συνερχεσθε).

[27] Rom 16.5 (απερχη το απερχης).

[28] At this stage, I am assuming tentatively that any deviation from NA-UBS text is an error committed by the scribe, to portray the relationship of the visual location of the variation and the physical material and to explore what inferences can be made from this. However, attempts are made in the ensuing pages to demonstrate which of these variations are possibly from the *exemplar* (i.e., "*exemplaric* variations") and which ones are most likely from our scribe.

[29] Breakdown: 177 variations *before* (1Cor 1.1-12.2) and 137 (1Cor 12.3-16.22) *after* the midpoint.

Whilst the side location of a page is a crucial factor in terms of character input, this table reveals that in terms of error commission this is not a decisive contributing factor, since figures for both right-hand and left-hand pages are cumulatively almost equal—699 vis-à-vis 705. It must be noted that though there are more variations committed on the right-hand side than left-hand when the scribe was copying Rom (^{ls}=87 < ^{rs}=121), the pattern was slightly reversed in Heb (^{ls}=169 > ^{rs}=143), and somehow "stabilised" in the rest of the Epistles. This implies that, for all intents and purposes, he was practically unaffected by the binding centre in the commission of errors.

This table also reveals that the number of errors committed before the midpoint (i.e., f52^{r} and f53^{r} [1Cor 11.26-12.2 and 12.3-12]) of the codex is just as many as the ones committed after the midpoint, i.e., 697 *before* and 707 *after* the midpoint, one implication of which is that our scribe's level of concentration[30] did not drastically falter throughout the copying process. The level of his copying accuracy did not increase as the copying progresses but the level of his copying error did not drastically increase either. This may be suggestive that the proximity of the end of his copying project was not a factor in the commission of errors, that is, our scribe apparently went on "business as usual" although he was already nearing the end.

Fibre orientation does not seem to be a critical factor as well, as can be seen from Table 4-A4, where totals for each are also cumulatively almost the same (707 verso > 697 recto). Just as with "side distribution" the pattern to be noticed in regard to fibre orientation is that whereas in Rom there are more variations on the sides with vertical orientation than horizontal, this was reversed in Heb, and somehow "normalised" in the rest of the Epistles.

[30] Of course, this is relative only to our scribe's commission of errors and not to his copying accuracy.

TABLE 4-A4 VARIATION DISTRIBUTION TABLE PER FIBRE ORIENTATION				
		VERTICAL	HORIZONTAL	TOTALS
ROM		121	87	208
HEB		143	169	312
1COR	1.1-12.2	88	94	182
	12.3-16.22	62	70	132
2COR		105	102	207
EPH		55	48	103
GAL		66	54	120
PHIL		38	40	78
COL		27	32	59
2THESS		2	1	3
TOTALS		**707**	**697**	**1,404**

Did our scribe commit more errors at the upper lines or lower lines of a page? There is a built-in methodological difficulty in this question since the number of lines to a page differs substantially as the copying progresses. However, since we have already established in Chapter Three[31] that the increased pattern of line-input can be framed in terms of quarter-intervals, we can thus also use that same information for our reference:[32]

TABLE 4-A5 SUMMARY OF NUMBER OF LINE INPUT PER QUARTER			
QUARTER DIVISIONS	AVERAGE # OF LINES	UPPER PART	LOWER PART
First quarter (f08^{v}-f26^{r})	25-28 lines	II$^{01\text{-}13}$	II$^{14\text{-}25(\text{-}28)}$
Second Quarter (f27^{v}-f52^{r})	25-30 lines;	II$^{01\text{-}14}$	II$^{15\text{-}25(\text{-}30)}$
Third Quarter (f53^{r}-f75^{v})	26-30 lines	II$^{01\text{-}14}$	II$^{15\text{-}26(\text{-}30)}$
Last Quarter (f76^{r}-f97^{v})	28-32 lines.	II$^{01\text{-}15}$	II$^{16\text{-}28(\text{-}32)}$

Assuming these figures, Table 4-A6 shows how the variations are committed in terms of this location variable:

TABLE 4-A6 VARIATION DISTRIBUTION TABLE PER UPPER-LOWER LOCATIONS			
	UPPER	LOWER	TOTALS
ROM	113	95	208
HEB	176	136	312
1COR	187	136	314
2COR	112	95	207
EPH	65	38	103
GAL	68	52	120
PHIL	52	26	78
COL	36	23	59
2THESS	3	0	3
TOTALS	**812**	**592**	**1,404**

[31] For more detailed discussions about line inputs per page, see pp. 114-18.

[32] Note however that since there are erosions at the bottom, the figures presented henceforth pertain only to those that are extant—reconstructed portions are excluded.

Unlike the side and the fibre orientations, the difference between the numbers of variations on the upper lines and those in the lower lines is more profound at 220. Furthermore, it will be noted that in all the books, variations in the upper lines are always higher than those in the lower lines; the biggest gaps are recorded in 1Cor and Heb at 51 and 40, respectively. What inference can be made of this information? Nothing much can be asserted from this except to suggest the possibility that the *exemplar* may have been placed on top of our scribe's codex during the copying process which makes the lower lines visually closer to his eyesight, and he therefore committed fewer errors therein. This seems to be supported by the figures generated from the side and fibre orientations. Admittedly, the inevitable assumption is that the *exemplar*'s text lay-out is the same as our scribe's, so that there is some form of visual correspondence in the locations of text in both manuscripts.[33]

It should be obvious by now that our scribe, like any other ancient scribe, is capable of producing errors of all sorts, at any time, and at any point of his codex. There seems to be no justification for a claim, however, that the kinds of errors he made are laden with systematic or programmatic intentions. As I see it, many of them seem to be occasioned more by human frailty (of our scribe and the scribes before him) than anything else. If so, what derivable factors influenced or affected him then?

In what follows, an attempt is made to articulate probable factors—aside from the traditional ones—as to why our codex reflects alterations that are unique in themselves and/or anticipated in the manuscript tradition, and thus to identify what implications they pose for the bigger context of ancient book production enterprise.

[33] I do recognise, however, that the correspondence between the *exemplar* and the codex cannot be taken as strictly one-to-one, due to the presence of incremental omissions in $\mathfrak{P}^{46}$ (where substantial number of characters had been lost accidentally).

II. *EXEMPLAR*-RELATED VARIATIONS

There are variations in $\mathfrak{P}^{46}$ indicating that our scribe's *exemplar* is itself embellished with difficult-to-comprehend and unique readings.[34] In this thesis these are referred to as "*exemplaric* variations"—readings that are best explained as *already present* in our scribe's *exemplar*, which could have been 1) originally marginal glosses that got assimilated accidentally into the text of the *exemplar*, either in the previous layers of its transmission history or in the *exemplar* itself for the first time;[35] or 2) because the *exemplar* was sorely damaged or annotations (corrections and others) have been badly written at those particular junctures which our scribe tried to make sense of but failed; or 3) because the transmitted underlying text is already editorially altered. Admittedly, as to precisely how far we can detect the beginning of the "intervening" process, this is a matter of speculation.[36] It is fair to say that until now we have yet to behold a method that will address most satisfactorily this question. At any rate, every case has to be critically weighed.

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[34] This phenomenon is equally recognized by Royse, *SH-M*, 100, "The scribe's task would have been made more onerous by indistinct or even illegible writing in the exemplar, as well as by marks of correction or marginal annotations." But previous to him, Zuntz had already convincingly argued that "the *Vorlage* of P^{46} contained alternative readings" (p. 255).

[35] Apart from Zuntz, Kenyon, *Our Bible and the Ancient Manuscripts*, 51, also provided a general description of this phenomenon, "Again, similar letters may be confused, abbreviations and contractions misunderstood; sometimes the MS. from which he is copying is furnished with short explanatory notes or glosses in the margin, and he fails to see were the text ends and the notes begins, and so copies the note into the text itself. Mistakes of this kind are bound to occur at all stages in the manuscript tradition, and the mistakes of one copyist are repeated and added to by the next." A slightly nuanced study of variations in the text of Romans in $\mathfrak{P}^{46}$ has been conducted by Holmes ("The Text of $\mathfrak{P}^{46}$: Evidence of the Earliest 'Commentary' in Romans") arguing that some of these variations may have originated as marginal glosses—or what he calls as "commentaries"—in the *exemplar* or the ancestors of $\mathfrak{P}^{46}$.

[36] In fact, even Zuntz, *TEDCP*, 254, could only talk of $\mathfrak{P}^{46}$'s "ancestors", "parent manuscript", and "distant predecessor" to describe the extent of the "philological technique (that) was applied to the text of the Epistles already in the second century." Gordon Fee, "The Significance of Papyrus Bodmer II and Papyrus Bodmer XIV-XV for Methodology in New Testament Textual Criticism," PhD Dissertation, University of Southern California, 1966, 118, himself recognized, "While it is true that a certain number of variants have come into the tradition as 'errors' on the part of scribes, and therefore are the result of 'non-editorial' activity, it is also true that a large part of the variation is the result of scribes choosing to add, delete, or alter certain words. How many variations in a given MS can be attributed to either of these processes and how many are already in the scribes' exemplars is not at all easy to determine."

A. First Things First: Copying to Dictation or Visual Copying?

Before any progress is to be made in this area, however, we must first ask a prior question of methodological import: Was $\mathfrak{P}^{46}$ composed through dictation or through visual copying?[37] It is essential to raise this question at this point than outright assume that $\mathfrak{P}^{46}$ is a product of the latter without the benefit of proofs.

The phonetic variations in $\mathfrak{P}^{46}$ perhaps may be conjured as a compelling argument for a dictation context. This is particularly important considering that most of the variations in $\mathfrak{P}^{46}$ are orthographic in nature. In fact, there are numerous and widely scattered occurrences of **ο-ω/ω-ο**, **ου-ω**, **υ-η/η-υ**, **ι-ε/ε-ι**, **αι-ε/ε-αι**, and **c-ζ/ζ-c** interchanges—similar phonetic features prompted Milne and Skeat to espouse a dictation context for Codex Sinaiticus.[38]

As is widely recognised, the initial vowel in **υμεις** and **ημεις** came to be phonemically pronounced alike as **ιμεις**,[39] and can be readily confused in a dictation context. In fact, of the 22 orthographic variations involving the derivative forms of this pronoun, 17 are to the direction of the 1st person plural (**υμεις** to **ημεις**)[40] but there

[37] On this debate see, T.C. Skeat, "The Use of Dictation in Ancient Book Production," *Proceedings of the British Academy* 42 (1956): 179-208; repr. *CBW-Skeat*, 1-32, who provided a very informative historical summary of the debate since the 18th century. See also, Alphonse Dain, *Les Manuscrits* (Paris: Les Belles Lettres, 1975), 20-22, 41-45; Klaus Junack, "Abschreibpraktiken und Schreibergewohnheiten in ihrer Auswirkung auf die Textüberlieferung," in *New Testament Textual Criticism* (eds. E.J. Epp and G.D. Fee; Oxford: Clarendon, 1981), 277-95. For specific manuscripts, see H.J.M. Milne and T.C. Skeat, *Scribes and Correctors of Codex Sinaiticus, with contributions by Douglas Cockerell* (London: British Museum, 1938), and Jongkind, *Scribal Habits*, 21-24, for Codex Sinaiticus; Sheldon MacKenzie, "The Latin Column in Codex Bezae," *JSNT* 6 (1980): 58-76, and David Parker, "A Dictation Theory of Codex Bezae," *JSNT* 15 (1982): 97-112; repr. *Manuscripts, Texts, Theology*, 5-18, for Codex Bezae. See also, Wayment, "Scribal Characteristics of the Freer Pauline Codex," esp. pp. 252-54.

[38] But cf. Kirsopp Lake, Review of H.J.M. Milne and T.C. Skeat, *Scribe and Correctors of Codex Sinaiticus*, *CP* 37/1 (1942): 91-96, esp. 94-95; and Henry Sanders, Review of H.J.M. Milne and T.C. Skeat, *Scribes and Correctors of Codex Sinaiticus*, *AJP* 60/4 (1939): 486-90, esp. 487-89.

[39] For instance, see Aland and Aland, *Text of the New Testament*, 286.

[40] Rom 15.5a; Heb.10.34b, 35; 1Cor 7.15b; 2Cor 7.15a; 8.8 (υ/ημετερας); 9.14a, b; Eph 1.18b; 3.13c, d; 6.22; Gal 1.6b; Phil 2.5b; Col 1.7c; 2.4a, 13a.

are also five cases to the other direction (ⲏⲙⲉⲓⲥ to ⲩⲙⲉⲓⲥ).[41] Regarding ⲟ-ⲱ/ⲱ-ⲟ interchange, the distribution is at eight and nine apiece.[42] But in regard to verbs ending with ⲁⲓ-ⲉ/ⲉ-ⲁⲓ, the pattern goes in favour of the latter—nine out of fourteen.[43]

Furthermore, there are other variations that are best viewed as having resulted from sound confusion. For instance, in Heb 11.36 the genitive singular πυλακης was altered to the plural dative πυλακαις.[44] 2Cor 9.11 (ητις το ει τις [*cum* D*]) no doubt is also a product of sound confusion, and so is 1Cor 12.31 (και ετι το και ει τι). Some other examples may be derived from our list, the point being that there seem to be indications that some orthographic variations are mediated by sound confusion which can point to a dictation context in the transmission process.

There is, however, evidence from the same pool of phonetic variants that indicates otherwise. First, we have lexical units that are, orthographically-speaking, consistently formed. In particular, we can cite the itacistic representation of ⲓ as ⲉⲓ, in the dative pronoun ⲏⲙⲉⲓⲛ and ⲩⲙⲉⲓⲛ, which is consistently reflected throughout with -ⲉⲓ- than with -ⲓ-.[45] The absence of interchange does not favour a dictation context.

Second, looking at the nasals, particularly compounds with the prefix ⲥⲩⲛ-, shows a clear attempt toward "formal" consistency as well,[46] as shown in Table 4-A7:

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[41] Rom 16.1b; 2Cor 1.11c; 5.12c; Eph 6.12a; Col 4.8b.

[42] -ⲟ to -ⲱ: Rom 6.2; Heb2.3; 12.1c; 1Cor 15.49; 2Cor 5.8a, 11a; Phil 1.22b; and Col 2.18;
-ⲱ to -ⲟ: Rom 9.17, 29c; 10.14a; Heb7.3; 10.22a; 12.28a; 1Cor 13.12b; 16.2b; and Gal 6.12b.

[43] -ⲁⲓ to -ⲉ: Rom 13.5c; 1Cor 6.1; 10.27a; and Eph 4.23, 24;
-ⲉ to -ⲁⲓ: 13.14a; Heb 10.29a, 32; 12.3a; 1Cor 6.7c, d; 15.17a; 2Cor 12.13; and Col 2.10a.

[44] Royse, *SH-M*, 319, noted "Although the dative plural seems meaningful here, a sound confusion is quite possible. Cf. Hoskier, *Commentary*, 56.

[45] In fact, in Gal 4.16 (f84^{r}-I^{26}) where our scribe mistakenly transcribed ⲩⲙⲉⲛ at the first instance, the desire for consistency is also made evident, since he also (perhaps *in scribendum*) corrected it by supralinearly inserting the missing *iota*.

[46] Note that συμ + φ- is consistently not transformed to συνφ-; but συμπ is transformed to συνπ-.

TABLE 4-A7 TABLE OF MORPHOLOGICAL TRANSFORMATION OF NASAL PREFIXES	
συγκ- → συνκ-	Rom 8.17b; 11.10, 17b; Heb 4.2; 11.9b, 25; 1Cor 2.13a; 9.23; 2Cor 6.16a; Eph 3.6a; 5.11b; Gal 3.23
συγχ- → συνχ-	1Cor 12.26d; 13.6; Phil 2.17b, 18b
συγγ- → συνγ-	Rom 9.3b; 16.7c, 21b; 1Cor 7.6
συζ- → συνζ-	1Cor 1.20; 2Cor 7.3b; Phil 4.3b
συλλ- → συνλ-	Phil 4.3c
συμβ- → συνβ-[47]	1Cor 4.8b; Eph 4.16a; Phil 2.2
συμμ- → συνμ-	Rom 8.16; 8.29; 1Cor 9.13c; Eph 3.6c; Phil 3.17a
συμπ- → συνπ-	Heb 4.15; 1Cor 12.26b
συμψ- → συνψ-	Phil 2.2
συσ- → συνσ-	Eph 3.6b; Gal 4.25c; 2.25b

Again, the lack of interchange adds weight to our observation that this could not have been possible in a dictation context—only a visually-guided copying environment could have resulted from this remarkable orthographic consistency. This then leads us to another component, that is, the visual lay-out of our codex, particularly in portions that are marked with variations that cannot be attributed otherwise but only to the visual use of an *exemplar*. For this, we now turn to some examples.

B. Some Minor Examples

1. *2Cor 12.19 (F74^{r})*

L^{01}: $\overline{\pi\nu}$ι περιεπατησαμεν ου τοις αυτοις ϊχνεσ$\overline{\iota}$
L^{02}: $^{12.19}$ **ογ** παλαι δοκειτε οτι ϋμειν απολογουμεθα
L^{03}: κατεναντι $\overline{\theta\upsilon}$ λαλουμεν τα δε παντα αγα|

In his apparatus, Sanders correctly discerned a correction event here and assigned it to our scribe.[48] Except for the assignment,[49] I concur that there is indeed a correction attempt here, albeit incomplete,[50] as revealed in this magnified image:

[47] The lone anomaly here is 1Cor 2.16, where our scribe copied συμβιβα=||σει.

[48] Sanders, *TCPC*, 84, "ου *man 1, sed scr supra rasuram*".

[49] A later corrector is more likely than our scribe, since erasure (by water and sponge) is not characteristic of our scribe; he either used expunging dots or(/and) a slashing stroke to effect deletion.

[50] Royse, *SH-M*, 217, disagreed with Sanders and noted, "… while there is some discoloration of the papyrus here, I can see no sign of erasure." But I think Royse misjudged the "discoloration" component in this instance, as the "discoloration" is not *of* the papyrus itself but a result of an erasure (most likely through water and sponge), hence, a correction by a later hand is in view here. It must be added, however, that the alteration was not completely successful, as the particle was not fully erased.



Figure 4-1.1 Enlarged image of 2Cor 12.19 showing the erasure on ⲟⲩ.

Royse classified this variant under his "harmonization to the context".[51] However, it is difficult to imagine how the harmonisation occurred in this instance, since the two clauses are conceptually unrelated as well as visually dissimilar (i.e., ⲟⲩ ⲧⲟⲓⲥ ⲁⲩⲧⲟⲓⲥ [v.18] vis-à-vis ⲟⲩ ⲡⲁⲗⲁⲓ). In fact, under present circumstances, it is extremely difficult to think of any way how this variation might have appeared in 𝔓[46], unless this reading was already in the *exemplar*, which our scribe then reflected in his codex. But the annotation must have been puzzlingly marked. Since the tradition is divided[52] it might have been marked for replacement in the *exemplar*, as "ⲟⲩ ⲡⲁⲗⲁⲓ" (i.e., "not" ⲡⲁⲗⲁⲓ) with an intent of substituting ⲡⲁⲗⲁⲓ with the more familiar ⲡⲁⲗⲓⲛ, but the plan was not fully consummated, since the replacement word was not written or was perhaps unintelligibly written in the *exemplar*, and therefore our scribe left the line confusing as it is now; which in turn triggered the correction attempt (later?) by incompletely obliterating ⲟⲩ to resolve the conundrum. Such a perplexing scenario seems to account better for the emergence of this difficult variant. That there were confusing or unintelligible markings in the *exemplar* is further demonstrated in the next example.

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[51] Royse, *SH-M*, 269, 344.

[52] 𝔓[46] is alone reading ⲟⲩ ⲡⲁⲗⲁⲓ. But the textual tradition is divided between the less familiar adverb ⲡⲁⲗⲁⲓ (א*ABFG 0243 6 33 81 256 365 424[c] 1175 1319 1573 1739 1881 2127 it[ar, b, d, f, r] vg cop[sa] Ambrster) and the more common ⲡⲁⲗⲓⲛ (א[2]DKLPΨ 075 0150 0278 104 263 424* 436 459 630 1241 1505 1852 1912 1962 2200 2464 it[g, o] vg[mss] syr[p, h] cop[bo] arm geo slav Chrys).

2. *1Cor 15.51 (F59^{v})*

L^{01}: παντες ος κοιμηθησομεθα ος παν
L^{02}: τες δε αλλαγησομεθα $^{15.52}$ εν ατομω

The transmission history of this passage is marked with considerable difficulty, transcending the traditional "text-type" boundaries. Gordon Fee identified at least five basic variant forms:[53]

παντες ος κοιμηθησομεθα παντες δε αλλαγησομεθα	(BD^{c}KPΨ 81 614 𝔐 *al*)
παντες κοιμηθησομεθα ος παντες δε αλλαγησομεθα	(ℵC 33 1739 *al*)
παντες ος κοιμηθησομεθα ος παντες δε αλλαγησομεθα	(𝔓^{46} A^{c} Origen)
παντες κοιμηθησομεθα παντες δε αλλαγησομεθα	(A*)
παντες αναστησομεθα ος παντες δε αλλαγησομεθα	(D* Mar)

No doubt, 𝔓^{46}'s reading can hardly compete as the *Ausgangstext*, as it cannot satisfactorily account for the emergence of the rest of the readings, despite the fact that it is the earliest surviving witness to the text. Conversely, its reading is best explained as arising from a conflation of readings #1 and #2[54] that must have transpired at a very early period (even earlier than 𝔓^{46}'s *exemplar*), accidentally getting assimilated into the *exemplar* or its ancestors,[55] since supports for both locations of the negative particle are traceable in the textual tradition as shown above.[56] In fact, Zuntz also explains that this duplication of the negative particle "betrays the combination of a variant with the basic text."[57]

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[53] Gordon Fee, *The First Epistle to the Corinthians, NICNT* (Grand Rapids, MI: Eerdmans, 1987), 796, n3.

[54] So Zuntz, *TEDCP*, *255;* Fee, *First Epistle to the Corinthians*, 796, n3. See also Metzger, *TCGNT*^{2}, 502.

[55] That the presence of ου in the second clause is also supported by D* Marcion Tertullian and also by ℵBC as well as by DF (although they reflected the conjunction ουν, [I reckon that the reading is related to the variation]), strongly suggests that the "error" must have transpired very early in the tradition, which was then carried onto the manuscripts that were copied from it.

[56] Both Jean Héring, *The First Epistle of Saint Paul to the Corinthians* (trans. A.W. Heathcote and P.J. Allcock; London: Epworth, 1962), 180, n63 and 181, n64, and C.K. Barrett, *A Commentary on the First Epistle to the Corinthians: Black's New Testament Commentary* (London: Black, 1968), 380-81, suggested that the emergence of the various readings could have been theologically motivated: Paul and his contemporaries died already and the *parousia* is still yet to come, hence, the transposition of the negative particle.

[57] Zuntz, *TEDCP*, 255, "In 1 Cor. XV.51 ('we shall [not] all sleep, but we shall [not] all be changed') the papyrus has a *ου* both in the first and in the second clause. There is ample and very ancient evidence

3. *1Cor 7.34 (F46^{r})*

L[11]: η γυνη η αγαμος και η παρθενος
L[12]: η αγαμος μεριμνα τα του κῡ ϊνα

Comparing 𝔓[46]'s reading against UBS[4]-NA[28]'s ἡ γυνὴ ἡ ἄγαμος καὶ ἡ παρθένος μεριμνᾷ (*cum* 𝔓[15] BP) shows that the phrase η αγαμος was copied twice and gives the appearance of a dittography, especially because of the visual proximity of the phrase. However, what we in effect see unfolding here is a further revelation of how the *exemplar* might have looked—it is already embellished with dual-locations of the phrase since other manuscripts already reflect a juxtaposed reading, i.e., η γυνη και η παρθενος η αγαμος μεριμνα (DFGKLΨ 056 0142 0150 0151). This dual-location was faithfully reflected in the codex, perhaps after a painstaking decision on which reading to chose. But are there any indications that our scribe indeed had a habit of indecision when confronted with difficult readings he discovered in his *exemplar*? The following major examples offer answers to this question.

C. "Let the corrector settle that":[58] Some Major Examples

1. *1 Cor 15.2; Heb 10.10; and 1Cor 3.2*

L[17]: [15.2] δι ου και σωζεσθε τινι λογω ευηγ
L[18]: γελισαμην ϋμειν ----------
L[19]: ќάτὲχὲϊν ει κατεχετε εκτος ει μη
L[20]: εικη επιστευσατε [3] παρεδωκα γαρ

Our scribe's difficulty in comprehending some portions of his *exemplar* is inescapably demonstrated in this example, which is one of the most visually abnormal in the whole of our codex. In fact, Zuntz describes the correction event here as "the decisive passage" proving that "philological technique was applied to the text of the Epistles

for either position of the negation. The (unique) duplication in P[46] betrays the combination of a variant with the basic text. The corrector of 𝔓[46] overlooked this conflation…"

[58] The phrase is by Zuntz, *TEDCP*, 254, which he used to caricature our scribe's recourse to leaving spaces for instances that were unclear to him, either because the *exemplar* was illegible or was defective at those instances. I used it here to underscore the same.

already in the second century."[59] I concede that Zuntz is at his best in his analysis of this one, especially in the way he depicted how the *exemplar* might have looked.

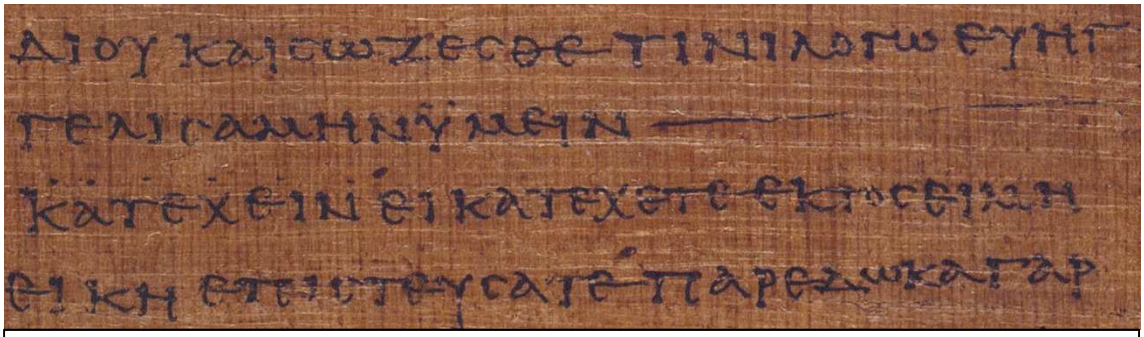


Figure 4-1.2 F57^{r}-II$^{16-20}$: What could have been in the *exemplar* that prompted the scribe to leave a blank mid-way through the line?

As Fig. 4-1.2 shows, after the pronoun ΥΜΕΙΝ in l^{18} is a rather long line filler, measuring 4.1 cm—which can account for at least 11-13 characters—as well as the infinitive ΚΑΤΕΧΕΙΝ in l^{19} with six expunging dots above it.[60] Hinting at the possibility of the presence of a "Western" reading, Kenyon noted in his apparatus, "*tanquam* οφειλετε κατεχειν*, quod habent* D*FG".[61] Zuntz, in effect, developed this line of argument by proposing that ΟΦΕΙΛΕΤΕ was in the *exemplar* already but was written very badly or puzzlingly marked, prompting the scribe to leave an empty space. Zuntz added that in the *exemplar* ΚΑΤΕΧΕΙΝ was also marked for deletion but was still copied because "this was not clearly indicated".[62] Eventually, a corrector put right this terrible blunder by marking the vacant space with a line-filler, and purging the intrusive κατεχειν.[63] Except for the fact that the empty space could account for 11-12 characters (οφειλετε is eight

[59] Zuntz, *TEDCP*, 254.

[60] There are also what appear to be ink smudges in between the final -Ν and the initial Ε- of the proceeding conjunction. But this seems to most likely to be a reading mark, due to its ink density and direction of the stroke (see also the same mark in the following line).

[61] Kenyon, *CBBPIII-1936*, 86.

[62] Zuntz, *TEDCP*, 254.

[63] Zuntz, *TEDCP*, 254.

only)[64] and given that no contrary evidence presents itself, Zuntz's proposal best accounts for the facts,[65] which in turn corroborates our observation about our scribe's intermittent visual problem with his *exemplar*.

Heb 10.10 (f31^{r}-ll$^{03-04}$) is another instance showing our scribe's recourse to leaving blank spaces in view of his difficulty reading or comprehending his *exemplar*. As shown in Fig. 4-1.3, our scribe copied **ΠΡΟC** only and left a blank space of about 1.3 cm (equal to 4-5 characters). The contextually nonsense reading and the blank space cannot be satisfactorily explained except as another *exemplaric* problem.[66] A corrector (a later hand) supplied the correct reading by intralinearly inserting **ΦΟΡΑC** into the vacant space.

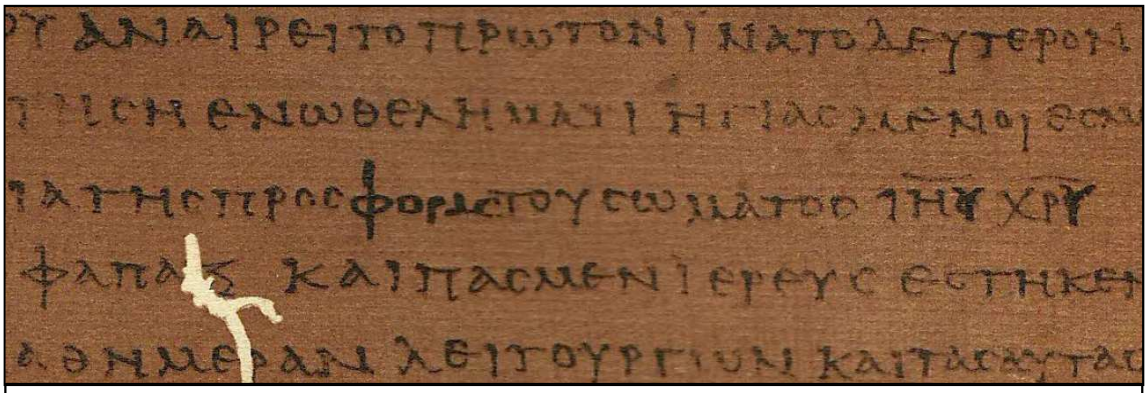


Figure 4-1.3 F31^{r}-ll$^{03-07}$: Filling-in the Gap—**ΦΟΡΑC** inserted to account for the gap.

We have yet one more example of this phenomenon: 1Cor 3.2 (f40^{r}-ll$^{19-20}$)—a variation that has never been recorded until now. But earlier students cannot be arbitrarily faulted for missing this since neither Kenyon's transcription nor his facsimile make it obvious; only through a meticulous examination of the actual manuscript's inking features is this textual problem revealed.

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[64] This will also make the projected total number of characters in this line to only 21, which would be the shortest on the page.

[65] See also, Zuntz, *TEDCP*, p. 254, n3. Royse, *SH-M*, 230-31, presented a slightly variant form of the reading found by our scribe in his *exemplar*. But whether Zuntz's or Royse's proposal is taken, the likelihood remains that there was ambiguity in the *exemplar* prompting our scribe to do what he did.

[66] Royse, *SH-M*, 235-36, citing Zuntz, also sees the probability that the *exemplar* is defective at this point. In fact, the grammatical confusion of the end-line *nomina sacra* (i.e., $\overline{\text{ιη}}$ >ς/υ< $\overline{\chi\rho}$>ς/υ<) must be viewed in relation to this problem also.

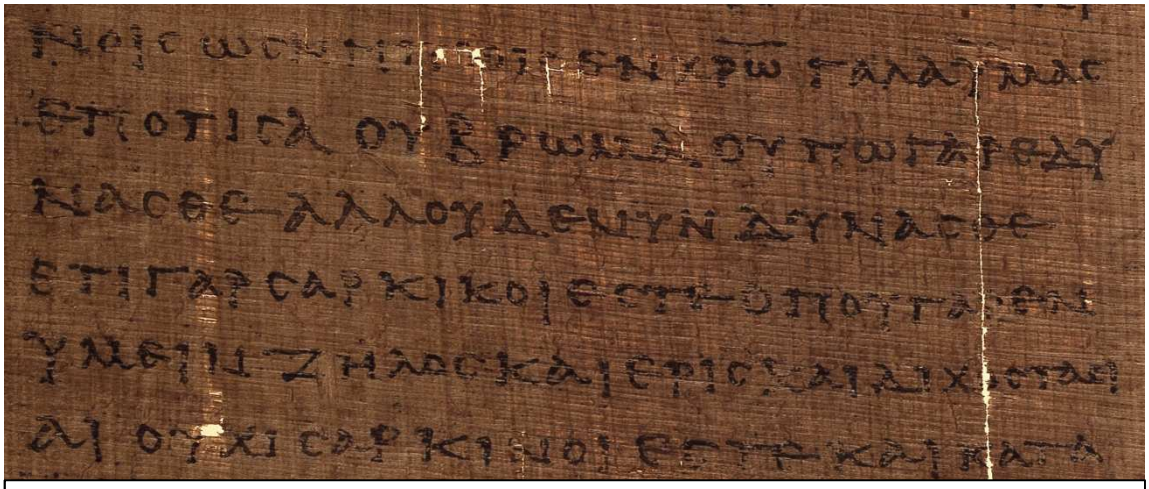


Figure 4-1.4 F40^{r}-II$^{16-23}$: The inking of εδυνασθε αλλου δε νυν (II$^{19-20}$) differs from the rest of the lines, having been written in a slightly paler black ink.

As shown in Fig. 4-1.4, the phrase εδυ||νασθε αλλ ουδε νυν in II$^{19-20}$ is written in a slightly paler black ink (a bit greenish black in the actual manuscript).[67] Palaeographically-speaking, the hand is definitely from our scribe, but the ink residue is unquestionably different from the rest (notice that the inking density of εποτισα ου βρωμα ουπω γαρ [I^{19}] and δυνασθε [I^{20}] are very similar). Hence, given that our scribe exhibits a practice of leaving vacant spaces, it is possible that at the first instance, our scribe skipped copying this phrase but consciously allotted enough space (a combined total length of about 7.5 cm) for it or at least what he thought was sufficient.[68] He then wrote the phrase at a later time, when he was more certain of the text to copy. But what could have prompted him in the first place to leave such a long space (in fact, much longer than the vacant space in 1Cor 15.2!)? An answer might be hinted by the fact that a reading without this phrase (or very similar to this phrase, i.e., αλλα ουδε ετι νυν δυνασθε) is also read by 049 056 0142,[69] manuscripts that are not infrequent

[67] For a clearer image of the page showing the inking difference, see <http://ntvmr.unimuenster.de/community/modules/papyri/?zoom=18&left=0&top=0&site=INTF&image=10046/89234/620/10/126>.

[68] As it turned out, the scribe left out ετι—perhaps due to lack of space or due to the reading he found in the *exemplar* that he eventually used.

[69] See *DNTAP*$^{2.1}$, 172, which interprets this omission as a case of *homoioteleuton*.

supporters of $\mathfrak{P}^{46}$'readings. Hence, the *exemplar* might have been visually problematic again at this instance (either illegible or defective), which was resolved at a later time, perhaps after having consulted another *exemplar* (with the omitted ετι [*cum* B 0185]).

2. *2Cor 10.8 and Rom 16.7*

There are instances where our scribe seems to have understood fairly the exegetical ramifications of the presence of conflicting readings in his *exemplar*. Either one reading was in the main text and the other in the margins (or both were already incorporated in the text), as can be evidenced in many correction examples where our scribe, whether *inter scribendum* or during his second reading, interlinearly placed "corrections" but never effectively enforced them, as it were, since he did not accordingly put expunging dots ("˙") or slashing strokes ("/" or "—") on the character/s at issue (perhaps intending them to be considered as "alternative readings" only).[70]

But there are also clear cases where our scribe had been baffled by the presence of two conflicting readings in his *exemplar*, and perhaps scratching his head in confusion, resorted eventually to a conflated reading, albeit exegetically opposed! One such example is 2Cor 10.8 ($f70^{v}$).

L^{18}:	θως αυτος ο $\overline{\text{χρς}}$ ουτως και ημεις $^{10.8}$ εαν
L^{19}:	γαρ περισσοτερον τι ΚΑΥΧΗϹΩΜΑΙ
L^{20}:	ΚΑΥΧΗϹΟΜΑΙ περι της εξουσιας ημω̄

In this instance, our scribe's confusion becomes evident as he conflated two readings, each with respectable manuscript supports, producing an unattested version of his own.[71] We have no way of knowing precisely how his *exemplar* looked like at this point, whether

[70] For instance: Rom 9.17 ($f12^{r}$-l^{16} ενδειξ<ο/ω>=||μαι); Heb 10.34b ($f32^{r}$-l^{09} <η/υ>μων); 12.28 ($f36^{r}$-l^{23} εχ<ο/ω>μεν); 1Cor 13.12 ($f55^{r}$-l^{03} προς<ο/ω>πον); 15.17 ($f58^{r}$-l^{01} εστ<αι/ε>); Eph 1.18 ($f75^{v}$-l^{18} <η/υ>μας); and 6.22 ($f81^{r}$-l^{06} <η/υ>μων). Conversely, the following seems to have been intended to be "genuine corrections", as expunging dots and/or slashing strokes are placed on them: 2Cor 1.11 ($f61^{v}$-l^{12} υπερ υ̇<ηˍ>μων); Gal 1.6 ($f81^{r}$-l^{22} ꙗ<υ>μας); Phil 3.14 ($f89^{r}$-l^{09} σκοπω̣<ο>ν); and 3.15 ($f89^{r}$-l^{11} φρ>ω/ο<νωμεν).

[71] ΚΑΥΧΗϹΩΜΑΙ is supported by BCDFGHKΨ 049 075 0150 0151^{c}, whilst ΚΑΥΧΗϹΟΜΑΙ is read by ℵLP 0151* 0209 0243.

it was defective or laden with difficult correction *sigla* (which he could not understand), but what we know is that both readings must have been puzzlingly *in* it already,[72] rather than a random product of our scribe's creative mind! What may be further noted about this variant is the fact that it is the only instance in $\mathfrak{P}^{46}$ where our scribe fully reflected two readings distinguished only by the **ο-ω** sound confusion.[73] This strongly indicates that the correction note in his *exemplar* was also written in full and not the usual interlinear vowel annotation only.

Rom 16.17 ($f21^{v}$) in the *exemplar* must have been equally perplexing, as **ποιουντας** was not only written twice at different places but also with an expanded element, as shown in the following transcription:

L^{07}:	αι εκκλησιαι πασαι του $\overline{\text{χρυ}}$ $^{16.17}$ παρακαλω
L^{08}:	δε υμας αδελφοι σκοπειν τους τας διχο
L^{09}:	στασιας και σκανδαλα παρα την διδαχην
L^{10}:	**ποιουντας** ην ϋμεις εμαθετε **η λεγον**
L^{11}:	**τας η ποιουντας** εκκλεινατε απ αυτων

It has been suggested that this quite long addition variant is a harmonization to the context.[74] However, given the above examples it seems more probable that what we witness here is another case of *exemplaric* variation, further exposing the maladies of our scribe's textual base. Holmes rightly noted this as a "deliberate" expansion.[75] But to this,

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[72] Zuntz, *TEDCP*, 254-55, described this variation as a case where our scribe "unblushingly had put καυχήσωμαι and καυχήσομαι side by side". But prior to Zuntz, Sanders, *TCPC*, 36, had already noted this "doublette" and suggested, "This doublette in $\mathfrak{P}^{46}$ probably indicates that corrections were being made in the manuscripts before the time of the recensions. In the parent of $\mathfrak{P}^{46}$ καυχήσωμαι, which has Western and Antiochian support, stood in the text. Later someone had written above it καυχήσομαι, which was then copied into $\mathfrak{P}^{46}$. The second reading became popular in Egypt and was adopted for the Alexandrian recension." Royse, *SH-M*, 338, n777, suggested that a reversed order is equally tenable. At any rate, all agree that there must have been a confusing marginal notation in this part of the *exemplar* already which puzzled our scribe.

[73] In other similar corrections, only the letter—either **ο** or **ω**—had been reflected. For instance: Rom 9.17 ($f12^{r}$-l^{16} ενδειξ<ο/ω>=||μαι); and Heb 12.28 ($f36^{r}$-l^{23} εχ<ο/ω>μεν).

[74] Royse, *SH-M*, 269, 343-44. Kenyon and Sanders simply noted the variation in their apparatuses, so is *DNTAP*[2.1].

[75] Holmes, "Earliest Commentary on Romans," 193. This view finds some resonance in C.E.B. Cranfield, *A Critical and Exegetical Commentary on the Epistle to the Romans: International Critical Commentary Vol. II* (Edinburgh: T & T Clark, 1979), 797, n2, who noted that "The addition of (η $\mathfrak{P}^{46}$)

we must quickly add: not added by our scribe, but something that is already embellished in his *exemplar.* As to which direction it originated from, it is difficult to tell, however, especially with the double occurrence of ποιουντας at different places. It is much easier to account for the η λεγοντας η ποιουντας as we already see this reading in DFG (sans the first particle η). One possibility is that we have two variants here that independently arose, at different stages, which have become amalgamated in $\mathfrak{P}^{46}$. At any rate, this variation cannot be accidental, but is a deliberate attempt to further the scope of Paul's injunction[76]—an emendation that must have predated our manuscript.

3. *Rom 15-16*

But the seeming peculiarity of Rom 16.17 is actually not an independent textual problem; it is part and parcel of the bigger transmission question of the last two chapters of Romans, for which our manuscript presents some special readings of its own.

Whilst Rom 16.7 is equally at the centre-stage of gender debate, whether a person named ιουνιαν is a male or female "apostle" (hanging its fate on an accent),[77] our codex reflects here a rather anomalous reading--ιουλιαν.[78] Comfort explained the alteration mechanically: "It is possible that this variant was the result of a transcriptional

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λεγοντας η before ποιουντας by $\mathfrak{P}^{46}$ DG *m* looks like an unsuccessful attempt at improving the sentence by someone who felt that Paul's meaning would be more adequately expressed if a reference to speaking (contrary to true doctrine) were included, but failed to notice that the effect of the addition would be to leave τας διχοστασιας και τα σκανδαλα in the air."

[76] As Holmes, "Earliest Commentary on Romans," 204, commented, "Here Paul exhorts the Roman congregation to "take note of" and "avoid" those who cause (ποιουντας) dissensions and difficulties. The text of $\mathfrak{P}^{46}$, with its repeated η (apparently lost from DFG when the comment was incorporated into the text), has every appearance of a marginal comment—"either speaking about or causing"—that both affirms and expands somewhat Paul's injunction."

[77] Ἰουνίαν is supported by $B^2D^2L\Psi^{vid}\mathfrak{M}$ 0150 33 81 104 256 263 365 424 436 459 1175 1241 1319 1573 1739 1852 1881 1912 1962 2127 2200 Chrysostom. On the other hand, ℵAB*CD*FGP are without accent but are believed to have read the masculine Ἰουνιᾶν. For a recent review of this debate from a text-critical perspective, see E.J. Epp, "Text-Critical, Exegetical, and Socio-Cultural Factors affecting the Junia/Junias Variation in Romans 16,7," in *New Testament Textual Criticism and Exegesis: A Festschrift for J. Delobel* (Bibliotheca Ephemeridum Theologicarum Lovaniensium CLXI; ed. A. Denaux; Leuven: Leuven University Press/Peeters, 2002), 227-91; see also his "Minor Textual Variants in Romans 16:7," in *Transmission and Reception,* 123-41.

[78] Also read by 6 606 1718 2685 $it^{ar,b}$ vg^{mss} cop^{bo} eth Jerome.

error—the Greek *nu* was made a *lambda*."[79] But this is highly farfetched in view of the morphological differences of the two letters, regardless of whether the *exemplar* was a majuscule or minuscule. On the other hand, Epp suggested that the alteration is an influence from the feminine ΙΟΥΛΙΑΝ in 16.15.[80] Whilst this is a logical possibility, this is not without complications either, since 16.15 in $\mathfrak{P}^{46}$ presents another of its idiosyncratic readings. Instead of ἀσπάσασθε Φιλόλογον καὶ Ἰουλίαν, Νηρέα καὶ τὴν ἀδελφὴν αὐτοῦ $\mathfrak{P}^{46}$ has ΑCΠΑCΑCΘΕ ΦΙΛΟΛΟΓΟΝ ΚΑΙ ΒΗΡΕΑ ΚΑΙ ΑΟΥΛΙΑΝ ΚΑΙ ΤΗΝ ΑΔΕΛΦΗΝ ΑΥΤΟΥ. $\mathfrak{P}^{46}$ is again alone in this reading but its version shows a three-level alteration: 1) the transposition of ΙΟΥΛΙΑΝ and ΝΗΡΕΑ; 2) the alteration of the first letters of the two proper names, i.e., Ν- to Β- and Ι- to Α-; and 3) the addition of ΚΑΙ. Royse has an ingenious explanation for this unattested reading,

> In the exemplar the names were marked for transposition by the use of the letters A and B, as is known from other manuscripts… However, our scribe misinterpreted the letters as being intended to replace the letters over which they were written, and thus created βηρεα αουλιαν.[81]

This is a very attractive solution to the rather intriguing name changes in this passage. The only problem with this, I think, is that it does not sensibly account for the other transposition variants on the same page (ll[1,10, and 16]), if indeed in our scribe's *exemplar* the A-B transposition *siglum* was ever used in this part of $\mathfrak{P}^{46}$ (or elsewhere for that matter). It also sheds no direct light to the name alteration in 16.7. But what is clear from Royse's proposal is that our scribe's *exemplar* was likely visually marked at this point.

There is no easy answer to this convoluted textual scenario. I, too, do not have any surprise solution. But what may be suggested is that, instead of looking at these

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[79] Philip Comfort, *New Testament Text and Translation Commentary: Commentary on the Variant Readings of the Ancient New Testament Manuscripts and How they relate to the major English Translations* (Carol Stream, Il.: Tyndale House, 2008), 476.

[80] Epp, "Text-critical, Exegetical, and Socio-Cultural Factors" 265, "(Ἰουλίαν) is recorded… but doubtless influenced by the Ἰουλίαν of Rom 16,15"; see also, Idem, "Minor Variants in Rom 16:7," 131.

[81] Royse, *SH-M*, 333-34.

variations piecemeal, it might help us to appreciate the conundrum better if the variations in these last two chapters are taken as representing an organic textual problem that has to do with the bigger question of the extent of collation and transmission of this epistle, that has been preserved quite idiosyncratically in the *exemplar* of $\mathfrak{P}^{46}$. A look at the visual lay-out of f21^{v}-ll$^{01\text{-}18}$ helps drive this point:

L^{01}: [14] φλεγοντα ερμην ερμαν[82] πατροβαν και
L^{02}: τους συν αυτοις αδελφους [15] ασπασασθε
L^{03}: φιλολογον και βηρεα και λουκιαν[83] και
L^{04}: την αδελφεν αυτου και ολυμπαν και
L^{05}: τους συν αγιοις[84] αγιους [16] ασπασασθε αλληλους
L^{06}: εν φιληματι αγιω ασπαζονται ὑμας
L^{07}: αι εκκλησιαι πασαι του $\overline{\text{χρυ}}$ [17] παρακαλω
L^{08}: δε υμας αδελφοι σκοπειν τους τας διχο=
L^{09}: στασιας και[85] σκανδαλα παρα την διδαχην
L^{10}: ποιουντας ην ὑμεις εμαθετε η λεγον=
L^{11}: τας η ποιουντας[86] εκκλεινατε[87] απ αυτων
L^{12}: [18] οι γαρ τοιουτοι τω $\overline{\text{κω}}$ ημων $\overline{\text{χρω}}$ ου δουλευ̣=
L^{13}: ουσιν αλλα τη εαυτων κοιλια και δια της
L^{14}: χρηστολογιας και ευλογιας εξαπατωσιν τ̣ας
L^{15}: καρδιας των ακακων [19] η γαρ ὑμων ὑπακοη
L^{16}: εις παντας αφεικετο χαιρω ουν εφ υμ[ειν][88]
L^{17}: και[89] θελω δε υμας σοφ[ο]υς ειναι εις το αγαθοv
L^{18}: ακεραιους δε εις το κακον [20] ο δε $\overline{\text{θς}}$ τη[ς]

Undoubtedly, this page alone swarms already with remarkably intelligible variations! The only logical conclusion from this is that these alterations show that they are more than just "scribal slips"—they are deliberate sensible emendations that

[82] $\mathfrak{P}^{46}$ is alone in this reading, but D^{1}LΨ 049 056 0142 0151 also reflect a transposed reading ερμαν πατροβαν ερμην. ℵABCD*FGP 0150 have ερμην πατροβαν ερμαν, the reading favoured in many Greek text editions to be the "original" sequence.

[83] Φιλόλογον καὶ Ἰουλίαν, Νηρέα καὶ τὴν ἀδελφὴν αὐτοῦ is read by ℵABC^{2}D(D* αυτο)LP(om και^{3})Ψ 049 056 0150 0151 6 33 81 104 256 263 365 424 436 459 1175 1241 1319 1573 1739 1852 1881 1912 1962 2127 2200 2464 it$^{ar, b, d, f, g, mon, o}$ vg syr$^{p, h}$ cop$^{sa, bo}$ arm (eth) geo slav Origen^{lat} Chrysostom; and Pelagius. A and 0142 read Φιλόλογον καὶ Ἰουλίαν, Νηρέαν καὶ τὴν ἀδελφὴν αὐτοῦ whilst Φιλόλογον καὶ Ἰουνίαν, Νηρέαν καὶ τὴν ἀδελφὴν αὐτοῦ is surprisingly read by FG (and C* Νηρέα)!

[84] παντας is omitted here by $\mathfrak{P}^{46}$.

[85] Only $\mathfrak{P}^{46}$ attests to an anarthrous reading here.

[86] $\mathfrak{P}^{46}$ omitted the conjunctive και here before the imperative command.

[87] $\mathfrak{P}^{46}$ is joined here by ℵ^{2}ADFGLP 049 056 0142 0151 in reading the aorist εκκλιναTε, whilst ℵ*BCΨ 0150 read the present εκκλινετε.

[88] ℵ*ABCLP 0150 81 365 read εφ υμιν ουν χαιρω, whilst ℵ^{2}D^{1}Ψ 049 056 0142 0151 33 1739 read χαιρω ουν το εφ υμιν. $\mathfrak{P}^{46}$ aligns here with D*FG 323 1881 in reading χαιρω ουν εφ υμ[ειν].

[89] $\mathfrak{P}^{46}$'s additional και is also read by DFG, but without the adversative δε. ℵABC *rell* read the simpler θελω δε.

must have transpired *before* the copying session. These are already in our scribe's *exemplar*—they prove neither the creativity of our scribe nor his ineptness, but what they demonstrate is a revelation about his *exemplar*.

Perhaps the best example pointing to this *exemplaric* idiosyncrasy is the so-called "floating doxology" which we find located at the end of chapter 14 in some mss and after 16.23 or 24 in some others, but which we find in our codex at the end of 15.33 and then immediately followed by 16.1ff (Fig. 4-1.5).[90]

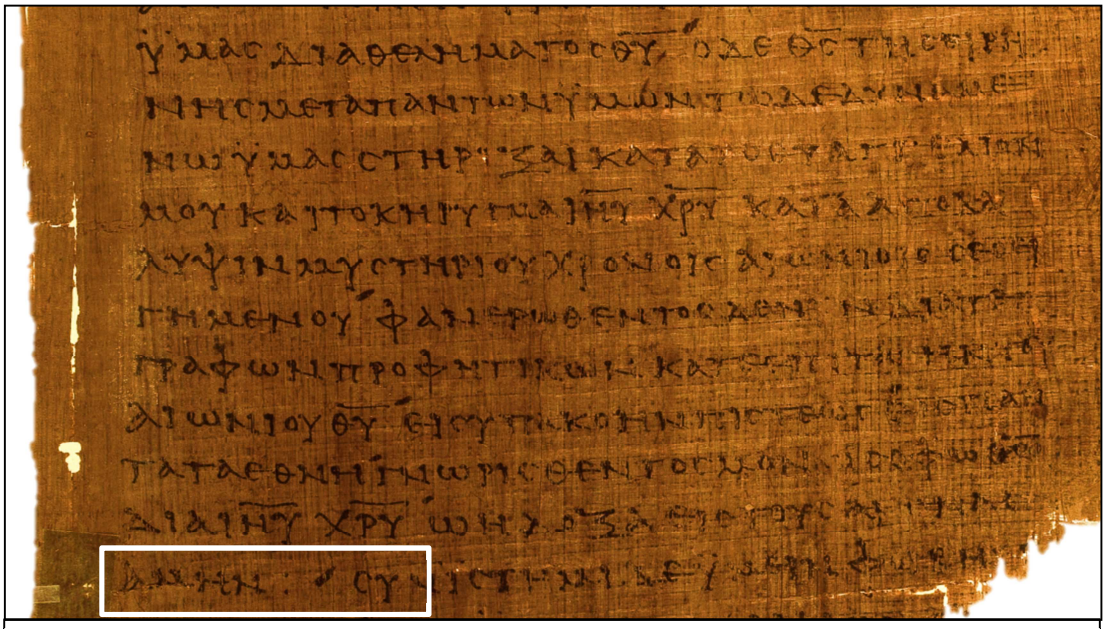


Figure 4-1.5 F20^{v}-II$^{09-19}$: Rom 15.33, 16.25-27, and 16.1 in focus.

[90] For the various locations of the doxology in Romans and citations of evidences for each, see Kurt Aland, "Der Schluß und die ursprüngliche Gestalt des Römerbriefes," in *Neutestamentliche Entwürfe* (München: Kaiser, 1979), 284-301, who sees at least fourteen different versions from the textual tradition; Peter Lampe, "Zur Textgeschichte des Römerbriefes," *Novum Testamentum* 27 (1985): 273-77; Gamble, *Textual History of the Letter to the Romans*; and Metzger, *TCGNT*^{2}, 471.

Kenyon, *CBBPIII-1936*, xviii, underscored the importance of the discovery of $\mathfrak{P}^{46}$ for the discussion of the three possible text-forms of Romans, in that it provided a material witness to the conjectural proposal of a 15-chapter-epistle, that has been first flaunted by Caspar Gregory. However, he too was unsure how to decisively settle that question through the evidence of $\mathfrak{P}^{46}$. Colwell, Review of Sanders, 98, was equally enthusiastic about the discovery of $\mathfrak{P}^{46}$ in relation the Romans debate and almost predicted that that manuscript-less proposal supporting a 15-chapter-epistle "receives its first manuscript tradition". As it turned out, however, $\mathfrak{P}^{46}$ has been the first and the last thus far. Gamble, *Textual History of the Letter to the Romans*, in fact, warned, "Although the value of ($\mathfrak{P}^{46}$) is great in these and other respects, it has not worked a revolution in our understanding of the history of the Pauline text, either in general or in respect of particular readings."

Many scholars regard Rom 16.25-27 as "re-located".[91] However, in the case of $\mathfrak{P}^{46}$ these verses must have been here in the *exemplar* already which our scribe copied, consciously aware of the concomitant questions governing its location. A few things need to be noted in view of this observation. First is the absence of any first hand correction activity in these verses; that the "re-location" was cast in 10 lines is undoubtedly a very recognizable "aberration" if indeed the scribe randomly and personally "re-located" the doxology, deviating from his *exemplar*, and therefore could have been easily corrected by the "proof-readers". Second is the presence of paratextual elements at the *end* of the doxology: a *dicolon*, a space-gap, and a reading mark (see inset, Fig. 4-1.5). The first two are definitely from the first hand and they seem to compositely function here as variation indicators, denoting our scribe's textual awareness about the peculiar location of the passage. The reading mark is from another hand, cognitively and visually conveying a reading unit, which in this case encompasses ω η δοξα εις τους αιωνας αμην.[92] What must be noted further in this regard is the unambiguous fact that there is no space-gap or a *dicolon* (or any first hand marker) at the *beginning* of the doxology (i.e., between υμων and τω in l[10])—and yet the reading marker (i.e., the *lector*) does not seem bothered any bit by it! But what is clear is the presence of a space-gap and a reading

[91] Whilst its history of research is interestingly important, this is not the place to discuss in length the debate on the authenticity of the "wandering doxology". The following may be consulted instead: Luceta Mowry, "The Early Circulation of Paul's Letters," *JBL* 63/2 (1944): 73-86, esp. 79-80; Gamble, *Textual History of the Letter to the Romans* (vis-à-vis Larry Hurtado, "The Doxology at the End of Romans" in *New Testament Textual Criticism, Significance for Exegesis: Essays in Honour of Bruce Metzger* [eds. E.J. Epp and G.D. Fee; Oxford: Clarendon, 1981], 185-99, who is also open to the [less likely] possibility that the doxology "arose in connection with a 15-chapter edition of Romans" [p. 198, n57], which finds its only support in $\mathfrak{P}^{46}$); Raymond F. Collins, "The Case of a Wandering Doxology: Rom 16,25-27," in *New Testament Textual Criticism and Exegesis: A Festschrift for J. Delobel*, 293-303; Parker, *New Testament Manuscripts and their Texts*, 270-74. These materials include a wide array of other materials dealing with the subject, including those outside of the text-critical discipline.

[92] Gamble, *Textual History of the Letter to the Romans*, 33, n85, has (mistakenly) proposed that Chapter 16 is "set-off from what precedes by a small diagonal slash". However, this is not tenable on two grounds: first, the reading mark (what he described as "small diagonal slash") is not from the first hand; second, this particular reading mark is not meant to "set-off" Chapter 16 but to restrict the reading unit to ω η δοξα εις τους αιωνας αμην.

mark at the *beginning* of the benediction in v.33 as well as the logical omission of the concluding ΑΜΗΝ![93] This implies that our scribe had taken Rom 15.33 and 16.25-27 as a coherent unit,[94] and this seems corroborated also by the presence of the eight reading marks within this unit.[95]

What point issues from all this is that these examples from the last two chapters of Romans could not have been the result of random creativity *during* the actual copying process; they must have been decided *before* the copying session, as this kind of well-ordered sensible "editorial" alterations requires cognitive and resource processing more than what is available to the scribe during the copying session.[96] Bruce Metzger indicated that the UBS Committee had difficulty deciding whether the reading of $\mathfrak{P}^{46}$ is "merely one of the several idiosyncrasies of the scribe of $\mathfrak{P}^{46}$, or somehow reflects a stage during which Romans circulated without chapter 16."[97] Idiosyncrasy it is indeed, not by "the scribe of $\mathfrak{P}^{46}$" but by his *exemplar* and the layers of traditions undergirding it!

How then might we conclude this sub-section, in regard to our scribe's *exemplar*? One answer comes from Zuntz:

> (Heb 10.1) is a wilful alteration, an alteration which could readily suggest itself to an attentive reader assessing the original text by the standards of educated Greek thought and mode of expression. *We shall meet before long with similar instances… Such variant readings in $\mathfrak{P}^{46}$ evidently cannot be ascribed the same origin as the numerous errors previously instanced. They are not due to a scribe's slovenliness: they are conjectures, and indeed*

[93] AFG 330 436 451 1506 1739 1881 2200 ℓ1021 $it^{f,g,mon,\ o}$ vg^{ms} also omitted ΑΜΗΝ here.

[94] Of course, we are cognizant also that the resulting reading is a bit grammatically awkward, randomly shifting focus from the "blessee" (ΠΑΝΤΩΝ ΥΜΩΝ) to the "Blesser" (ΤΩ ΔΥΝΑΜΕΝΩ).

[95] Reading marks are located in l^{09}, l^{14}, l^{15}, l^{16} (2x), l^{17}, l^{18}, and l^{19}. What may be inferred from these marks is that within the circle of this *lector* the location of the doxology at this place is a recognized fact.

[96] Whether the doxology is authentically Pauline or not is beyond the scope of this section, but in regard to our present inquiry I find myself in agreement with the observation of Schmid, "Scribes and Variants," 15: "(The doxology) is a purposeful creation, very well thought through. It is therefore highly unlikely that it has been created by a scribe on the fly… It is a conscious action that involves reasoning, preparation and resources in order to be carried out in the way we find it in our tradition." Gamble, *Textual History of the Letter to the Romans*, 34, has also recognized this: "… many special readings are to be found in ($\mathfrak{P}^{46}$), but the vast majority of these must be set down to scribal error and alleviative conjectural emendations. Yet the placement of the doxology at the end of Rom 15 clearly cannot be accounted for in these ways."

[97] Metzger, *TCGNT*[2], 473.

> *ingenious conjectures, witnessing to attentive study of the text and perfect command of the Greek language. Our scribe found them in his copy* and it is most unlikely that he should have been alone in propagating them.[98]

This speaks well of the text underlying $\mathfrak{P}^{46}$. The other side of the coin, however, is that despite the "excellent quality of the text" underlying it, the examples we cited thus far demonstrate that our scribe's *exemplar* was equally very imperfect in ways that influenced our scribe to commit "grievous mistakes"—these *exemplaric* deficiencies must not be attributed to our scribe.

III. CHARACTER-GENERATED VARIATIONS IN THE TEXT OF $\mathfrak{P}^{46}$

Alongside *exemplar*-related variations that competed against our scribe's concentration are variations that have to do visually with the morphology (formation) of a letter or group of letters. This is widely recognized in the literature, but herein I shall descriptively call this feature "character-generated variations,"[99] broadly defined as variations due to graphic similarity of certain letters that have been *accidentally* created by our scribe or could have been present already in his *exemplar* at the time of the production of his manuscript. The demarcation line between the two is admittedly hair-thin. At any rate, the confidence that we have about this type of variation is the high probability that they are character-related, which consequently resulted either in the shortening or lengthening of certain passages in our codex. They have been traditionally called in the literature as *haplography* and *dittography*, respectively.

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[98] Zuntz, *TEDCP*, 22-23. (Emphasis added).

[99] This is a familiar scribal phenomenon in the manuscript tradition, and is widely documented in the literature. Specifically, in relation to scribal activities, Colwell, "Scribal Habits," 112, described this phenomenon as a case of a "misplaced scribe": "The scribe loses his place, looks around and finds the same word, or at least same syllable or letter, and starts from there. If he looks ahead to find his place, the result is a gap in the text. If he looks back, the result is a text twice written (dittography). A special case of a gap caused by the leap is that where a word, or a least a syllable or a letter, is repeated immediately in the text. The writing of only one of these (haplography) causes the loss of the other."

A. Haplographies

Even the most careful of scribes was prone to accidentally omitting a letter, a word, or group of words, due to similar letter groupings or letter formation.[100] Collated against UBS[4]-NA[28] common text, I have documented 432 cases of omissions in $\mathfrak{P}^{46}$; 363 of which are grammatical in nature (e.g., nouns, verbs, etc.). The rest involves lengthy omissions. For our present purposes, I shall limit my discussion to the more quantitatively substantial omissions, the "incremental omissions"—*accidental* omissions due to optical forward leaps that quantitatively affected the length of a line(s),[101] and therefore ultimately the quality (exegesis) of the text.[102]

I recorded 70[103] instances of incremental omissions in $\mathfrak{P}^{46}$, accounting for a loss of 289 words or a cumulative total of 1,402 characters.[104] This indicates that our scribe

[100] Of course, some of these omissions may vie for the coveted title of being the "original reading" and some may even be "intentional". As my purpose in this section is focused on the visual factors that might have caused omissions in general, I shall not deal directly with these possibilities as sub-categories but nonetheless will mention them at appropriate junctures. The works of Zuntz and Royse on these categories may be consulted instead.

[101] This takes special significance when viewed against the observations of the earlier students of $\mathfrak{P}^{46}$. For instance, Zuntz, *TEDCP*, 19, commented, "… the omission of whole clauses owing to homoioteleuton is an outstanding characteristic of P46". Also, P. Benoit, "Le Codex Paulien Chester Beatty," *RB* 46 (1937): 58-82, p. 63, "*Les omissions par homoioteleuton sont vraiment frequentes et parfois considerable*"; see also, Hoskier, "Study of the Chester Beatty Codex," 162-63. Royse, *SH-M*, 289, although based only on the "singularly" attested readings, described this phenomenon as "one of the scribe's pervasive tendencies".

[102] The exegetical effect that longer omissions present has been duly recognized also by Royse, *SH-M*, 297, commenting that, "It is true that some of the other omissions alter the sense of the text in a striking fashion." In n558, he added, "This is especially true for some of the longer leaps…"

[103] Royse, *SH-M*, 283, n483, added one more possible case (Rom 14.18 [f18^{v}-l^{20}]). However, this is not only lacunose but also very fragmentary (no complete line is kept intact on this page), and therefore involves a high degree of speculation as can be seen from the conflicting suggestions of Kenyon, *CBBPIII-1936*, 15; *DNTAP*$^{2.1}$, 123; and Comfort-Barrett, *Text of the Earliest NT Greek MSS*, 220; hence, I excluded this from my list. For our detailed list of all these incremental omissions, see Appendix L.

[104] Both in his dissertation and monograph, Royse consistently used word count to reflect long omissions. This is a valid representation of the facts. However, I have here added also the total number of character losses since it is graphically easier to appreciate the amount of loss when incremental omissions are viewed in the context of actual number of characters to a line on a particular page. Accordingly, when a variation involves any of the nine regular *nomina sacra* in $\mathfrak{P}^{46}$, I have counted the longest *nomen sacrum* form rather than its *plene* form.

accidentally omitted an average of eight (8) characters per page in the 172 extant pages,[105] due to this type of error. As we noted in the previous chapter, it would be unwise to impute intentionality in these variations, since they are products of accidents not intention.[106] This type of variation does not prove anything about the scribe's intention to shorten the text of his *exemplar*; but it effectively depicts our scribe's recurring optical difficulty in dealing with similar letter morphology.[107]

Haplographies are of various lengths, some as short as one word and some as long as 23 words. The shortest, committed near the end of a line, is 1Cor 1.8 ($f38^{r}$-l^{09}) involving the omission of the adverb ϵωc due to letter similarity with the final character of the previous word (ϋμac).[108] In this optical oversight, three letters were lost.[109] On the other hand, one of the longer omissions is at 2Cor 1.6-7 ($f61^{r}$-l^{22}). Clearly occasioned by *homoioteleuton*, this line-end variant was committed when our scribe's eyes inadvertently jumped from the first παθηματων to the second,[110] eventually losing a long series

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[105] This figure excludes the 363 instances of grammatical omissions totalling to 1,176. If combined, this yields a ratio of 15 lost letters per page throughout the 172 extant pages; see related discussion in pp. 220-22.

[106] See Colwell and Tune, "Method in Classifying and Evaluating Variant Readings," 103. In his article, "Textual Criticism of the New Testament," in *Hearing the New Testament: Strategies for Interpretation* (ed. J.B. Green; Grand Rapids, MI/Carlisle: Eerdmans/Paternoster, 1995), 127-45, p. 136, Bart Ehrman was cautiously repetitious in underscoring the "accidental" nature of this type of variation.

[107] The difference between "tendency" and "intention" is admittedly hair-thin, and confusing the two may to some extent be allowed. But confusing obvious "accidence" with "intention" is another story.

[108] Royse, *SH-M*, 327, 350, more elaborately classified this variant as a harmonization to context, particularly with the proceeding ανεγκλητους. However, an accidental omission seems the simplest explanation to account for the variation. And even if Zuntz, *TEDCP*, 20, is correct in his suggestion that "(t)he isolated variant in $\mathfrak{P}^{46}$ may derive from a (correct) gloss τελειως", the possibility of letter visual confusion still best accounts for the rise of the variation than harmonization.

[109] Three letters were also lost in 2Cor 12.6 ($f73^{r}$-l^{17}) τις εις εμε, and in Gal. 1.15a ($f81^{v}$-l^{13}) ευδοκησεν ο $\overline{\text{θc}}$ ο αφορισας.

Four letters: Rom 11.16b ($f15^{v}$-l^{07}) ριζα αγια; Eph 1.1a ($f75^{r}$-l^{03}) τοις αγιοις τοις.

Five letters: Heb 10.17a ($f31^{r}$-l^{22}) αμαρτιων αυτων; 11.39 ($f35^{v}$-l^{15}) και αυτοι; 1Cor 3.10a ($f41^{v}$-l^{09}) χαριν του θεου την; 2Cor 10.10 ($f70^{v}$-l^{24}) επιστολαι μεν φησιν; Eph 3.8b ($f77^{r}$-l^{20}) παντων αγιων.

And six letters: Rom 16.15d ($f21^{v}$-l^{05}) αυτοις παντας αγιους; 1Cor 14.19c ($f56^{r}$-l^{11}) μυριους λογους; Phil 1.30b ($f87^{v}$-l^{02}) ακουετε εν εμοι ει τις.

[110] The textual tradition for this is a complicated one, involving at least five variant forms, but all similarly characterized by *homoioteleuton*; for details, see the variation unit in UBS^{4}, 610-11, n1. I think, however, that the variation committed by $\mathfrak{P}^{46}$ and 2127 should be classified as another variant reading and

of 18 words (=81 characters), i.e., ΤΩΝ ΑΥΤΩΝ ΠΑ=‖θηματων ων και ημεις πασχομεν και η ελπις ημων βεβαια υπερ υμων ειδοτες οτι ως κοινωνοι εστε των πα=‖ΘΗΜΑΤΩΝ ΟΥΤΩϹ. The loss may have been equal to 2-3 lines of the *exemplar.*

Another remarkable case is 2Cor 8.19-20 ($f69^{r}$-l^{16}), not only because this is the longest omission on record,[111] involving 23 words, but more so because it involves multi-level incremental omissions on the same page (*cum* 2Cor 8.18, involving 9 words). The transcription of ll^{12-18} of $f69^{r}$ helps illustrate our point:

L^{12}:	ΥΜΑϹ [18] ϹΥΝΕΠΕΜΨΑΜΕΝ ΔΕ ΜΕΤ ΑΥΤΟΥ	(26)	
L^{13}:	ΤΟΝ ΑΔΕΛΦΟΝ ΟΥ Ο ΕΠΑΙΝΟϹ ΕΝ ΤΩ ΕΥΑΓ	(28)	
L^{14}:	ΓΕΛΙΩ ΔΙΑ ΠΑϹΩΝ ΤΩΝ ΕΚΚΛΗϹΙΩΝ	(25)	
	ου μονον δε αλλα και χειροτονηθεις υπο των εκκλησιων		(***44***)
L^{15}:	[19] ϹΥΝΕΚΔΗΜΟϹ ΗΜΩΝ ϹΥΝ ΤΗ ΧΑΡΙΤΙ ΤΑΥ	(28)	
	τη τη διακονουμενη υφ ημων προς την αυτου του $\overline{\text{κυ}}$ δοξαν και προθυμιαν ημων [20] στελλομενοι τουτο μη τις ημας μωμησηται εν τη αδροτητι ταυτη		(***113***)
L16:	ΤΗ ΔΙΑΚΟΝΟΥΜΕΝΗ ΥΦ ΗΜΩΝ [21] ΠΡΟΝΟ	(25)	
L17:	ΟΥΜΕΝ ΓΑΡ ΚΑΛΑ ΟΥ ΜΟΝΟΝ ΕΝΩΠΙΟΝ	(26)	
L18:	ΤΟΥ $\overline{\text{ΘΥ}}$ ΑΛΛΑ ΚΑΙ ΕΝΩΠΙΟΝ $\overline{\text{ΑΝΩΝ}}$ [22] ϹΥΝ	(26)	

As the transcription shows, the first level omission transpired in l^{14} when our scribe's eyes accidentally jumped from the first ΤΩΝ ΕΚΚΛΗϹΙΩΝ to the second, then wrote the text of what is now l^{15}. Surprisingly, at the end of the same line the second level omission was triggered when the word ΤΑΥΤΗ was divided and ran through the next line. But instead of returning to the first τη διακονουμενη υφ ημων (v19), the scribe's eyes returned wrongly to τη[112] διακονουμενη υφ ημων of v.20.[113] The further importance of this two-level omission is the kind of information it reveals

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should not be lumped with ℵAC *et al*; it is more likely that the reading of $\mathfrak{P}^{46}$ independently arose and 2127's same error is coincidental.

[111] As per Royse, *SH-M*, 288, n513, this is the longest omission in the six papyri he analysed.

[112] At this point in the *exemplar*, the dative ΤΗ may have been the first word of the line, hence, the optical jump. Considering the line-end location of the variation, this is the simpler explanation than assuming that this is a case of "a leap within a leap" (*contra* Royse, *SH-M*, 288, n512).

[113] Kenyon, *CBBPIII-1936*, 108, noted in his apparatus, "*Post* ημων 2° om. προς την ... υφ ημων(v. 20) *per homoiotel.*" Royse supported this interpretation in both his dissertation (p. 256) and monograph (281-82, 288). (Although the presentation in p. 288 is a bit confusing and seems to suggest at first glance that the leap started with χαριτι ταυτη of v.19). However, it seems more likely that the long omission was triggered by the way ΤΑΥΤΗ was divided at line-end.

about the possible lay-out of the *exemplar*. Note that both these examples are line-end omissions. If both these cases were also located at line-ends in the *exemplar* we are then afforded a glimpse of how it might have been formatted in terms of character to a line, at least for this part of epistle.[114] The scribe lost 44 characters in the first level and 113 in the second, perhaps cumulatively accounting for 4-6 lines of the *exemplar*.

Another of the longer omissions is Heb 12.6-7 ($f35^{r}$-l^{15}). In this mid-line omission due to *homoioarcton*,[115] the optical leap transpired after the *nomen sacrum* $\overline{\text{ΚϹ}}$ was copied but instead of returning to the first ΠΑΙΔΕΥΕΙ (v.6), the scribe mistakenly copied the second ΠΑΙΔΕΥΕΙ of v.7. In the process, 21 words (=97 characters) were lost, which may have been equal to 3-4 lines in the *exemplar*.

Whilst I can confidently talk about the number of lost characters per example, I can only refer to the lay-out of the *exemplar* in proximate terms, due to the inherent methodological difficulty of the exercise. For one, we must first be able to establish the format of the *exemplar* whether it was a codex or a roll; admittedly, we can only speculate in this regard, but the main difference between the two is the number of characters per line per column. Whilst it is tempting to readily presume a codex due to its preponderance among our earliest extant papyri, a roll cannot be entirely ruled out. Second, even if we are able to hypothesise the format of the *exemplar*, we still need to establish the average line number per page and the average character number per line, and here evidence from the manuscript tradition provides various models.

[114] The inevitable presupposition is that the *exemplar* was also in codex format. But even then, the location is also emphasised since it would be faulty to assume that the amount of character-input per page is constant due to the codicological fact that middle sheets in a codex would receive lesser number of text in view of the established practice of trimming codices for aesthetic purposes.

[115] Kenyon, *CBBPIII-1936*, 47; *DNTAP*$^{2.2}$, 343; and (indirectly) Royse, *SH-M*, 281 all see a case of homoioteleuton here, but it seems more likely that the omission was triggered by the similarity of the first letters of ΠΑΙΔΕΥΕΙ than the last ones, due to the fact that ΜΑCΤΙΓΟΙ in v.6 and ΠΑΤΗΡ in v.7 are morphologically dissimilar. Homoioarcton better explains the rise of the omission.

Having said that, the kind of information our codex provides, through these incremental omissions, may point to another equally important area of methodological discourse in scribal studies: Who *really* omitted what?

Turner, inquiring whether the copying context (i.e., copying to dictation or visual copying) of a manuscript can be determined from its remnant errors, opined that "evidence from the books themselves is open to ambiguous interpretation", and argued that phonetic errors can also be a product of a scribe's reading the text of his *exemplar* to himself before copying.[116] But on a particular example of scribal error, Parker challenged this assumption, arguing that not all "errors of parablepsis are equally attributable to either manner of copying".[117] What relevance do these two viewpoints offer to our present inquiry?

Thus far, we have examined two basic types of incremental omissions according to their visual locations in our codex: line-end and mid-line omissions. Looking at Appendix L, we are provided with a visual summary of the locations in our codex where the incremental omissions transpired. From this same appendix we can conclude, with high degree of confidence, that there are clear examples where we can indeed witness our scribe's own blunders, jumping forward from one line to another, relative to character similarity and/or formation. This is specifically true for cases of long omissions at *line-ends* and *line-beginnings*, where optical leaps are theoretically easier to conceive. However, there is another twist to this observation—there are equally many cases where incremental omissions have

[116] Turner, *GMAW*[2], 17.

[117] Parker, "Dictation Theory," 16. He added, "Errors of pronunciation are more certainly ambiguous; the statement that a copyist always repeated aloud his text has not been challenged" (16).

taken place at *mid-lines*.[118] Indeed, similar words or syllables are involved, but the fact that they are situated at the middle of a line should caution us from impetuously pointing to our scribe as *the actual* originator of those haplographies. In fact, at most, we can only state that a particular substantial omission is *in* $\mathfrak{P}^{46}$; to state more boldly that these were all originally made *by the scribe* of $\mathfrak{P}^{46}$ would be a misinterpretation of the facts—conscious distinction between the two will do justice to our scribe's own facility.

This phenomenon of incremental omissions is open to two interpretations. First is that the scribe's eyes jumped forward because the words he was copying were at line-ends or at line-beginnings in his *exemplar*, hence, the blunders are his own doing, i.e., he wrongly transcribed a correctly copied tradition. A relevant implication of this is that the lay-out (in terms of character input to a line) of his *exemplar* is different from the way he laid-out his own manuscript.

The second, however, one which is of equal weight, is that our scribe's *exemplar* contained these mid-line incremental omissions *already* (and perhaps even in its ancestors).[119] This means that our scribe was "correctly" reflecting a wrongly copied tradition. Admittedly, there is no easy way of precisely telling which of the 25 mid-line

[118] The breakdown of the 70 IO's according to their location in our codex is thus:

	LINE-BEGINNING	NEAR LINE-BEGINNING	MID-LINE	NEAR LINE-END	LINE-END	TOTALS
ROM	0	3	5	0	1	**9**
HEB	0	1	5	3	3	**12**
1COR	0	2	3	2	7	**14**
2COR	1	2	5	0	4	**12**
EPH	0	2	3	1	4	**10**
GAL	0	2	0	0	3	**5**
PHIL	0	0	3	0	2	**5**
COL	0	1	1	1	0	**3**
1THESS	0	0	0	0	0	**0**
TOTALS	**1**	**13**	**25**	**7**	**23**	**70**

[119] Here again I must underscore the methodological difficulty of ascertaining with utmost precision how far the origin of a variation can be confidently established, hence, it can go in any direction depending on the viability of evidence one can amass. As Parker, "Scribal Tendencies and the Mechanics of Book Production," 175, noted, "Every accidental change must be the work of a scribe (either the scribe of a manuscript under scrutiny or of an earlier copyist, or of several)."

incremental omissions belong to the first and which belong to the second. But the caveat this observation brings to fore is the need for caution from randomly attributing across-the-board all incremental omissions to the scribe of $\mathfrak{P}^{46}$–the scribe of his *exemplar* may have also been a culprit in many of these "sins of textual omissions".

B. Dittographies

As noted above, there are 148 cases of additions in $\mathfrak{P}^{46}$ (cumulatively totalling to 159 words or 517 characters).[120] We shall focus here on the addition variants caused by copying certain letters or words twice. Compared with haplographies, dittographies are fewer[121] in $\mathfrak{P}^{46}$—I documented 18 instances only.[122] Quantitatively, there are two types: word or phrase dittography and letter[123] dittography. As with incremental omissions, dittographies are obviously "errors of the eyes", and likewise caution must be taken from vesting them indiscriminately with scribal intentionality, i.e., intention to produce a long text. Quantitatively, variations like this have either significantly[124] or insignificantly[125] lengthened the text, depending on how far the momentary mental-optical lapse has covered. On the other hand, qualitatively, the effect or result may be described as three-

[120] See related discussion in pp. 219-20.

[121] Hoskier, "Appendix to an Article on the Chester Beatty Papyrus of the Pauline Epistles," 6, ‡, was not incorrect with his observation when he stated, "The scribe does not often reduplicate".

[122] Rom 8.38b (f12^{v}-l^{03}) αρχαι ου ουτε; 11.7a (f14^{r}-l^{10}) ο ουκ επετυχεν; 16.17 (f21^{v}-ll$^{10-11}$) ποιουντας ην υμεις εμαθετε η λεγοντας η; 16.26 (f20^{v}-l^{15}) κατε επιταγην;

Heb 6.13 (f26^{r}-l^{02}) κα||θε εαυτου; 13.21 (f38^{v}-l^{03}) θελημα αυτου αυτο;

1Cor 2.4a (f40^{v}-l^{04}) πειθοις σοφιας; 7.34a (f46^{r}-ll$^{11-12}$) |η γυνη η αγαμος και η παρθενος||η αγαμος μεριμνα; 16.19b (f60^{v}-l^{19}) πρεισκας συν;

2Cor 3.11 (f63^{v}-ll$^{20-21}$) τ̣ο̣ το καταργουμενον; 4.6a (f64^{v}-l^{06}) εκ σκοτοτους;

Eph 2.2a (f76^{r}-l^{10}) ποτε επεριεπατησατε; 4.18 (f78^{v}-l^{13}) πορρωσιν;

Gal 2.1a (f82^{r}-l^{04}) βαρναβας συνπαραλαβων;

Phil 2.18 (f88^{r}-ll$^{09-10}$) και ϋμεις||χαιρετε και υμεις χαιρετε και συνχαιρετε; 2.25-26a (f88^{r}-ll$^{23-25}$) πε[μψαι]||προς υμας επειδη επιποθων ην π̣ε̣μ̣[ψαι]||προς υμας και αδημονων; 4.6b (f89^{v}-ll$^{11-12}$) μετα ευχα=||ριστ<ε>ιας μετα ευχαριστειας; and 4.18a (f90^{r}-l^{09}) δε δεξαμενος.

[123] Rom 16.26 [κατ**Ε** **Ε**πιταγην]; Heb 6.13 [κα|θ**Ε** **Ε**αυτου]; 1Cor 2.4 [πειθοι**C** **C**οφιας]; 16.19 [πρεισκα**C** **C**υν]; Eph 2.2 [ποτ**Ε** **Ε**περιεπατησατε]; 4.18 [**Δ**Ε **Δ**Εξαμενος]; and Gal 2.1 [βαρναβα**C** **C**υνπαραλαβων].

[124] For this, we can mention the two cases in Philippians: 2.18 and 4.6b.

[125] This is especially true for letter dittographies identified previously.

fold:[126] 1) dittographic variations resulting in nonsense readings; 2) dittographic variations causing no change in meaning; and 3) dittographic variations causing change in meaning. Some examples are in order.

1. *Dittographic variations resulting to nonsense reading*

Six[127] cases of dittography may be classified as *nonsense readings*. Three are lexical nonsense (Rom 16.26 [f20^{v}-l^{15}] κατε επιταγην; Heb 6.13 [f26^{r}-l^{02}] κα||θε εαυτου; and 2Cor 4.6a [f64^{v}-l^{06}] εκ σκοτοτους[128]) and three grammatical nonsense (Rom 8.38b [f12^{v}-l^{03}] αρχαι ου ουτε;[129] Heb 13.21c [f38^{v}-l^{03}] θελημα αυτου αυτο;[130] Eph 2.2 [f76^{r}-l^{10}] ποτε επεριεπατησατε). Whilst these resulted in nonsense readings, yet they provide valuable information about our scribe. In three of this we are afforded once more a glimpse of another recurring error of our scribe: his tendency to copy an initial *epsilon* twice when the preceding word is elided. This tendency is augmented when the two words are copied as a visual unit, in rapid succession.[131] This happens also with other letters.

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[126] There is another type actually, but it seems to me more attributable to the *exemplar* than to the optical lapse of our scribe. Hence, it shall be discussed in the next sub-section.

[127] Strictly speaking, the longer instances in Phil 2.18 and 4.6b are also nonsense contextually. However, as they are comparatively longer, they shall be classed separately.

[128] *DNTAP*$^{2.1}$, 345, noted "*unkorrigierte Dittographie in* σκοτους". Royse, *SH-M*, 254, added, "... we probably have an internal dittography caused by a backwards leap from the second ο to the first in σκοτους".

[129] In this instance, the recurring occurrences of the particle ΟΥΤΕ (four times already prior this error) might have contributed to the dittography. Notable also is the presence of an observable (1-letter) space-gap between ΟΥ and ΟΥΤΕ, and that ΟΥΤΕ commenced at the point of junction between two vertical papyrus strands. This may have also momentarily distracted our scribe.

[130] Royse, *SH-M*, 252, n279, has classified this as an interchange of ο and ω, arguing that a dittography is unlikely here since "$\mathfrak{P}^{46}$ and the ancestor(s) of ℵ* A C* *al* would have independently duplicated αυτου here." But a dittography is still conceivable if viewed from the perspective that this arose very early in the lineage of our scribe's *exemplar* and other related mss, hence, the multiple attestation in different forms (i.e., αυτου αυτο, αυτου αυτω, and αυτου αυτος). Furthermore, the variation seems to have been an "incomplete" dittography which was unfortunately left uncorrected, than a simple ο-ω interchange.

[131] The phrase ΚΑΤΕ ΕΠΙΤΑΓΗΝ (=12 letters) in Rom 16.26 [f20^{v}-l^{15}] was written rapidly as one continuous visual unit, without any space-gap, which might also contributed to the dittography. Note that the horizontal *hasta* of the first *epsilon* touches the downward curve of the second *epsilon*. On the other hand, the phrase ΠΟΤΕ ΕΠΕΡΙΕΠΑΤΗ CΑΤΕ (=18 characters) in Eph 2.2 [f76^{r}-l^{10}] was also written in rapid succession as one visual unit, but with a slight space-gap on the penultimate syllable of the second word, presumably generated when the scribe slightly moved his hand to the right. Similarly, the horizontal stroke of the first *epsilon* touches the downward curve of the second *epsilon*, at the point of junction with its horizontal *hasta*.

2. Dittographic Variations causing no change in meaning

Not all dittographies have unintelligible results; some make sense in context, both lexically and grammatically, yet they do not essentially disturb the sense (meaning) of a passage. The following examples demonstrate this.

a. Gal 2.1

L^{02}	ξαζον εν εμοι τον θν̄ [2.1] επειτα δια δεκατεσ=
L^{03}:	σαρων ετων παλιν ανεβην εις ϊεροσολυ=
L^{04}:	μα μετα ΒΑΡΝΑΒΑC̲ C̲ΥΝΠΑΡΑΛΑΒΩΝ και τιτ̄ο

In l^{04} of $f82^{r}$ (Gal 2.1), the initial *sigma* of συνπαραλαβων was copied twice, producing a reading which Royse described as a form "for which there seems to be no parallel".[132] Here the mistake is most likely generated by the text itself, and like in the previous case, this seems to be part of that similar tendency—a visual difficulty with initial *sigma* immediately following an open vowel. 1Cor 2.4 ($f40^{r}$-l^{04}) is another case in point.

b. 1Cor 2.4

L^{03}:	[2.4] και ο λογος μου και το κηρυγμα μου ου=
L^{04}:	κ εν ΠΕΙΘΟΙC̲ C̲ΟΦΙΑC αλλα εν απο=
L^{05}:	δειξει πν̄ς̄ και δυναμεως [5] ινα η πι=

In l^{04}, the initial *sigma* of ΟΦΙΑC was copied twice, producing an unattested lexical form (i.e., *hapax legomenon*), not only in biblical Greek, but also in the entire Greek literature.[133] Furthermore, the textual tradition also reveals that the error had eventually spawned opportunities for scribal clarificatory expansions, from the straightforward ΕΝ ΠΕΙΘΟΙ ΟΦΙΑC to the more elaborate ΕΝ ΠΕΙΘΟΙC

[132] Royse, *SH-M*, 332. Royse classed this variant under the heading Substitution of "Proper Names", but in n755, he noted of Kenyon's "*per errorem*" and Zuntz's view that this is a case of dittography.

[133] On this, see the excellent treatment by Zuntz, *TEDCP*, 23-25, where he first enunciated the view that the textual conundrum εν πειθοις ανθρωπινης σοφιας λογοις has its origin from the original εν πειθοι which unfortunately suffered from later scribal proclivity of clarificatory expansions. Fee, *First Epistle to the Corinthians*, 88, n2, picked up Zuntz's view and on the principle of *lectio difficilior potior* argued, "with some reluctance", that $\mathfrak{P}^{46}$'s reading (*cum* FG) is the most plausible candidate to be the "original reading". For a more recent treatment of the problem with a view of demonstrating the continuing validity of the age-old axiom "*lectio brevior potior*" for some text-critical problems, see Edgar Ebojo, "How Persuasive is the 'Persuasive Words of Human Wisdom'? The Shortest Reading in 1 Corinthians 2.4," *TBT-Technical Papers* 60/1 (2009): 10-21.

ⲁⲛⲑⲣⲱⲡⲓⲛⲏⲥ ⲥⲟⲫⲓⲁⲥ ⲗⲟⲅⲟⲓⲥ.[134] Fortunately, this (and the previous) example does not fundamentally alter the exegetical configuration of the passage.[135]

3. Dittographic variations causing change in meaning

a. 1Cor 16.19: "Sex-change by Transcription"

Some dittographies, however, can perniciously (but unintentionally) alter the meaning of the text.[136] 1Cor 16.19 is a perfect example, where the name Πρισκιλλα in Acts is presented in its shorter form[137] but with a distinct twist:

L[17]:	ουν τους τοιουτους [16.19] ασπαζονται
L[18]:	υμας εν κ̅ω̅ πολλᾲ ακυλας και
L[19]:	ⲡⲣⲉⲓⲥⲕⲁⲥ ⲥⲩⲛ τη κατ οικον αυτων
L[20]:	εκκλησια [20] ασπαζονται ϋμας οι α=

Some have discerned in this variation an alteration motivated by "anti-women sentiments". For instance, Haines-Eitzen argued that passages in the Pastorals curtailing the role of women in the church "provide a counterpart to the singular reading we find in 𝔓[46]."[138] Also, Kurek-Chomycz, reviewing passages in Acts and other Pauline Epistles,

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[134] The nature of variation includes dittography, e.g., πειθοι σοφια to πειθοις σοφια (𝔓[46] FG Chrys[mss]); explanatory expansion, e.g., πειθοις σοφια to πειθοις σοφιας λογοις (BD 0150 33 1175 1506 1739 1852 1881 1912 it[r] vg[ww, st] geo[1] Or[gr4/7, lat2/3] Eus Did[1/3] Chrys[1/2] Sev Ambr[1/7] Jer[4/5] Pel Ver); and πειθοις σοφιας λογοις to πειθοις ανθρωπινης σοφιας λογοις (א[2]ACΨ 6 81 104 256 263 365 424 436 459 1241 1319 1573 2127 2464 *l*592 it[o] vg[cl] geo[2] slav Or[gr1/7, lat1/3] Ps-Athan Cyr-Jer Apollinaris Did[2/3] Chrys[1/2] Cyrl[2/3] Ambr); transposition, e.g., ανθρωπινης σοφιας to σοφιας ανθρωπινης (1962 2495); and singularisation, e.g., λογοις to λογος (א*).

It is noteworthy that the common text of UBS[4]–NA[28] reflects ἐν πειθοῖ[ς] σοφίας [λόγοις], whereas Holmes' *Greek New Testament: SBL Edition* reflects the shortest reading: ἐν ⸢πειθοῖ σοφίας⸣.

[135] Roger Omanson, *A Textual Guide to the Greek New Testament: An Adaptation of Bruce M. Metzger's Textual Commentary for the Needs of Translators* (Stuttgart: German Bible Society, 2006), 329.

[136] Emphasis on delineating accidence from intentionality is here necessary, at least at theoretical level, since recent studies within the discipline has to some extent directly attributed sensible alterations with semblance of ideological motivations to scribes of particular manuscripts or group of manuscripts. Bart Ehrman's recent works, most notably in his *Orthodox Corruption of the Scriptures*[1,2], best represent this line of development. But are these sensible alterations really ideologically motivated or can the mechanics of ancient book copying explain these? On this question, see Parker, *Manuscripts and Their Texts*, 133-58, esp. 152-53.

[137] The manuscript tradition is divided between the diminutive and the non-diminutive: ⲡⲣⲓⲥⲕⲁ read by אBP 0121 0243 33 226 1175* 1739 1881* vg[st] sa bo[pt] and ⲡⲣⲓⲥⲕⲓⲗⲗⲁ supported by CDFGKLΨ 049 056 075 0142 0150 0151 1881[c] it vg[cl] sy bo[pt] Ambst Pel. Whilst 𝔓[46] sides with the diminutive form, it is nonetheless longer, both because of the itacised form of the medial –ι– and the additional ⲥ at the end, hence, ⲡⲣⲉⲓⲥⲕⲁ.

[138] See for instance, Haines-Eitzen, *Guardians of Letters*, 115-16, who concluded, "(T)he simple addition of a final sigma to Πρίσκα changes the name from a feminine form to a masculine one, thereby identifying in this passage two men, rather than a man and woman. It must be admitted that such an

concluded, "The singular reading in $\mathfrak{P}^{46}$ suggests that the copyist was either careless and ignorant, or otherwise that he (less likely she) consciously added Σ in order to reduce the role women play in the NT... (A)lthough the intentions of particular scribes cannot be easily ascertained, specific readings have a negative influence on the overall picture of Prisca."[139] Furthermore, Royse suggested that "the scribe took this person to be a man, perhaps under the influence of the preceding ακυλας."[140] However, given the examples mentioned above about our scribe's copying tendency to accidentally double initial *sigma* preceded by a word terminating with an open vowel makes this "sex-change by transcription" view neither necessary nor warranted. Conversely, it is easier to explain this particular variant as a product of transcriptional accidence, than a programmatic ideological alteration to denigrate the role of women in the early church.[141]

4. Visually parallel ("Glance-up") Dittographies

These are a case of "wandering-eye" errors. Our first example is 1Cor 1.20-21:

L[17]: ϲοΦιαν ṭου κοϲμου [1.21] επειδη γαρ εν̣
L[18]: τη ϲοΦι̣ᾳ̣ του κοϲμου ουκ εγνω ο κοσ=
L[19]: μος ...

In this instance, the scribe's eyes accidentally glanced up at the previous line with exactly the same visual construction and thereby copied the object of the phrase

addition may simply be due to the influence of the ending of Ακύλας**.** But there may be more at work in the change, for we know that the role of women was fiercely contested in early Christianity. Already in the pastoral Epistles, for example, we find the roles of women restricted and controlled… Furthermore, we have other instances in which textual changes appear to be motivated by antiwomen sentiments, and these provide a counterpart to the singular reading we find in P[46]."

Similarly, whilst cognizant of the palaeographical possibilities for the emergence of this reading, Kurek-Chomycz, "Is there an 'Anti-Priscan' Tendency," 111, insisted, "(W)e cannot be sure that this singular reading should be explained as a mere mechanical error. The outcome at any rate is obvious: the variant under discussion reduces the number of women mentioned in 1 Corinthians. This, however, does not yet facilitate any far-fetched conclusions with respect to the possible animosity toward women in $\mathfrak{P}^{46}$…"

[139] Kurek-Chomycz, "Is there an 'Anti-Priscan' Tendency," 128.

[140] Royse, *SH-D*, 274; *SH-M*, 332; adopted by Haines-Eitzen (p. 116) and Kurek-Chomycz (p. 111).

[141] I concede, however, that some passages within $\mathfrak{P}^{46}$ itself—passages that are palaeographically inexplicable—*can* be interpreted, in terms of *exegetical effect*, in light of the modern gender question; on this, see Edgar Battad Ebojo, "The Way I See It: $\mathfrak{P}^{46}$ as a Paradigm Reader-Response Criticism," *TBT-Technical Papers* 60/1 (2009): 22-36, esp. 31-35.

σοφιαν του κοσμου, although he might have been vocalizing σοφια του θεου.[142] Hence, as it presently stands, $\mathfrak{P}^{46}$ has in effect rendered Paul's rhetorical discourse on the irony of worldly wisdom as part and parcel of the *cosmic wisdom* itself that eventually leads to a salvific experience for those who would believe, and no longer as a product of God's own omniscient initiative.

That our scribe tended to be visually affected by what is written directly above his still vacant line of writing is equally demonstrated elsewhere. Rom 13.12 ($f17^{r}$-l^{02}) is a more graphic example of this tendency.

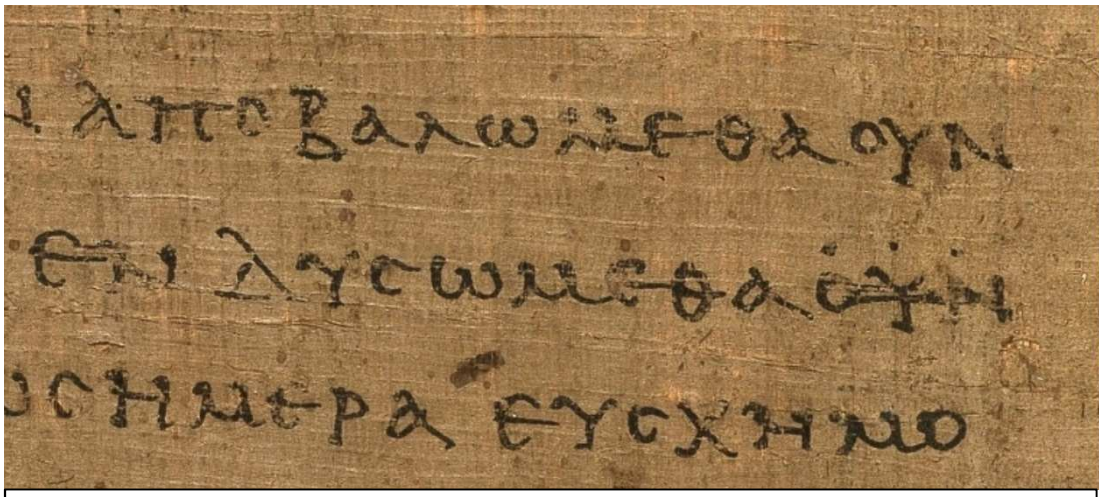


Figure 4-1.6 $F17^{r}$-II^{01-03}: Orthographic similarities resulting in dittography.

In this case, his *exemplar* must have correctly read ενδυσωμεθα τα οπλα του φωτος.[143] However, after copying ενδυσωμεθα he must have paused for a split second to glance at his *exemplar* for the next word/s to copy, but because of the visual similarity of the verb ending of the previous line (which by this time was already directly above where

[142] UBS^{4}-NA^{28} reading has the best external attestations ($\mathfrak{P}^{11}$ ℵABCDF^{c}GLPΨ 049 056 0142 0150 0151 88 614 1739) and most likely reflects the "original reading". Apart from $\mathfrak{P}^{46}$'s reading (read also by the minuscule 623), other variant readings include: του θεου τη σοφια (r vg vg^{ms} F^{c} Mcion) and ο θεος την σοφιαν (b). Royse, *SH-M*, 319, 349, classified this as a case of harmonization in context. Whilst it is a plausible suggestion in terms of *result*, in terms of *cause*, it is best explained by an "optical parallel leap".

[143] UBS^{4}-NA^{28} common text reads ἐνδυσώμεθα [δὲ] τὰ ὅπλα τοῦ φωτός (supported by ABC*D*P 048 630 1506 1739 1881 *pc* Cl).

he stopped), i.e., ΑΠΟΒΑΛΩΜΕΘΑ ΟΥΝ, he thus accidentally copied ΟΥΝ.[144] Fortunately, the glitch was spotted and appropriately corrected, more likely by the scribe himself,[145] thereby preventing the transmission of a mistaken reading. However, not all the "glancing-up-generated" errors have been correspondingly corrected. One such example, with glaring exegetical effect is Phil 2.25-26:

L^{20}:	... [25] αναγκαιον δε ἠ[γη=]
L^{21}:	σαμην επ̣αφροδειτον τον αδελφον κ̣α̣[ι συν]
L^{22}:	εργον και συνστρατιωτην μου ὑμων δε [απο=]
L^{23}:	στολος και λειτουργον της χρειας μου ΠΕ[ΜΨΑΙ]
L^{24}:	ΠΡΟΣ ΥΜΑΣ [26] επειδη επιποθων ην π̣ε̣μ̣[ψαι]
L^{25}:	ΠΡΟΣ ΥΜΑΣ και αδημονων διοτ̣ι η[κουσατε]
L^{26}:	οτι ησθενησεν ...

After copying the fifth word of l^{24} (i.e., ΗΝ), instead of παντας υμας,[146] the scribe accidentally (re-)copied ΠΕΜΨΑΙ ΠΡΟΣ ΥΜΑΣ,[147] not only because of its morphological similarity with παντας υμας but also because of the visual proximity of the phrase πεμψαι προς υμας at the spot where the scribe may have momentarily paused. Unfortunately, because of this dittography, the phrase παντας υμας was eternally lost in $\mathfrak{P}^{46}$ for it was never restored.[148] Hence, instead of Epaphroditus "longing for *you all*" (παντας υμας = the

[144] This is also the explanation of Zuntz, *TEDCP*, 258-59: "the scribe's mistake was due to his looking at ἐνδυσώμεθα οὖν at the end of the preceding line." Royse, *SH-M*, 229, n149, described Zuntz's proposal as "most plausible to me".

[145] The assignment of correction in previous studies is divided. Sanders, Kenyon, and Zuntz all see a second hand correction; *DNTAP*[2.1], 115, suggested with doubt a second also, i.e., "2. Hand?"; whilst Kim and Royse simply assigned it to a corrector. Whilst it is definitely more difficult to assign corrections dealing with expunging dots and slashing mark, it seems reasonable to assign the correction to the first hand in this instance as the two other corrections ($II^{06, 08}$) on this page as well as the one on the opposite page ($f18^{v}$-I^{16}) are all from him, plus the fact that ink colour is closely similar with that of the text than that of the second hand. See also related note in p. 318, n113.

[146] Read by $\aleph^{2}$FGKLPΨ 056 0142 0151. The transposed υμας παντας is read by B. $\aleph^{*}$ACDI^{vid} 0150 0278 support an expanded reading παντας υμας ιδειν, whilst 075 has the transposed version of this long reading ιδειν παντας υμας.

[147] Kenyon, *CBBPIII-1936*, 145, commented, "*repetitum per errorem*". Similarly, *DNTAP*[2.2], 108, noted, "*Der Schreiber kopierte versehentlich nicht παντας υμας aus der Vorlage, sondern wiederholte πε[μψαι]*|[24] *προς υμας (2,25) genau unter diesen Wörtern*".

[148] Phil 2.18 ($f88^{r}$-II^{09-10}) is another interesting case, for the conjunctive clause ΚΑΙ ΥΜΕΙΣ ΧΑΙΡΕΤΕ was copied twice in direct sequence without the scribe correcting it (there are no other corrections on this page), and as a result his text is now longer by 15 letters. Exactly the same circumstance is Phil 4.6b ($f89^{v}$-II^{11-12}) where the prepositional phrase ΜΕΤΑ ΕΥΧΑΡΙΣΤΕΙΑΣ was copied

Philippian church members), the verse in $\mathfrak{P}^{46}$ now reads "for he has been yearning to *send you* (πεμψαι προς υμας)", vesting authority upon Epaphroditus.

What may be inferred from these two examples is that the codex's text lay-out had contributed to the creation of these errors; both transpired at line-ends. This seems to be corroborated to some extent by the two other longer dittographies in Philippians (2.18 and 4.6b) which both transpired near line-beginnings, as shown in the following:

Phil 2.18 **...** και ϋμεις|
|χαιρετε ΚΑΙ ΥΜΕΙC ΧΑΙΡΕΤΕ και συνχαιρετε μοι|

Phil 4.6b **...** μετα ευχα=|
|ριστ<ε>ιας ΜΕΤΑ ΕΥΧΑΡΙCΤΕΙΑC τα αιτημα=|

5. *"Dittographic Omissions"*

These errors are actually omission variants. But they seem to point to another proclivity of our scribe that has to do with dittography—at least as a mental construct. "Dittographic omissions" are variants that have been likely committed due to the scribe's misgiving that his *exemplar* suffers from dittography, as though the scribe was "correcting" his *exemplar*'s error. Admittedly, this type cannot be strongly asserted as with others, and there are not many examples of this type, but they are intermittently scattered throughout the codex. A few may be cited. For instance, in Heb 10.37 UBS[4]-NA[28] has μικρὸν ὅσον ὅσον ὁ ἐρχόμενος but $\mathfrak{P}^{46}$ originally lacks the second ὅσον.[149] Many text critics regard this as a case of haplography[150] and it is difficult to suggest otherwise, for the morphological similarities of the characters are unmistakable. At any rate, there

twice also in immediate succession. Interestingly, whilst there was a correction event in the first ΕΥΧΑΡΙCΤ<Ε>ΙΑC, conforming it to the usual itacistic form, the dittography was never corrected!

[149] The word was restored though, by the second hand (so is Zuntz, *TEDCP*, 253; *DNTAP*[2.2], 322).

[150] Zuntz, *TEDCP*, 253; Royse, *SH-M*, 238.

seems to be more at play here than just simple haplography, especially if the other examples are called upon to provide witness, thus:[151]

Heb 10.1	θυσιας <u>ας</u> (UBS^4-NA^{28})
	|ταις θυσιας <^{ας}> ... | ($\mathfrak{P}^{46*}$ A)
Heb 11.16b	ο θεος <u>θεος</u> επικαλεισθαι αυτων (UBS^4-NA^{28})
	| ... ο $\overline{\theta ς}$ επικαλεισθαι αυτων ... | ($\mathfrak{P}^{46}$)
2Cor 1.17	το ναι <u>ναι</u> και το ου <u>ου</u> (UBS^4-NA^{28})
	| ... το ναι και||το ου ... | ($\mathfrak{P}^{46}$ 0243)
Rom 8.17a	κληρονομοι <u>κληρονομοι μεν</u> θεου συγκληρονομοι (UBS^4-NA^{28})
	|[και] κ̣ληρονομοι $\overline{\theta υ}$ συγκληρονομοι... ($\mathfrak{P}^{46}$)

The common denominator for these examples has to do with the fact that the words at issue occurred twice originally, which of course reflect the correct readings. However, for a scribe in a fast copying mode, the chance of mistaking them for dittographies is equally real.

IV. SOME HABITS IN THE NON-TEXTUAL "ERRORS"

A. Confusing *Nomina Sacra* Overlines

Our scribe committed a few (interesting) mistakes in employing this component of the *nomina sacra*. For instance, in f38^v-l^{22} (1Cor 1.2), the genitive $\overline{\text{ιηυ}}$ does not have the superscript line, although corresponding overlines have been written above the preceding $\overline{\text{κυ}}$ and the immediately proceeding $\overline{\text{χρυ}}$ on the same line (Fig. 4-1.7). This must have been a simple case of oversight.[152] Yet despite this singular blunder of this type,[153] the scribe of $\mathfrak{P}^{46}$ is undoubtedly a scribe most cognizant of the essential role of the crossbar in designating a word as a *nomen sacrum*.[154]

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[151] Perhaps the following might have been mentally perceived as letter dittographic variations:

2Cor 11.25	τρις ερ̣ραβδισθην (UBS^4-NA^{28})
	τρις εραβδισθην ($\mathfrak{P}^{46}$ cum ℵBDFGHKLPΨ 056 075 0142 0150 0151 0243)
Gal 5.14c	ως σεαυτον (UBS^4-NA^{28})
	... ω[ς]||εαυτον ($\mathfrak{P}^{46}$ *cum* FGLPΨ 056 075 0122* 0142 0150 0151 0278]

[152] In his 1936 edition, Kenyon also failed to indicate the superscript lines in three instances involving this genitive form (1Cor 1.9; 15.57) and the accusative (2Cor 4.5) as well as in 1Cor 7.34 for the genitive κυ. These, too, must have been a simple case of oversight on the part of the Kenyon's proof-readers.

[153] Royse, *SH-M*, 896, reported that Rom 16:8 (f20^r-l^{14}) involving the *nomen sacrum* $\overline{\text{κω}}$ is another instance. At first glance, this seems to be the case if one is investigating from Kenyon's facsimile.



Figure 4-1.7 F38^{v}-l^{22} with the three *NS*; the mid-line *nomen sacrum* ΙΗΥ does not have the overline.

The absence of an overline on a contracted *NS*, whilst an interesting trivium, would have presented little difficulty to the user-readers of $\mathfrak{P}^{46}$ as the contraction ΙΗΥ has been recurrently used throughout. But to see the overline outside the "standard" *NS* words can leave one scratching his head in confusion, especially if a word is *not* contracted. A case in point is f30^{v}-l^{15} (Heb 9.14), where an overline above the word ΑΙΜΑ is present.[155] This is the only instance in $\mathfrak{P}^{46}$ where ΑΙΜΑ occurred with an overline. Paap interpreted this as a legitimate instance of a *nomen sacrum* and argued for a possible *Eucharistic* motivation behind the occurrence.[156] Roberts, on the other hand, argued that this is a sorry case where an equally important theological word is "surprisingly omitted"

However, as the magnified image below shows, there is a recognizably distinct ink residue above the abbreviation. Hence, this is excluded.



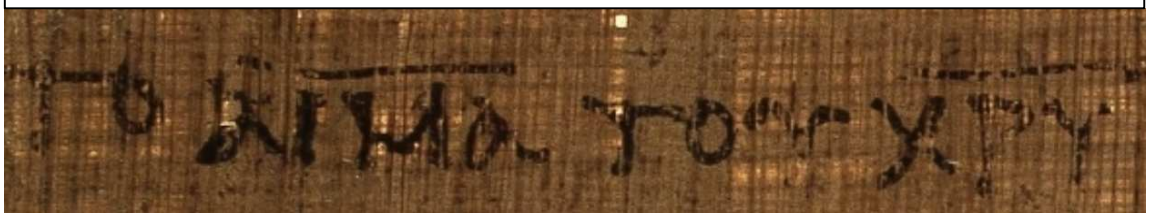
[154] Our scribe is far more cognizant when we compare his singular blunder with that of other manuscripts. For instance, the scribe of the Septuagint manuscript, J. Harris Bibl. Fr. Sinai No. 15 (about 4th century A.D.) failed to put overlines 19 times out of the 32 extant instances of *NS* in this manuscript; see Paap, *Nomina Sacra*, 40-41, 102.

[155] It is equally interesting to note that the page numerations for f46^{v} and f52^{r} also have above them what seem to have been crossbars.

[156] Paap, *Nomina Sacra*, 114.

in the system but is "sometimes felt".[157] More recently, Comfort furthered this proposal and claimed that the scribe "*decided* to make the Greek word for blood... a nomen sacrum".[158] However, a second look at the evidence, particularly from the standpoint of palaeography, proves these proposals unconvincing, to say the least.

Figure 4-1.8 F30^{v}-I^{15}: The corrected ΑΙΜΑ with an overline written out *plene*.



It is very unlikely that our scribe originally intended to treat the uncontracted ΑΙΜΑ in Heb 9.14 as a *nomen sacrum*, for its Eucharistic overtone. A few reasons can be cited against this proposal. First, the phrase "blood of Christ" (ΑΙΜΑΤΟC/ΑΙΜΑΤΙ ΤΟΥ $\overline{\text{ΧΡΥ}}$) also occurred in 1Cor 10.16 and Eph 2.13 and in both instances ΑΙΜΑΤΟC/ΑΙΜΑΤΙ were similarly written out in full, *without* any superscripted lines—this underscores the problem of consistency.[159] Second, and more importantly, Paap, Roberts, and Comfort failed to reckon with the fact that in this particular instance the ΑΙΜΑ reading is a *corrected* reading (Fig. 4.1-8). The original reading was $\overline{\text{ΠΝΑ}}$ ΤΟΥ $\overline{\text{ΧΡΥ}}$ ("Spirit of Christ"), which may have been reflective of his *exemplar*'s reading which he faithfully copied. But whilst such a reading made sense in context, it was a reading unsupported by other manuscripts, and therefore the correction was carried out once the "mistake" was spotted. The rectification might have been undertaken at a later

[157] Roberts, *Manuscripts, Society, and Belief*, 40, asserted, "Why should (some theological terms) be excluded? More striking still is the omission of the *eucharistic* words αιμα, αρτος, οινος, σαρξ, σωμα, an omission that was sometimes felt as can be seen from the Chester Beatty Pauline Epistles… in which 9.13 a line is placed above αμα, though the word itself is not contracted (p. 40)". See also his Review of Sanders, 133.

[158] Comfort, *Encountering the Manuscripts*, 249-50. Emphasis added.

[159] I am aware, nonetheless, that the application of the system across the manuscript tradition is quite "inconsistent" in a number of instances.

time by a hand *other* than the original scribe.[160] The first vertical stroke of ⲡⲧ was overwritten with *alpha* (ⲁ) and the second stroke with *iota* (ⲓ), and ⲛ was overwritten with the *mu* (ⲙ).[161] But what the corrector failed[162] to do was to erase the overline on the original contraction which was a legitimate *nomen sacrum.* Such (intentional?) failure to erase the overline is conceivable since it is quite difficult to satisfactorily undertake corrections of such nature, without generating a confusing result, *unless the whole word is totally erased first* and the correction re-written.[163] Hence, it is quite imprecise to argue that the crossbar was ever connected to the intention of the scribe to treat ⲁⲓⲙⲁ as a legitimate *nomen sacrum*; the crossbar in this instance rightly belongs to ⲡⲛⲁ;[164] the crossbar is prior to the correction.[165]

B. Line Habits Exposed

1. *The First Lines (l^{01})*

The first line of a page, as noted in the previous chapter, sets the line length for the rest of the page. Additionally, it also theoretically functioned as a visual guide for the straightness of the lines on a given page. This intent is disturbed, however, when the papyrus strips are aesthetically problematic, as in the case of f82^{v} where the first line beautifully set the tone

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[160] Palaeographically, the ink of the correction is a bit darker and denser than the ink in the text. Also, the rounded stroke of the alpha is sloping a bit to the right whilst the original text is a bit more erect and the slanting stroke ends with a serif pointing upward (which the correction does not have).

[161] It is likely that only the middle sloping stroke of ⲙ was written by the corrector in between the two existing vertical strokes of the original ⲛ.

[162] Royse, *SH-M*, 236, n181, sees here a case of simple negligence: "the corrector simply ignored, or forgot to erase, the supralinear line that is now superfluous".

[163] The case for Phil 1:23 is different since the correction does not only involve the *nomen sacrum* χ̄ω̄ but also the preceding preposition εν, which was then forged into just one word, i.e., εν̀χ̄ω̄ν. There the erasure of the crossbar is indeed warranted to avoid confusion.

[164] There are still many other peculiarities in the employment of the convention on ⲡⲛⲉⲩⲙⲁ. However, since they require more space, it is deemed best to reserve its discussion in Section Three of the present chapter where the *Nomina Sacra* in $\mathfrak{P}^{46}$ are treated in full.

[165] *Contra* Roberts, *Manuscripts, Society, and Beliefs*, 40, who, perhaps also unaware of the correction, noted "a line is *placed* above αιμα" (emphasis added); see also Kenyon, *CBBPIII-1936*, xiv.

for the page but the papyrus caused most of the lines to be written pretty badly, as shown in Fig. 4-1.9. Fortunately, examples like this are not many.[166]



Figure 4-1.9 F82^{v} showing the right upward direction of the lower lines.

One perhaps might expect that the chances of committing variations at the beginning of a new page would be very slim, since the scribe would have expectedly become more composed after a brief respite from the previous page—however

[166] Other examples include, either exhibiting upward or downward or wavy lines: f14^{r}, f16^{r}, f17^{r}, f19^{v} and f19^{r}, f39^{r}, f45^{r}, f48^{r}, f69^{v}, f78^{r}, f87^{r} and f87^{v}.

momentary that time lapse had been. This is untrue for our codex, however, since of the 156 fully extant first lines, we noted 64[167] variations/errors of different sorts in 58 folios, most of which (29 instances) transpiring at mid-lines (ML).[168] Ten cases are line-beginnings (LB),[169] three at line-ends (LE),[170] twelve near line-beginnings (NLB),[171] and 10 near line-ends (NLE).[172]

2. The Right Text Margins

Note has been made already about our scribe's remarkable consistency in keeping the left text margin visually straight from top to bottom,[173] but the same cannot be said of his right text margin. Whilst there seems to be a desire to keep it aesthetically straight, the inconsistency is more pronounced. Note also that the irregularity of the right text margin seems more obvious on the left-hand pages than the right-hand, most likely due to the physical and visual impediments presented by the binding centre as his writing hand draws closer to it.

Accordingly, some letters or group of letters at line-ends are written in smaller sizes in not a few instances, especially letters with roundish strokes (ⲟ, ⲱ, ⲥ, and ⲉ). Sometimes ligatures are also formed at line-ends (e.g., f15^{r}-ll$^{06, 08, 13, 18,}$).

3. The Abbreviated Line-end Final -ⲛ

How the abbreviated line-end final ⲛ's were used are a direct indicator of our scribe's understanding of this widely-recognized literary convention—he used it conservatively

[167] Actually, the number of variations derived from Appendix K—Table of Variations in 𝔓^{46} is 77 but thirteen (13) of this number are itacisms and nasals, hence, excluded.

[168] ML: F16^{v}, f19^{r}, f21^{v}, f27^{v}, f31^{v}, f32^{v}, f34^{v}, f50^{r}, f51^{v}, f52^{r} (2x), f56^{r} (2x), f56^{v}, f58^{v}, f60^{r}, f67^{r}, f67^{v}, f72^{r}, f77^{v}, f78^{v}, f79^{v}, f81^{r}, f86^{r}, f88^{r}, f89^{r}, f90^{r}, and f91^{v}.

[169] LB: F23^{r}, f29^{v}, f32^{r}, f45^{r}, f47^{r}, f54^{r}, f54^{v}, f63^{r}, f81^{r}, and f82^{v}.

[170] LE: F42^{r}, f66^{r}, and f78^{v}.

[171] NLB: F19^{r}, f27^{r}, f28^{v}, f29^{r}, f30^{r}, f32^{v}, f58^{r}, f59^{r}, f59^{v}, f71^{r}, f80^{v}, and f83^{r}.

[172] NLE: F15^{v}, f17^{r}, f27^{v}, f34^{v}, f43^{r}, f50^{r}, f55^{v}, f63^{v}, f78^{v}, and f89^{r}.

[173] See related discussion in pp. 113-18.

and somewhat arbitrarily. I counted 614 instances[174] where a line ends with a complete word with a final –ⲛ. Of this number, our scribe abbreviated the final –ⲛ in 87 cases only, irregularly covering 65 pages.[175] Notably, there are long stretches of pages without the abbreviation despite many legitimate opportunities to do so.[176] In fact, f79^{r}-ll$^{05\text{-}06}$[177] is the sole instance where he used it in two consecutive lines. It is also notable that there is no distinct preference in using the convention on a particular side, since the distribution is almost even: 36 left-hand and 29 right-hand.

There is a marked arbitrariness in his use as well; his choice of which to abbreviate is almost pattern-less. The most that he used the convention on a page is three,[178] in two pages in 1 Corinthians.[179] At first glance, he seems to exhibit particular fondness with the genitive plural and dative singular forms of the pronoun ⲩⲙⲉⲓⲥ/ⲏⲙⲉⲓⲥ, abbreviating them 14 times, but on a second look, it is upset by the fact that he did not abbreviate these in 43 instances,[180] some of which are even on the same

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[174] This figure covers 161 pages out of the 172 extant pages. Pages without line-end final –ⲛ includes: F11^{r}, f12^{v}, f13^{v}, f15^{v}, f42^{v}, f42^{r}, f45^{v}, f45^{r}, f54^{r}, f56^{r}, and f74^{r}. The following are fragmentary or the right text margin eroded already: fo8^{v}, f18^{v}, f94^{v}, and f97^{r}. For more details, see Appendix M—Table of Abbreviated Line-End Final –ⲛ.

[175] Sanders, *TCPC*, 16, without mentioning their actual locations, stated that there are two instances of mid-line final –ⲛ. However, I have not found any case of a mid-line abbreviated final –ⲛ during my autopsy of the Michigan leaves.

[176] For instance, in the 23 extant folios encompassing the Epistle to the Romans there are only three cases of final –ⲛ abbreviations (f12^{r}, f15^{r}, and f17^{r}), although there are 68 other cases of line-end complete words with final –ⲛ's.

[177] But even on that page, two line-end complete words with a final –ⲛ were never abbreviated: l^{03} (ⲭⲉⲣⲥⲓⲛ), and l^{04} (ⲭⲣⲉⲓⲁⲛ).

[178] The following have two: F22^{r}, f23^{v}, f29^{r}, f30^{r}, f35^{v}, f39^{r}, f48^{r}, f53^{r}, f54^{r}, f56^{r}, f56^{v}, f58^{r}, f58^{v}, f76^{v}, f77^{r}, f79^{r}, f84^{r}, and f90^{r}.

[179] F38^{r} (l^{01} [ⲩⲙⲉ̄ⲓ], l^{05} [ⲩⲙⲉ̄ⲓ], and l^{14} [ⲏⲙⲱ̄]) and f40^{v} (l^{06} [ⲁⲛⲑⲣⲱⲡⲱ̄], l^{08} [ⲥⲟⲫⲓⲁ̄], and l^{17} [ⲁⲩⲧⲱ̄]). But note that in these two pages there are other opportunities for abbreviation as there are nine cases of line-end complete words with final –ⲛ.

[180] Rom 6.12 (fo8^{r}-l^{18}); 15.5 (f18^{r}-l^{15});

Heb 7:26 (f28^{v}-l^{14}); 12.5 (f35^{r}-l^{12});

1Cor 1.12 (f38^{r}-l^{21}); 2.1 (f39^{r}-l^{24}); 3.18 (f41^{r}-l^{07}); 5.11 (f43^{r}-l^{09}); 9.11 (f48^{v}-l^{20}); 10.6 (f49^{r}-l^{12}), 27 (f50^{r}-l^{16}); 11.30 (f52^{r}-l^{12}); 12.21 (f53^{v}-l^{19}); 14.37 (f57^{r}-l^{08}); 15.1 (f57^{r}-l^{15}), 57 (f59^{v}-l^{16}), 58 (f59^{v}-l^{21});

2Cor 1.3 (f61^{r}-l^{08}),12 (f61^{v}-l^{14}), 18 (f62^{r}-l^{07}), 21 (f62^{r}-l^{15}), 22 (f62^{r}-l^{18}); 4.12 (f64^{v}-l^{25}), 14 (f65^{r}-l^{04}); 5.12 (f65^{v}-l^{23}), 20 (f66^{r}-l^{18}); 7.5 (f67^{v}-l^{01}), (f68^{r}-l^{02}); 8.1 (f68^{r}-l^{18}), 16 (f69^{r}-l^{08}); 9.2 (f69^{v}-l^{04}); 10.16 (f71^{r}-l^{17}); 11.8 (f71^{v}-l^{17});

Eph 2.1 (f76^{r}-l^{09}); 3.11 (f77^{v}-l^{01}); 4.23 (f78^{v}-l^{23}); 5.19 (f79^{v}-l^{19});

page where he effected abbreviations.[181] The same can be said of ЄCTIN (9 times), the articles TѠN/THN (7), AYTOC (6), and the adjective ΠAC (5). This arbitrariness is further exposed when we reckon that despite the fact that he abbreviated 22 line-end nouns, he never abbreviated the same noun more than once,[182] although they appeared elsewhere. The same can be said of the 18 cases of abbreviated verbs and participles and five cases of adjectives.[183]

But there is one observable pattern that must be noted. There seem to be some words that our scribe intentionally avoided abbreviating. A few grammatical units such as certain conjunctions (ЄAN, OYN), prepositions (CYN, ЄN), particles (MЄN, AN), adjectives (MIAN, TPITON), adverbs (NYN, ΠAΛIN, MAΛΛON), and relative pronouns (HN, ON) were not subjected to this convention even if they were at line-ends.

It is now conspicuous that our scribe's use of this convention is a random one, and except for the lone pattern we mentioned in the previous paragraph, it is unclear what criteria our scribe employed in effecting the abbreviated final -N. But what is clear is that the choice he eventually made, however intermittent and arbitrary, is our scribe's own doing and most likely not dictated by his *exemplar*.

CONCLUSION

$\mathfrak{P}^{46}$ is indeed replete with textual and visual errors and idiosyncrasies, of almost every conceivable kind. However, this indelible fact does not put $\mathfrak{P}^{46}$ above and beyond the rest of the extant earliest manuscripts in terms of error-commission. On the contrary,

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Gal 4.26 ($f84^{v}$-l^{15});
Phil 4.5 ($f89^{v}$-l^{08}), 9 ($f89^{v}$-l^{22});
Col 2.5 ($f91^{v}$-l^{21}); 3.4 ($f92^{v}$-l^{08}); 4:6 ($f93^{v}$-l^{05}).

[181] E.g., whilst our scribe made three abbreviations in $f38^{r}$, ΥMѠN in l^{21} was left un-abbreviated.

[182] The lone anomaly here is the word NOMON where he abbreviated it twice ($f32^{v}$-l^{18} and $f76^{v}$-l^{16}), otherwise all the rest were abbreviated only once despite their recurrence elsewhere.

[183] For details, see Appendix M—Table of Line-End Abbreviated Final N's.

$\mathfrak{P}^{46}$ is in fact a very "normal" manuscript in every way, for there is not one manuscript that is inerrant and non-idiosyncratic—all manuscripts, however extensive or fragmentary, leave traces and evidences of errors and deviations. In this regard, all manuscripts are equal—"all have fallen short".

Having said that, the variations in $\mathfrak{P}^{46}$, whether from its scribe or from the transmitted text of its *exemplar*, are an excellent resource for framing the sociology of ancient manuscript production using the very lenses its scribe used whilst producing his codex and not exclusively from the standpoint of seeing how this scribe blundered in safeguarding the "authoritative" readings of the New Testament text. What have been discussed in this section highlighted the *human face* of the transmission of the New Testament text—this calls for a re-appreciation of the work of our scribe.

SECTION TWO
SCRIBAL PATTERNS IN THE CORRECTIONS OF $\mathfrak{P}^{46}$

INTRODUCTION

Corrections, according to the received wisdom, are textual fidelity indicators.[1] This section, however, will show that they are also a portal to know more about our manuscript, its scribe, and its immediate users.[2] From our vantage point, the "correctors" of $\mathfrak{P}^{46}$, like the main hand, are unknown, but by piecing together the extractable details of what *and* how they effected correction and initiated correction events[3] we can further expand our information database about the scribe and his colleagues in the trade who left their imprints, however sporadic, on our manuscript.[4]

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[1] For recent general treatments on how "corrections" in the earliest surviving manuscripts have played in the transmission of the New Testament texts, see Parker, *NT Manuscripts and Their Texts,* 141-48; Michael Holmes, "Text and Transmission in the Second Century," in *The Reliability of the New Testament*, 61-79, pp. 74-78; K. Martin Heide, "Assessing the Stability of the Transmitted Texts of the New Testament and the *Shepherd of Hermas*," in *The Reliability of the New Testament*, 125-59.

[2] Tasker's enthusiasm is contagious: "The study of these corrections is one of the most fascinating portions of the book, for he must surely be lacking in imagination who does not feel some thrill at such an attempt to see reflected in the mistakes and corrections of a document written over seventeen hundred years ago images of the 'life-situation' which produced them" ("The Text of the *Corpus Paulinum*," 190).

[3] The first attempts at distinguishing various correcting hands in $\mathfrak{P}^{46}$ have been undertaken by Kenyon, *CBBPIII-1934* and *CBBPIII-1936*, as well as by Sanders, *TCPC*. But it was in Zuntz's *Text of the Epistles* that the characteristics of the various correctors and their textual implications have first come into fruition. Kim, "Palaeographical Dating," proposed an expanded pool of correctors. Comfort and Barrett, *Text of the Earliest*, also assigned hands to the various corrections noted in their transcriptions. Jaroš, on the other hand, made no attempt to assign corrections. Thus far, *DNATP*$^{2.1}$ and *DNTAP*$^{2.2}$ have made the most exhaustive analysis of individual corrections. However, the most recent analysis on the corrections in $\mathfrak{P}^{46}$ with specific slant toward scribal studies is Royse's monograph.

[4] As Parker, *Codex Sinaiticus*, 79-80, put it succinctly, "The detailed study of the correctors and their corrections is worthwhile, because of the insight it offers into the ways in which readers studies and used a biblical manuscript in antiquity; because learning the ability to differentiate between them provides an excellent palaeographical training; and because the study of the ways in which they altered the text can be used to document some of the details of how the text of the Old and New Testaments changed as time went by."

I. STATISTICS AND METHODOLOGICAL CHALLENGES

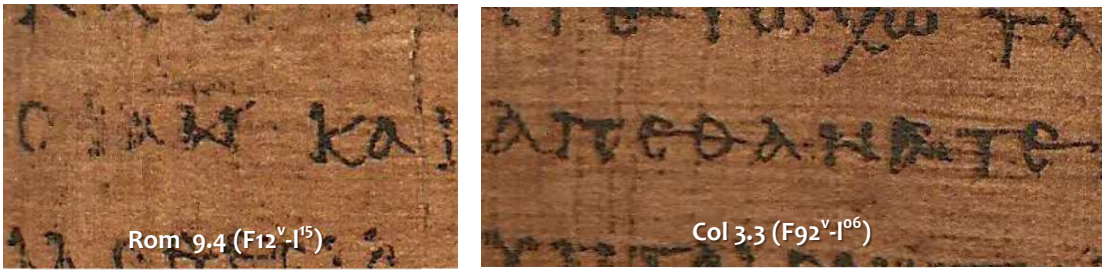


Figure 4-2.1 The First and the Last Extant Corrections in 𝔓46

The first recorded correction event in the extant pages of 𝔓46 is Rom 9.4 (f12v-l15) where the ⲛ of the original υιοθεσιαν[5] was cancelled *in scribendo* via diagonal slash (i.e., υιοθε̣||σιαⲛ̸), whilst the last recorded is Col 3.3 (f92v-l06) involving another *in scribendo*[6] correction overwriting ⲁ on what was originally an ϵ (i.e., απεθαν>ϵ/ⲁ<τε).[7] There are no annotations in 𝔓46 bashing other scribes, as in the infamous contra-scribal marginal note in Heb 1.3 of Codex Vaticanus. In fact, there are no "marginal" remarks throughout, only supralinear and intralinear insertions of *prescriptive* and *suggestive* corrections.

[5] The accusative υιοθεσιαν is alone against all other witnesses reading the nominative υιοθεσια.

[6] Kenyon, *CBBPIII-1936*, *DNTAP*[2.1] ("*wahrscheinlich von der 2. Hand*"), and Royse suggested the correction to be by the paginator-corrector, dubbed variously as "m.2" or "man 2". However, it seems more plausible to me that the overwriting of the original ϵ is a product of an *in scribendo* correction, in view of the looping stroke of the ⲁ which is comparable with the loop of the *alphas* on the same line; ink colour is also the similar with that of the text.

[7] In this section (and in Appendix O) the following *sigla* were used to represent correction events:

<α> character/s *inserted* within or on the line, in between letters. For fidelity purposes, I have put in superscript those that are actually written above the line in 𝔓46 (e.g., f16r-l09 φρονου<ⲛ>τες) and retained in normal size those squeezed in between letters (e.g., f27r-l04 <ⲁ>αρων);

>α/β< character/s *written over* with another character/s (e.g., f22r-l12, τι>ϲ/ττ<αρ);

α̣ character/s cancelled by way of an expunging dot and/or a diagonal slash; sometimes the cancellation is indicated only by the expunging dot above the letter/s (e.g., f25v-l21 ἐπ̣ὲὺξ) or a diagonal slash only (e.g., f29r-l20 μηπωϛ). Sometimes the slash is horizontal (e.g., f26r-l24 ⸗ὼ αβρααμ);

α̣+_ a space was *intentionally allotted in scribendo* immediately after a correction event (e.g., f12r-l23 πλασαντι ἀα̣+_ [this siglum is used in this section only]).

A. Correction and Non-Correction of Errors: A Conceptual Question

I have documented 193[8] cases of correction throughout the 172 extant pages of $\mathfrak{P}^{46}$, the breakdown of which per book is shown in Table 4-B1.

TABLE 4-B1 CORRECTIONS DISTRIBUTION PER BOOK	
ROM	16
HEB	88
1COR	38
2COR	22
EPH	8
GAL	9
PHIL	8
COL	4
1THESS	0
TOTAL	**193**

The first most remarkable fact about this table is that nearly half of the total number of corrections is to the text of Hebrews (in fact, all the correctors have had their hands on this book); then the number decreases accordingly.

Translated statistically, this means a ratio of a little over one correction every page or an average of one correction for every 1000 characters. However, this would be a deceptive portrait if it creates the impression that the first hand committed only

[8] For a complete list of the corrections, see Appendix N—Table of Corrections in $\mathfrak{P}^{46}$. Kenyon's 1936 edition documented corrections, while Zuntz noted of only a few of the corrections, particularly those involving Hebrews and 1 Corinthians. Kim reflected 134 corrections in his "Figure 2—Classification of Added Hands". On the other hand, Comfort and Barrett recorded 198 corrections.

In his dissertation (pp. 235, 627-40 [list]), Royse listed 160 corrections. Having closely consulted studies that came out after his dissertation (i.e., Kim, Comfort-Barrett, and *DNTAP*[2.1, 2]), his figure rose to 183 in his monograph (pp. 211, 850-56 [list]). In this new list, he already—and rightly I should say--excluded the lone first-hand ligatured abbreviation in Heb 13.23. His additional entries include:

Rom (2): 8.30; 13.9;
Heb (8): 2.7; 9.5b; 9.14a; 10.2, 7; 11.7; 13.4, 5;
1Cor (3): 3.21; 11.27; 15.24a;
2Cor (6): 1.14; 3.11; 7.10, 13; 8.14; 9.14;
Eph (3): 2.3, 12; 6.20;
Gal (1): 1.13; and
Phil (1): 3.10.

Note, however, that I excluded six of his entries, two of which are merely expansions of the same correction events (2Cor 1.14a & 1.14b; and Eph 2.12b & 2.12c) and four that are more likely to have no correction events at all (Rom 8.30; Eph 6.20; and Phil 3.10 & 15). The new entries in my list include:

Heb (5): 1.9; 6.2; 9.12; 10.25; 12.13;
1Cor (5): 2.6; 3.2; 4.5; 5.5, 11.3; 16.3;
2Cor (4): 9.12; 10.4, 12; 12.19.

one error per page (or close to that). This is far from the truth. Although Zuntz did not give actual details,[9] he is not incorrect in asserting that the number of corrections decreases toward the end of the codex even though the text is badly in need of rectifications.[10] The truth is, of the 172 extant pages, only 102 (=59%) bear marks of corrections leaving 70 pages (=41%) without any correction at all (Table 4-B2).

TABLE 4-B2
TABLE OF PAGES WITH AND WITHOUT CORRECTIONS

BOOK	TOTAL # OF CORRECTIONS	TOTAL # OF PAGES	# PAGES WITH CORRECTIONS	# PAGES W/OUT CORRECTIONS
ROM	16	21	11	10
HEB	88	36	29	7
1COR	38	44	27	17
2COR	22	28	15	13
EPH	8	14	7	7
GAL	9	8	6	2
PHIL	8	8	3	5
COL	4	9	4	5
1THESS	0	4	0	4
TOTALS	**193**	**172**	**102**	**70**

The bulk of the 193 corrections appear in the two largest books, i.e., Hebrews (36 pages) and 1 Corinthians (44 pages), albeit unevenly at 88 and 38.[11] More notable is the fact that the number of corrections decreases substantially as the codex nears its completion. Expressed differently, of the 193 corrections 125 (=65%) happened on the first half of the codex and the other 68 (=35%) on the second half. To further illustrate: In the two extant leaves of 1Thess (f94 and f97), we have no record of any correction attempt despite the fact that there are two significant variants here, both unsupported readings.[12] But since 1Thess leaves are very fragmentary, the Col leaves make a more

[9] This lack of details has been noted also by Royse, *SH-M*, 212, n73.

[10] Zuntz, *TEDCP*, 252, observed, "Of his innumerable faults, only a fraction (less than one in ten) have been corrected and even that fraction… grows smaller and smaller towards the end of the book. Whole pages have been left without any correction, however greatly they were in need of it."

[11] Stated differently, only 81% of the 36 Hebrews pages have been corrected. The decrease in 1 Corinthians is more remarkable at only 61% of the 44 pages!

[12] 1Thess 1.10 ($f94^{v}$-I^{06}) reads υπομ̣ε[νειν] against all other manuscripts' αναμενειν. On the other hand, νηψωμεν in 5.6 ($f97^{r}$-I^{04}) is alone against the widely attested νηφωμεν.

graphic case, where only four corrections are recorded (three by the first hand).[13] However, when viewed against the number of instances requiring corrections (involving 71 cases[14]), the disparity is exposed.[15] Even if we exclude cases involving *itacisms* and transpositions (26 cases in all), we still have 41 cases genuinely needing rectification but simply left untouched as our codex presently stands! In fact, even in books where corrections are the most dense, the disparity between the corrections and the instances needing corrections are even more pronounced. Let us take the case of f32^{v} (Heb 10.22-30) where the seven corrections give the highest recorded number of corrections on a single page.[16] I have noted 20 cases of variations just on this page, but four are *itacistic*,[17] hence, excluded. But despite this elimination, this still means that the correctors of this page—which interestingly involved three correctors (sans the first hand)—missed correcting nine legitimate cases of variations![18]

Turner already expressed his observation about the "superficial" type of corrections across several ancient papyri.[19] But it was Zuntz who first noted the highly selective concentration and decreasing fashion of the corrections in $\mathfrak{P}^{46}$, and seconded by

[13] Col 1.20 (f91^{r}-l^{12} α$^{\langle\gamma\rangle}$του), 28 (f91^{v}-l^{04} νουθετουν<Θ$^{/\tau}$>ες); and 3.3 (f92^{v}-l^{06} απεθαν>Ε/Α<ετε). 1.7 (f90^{v}-l^{08} συνδο$^{\langle}$γ$^{\rangle}$λου) is from the corrector with pale black ink.

[14] On this, see Appendix K—Table of Variations in P^{46}.

[15] The disparate picture becomes even more pronounced when we note that most of the corrections in the latter half of the codex have been by the first hand; see discussion below.

[16] Heb 10.22a (l^{01}, προσερ=||χ<Ο/$^{\omega}$>μεθα), 22b (l^{01}, Ἰ$^{-}$ΑΡ μετα), 22c (^{l01}, αληθει$^{\langle NH\rangle}$Α/ς), 24 (l^{07}, κατανο<ΗC>ωμεν), 25a (l^{09}, την ΕΠΙ=||συναγωγην), 25b (l^{10}, εαυτων $^{\langle\text{KAΘ}\omega\text{C}\rangle}$), and 25c (l^{10}, τι$^{\langle\text{CIN}\rangle}$).

[17] Heb 10.22d (συνιδησεως), 27 (εσθειειν), 28 (οικτειρμων), and 29c (τειμωριας).

[18] Three involve compound verbs (10.25a [καταλειποντες]; 26d [καταλειπεται]; and 29b [καταξιωθησεται]); one replacement (10.24b [εκ παραξυσμου]); one omission (10.26a [αμαρτοντων]), four grammatical (10.22e [λελουμενοι]; 26b [αμαρτοντων]; 26c [περι αμαρτιας]; 29a [δοκειται]).

[19] Assuming a "professional copying-houses" (i.e., *scriptoria*) context, Turner, *Greek Papyri*, 93, asserted, "The manuals of palaeographers tell us that it was the task of professional copying-houses to 'proofread' their texts; the reader (*diorthotes, corrector* in Latin) was expected to sign at the end of the book, in such monograms as δι… or αντεβλεθη. But several of our surviving manuscripts, and especially those which are beautifully written, contain such serious un-noted errors that it is clear their 'proof-reading' was of a summary, superficial kind, if done at all. This phenomenon has long been known to palaeographers and textual critics."

Royse.[20] What picture then emerges from this? Certainly not an affirmative one. The most logical conclusion to be drawn from this is that "other-hand" corrections in $\mathfrak{P}^{46}$ were done randomly and unsystematically, i.e., spot-checking. This observation becomes more concrete when one seriously considers the uncorrected cases of incremental omission (IO)—substantially long haplographies that quantitatively shortened the text. Of the 70 IO cases[21] scattered throughout the codex, only two[22] were ever correspondingly corrected recovering 33 letters back to $\mathfrak{P}^{46}$, and three were simply noted with an *ancora* (↑) to *indicate* the missing texts,[23] cumulatively totalling to 175 letters.[24] However, most of the other equally or more serious omissions were curiously left unchecked, prompting one to ask whether the *exemplar/s* used for checking *already* did not have those uncorrected omissions as well or whether the correctors were simply incompetent in doing a good "job". The more serious question, however, is whether the concept of "textual fidelity" of this era (or at least for this manuscript) is different from our own understanding of the term, with which we use to evaluate these alterations.

[20] Zuntz, *TEDCP*, 252; Royse, *SH-D*, 235; Ibid, *SH-M*, 223-24, in relation to his "man 2" (our M^2), noted, "It is evident that the activity of man 2 was devoted mostly to Hebrews; indeed, more than half of all the 74 corrections by later hands are by man 2 in Hebrews, and these moreover are concentrated in the latter part of that book. And, as is often true, the frequency of corrections decreases sharply toward the end of the codex." What is noticeable, however, in both Zuntz's and Royse's observations is their absence of discussion as to the probable reasons for such selective correction events, and their implications to the principle of "text guarding" role of corrections.

[21] For the list of incremental omissions in $\mathfrak{P}^{46}$, see Appendix L.

[22] The first involves the phrase ΚΑΙ ΠΑΝΤΑϹ ΤΟΥϹ ΑΓΙΟΥϹ with a long line filler was superscripted in Heb 13.24b, by a corrector with heavy black ink and rather thick-nib pen (our "M^{2*}"). The second involves the supralinear restoration in Heb 11.12c of the phrase *Η ΠΑΡΑ ΤΟΥ ΧΕΙΛΟϹ*, by a cursive writing hand (our "M^{3}").

[23] Of course, one may contend at least for the possibility that these omitted words may have been inscribed at the (now lost) bottom margins of the page (thus, Zuntz, *TEDCP*, 253; Comfort and Barrett, *Text of the Earliest*, 237; Royse, *SH-M*, 235). However, this is implausible in these particular cases due to the upward direction of the *ancora* and the absence of a word indicating that the omitted words were annotated at the bottom margins; on this point, see Turner, *GMAW*^{2}, 16.

[24] Heb 8.8b (23 letters); 9.14a (55); and 12.6-7 (97).

Note also that these five "corrections" all transpired in Hebrews only and, except for Heb 11.12c, mostly were altered by only one and same scribe![25] Consequently, because these text-shortening variations were never properly addressed, $\mathfrak{P}^{46}$ has forever lost a cumulative total of 1,194 characters—a figure tantamount to more than a page in the context of the overall average number of letters per page in $\mathfrak{P}^{46}$.[26]

This point raises serious implications for our view of the functional structure of the ancient scribal trade. If the number of uncorrected "errors" is substantially far higher than the number of effected corrections, does this exude the confidence necessary to assume that corrections were really intended to function as safety nets "to guard the integrity of the text" being transmitted,[27] especially for a text considered by its intended users as *sacra pagina*? In principle, the concept of correction (by a διορθωτης or by the main scribe himself) as part and parcel of the professional scribal structure "to ensure greater accuracy"[28] could certainly generate a positive sense of trust upon the scribal trade, as Haines-Eitzen has argued.[29] Perhaps manuscripts with more obvious systematic and extensive traces of (historically multi-layered) correction events, e.g., Codex

[25] All these four "corrections" are the handiwork of a scribe who used a heavy black ink and a rather thick-nib pen, and copied the letters in an upright angle (i.e., our "M^2").

[26] On this, see related discussion in pp. 213-223.

[27] For instance, see Hurtado, *Earliest Christian Artifacts*, 186, who underscored, "… corrections reflect a mentality toward the text in which its wording is invested with some significance and concern."

[28] Metzger and Ehrman, *Text of the New Testament*, 25.

[29] Haines-Eitzen, *Guardians of Letters*, 109, argued, "Scribes frequently reread and corrected their work, and in doing so they demonstrate an awareness of their own fallibity as well as an awareness of some 'standard' of reproduction they are to meet… Sometimes corrections were made after consulting another exemplar, but more often scribes simply reread the copy and corrected errors… ". Using $\mathfrak{P}^{66}$ as a specific example, she further stated, "To be sure, this scribe appears to have produced many readings that called for correction. But would a careless scribe take the trouble to correct his/her own work? Or do the corrections actually suggest the care the scribe took in attempting to produce a good copy? The scribe of $\mathfrak{P}^{66}$ reread the copy and made corrections, and also made corrections according to a second exemplar. Such work does not indicate carelessness, it seems to me, but rather deliberate care and a desire *to get the text right…*" See also, Hurtado, *Earliest Christian Artifacts*, 186-87, who proposed distinct levels of implications after various hands involved in the correcting of a manuscript have been analysed.

Sinaiticus,[30] can bear this argument out. However, the overall picture of (non-) corrections in $\mathfrak{P}^{46}$ clearly does not warrant a comparably affirmative appreciation. In fact, it directly casts doubts against the very assumption about the "quality-control" role[31] corrections supposedly played in ancient book production setting,[32] whether in the context of a *scriptorium* or of a private network setting![33] Furthermore, this point also brings to surface a methodological loophole in the way corrections in a particular manuscript are usually analysed. More often than not, the most prominent aspects of correction studies are concentrated on identifying and then *comparatively* analysing the number of corrections against *other* manuscripts (to probe the degree of [dis-]agreement, and therefore establish the "correction flow"[34]), but rarely, if ever, are these corrections

[30] On the corrections in ℵ, see Jongkind, *Scribal Habits*, 9-18, 44-48, 159-60, 167-69; also, Idem, "Singular Readings in Sinaiticus: The Possible, the Impossible, and the Nature of Copying," in *Textual Variations*, 35-54, pp. 38-41. See also the earlier analysis of Milne and Skeat, *Scribes and Correctors*, 40-50.

[31] On this role as attributed to corrections undertaken by hands contemporary to the main hand(s), see Hurtado, *Earliest Christian Artifacts*, 187-8, who argued, "The particular importance of manuscript corrections by a hand contemporary with the original copyist is that they point to a setting in which the work of a copyist was reviewed and supervised by someone else, someone with authority to correct the copyist's work."

[32] This observation indirectly finds support in Barbara Aland's concept of early Christian communities' "textual consciousness", arguing that minor differences/errors in the manuscripts were not corrected systematically because they were "inconsequential" and to some degree "were tolerated" ("Significance of the Chester Beatty," 117-18).

[33] Here I allude only to the debate whether Christian *scriptoria* already existed in the second-third centuries (Zuntz, Colwell, Fee, Roberts) or whether a private scribal network was at work instead in the production of early Christian manuscripts (Haines-Eitzen); on this debate, see Zuntz, *TEDCP*, 273; Colwell, "Scribal Habits," 116-18; Gordon Fee, "P^{75}, P^{66}, and Origen: The Myth of Early Textual Recension in Alexandria," in *Studies in the Theory and Method of New Testament Textual Criticism* (Grand Rapids, MI: Eerdmans, 1993), 247-73, p. 258; Roberts, *Manuscripts, Society, and Beliefs*, 24; and Haines-Eitzen, *Guardians of the Letters*, 83-91. Haines-Eitzen, *Guardians*, 85-86, doubts the existence of Christian *scriptoria* early on in the history of the nascent Church, arguing that, among others, corrections in Christian mss of the 2nd-3rd centuries do not conclusively point to such existence, as their presence can be done by any scribe with an intention of producing a good copy. However, whether it was members of a *scriptorium* or a private scribal network who have undertaken corrections is not directly relevant to the point we are raising here, for both camps seem to share the same presupposition that, in principle, corrections were meant to produce a "good (faithful) copy", whether in favour of the scribe's *exemplar* or to a particular "recension" (a.k.a. "texttype"). Moreover, both camps equally fail to address the question of disparity of the "principle" of correction against the "actual (quantitative and qualitative) output" of correction in the early NT papyri in general, and in $\mathfrak{P}^{46}$ in particular.

[34] For instance, both Zuntz and Royse have attempted to demonstrate that corrections in $\mathfrak{P}^{46}$ were toward the "Alexandrian" text; an interesting observation that needs to be looked at again. On another manuscript, A.F.J. Klijn, "Papyrus Bodmer II (John i-xiv) and the Text of Egypt," *NTS* 3 (1956-57):

put side by side with the *actual* number of "errors" on the same pages of the same manuscript where corrections exist, to validate whether each correction event was truly intended to guard the integrity of the text for the sake and benefit of its end-users (the commissioning community), or to make it conform to a particular "texttype", or to effect editorial improvisations, or whether simply to check the general affinity of the manuscript with the ones the commissioning community are familiar with already, with conscious leeway given to the presence of minor grammatical and syntactical divergences.[35] Thus, it is not infrequent to read in the literature very general statements about the presence of correction events (or the lack thereof) in particular manuscripts under investigation.[36] But this does not look well if the goal of an inquiry is profiling scribal habits, with an end-goal of (re-)constructing the sociology of ancient book production![37]

327-334, pp. 333-34; H.M. Teeple and F.A. Walker, "Notes on the Plates in Papyrus Bodmer II," *JBL* 78 (1959): 148-52; and Gordon Fee, "Corrections in Papyrus Bodmer II and Early Textual Transmission," *NTS* 7 (1965): 247-57, all proposed that some of the corrections in $\mathfrak{P}^{66}$ are indicative of corrections toward an *exemplar* different from what the main hand originally used.

[35] It goes without saying that this tendency for the most part is influenced again by the traditional "*text*-focused" goal of textual criticism.

[36] One example may suffice at the moment. In giving a summary of his analysis of the corrections in $\mathfrak{P}^{45}$, $\mathfrak{P}^{66}$, and $\mathfrak{P}^{75}$, Colwell, "Scribal Habits," 118, concluded, "$\mathfrak{P}^{75}$ and $\mathfrak{P}^{66}$ represent a controlled tradition, $\mathfrak{P}^{45}$ represents an uncontrolled tradition. $\mathfrak{P}^{75}$ and $\mathfrak{P}^{66}$ are, according to their own standards, careful workmen. $\mathfrak{P}^{66}$ is careless and ineffective—although he is the only calligrapher of the three. He uses up his care, his concern, in the production of beautiful letters." As there are no appendices to his article, one wonders, however, whether Colwell equally took the pain of listing down all the uncorrected variations on the pages where these corrections have been located.

Unfortunately, some have taken this aspect of Colwell's method to be indicative of the degree of copying alertness of a particular scribe. For instance, referring to numerous corrections in $\mathfrak{P}^{66}$, Juan Chapa, "The Early Text of John," in *Early Text of the New Testament*, 144, claimed, "A writing containing so many corrections indicates a degree of carelessness, at least initially, and little preoccupation with formality." I am, however, uncertain whether the presence of fewer corrections conversely guarantees that a manuscript is indeed a qualitatively good and faithful copy. Needless to say, only an exhaustive profile of errors versus corrections can validate such a general claim.

[37] The inevitable question, of course, is whether the sporadic corrections in $\mathfrak{P}^{46}$ are also reflective of other manuscripts of comparative age—a notable and interesting subject in itself for a separate study. It would be very instructive to see how NT papyri of contemporary age, with Pauline Epistles (including Hebrews), fare against each other in terms of the quantitative and qualitative presence of corrections. We cannot, however, presently engage this question in view of space and time limitations. At any rate, one may perhaps start with the two editions of *DNTAP* containing Pauline Epistles as well as Royse's his latest

B. Methodological Difficulty of Corrector Assignment

Whether a "singular reading" is to be counted *before* or *after* a correction is a methodological departure point between Colwell and Royse,[38] even though the latter essentially adopted the former's model of locating scribal habits through "singular readings", arguing that taking a pre-correction variant is "unjustified and may give a very misleading impression of a scribe's activity."[39] One advantage of the method of locating scribal habits we adopted in this project, in contrast with Colwell's and Royse's, is that it does not problematize the temporal component of correction of a variant considered as "singular reading". In describing the habits of our scribe, we consider both the readings before *and* after their corrections, regardless of whether they are singularly supported in the manuscript tradition or not. In fact, it may be observed that profiling the *how* (manner of correction) provides a more graphic picture about scribal habits than exclusively concentrating on the *what* (content of correction).

Needless to say, counting correction events is a much easier task than assigning *who* corrected *what*—a critical but difficult task where palaeographical skill is put to test.[40] Admittedly, textual critics and palaeographers alike have not yet developed a fool proof methodology on how to assign corrections with utmost certainty,[41] which presently is largely based on one's level of familiarity with (and memory of?) the palaeographical

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article, "The Early Text of Paul (and Hebrews)," in *The Early Text of the New Testament*, pp. 175-203, with a broader view of collating all derivable data and not only the "singular readings".

[38] On this, see Head, "Scribal Behaviour and Theological Tendencies," 60-63.

[39] Royse, *SH-M*, 74.

[40] Kenyon (*CBBPIII-1936*) and Kim ("Palaeographical Dating") represent the two extremes of the palaeographical pendulum insofar as the corrections in $\mathfrak{P}^{46}$ are concerned. Kenyon assigned to "m.2" all other corrections that are not by the main hand (his "m.1"), whereas Kim, supposedly maintaining a very rigid palaeographical discriminants, classified about 19 different correctors (including $P46^{*1?}$, $P46^{10?}$, $P46^{11?}$, $P^{46?}$). Both of them did not give any particular descriptions to their designated correctors, though.

[41] This difficult task has already been underscored by Milne and Skeat, *Scribes and Correctors*, 40, 87; see also, Parker, *Codex Sinaiticus*, 80.

details of the corrections observable from the plates of facsimile editions.[42] (This problem is compounded when the facsimile is fraught with production inadequacies, e.g., inaccurate inking representation). Hence, it is not uncommon to see corrector re-assignments by the same scholars at a later period or by other scholars as soon as erstwhile unknown palaeographical details or more reliable (digital) images become available.[43] This method remains valid and serviceable, but should be used with a caveat, especially in cases where particular pages of the manuscript are seriously defective or where the ink residues are already almost non-existent, rendering palaeographical judgments only tentative at best,[44] even if one is working with the actual manuscript.

In this thesis, identification of correctors has been made largely on the basis of letter formations,[45] style (upright or cursive), and ink colour.[46] As those who have already made full-blown palaeographical analyses of any of the more extensive NT manuscripts would be ready to admit, there are indeed extremely difficult instances where these

[42] Turner, *GMAW*[2], 16, enumerated his criteria for identifying correctors in this manner, "The corrector's work will revealed by a different handwriting, different ink (often not easy to detect in a photographic reproduction), and the 'secondary' placing of his work in relation to the principal handwriting". It may be observed from this that Turner uses the term "corrector" in a strict sense, i.e., a "correcting" hand distinct from the main hand. However, this may not succinctly account for the fact that, at least for many of the early NT papyri, most of the corrections have been undertaken by the first hand; on this point, see Royse, *SH-M*, 77.

[43] A good example is given by Dirk Jongkind, "One Codex, Three Scribes, and Many Books: Struggles with Space in *Codex Sinaiticus*," in *NT Manuscripts: Their Texts and Their World*, 121-36, esp. 121.

[44] Fortunately, I am not starting from zero. Previous studies on the corrections of $\mathfrak{P}^{46}$, notwithstanding the intermittent debatable ascriptions, remain valuable resources to kick-off our own analysis. The two most extensive (and generally reliable) studies on the corrections on $\mathfrak{P}^{46}$ have been the two-volume *DNTAP* and Royse's dissertation-monograph. Though limited only to Hebrews and 1 Corinthians, I also benefitted from Zuntz's discussions on corrections as his comments are particularly helpful in portraying the various hands at work in $\mathfrak{P}^{46}$. I extensively interacted with these sources as I generated my initial list of corrections, but having checked the actual leaves I have found a few more previously undocumented corrections, including those that I call "unconsummated errors". In Appendix N, I indicated in the footnotes my rationale for assignment whenever I diverge from the majority classification.

[45] For instance, the presence or absence of finials characteristic of the main hand; the presence or absence of or divergence from the loops and hooks (especially for ⲁ, ⲗ, ⲙ, and ⲇ).

[46] Using the ink colour of the main text as the comparative base, heavy black ink and pale black become discernible throughout the pages of the codex.

straightforward palaeographical criteria do not always satisfy one's curiosities,[47] especially where the state of preservation and/or of the inking is not favourable toward this end. Except for a few extremely difficult cases, I have made judgments based on the most reasonably probable candidate of who might have undertaken a particular correction, and indicated this type of corrections with a question mark (if necessary),[48] instead of giving a generic ascription (i.e., "corrector");[49] this gives a more graphic picture of the activity of each "corrector". Additionally, the "*opening*" factor (i.e., facing pages) has also occasionally bolstered a more confident identification of a corrector. Since there is strong evidence for a spot-checking mode of correction in $\mathfrak{P}^{46}$, looking at this codicological feature helps to detect which hands were at work in a particular opening.

II. THE OTHER "CORRECTING" HANDS

Bearing all these cumulative criteria in mind, I have designated the 193 identified corrections under five demonstrably distinguishable hands, which I represent throughout as M^1, M^2, M^3, M^4, M^5, and a generic set of corrections which I call "ED" (i.e., *Extremely Difficult*), a brief description of each ensues. W*ho* then corrected *what* and *where*? Let me begin with the "correctors" other than the main hand, particularly the non-contemporaries.[50]

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[47] E.g., Comfort and Barrett, *Text of the Earliest*, 207, n7, clarified, "Although some different hands are clearly discernible in (Kim's) categorization, not every distinction is clear. Therefore, in this book we have not attempted to differentiate all the correctors throughout the transcription." Royse, *SH-M*, 249, himself underlined this difficulty by using a category he reflected as "$\mathfrak{P}^{46c}$" which he defined as "a correction that is not attributed more precisely." Royse is definitely correct about the difficulty involved in classifying the corrections. And it is in this regard that establishing the "correcting habits" of our main hand, and then the other hands, is of supreme assistance, as it can be a helpful classification criterion.

[48] Additionally, I have also placed the question mark in some first-hand corrections which could have been an *in scribendo* but for which certainty cannot be absolutely ascribed.

[49] Royse, *SH-M*, 249, 849, intimated this difficulty by marking a correction event with "$\mathfrak{P}^{46c}$", which he explained as indicating "a correction that is not attributed more precisely". On the other hand, Comfort-Barrett simply did not make any ascription. Zuntz, *TEDCP*, 253, noted some corrections as "unsure", referring to the difficulty of precisely identifying the corrector.

[50] Both Zuntz and Royse sequenced their discussion chronologically, commencing with the main hand, followed by the later hands. I have chosen to begin with the non-contemporaries as my focus

A. The Nib, the Mix, and the Style: Depicting Later "Users" of $\mathfrak{P}^{46}$

M^{5}, who made five corrections (Fig. 4-2.2), is distinguishable through the *thick-nib pen* and *heavy black ink*. He[51] must have been a "professional" of sort, as he can write in upright and cursive styles, and his corrections make sense and go beyond more than just orthographic concerns. For instance, on two opposite pages (an opening, f37^{r}-f38^{v} [Heb 13.18b and 21a]),[52] he restored the preposition **ϵN** twice (both longer readings supported by the manuscript tradition). Furthermore, in the newly documented correction in 1Cor 2.6 (f40^{v}-l^{07}), this scribe resolved the syntactic error by adding **N**[53] to the otherwise unsupported **COΦIA**. He also suggested[54] that the Latinized **CIABANOY** (*cum* DFG) in 2Cor 1.19 (f62^{r}-l^{10}) be read as **CIAOYANOY**[55] by supralinearly writing **OY** above **B** (without cancelling it).[56] But it is his correction in Rom 9.25 (f13^{v}-l^{07}) that intimates his connexion with our manuscript. At the first instance, instead of the quotation formula ωσηε λεγει, our scribe copied the unsupported and contextually nonsense **ωC H ϵλϵΓϵI**. As the line presently

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remains with the main hand. I am also guided by the conceptual structure proposed by Hurtado, *Earliest Christian Artifacts*, 186, that distinction of later, contemporary, and first hands indicates different levels of attitude toward manuscript use in the early church, e.g., preference for a particular reading, working environment, and attitude toward the task of copying.

[51] As in the main hand, the use of the masculine pronoun throughout this project for this and the rest of the correctors are generically meant, without prejudice to the contrary.

[52] *DNTAP*$^{2.2}$, 356-57, and Royse, *SH-M*, 856, attributed both corrections to C^{3} (the cursive corrector). However, the "cursive" criterion is not decisive in these instances as both corrections tended to be more upright than cursive. Thickness and ink colour together seems to be the deciding factors here.

[53] Except for 2Cor 1.19, I found the formation of the **N** as the most decisive factor in identifying this "new" corrector.

[54] Both Zuntz and Royse assigned this to the M^{3} (the *cursive* corrector). However, cursivity is not the decisive factor here, as the characteristic cursiveness of M^{3} is absent in this particular correction. In fact, *DNTAP*$^{2.1}$ could only ambiguously assign this to "*späterer Hand*". Accordingly, the inking seems more decisive in this instance, and M^{5}'s inking comes the closest.

[55] *Cum* ℵABCKLPΨ 1739 *rell*.

[56] Apart from their unsatisfactory attributions, Zuntz, *TEDCP*, 259-62 (unidentifiable corrector) and Royse, *SH-M*, 240-41 (cursive corrector), their respective discussions of this correction are very instructive.

stands, we see a short downward diagonal stroke above the ϖ (a later addition),[57] the colour of which is similar to the supralinearly added ν in ΕΛΕΓΕΙ, *as well as* to the reading marks on the page and its immediate environment. What this colour similarity infers is that this "corrector" is a later user of our manuscript, most likely as one of its "readers" who inscribed those reading marks in those pages of the codex, presumably to help him in his public reading task.



FIGURE 4-2.2 Collage of M[5] Corrections

On the other hand, **M[4]** is responsible for seven[58] corrections (Fig. 4-2.3), using a rather thick-nib pen also, but with a *diluted ink mixture*, resulting in a *pale black colour* characteristic,[59] which sometimes rendered his corrections unreadable,[60] if not almost

[57] *DNTAP*[2.1] sees this as part of the original reading. But the ink colour and thickness is very different from the text.

[58] Rom 12.16 (f16ʳ-I⁰⁹ φρονου<ν>τες); Heb 10.22a (f32ᵛ-I⁰¹ προσερ=||χ<ο/ω>μεθα); 1Cor 16.7 (f60ʳ-I¹² παρ<ο>δω); 2Cor 4.2 (f64ʳ-I¹⁸ απει||π<ᴀ/ο>μεθα); 9.12 (f70ʳ-I¹³ θω̄ <ο>τι), 14 (f70ʳ-I²³ <η/ʏ>μας); and Col 1.7 (f90ʳ-I⁰⁸ συνδο<ʏ>λου).

[59] Zuntz, *TEDCP*, 253, n3, assigned 1Cor 15.48 and 16.7 only, by virtue of their inking properties.

[60] *DNATP*[2.1] suggested a case of insertion of ο and subsequent erasure also by C[2] in 2Cor 4.2, arguing that "*da das ο recht scwach*". It seems to me, however, that there is no erasing event that has taken place here but more likely caused by an inking mixture problem.

non-existent (e.g., Heb 10.22a). His alterations (all supralinear) are written uprightly, without any fancy decorative flourishing, and are confined to orthographic errors (e.g., Rom 12.16; 1Cor 16.7; Col 1.7) rather than meaningful ones (i.e., exegetically significant variations). He makes no pretensions and ascribes no prescriptive authority to his corrections, as they seem to be mere *suggestions*.[61] What may be inferred from such sporadic and seemingly reluctant alterations is the likelihood that this hand's primary connexion with our manuscript is that of a "public reader" rather than a corrector.[62] Like M^5, this hand was a later user of $\mathfrak{P}^{46}$, too.

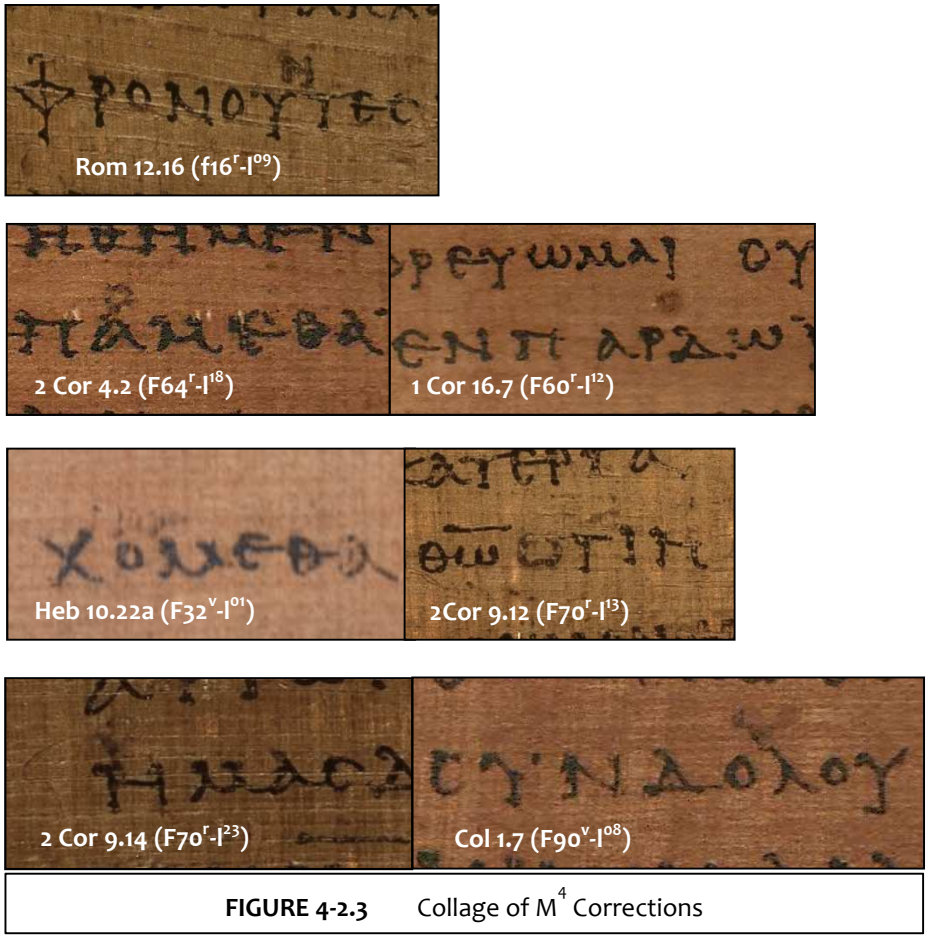


FIGURE 4-2.3 Collage of M^4 Corrections

[61] His corrections in Heb 10.22a and 2Cor 9.14, although they are alternative readings, are more likely meant to be suggestions as the original **ο** and **η** were not marked for cancellation.

[62] Starting f57r, the ink colour of the reading marks had become pale until f61r. No doubt this reading marks annotator is the one responsible for the correction in 1Cor 16.7 (f60v-l12), due to colour similarity. On this, see also Zuntz, *TEDCP*, 253, n3. These pale reading marks may be first noticed from f29r-f32r, then from f34v-f38v.

Finally, **M³**, who is best detected through his *cursive writing style*[63] and comparable letter formations,[64] is responsible for six restorative corrections (Fig. 4-2.4),[65] and like M⁴ and M⁵, this corrector made sporadic corrections only, all found in the longer books.[66] But unlike M⁴ his corrections are comparatively more textually substantial, not simply correcting "mere scribal errors" (*contra* Zuntz).[67] In fact, orthographic errors are the least of his concerns.[68]

Two things may be noted about this corrector. First, his corrections are all *prescriptive* and not merely suggestive. For instance, the alteration in Heb 10.22c involves the change from the nominal αληθειας to the adjectival αληθει<ᴺᴴ>ας (supported by the

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[63] Zuntz is to be credited for the identification of this corrector, whom he described as one who "writes a cursive hand which is easily distinguished from all others, and as C.H. Roberts tells me, should be dated late in the third century. That is to say, this is the hand of a later user of this manuscript who corrected a few places whose wording struck him as incorrect. Most of his corrections again bear upon mere scribal errors…" (pp. 253-54). This description has been adopted by Royse (*SH-D* and *SH-M* [man 3]), *DNTAP*[2.1, 2] [3. Hand], and Comfort and Barrett [C³].

[64] Specifically, the letters *ϻ*, *ϺH*, *ϺN*, and *ϵ*.

[65] Rom 15.26 (f19ʳ-l²⁰ αγιων <ᵀᵀᵂᴺ> εν ϊερουσαλημ), 31 (f20ᵛ-l⁰⁶ διακονια <ᴹᵞ>[=μου]); Heb 10.22c (f32ᵛ-l⁰¹ αληθει<ᴺᴴ>ας); 11.12 (f33ʳ-l¹² αμμος <ᴴ ᴨᴀᴩᴀ ᴛᴏ ᵪᴇɪᴀᴏᴄ>); 1Cor 12.20 (f53ᵛ-l¹⁶ πολλα <ᴹᴱᴺ>); and 14.10 (f55ᵛ-l¹³ τυχοι <ᵀᴱᴺᴴ>). Sanders basically distinguished between "man 1", "man 2", and sporadically "man 2 et 3", although there is no accounting for each corrector in the Introduction (as also noted by Zuntz, 252, n3). Accordingly, Zuntz, *TEDCP*, 254, assigned the inserted υμας in Rom 12.1 (erroneously printed as Rom xiv.10) to this corrector also. On the other hand, Royse, *SH-M*, 239-41, also attributed to this hand the following: Heb 3.6; 7.25; 13.18b, 21a; 2Cor 1.19; Eph 6.22; and Gal 5.17. However, I think that these are more rightly attributable to other correctors, which I discussed in Appendix N1.

[66] Royse, *SH-M*, 239, assigned Gal 5.17 (<ᵀᴼ> πν̄α̅) to this hand also, presumably because of its slight right-leaning look. However, I think that corrector is the first hand himself, perhaps even an *in scribendo*, due to similar ink colour and density. From the plates alone, ⲧ looks slanting to the right but autopsy reveals that there is actually a very small fragment of papyrus strand that is covering some parts of the vertical stroke of ⲧ, making it look like slanting; ο is definitely upright.

Although not cursive, Royse, *SH-M*, 220, surprisingly assigned Eph 6.22 (f81ʳ-l⁰⁶ <ᴎ/ϒ>μων) to his "man 3", arguing that it is "similar to the initial υμας of Rom 12.1". However, it is more plausible that this is also a first-hand correction, in view of similarity in ink colour and density with the main text. It may be true that the stroke is a bit different from the usual ϒ of the main hand, but this is due naturally to the little space available for the descender.

Finally, Royse, *SH-M*, 218, 223, also assigned Heb 3.6 to his "man 3", but (contra Royse as well as Kenyon [p. 24], Sanders [p. 62], and *DNTAP*[2.1] [p. 256]) it seems more likely that this is a first-hand correction, too, due to similarity in ink colour and the lower curve stroke of the ϲ (see Appendix N1).

[67] Zuntz, 254, 259, attributed the following corrections to the *cursive* corrector: Rom 12.1; Heb 11.12; 12.20; and 1Cor 14.10.

[68] Although Royse had different entries for this corrector, his general observation (*SH-M*, p. 241) agrees with mine: "The third hand is thus seen to be unconcerned about orthography except for the proper name at 2Cor 1.19…" Note, however, that I assigned 2Cor 1.19 to another corrector.

manuscript tradition). This is quite significant, because of the three later correctors he is the only one who made a correction via cancellation—indicative of the comparatively higher confidence level he has for his alterations. Second, it is noteworthy that the five other corrections involve the restoration of omitted words, toward readings supported by the majority of manuscripts rather than to a particular "texttype".[69] For instance, his correction in Rom 15.31 involves the restoration of the pronoun μου—albeit in abbreviated form, i.e., διακονια <ΜΥ> (=μου)[70]—a reading supported by the majority, except 0151.[71] Even his longest alteration in Heb 11.12 (η αμμος <Η ΠΑΡΑ ΤΟ ΧΕΙΛΟΣ>), correcting an error caused by haplography, is also a correction toward a reading with shared support.[72] Hence, what we have in M^3 is a hand who must have been involved also in the scribal trade, in one way or the other, or one who had access to other manuscripts with similar Pauline contents.

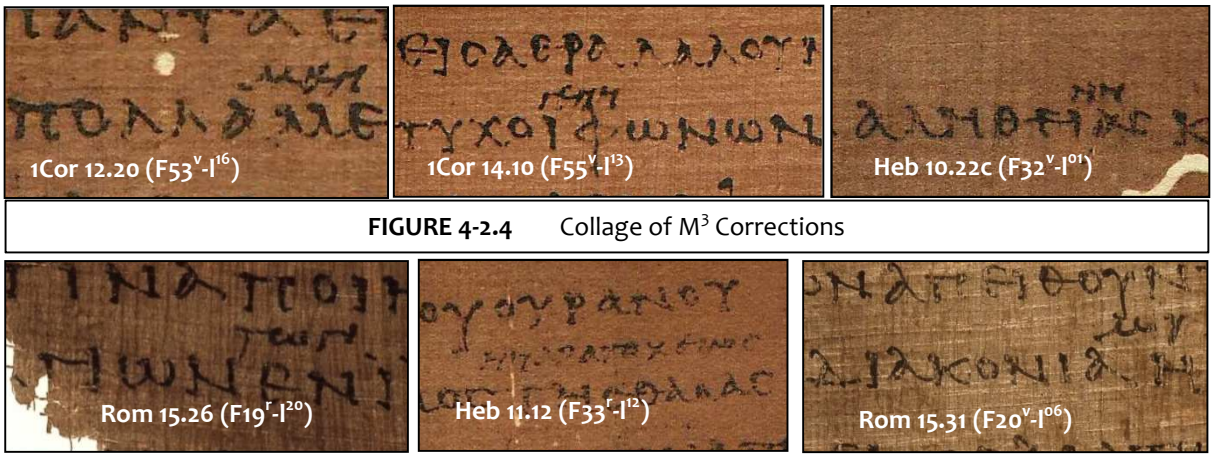


FIGURE 4-2.4 Collage of M^3 Corrections

[69] Also noted by Royse, *SH-M*, 241, "In fact, the fourteen (sic) corrections by the third hand (our M^3) are always to the majority text…"

[70] The only other abbreviated correction is the και (ϗ) compendium in Heb 7.25 (f28ᵛ-I¹¹), which is extremely difficult to assign a corrector.

[71] So is the restoration of the article ΤΩΝ in Rom 15.26 (supported by majority of the mss, including df vg), as well as the addition of ΜΕΝ in 1Cor 12.20 (supported by majority of mss, including DFG) and ΓΕΝΗ in 14.10.

[72] The longer reading is supported by both "Alexandrian" and "Western" representative mss, including D^c for the latter.

B. Unidentifiable "Other" Hands

There are at least nine cases which I find extremely difficult to assign to any of the more distinguishable correctors, for one reason or another: Rom 15.23[73] (εχ<Ɛ/ᴬ>ι<ᴺ>); Heb 6.2b (>χ/Ṇ<ε>ι/Ḳ<ρω̣ν); 7.25 (οθεν ᵏᵃ [=ᵏᵃᶦ]>); 9.8a (μη πωϛ); 11.1 (ελλεγχοϛ); 1Cor 8.9 (μη πως <ᴴ>); 2Cor 10.12 (αλλ<ᴀ> αυτοι); 12.19 (ᴏϒ παλλαι); Phil 1.20 (ζω<ᴴ>ϛ).

C. General Features of the Later Correctors

A brief summary of these later-hand corrections are in order. First, these corrections are very selective, and there are no recurring patterns in terms of page intervals, indicating a very random and non-systematic way of making corrections—precisely because they were not "correctors" in the strict sense of the word but "users" of that manuscript who effected correction only insofar as they encountered textual questions. Second, these corrections are not only selective but also mostly *suggestive* corrections, not imposing the readings they indicated.

D. The Contemporary Corrector

M², responsible for 67[74] corrections,[75] used a thick-nib pen, with heavy black ink mixture,[76] capable of writing in upright (close to biblical majuscule) and in semi-cursive styles, and mostly concentrated in Hebrews (58) and a few places in 1

[73] In Rom 15.23, the first-hand copied τοπον εχαι κλιμασι (unsupported) instead of τοπον εχων κλιμασι. This corrector then "suggested" changing the reading to εχειν by supralinearly adding ε and ν, without omitting ᴀ. This reading, however, is unsupported, too, and does not make sense in context.

[74] Royse, *SH-M*, *SH-M*, 223, 235, assigned 56 cases only to this corrector (note though that he stated 54 in p. 77.), 42 of which were in Hebrews.

[75] For a complete list of all the corrections under this corrector (and others), see Appendix N1, as well as Appendix N2 for images of these corrections.

[76] Sanders, *TCPC*, 15, described the page numbers as written in "larger and coarser" scripts and are contemporary of the main hand. Accordingly, Zuntz, *TEDCP*, 253, described this corrector as one who used "broad pen and very black ink". It seems to me though that after Zuntz mentioned about the *cursive* hand corrector, he already assumed that whenever he mentions "the corrector", he is ambiguously referring to the "second hand" (our M²).

Corinthians (9).[77] M^2 may have also been responsible for the inscription of the page numeration and the *stichos notations*,[78] and this observation seems strongly supported by palaeography.

M^2's correcting style involves the use of expunging dots and/or slashing strokes (for deletion), intra- and interlinear insertions, use of *ancora* for cases involving incremental omissions, and writing over certain letters. In manifest contrast with the previous correctors, M^2 inscribed corrections with imposing authority—all his corrections are *prescriptive*! In fact, Zuntz himself depicted M^2 as the "*ex-officio* corrector, who still in the scriptorium, applied the finishing touches to the work of the scribe".[79] This sense of authority is demonstrated by what he did.

Most of his corrections are of the scribal slips, i.e., correcting orthographic and nonsense readings. But to his credit he was able to remove meticulously some of the horrendous blunders of our scribe: Heb 5.6 ($f25^v$-l^{21}) <ΙΕΡΕΥC>ἐπὲὺξ; Heb 7.1 ($f26^r$-l^{23}) σα~~μου~~<Λ>ημλ; Heb 7.2 ($f27^v$-l^{04}) σαὰὸὺ<Λ>η<Μ>λ; etc.[80] He also ingeniously filled up some (visual) gaps in the manuscript, e.g., Heb 10.10a προς<ΦΟΡΑC> του σωματος.

Interestingly, a few of the mistakes he correctly altered concern serious factual errors regarding OT history. For instance, in Heb 11.21a ($f34^v$-ll^{08-09}), what our scribe mistakenly transcribed as "Jacob, whilst he was dying, blessed each of his sons"[81] was rightly corrected by M^2 to ϊακωβ απο||θνησκων εκαστον των υϊων

[77] The lonesome correction outside of these two books is 2Cor 7.13 ($f68^r$-l^{06} χαρ<Α> τι) where the inserted Α is very similar with the alphas of Heb 9.22a ($f30^r$-l^{12}). This decreasing number of corrections suggests that this corrector is self-conscious that he is only doing a "spot-checking" kind of correction.

[78] So is Zuntz, *TEDCP*, 253.

[79] Zuntz, *TEDCP*, 253.

[80] To his list of errors of "horrors", Zuntz, *TEDCP*, 253, added Heb 8.5 ($f28^r$-l^{17}) γ<α>ραφησει<ν>; and 9.22 ($f30^r$-l^{12}) νεκρω <αιματι>. Furthermore, Royse, *SH-M*, 235, added Heb 9.14a to his list of "most striking errors", on top of the ones already mentioned.

[81] UBS^4-NA^{28} common text reads Πίστει Ἰακὼβ ἀποθνήσκων ἕκαστον τῶν υἱῶν Ἰωσὴφ εὐλόγησεν.

~~αυ~~<ιωσεφ>||ϊöϋ ευλογησεν (Jacob, whilst he was dying, blessed each of the sons of Joseph).[82]

M^2 is not only intent on removals of bad readings, but is also in the business of restoring substantial grammatical elements that were not reflected in our scribe's original version. For example, he rightly restored the long omission **ΚΑΙ ΠΑΝΤΑϹ ΤΟΥϹ ΑΓΙΟΥϹ** in Heb 13.24 ($f38^{v}$-l^{11}) and the Pauline formulaic final **ΥΜΩΝ** of the concluding benediction of 13.25 (l^{13}). He also restored the omitted divine subject in 9.20 ($f30^{r}$-l^{09}) προς ϋμας<ο $\overline{\text{θϲ}}$>—the lone insertion involving a contracted *nomen sacrum*; the intensifying pronoun **ΠΑΝΤΩΝ** in Heb 12.23 ($f36^{r}$-l^{06}); the possibly perceived dittography in Heb 10.37 ($f32^{r}$-l^{17}) οσον<**ΟϹΟΝ**>; among others.[83]

But whilst M^2 seems to portray a commanding presence in *what he did*, his imposing authority as a strict corrector is somehow ironically tarnished by *what he did not do*. He, like the other "correctors", also unfortunately missed rectifying the other serious blunders in the manuscript. We should note that whilst he restored some of the incremental omissions in Hebrews (via use of *ancoras*, Heb 8.8b; 9.14b; and 12.6-7), he failed to correct six other omissions of this kind in Hebrews, cumulatively totalling to 70 characters, and restoring none of the incremental omissions in 1 Corinthians totalling to 303 characters.[84] This does not speak well about his efficiency. This leads to another point of a methodological nature: Did M^2 really systematically do his "job",

[82] To this, we may add the previously noted alterations in Heb 7.1-2, involving μελχισεδεκ βασιλευς σαμουηλ, which no doubt is a gross error by our scribe (unless it was the reading of his *exemplar*!). How could such a very prominent OT figure be mistaken to be a king, when in fact he was himself against the very idea of establishing monarchical rule in Israel (1Sam. 8.6-7)? It is a good thing that M^2 corrected this.

[83] Heb 1.9 ($f22^{v}$-l^{05}) ο θ$\overline{\text{ϲ}}$ <**ϹΟΥ**>; 5.11 ($f25^{r}$-l^{05}) <**ο**> λογος; 10.1 ($f31^{v}$-l^{11}) θυσιαις<**ΑϹ**>; 10.2 ($f31^{v}$-l^{14}) εχειν<**ΕΤΙ**>; 10.25b (32^{v}-l^{10}) εαυτων<**ΚΑΘΩϹ**>; 12.10 ($f35^{r}$-l^{24}) μεν<**ΓΑΡ**>; 12.25b ($f36^{r}$-l^{13}) <**ΤΟΝ**> χρηματιζοντα; 13.8 ($f37^{v}$-l^{16}) εχθες <**ΚΑΙ**>; 1Cor 13.5c ($f54^{v}$-l^{13}) το <**ΜΗ**>; 14.29a ($f56^{v}$-l^{15}) <**ΔΕ**> δυο; and 14.29c ($f56^{v}$-l^{16}) <**ΚΑΙ**> οι.

[84] Heb 8.5d (8 letters), 11-12 (31); 9.19b (12); 10.17a (5); 11.17b (9), and 39 (5). 1Cor 1.8a (3 letters), 25 (44); 3.10a (5); 6.12 (33); 8.12 (10); 9.7d (10), 19-20 (46); 10.19 (18), 28b (28); 14.19c (6), 23d (8); 15.40 (25), 54 (35); and 16.19a (32).

i.e., deliberately searching for errors to correct on each and every page? A look at his correction frequency highlights the implication of this question.

In total, M^2's 67 corrections are scattered in 32 pages, encompassing 21 openings. After his first two recorded corrections (Heb 1.1, 9 [$f21^r$-l^{05} and $f22^v$-l^{05}]), the third correction is found only after five pages (Heb 5.6 [$f25^v$-l^{21}]), followed by seven other corrections from $f25^r$-$f27^v$, a page without M^2 correction, then nine pages with a combined 26 corrections, an interruption of one more page with no M^2 correction, then another nine pages with 24 corrections. After this, his work is interrupted by nine pages without any correction, then three corrections on three different openings, then another nine-page without corrections, after which are seven intermittent corrections involving a total of five pages but punctuated by 30 pages with no M^2 corrections. Finally, the gap between the last correction in 1Cor (14.29c [$f56^v$-l^{16}]) and the lone correction in 2Cor (7.13 [$f68^r$-l^{06}]) is 23 pages ($f57^v$-$f68^v$). What this frequency pattern unambiguously reveals is that M^2 was *not* deliberately and systematically searching for errors to correct, since all these pages without M^2 corrections are laden with clear scribal errors and genuine variations![85] Furthermore, in pages where M^2 effected corrections, there are other variations that M^2 did not correct even if they are badly in need of corrections.[86] Indeed, Zuntz was not exaggerating when he stated that this corrector "did his work very carelessly".[87]

Furthermore, whilst M^2 corrected a few of the factual errors, other equally serious errors of fact were not amended. For instance, the omission of κατηγωνισαντο

[85] Indeed, Beare, "Text of the Epistle to the Hebrews," 383, was not incorrect when he keenly noted, "Sporadic corrections in a second hand appear on almost every page. These do not reflect anything resembling a systematic effort at revision through comparison with another manuscript. The majority of them are simply the rectifying of obvious blunders, and not even all of these are corrected."

[86] In fact, the opening where he made the most corrections (10 total) is $f36^r$-$f37^v$. This figure seems impressive at first glance until one looks at our Appendix of Variations—there are 16 variations that should have been corrected but were not!

[87] Zuntz, *TEDCP*, 253.

(*conquered*) and the substitution of βασιλειας (*kingdoms*) with **ΒΑCΙΛΕΙC** (*kings*) in Heb 11.33 (f34^{r}-ll$^{20-21}$) have totally altered the sense of the passage—instead of the heroes of faith (in v.32) doing the subjugation of kingdoms and enforcement of righteousness, a new set of heroes(!) have been introduced by the alterations, i.e., **ΟΙ ΔΙΑ ΠΙCΤΕΩC ΒΑCΙΛΕΙC ΕΙΡΓΑCΑΝΤΟ ΔΙΚΑΙΟCΥΝΗΝ** (*through faith, kings enforced righteousness*). M^{2} missed correcting this gross error although he made two corrections on this page, one of which (l^{19}) is very close to this one![88]

Worse still is that he, too, by some of his corrections, produced otherwise unattested or nonsense readings.[89] For instance, in Heb 10.25 (f32^{v}-l^{09}) M^{2} shortened επι||συναγωγην by cancelling the prefix through expunging dots. Also, his correction in Heb 12.26 (f36^{r}-l^{15}) produced a contextually nonsense reading, supported by no one else. Another unsupported reading is Heb 12.4c (f35^{r}-l^{10}) where he has overwritten **C** with **K**, producing an otherwise contextually nonsense reading, i.e., αν=||τικατεστη>**C**/**K**<εν.[90]

Another unattractive characteristic of M^{2} is his marked dis-interest for aesthetic appeal; many of his corrections made our codex aesthetically and literally dirty![91] This does not mean however that he could not write calligraphically beautiful

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[88] Another equally serious historical flaw that M^{2} left uncorrected—although he rectified a historical error on this opening (f34^{v}-l^{08})—is Heb 11.17 (f33^{r}-l^{27}) where our codex reflects a historically inaccurate reading: Isaac became the "offerer" instead of the "offering" (πιστι προσ||[ενηνοχ]ε̣ν̣ ισακ π̣ει̣ρ̣α̣ζομε̣νος instead of "Πίστει προσενήνοχεν Ἀβραὰμ τὸν Ἰσαὰκ πειραζόμενος" [UBS^{4}-NA^{28}]). Also, Heb. 9.2-3 (f29^{r}-l^{01}) was left uncorrected where the original version reflects our scribe's skewed schematic understanding of the tabernacle layout, confusing between the Holiest Place and the Holy Place. This is despite the fact that M^{2} made three corrections on this page: Heb 9.5a (ll$^{09-10}$), 6 (l^{14}), and 8a (l^{20}).

[89] Zuntz, *TEDCP*, 253, mentioned Heb 10.24 (f32^{v}-l^{07}) as an example.

[90] To this we may also add Heb 12.26 (f36^{r}-l^{15}) where the expunging of the particle **EI** produced an unattested nonsense reading, i.e., ἔι η φωνη.

[91] This corrector is generally a genius, but is not primarily concerned with aesthetics. For instance, in Heb 7.1, the first hand copied σαμουηλ—a serious error of facts—and having spotted this crucial mistake, M^{2} remedied the error by striking through the letters μου, then wrote λ on top of these letters. Afterwards, instead of crossing out the final λ, he instead wrote μ on top of it, restoring the correct reading σαλημ. This shows that this corrector had a good sense of resolving the problem, but the way he did it made the text a bit dirty, lacking the artistry and patience of the first hand.

scripts; in fact, he is capable of writing very good biblical majuscule as seen in Heb 9.22a (f30^{r}-l^{12}), but it seems that it is more of an exception than a rule.

III. THE FIRST HAND

Most of the recorded corrections in $\mathfrak{P}^{46}$ bear the imprints of its own scribe (**M^{1}**), accounting for 99 of the 193.[92] Unlike the other correctors, M^{1} has corrections on all the extant books (except 1Thess),[93] more than half of which transpired at the second half of the codex. In fact, of the 28 corrections after the larger books 26 are from the first hand,[94] divulging further the very selective corrections made by the other hands.

As in the main text, his corrections were written in upright style, with his usual flair for aesthetic beauty, carefully keeping his manuscript tidy, conspicuously avoiding making messy alterations, an attitude not shared by M^{2} who, on the other hand as we have seen, was more concerned with putting the "horrors", as Zuntz put it, of our manuscript right. Of course, Zuntz's comparative characterization is an unfair one, considering that M^{2} has also made horrendous mis-corrections and missed serious errors that needed to be rectified. Second, our scribe himself has also self-corrected a number of serious blunders in his *exemplar*, as well as avoiding potential errors (i.e., "unconsummated errors"). As a matter of fact, many of our scribe's own corrections have been effectively made in the course of copying (i.e., *in scribendo*), with some discernible patterns. *How* then did our scribe make corrections? Is there any reliable discernible *pattern* from these that can be clearly distinguished as his "correcting

[92] Rom (11), Heb (21), 1Cor (25), 2Cor (16), Eph (8), Gal (9), Phil (6), Col (3). This high degree of self-correction disproves Tasker's assertion that "The scribe who wrote the papyrus himself corrected in the course of writing a few of his many errors; but the large majority of the corrections of his slip-shod work and of the readings he was copying have been made with a broad pen and very black ink."

[93] Theoretically, it is possible for our scribe to have made some corrections in 1Thess as well in view of his self-correcting tendency in the extensively preserved books, but the fragmentary nature of the two surviving folios with 1Thess texts prevents any reasonable investigation along this line.

[94] Eph (8), Gal (9), Phil (6), and Col (3). It should be noted though that most of these are corrections during the first stage of copying (i.e., *inter scribendum*); for details, see Appendix N1.

habits"? To answer these, it is necessary to identify first the patterns that are more likely bred by his training and experience as a *Berufsschreiber*.

A. Linear-based Correction Patterns

Like any other experienced copyist conscious of his duty to come up with a manuscript as faithful as possible to his textual base, including effecting corrections when necessary, our scribe utilised linear-based correction patterns; I found no trace of any marginal correction. Linear-based corrections are either *intralinear* or *supralinear* alterations.

1. Intralinear Insertions

Intralinear insertions happened when our scribe, having spotted the absence of a character(s)—rendering a reading either nonsense or orthographically incorrect—carefully inserted into the line (space permitting) the missing character(s). In Gal 1.13 ($f81^{v}$-l^{08}), for instance, the scribe should have copied ιουδαισμω but missed the second ι. Discovering the unusual spelling, he corrected it by skilfully inserting the missing **ι** (with a somewhat elongated descender) in between **ϡ** and **ϲ**.[95] Similarly, the **ϲ** in πορευσονται of 1Cor 16.4 ($f60^{r}$-l^{06}) was not copied at the first instance, resulting in an unsupported reading. But upon detection our scribe arrested the problem by sensibly inserting the missing **ϲ**—although a bit smaller in size—in between **ϒ** and **ο**,[96] once again showing his preference for aesthetic beauty even in matters of correction.

2. Supralinear Insertions

Conversely, the second pattern results when the first option is not spatially possible—effecting corrections via *supralinear* insertions of missing letter/s to resolve orthographic or nonsense errors. For instance, the appositional clause ος εστιν η κεφαλη, χριστος in Eph

[95] Exactly similar process happened in Eph 2.3 ($f76^{r}$-l^{15}) involving the word επιθυμιαις.

[96] The same correction pattern was applied in Rom 10.13 ($f14^{v}$-l^{02} σωθησετ<**ϡ**>ι); Heb 7.11 ($f27^{r}$-l^{04} α<**ϡ**>ρων); 2Cor 12.5 ($f73^{r}$-l^{15} ασθεν<**ϵ**>ιαις); Phil 1.7b ($f86^{v}$-l^{06} εχ<**ⲉ**>ιν); and 3.18 ($f89^{r}$-l^{19} εχ<**ⲑ**>ρους).

4.15 (f78^{v}-l^{01})—supported by majority of the manuscripts—is rendered differently, that is, ο εστιν η κεφαλη του $\overline{\text{χρυ}}$. Whilst the variation in general is exegetically permissible, the neuter relative **ο** syntactically does not make sense in context, and therefore was remedied by supralinearly inserting the missed **ϲ** between **ο** and **ϵ**, to read **οϲ ϵϲτιν**. Rom 14.15 (f18^{v}-l^{16}) is another interesting case; although only 3.1 cm is preserved of the page, the correction event has survived, involving the supralinear insertion of **ϫ**, i.e., απο$^{\langle ϫ\rangle}$λ[υε].[97] Similarly, the orthographic problem in 2Cor 3.6 (f63^{v}-l^{06}) was solved by a supralinear insertion of an additional **ⲛ**.[98] A more complex[99] (and creative) correction event is Col 1.20 (f91^{r}-l^{12}) where our scribe originally copied δια του αιματος του $\overline{\text{στου}}$ **ⲇⲓⲁ ⲧⲟⲩ** ειτε (an unsupported reading), instead of the widely supported δια του αιματος του $\overline{\text{στου}}$ αυτου ειτε, probably caused by consecutive occurrences of genitive endings. To arrest the orthographic problem, **ⲩ** was supralinearly inserted between the **ⲁ** and **ⲧ**, skilfully transforming the reading to **ⲇⲓ ⲁ**$^{\langle ⲩ\rangle}$**ⲧⲟⲩ**.[100]

Supralinear insertions, however, are not restricted to orthographic problems; they also addressed word omissions. It is remarkable, however, that corrections of this type are limited to one full lexical word only, despite the above-mentioned large number of incremental omissions needing restoration. This observation may yet

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[97] Another supralinear insertion of **ϫ** is Gal 2.12 (f82^{v}-l^{02}), **ⲩⲡⲉⲥⲧⲉⲗ**$^{\langle ϫ\rangle}$**ⲉⲛ**, to agree with the majority of the manuscripts.

[98] So are Rom 13.5 (f17^{v}-l^{10} υποταϲ$^{\langle ϲ\rangle}$εσθε); Heb 11.22 (f34^{v}-l^{14} οστ$^{\langle ϵ\rangle}$ων); 1Cor 10.21c (f50^{r}-l^{03} τρ$^{\langle ⲁ\rangle}$πεζης), 25 (f50^{r}-l^{11} εσθ$^{\langle ϵ\rangle}$ιετε); 14.20 (f56^{r}-l^{13} ται$^{\langle ϲ\rangle}$ φρεσιν); 2Cor 3.6 (f63^{v}-l^{06} αποκτεν$^{\langle ⲛ\rangle}$ει); 6.12 (f66^{v}-l^{25} στενοχωρει$^{\langle ϲ\rangle}$θε); 10.15 (f71^{r}-l^{12} τα $^{\langle ⲁ\rangle}$μετρα); Eph 5.5 (f79^{r}-l^{23} $^{\langle ⲁ\rangle}$καθαρ̣[ος]); Gal 4.11 (f84^{r}-l^{17} εκοπι$^{\langle ⲁ\rangle}$σα), 16 (f84^{r}-l^{26} ὑμε$^{\langle ⲓ\rangle}$ν), and 22 (f84^{v}-l^{06} $^{\langle ϵ\rangle}$λευθερας).

[99] UBS^{4}-NA^{28} reads διὰ τοῦ αἵματος τοῦ σταυροῦ αὐτοῦ, [δι᾽ αὐτοῦ] εἴτε. Metzger, *TCGNT*^{2}, 554, explained that the Committee thinks that omission of the bracketed prepositional phrase in some manuscripts might have been accidental ("because of *homoioteleuton*") or deliberate ("because it is superfluous and obscure"). However, since $\mathfrak{P}^{46}$ is our earliest witness to this variation, it is not farfetched that it originated from our scribe—it is plausible that the longer reading was a case of a dittography, that is, our scribe's eyes jumped from the $\overline{\text{στου}}$ ending back to ειρηνοποι=‖ησας δια του (being at the line-end in his *exemplar*), thus, committing two errors, the omission of αυτου and the production of its $\overline{\text{στου}}$ δια του reading. Such scenario is supported by manuscripts without δια αυτου (BD*G 81 1739 it vg cop^{sa} arm eth).

[100] The same skilful correction display is demonstrated also in Phil 1.7a (f86^{v}-l^{05} του$^{\langle ⲧⲟ\rangle}$ φρονειν).

again betray our scribe's preference: either he was honestly unaware of these accidental omissions[101] or he was more concerned about the unpleasant aesthetic effect of restoring these missing words, especially in cases like Heb 12.6-7 where restoring them means an alteration involving no less than three long lines of 30-33 characters to a line. At any rate, some examples are in order. The typical Pauline paraenetic formula παρακαλω ουν ϒMAC αδελφοι was originally copied without the pronoun in Rom 12.1 (f16^{v}-l^{03}),[102] but was restored via supralinear correction, in the same way that the preposition ЄN in Heb 9.25 (f30^{r}-l^{25}) was restored.[103] The rest of the examples are one-letter words, particularly involving definite articles, as in 1Cor 7.15 (f45^{r}-l12 η $^{<H>}$ αδελφη); and 2Cor 10.6 (f70^{v}-l^{14} υμων $^{<H>}$ υπακοη).

B. *Suggestive* Alternative Readings

Apart from the three examples above, most of the supralinear corrections involve one-letter-insertion only.[104] But in addition to addressing nonsense/orthographic and omission problems, some one-letter supralinear corrections reflect *alternative readings*,

[101] Another possible scenario is that his *exemplar* already did not have these omitted words, which he only faithfully reflected in his manuscript.

[102] Previous studies attribute this correction to be either from M^{2} or M^{3}. However, I am more inclined to take this to be from the main hand in view of the formations of ϒ (with its usual finials) and C (especially the typical formation of its cusp); its being a cursive is not a decisive criterion in this instance.

[103] The interlinear restoration of the neuter article TO in Gal 5.17 (f85^{r}-l^{25}) is an interesting text-critical exercise, as the insertion is grammatically unnecessary if not syntactically impossible, raising the question as to whether the addition is a plain lapse due to fit of exhaustion on the part of our scribe or whether this is *exemplaric* in nature. Royse, *SH-M*, 241, assigning this correction to his "man 3", suggested that the later corrector misread the preceding $\overline{\text{ΠNA}}$TOΔЄ as ΠNЄYMATOC and then added the definite article. It is very unlikely, however, for 𝔓^{46}'s Δ to be overlooked as it is almost always written in unusually broader size than other letters. Note also that the line has $\overline{\text{ΠNC}}$__TO_ΔЄ (with the characteristic space-gaps) and not ΠNATOΔЄ. Furthermore, it is most likely, due to similar ink colour and density, that this is a first-hand correction than M^{3}. It may look like that τ is a bit slanting to the right, but autopsy reveals that there is actually a very small fragment of papyrus strand that is covering some parts of the τ, rendering it with a seemingly slanting stroke.

[104] I found two cases only of two-letter non-lexical supralinear insertions: first, the insertion of ЄC in Heb 3.6 (f23^{r}-l^{08}), to rectify the conflated οικοσμεν (read also by 075) το οικος $^{<ЄC>}$μεν (read by majority of mss), and second, the insertion of TO in Phil 1.7a (f86^{v}-l^{05}), transforming the articular infinitive του φρονειν (*cum* L) to the more widely supported demonstrative του$^{<TO>}$ φρονειν. 1Cor 10.2 (f49^{r}-l^{03}) is of another kind as it involves the insertion of CA, not as orthographic correction but as an alternative reading, i.e., from εβαπτιζοντο to εβαπτισαντο (read by most other mss).

although they seem to be intended merely as *suggestions.* For instance, above the -ⲁⲓ of the infinitive ενδυσασθαι ("to put on") in Rom 13.14a (f17^{r}-l^{06}, *cum* B) ⲉ was supralinearly inserted, alternatively suggesting the imperative ενδυσασθⲉ ("put on", *cum* ℵB).[105] Admittedly, the alternative reading does not make any significant meaning change.[106]

Accordingly, some suggestive alternative readings are no more than just syntactical corrections,[107] as in the case of Heb 9.24 (f30^{r}-l^{22}) where the dative articular phrase τω προσωπω was copied as τω ⲡⲣⲟⲥⲱⲡⲟⲩ,[108] hence, a supralinear insertion was written above the genitive ending -ⲟⲩ to agree with the preceding article. Conversely, in 2Cor 1.1 (f61^{r}-l^{04}), the article is syntactically at fault, i.e., ⲧⲟⲩ ουση instead of ⲧⲏ ουση (supported by the manuscript tradition), hence, the supralinear insertion of ⲏ. In both examples, however, is the absence of cancellation marks (i.e., expunging dot and/or diagonal slash), despite the fact that the original readings are clearly syntactically incorrect. Other examples of this correction pattern include:

ⲟ to ⲱ: Heb 12.28 (f36^{r}-l^{23}) εχ<ⲟ/^{ⲱ}>μεν;
Rom 9.17 (f12^{r}-l^{16}) ενδειξ<ⲟ/^{ⲱ}>||μαι
ⲏ to ⲩ: Heb 10.34 (f32^{r}-l^{09}) <ⲏ/^{ⲩ}>μων;
Eph 1.18 (f75^{v}-l^{18}) <ⲏ/^{ⲩ}>μας; 6.22 (f81^{r}-l^{06}) <ⲏ/^{ⲩ}>μων; and
Gal 1.6 (f81^{r}-l^{22}) <ⲏ/^{ⲩ}>μας;
Conversely, there is only one instance of reverse change:
2Cor 1.11 (f61^{v}-l^{12}) <ⲩ/^{ⲏ}>μων
Heb 13.22 (f38^{v}-l^{08}) <ⲁ/^{ⲉ}>πεστειλα
1Cor 4.14 (f42^{r}-l^{11}) νουθετ<ⲏ/^{ⲱ}>
1Cor 15.34 αμαρταν<ⲏ/^{ⲉ}>τε
Col 1.28 (f91^{v}-l^{04}) νουθετουν<ⲑ/^{ⲧ}>ες

[105] Another –ⲁⲓ to –ⲉ case without trace of cancellation marks is 1Cor 15.17 (f58^{r}-l^{01}) where the original εστⲁⲓ was suggestively corrected to be read as εστⲉ, perhaps toward another *exemplar.*

[106] There is also no significant meaning change in Rom 13.14b (f17^{r}-l08), from the accusative εις ⲉⲡⲓⲑⲩⲙⲓⲁⲛ to the genitive εις ⲉⲡⲓⲑⲩⲙⲓⲁⲥ (it has "corrected" the syntax flow though in consonance with the preceding preposition).

[107] See also 1Cor 16.1 (f59^{v}-l^{23} ⲉⲓ τους αγιους to ⲉⲓⲥ τους αγιους).

[108] 1Cor 13.12 (f55^{r}-l^{03}) is another supralinear correction, involving the word προσωπον which was originally copied as προσⲟπον (a nonsense reading). But as in Heb 9.24 the insertion of ⲱ surprisingly did not have accompanying cancellation marks (i.e., expunging dot and/or diagonal stroke), although the original reading is clearly orthographically incorrect.

These examples seem to give us a glimpse of the earlier oral stage of $\mathfrak{P}^{46}$'s textual ancestry, involving confusions of phonemically similar sounding vowels, which abound in our manuscript, both corrected and uncorrected. What is surprising here is the seeming reluctance of our scribe to effect prescriptive corrections—a practice he has done in a number of other instances.

C. *Emendatio Inter Scribendum*

1. "Unconsummated Errors"[109]

The first set of *in scribendo* corrections involves those that can be appropriately called "unconsummated errors", for which we have a few examples. In Heb 2.7, instead of his *exemplar*'s βραχυ ⲧⲓ παραγγελους, our scribe originally copied βραχυ ⲧⲓⲥ παραγγελους which does not make sense in context. It was just a momentary lapse, however, for the scribe immediately rectified the error by overwriting the final ⲥ with the first vertical stroke of the ⲧ, to agree with the reading of his *exemplar.*

Similarly, in the previously undocumented 2Cor 10.4, he mistakenly wrote the nonsense ⲇⲩⲛⲁⲧⲛ instead of ⲇⲩⲛⲁⲧⲁ, but immediately corrected the momentary error by overwriting the ⲛ's first vertical stroke with a looping stroke connected to the original oblique stroke and neatly erasing the second vertical stroke to form ⲁ.

In Phil 3.15, the initial looping stroke of the first ⲟⲱ was neatly transformed into an ⲟ which must have been the reading of his *exemplar.* Using existing strokes as base for corrections, whenever possible to still come up with neatly looking lines (with one exception),[110] is a recurring correction pattern by our scribe.[111]

[109] See related discussion in pp. 241-42.

[110] In 2Cor 7.10 ($f67^{v}$-l^{18}), our scribe originally copied the first letter of ⲅⲁⲣ as ⲕ, but the very instance he sensed the error immediately corrected it to a ⲅ. However, in the process of doing so, our scribe unnecessarily overwritten the original vertical stroke of ⲅ, resulting in a somewhat obvious thicker stroke.

2. Expunction Corrections

The second, and more widespread, *in scribendo* pattern concerns *expunctive* corrections using a right-to-left diagonal or horizontal stroke drawn through a letter/s and/or an expunging dot placed above a letter/s (or vice-versa). Examples of this type, which are all *prescriptive* in character, are plenty.

Apart from the usual expunging of letter/s to denote deletion, another observable pattern which normally happens when the erroneous letter/s at issue is at the end of a word[112] involves corrections *additionally* marked with a space-gap after expunctions, of which Rom 9.20 ($f12^{r}$-l^{23}) is an excellent example. In this instance, after copying πλασαν**ΤΙ**, our scribe's eyes accidentally jumped backward[113] to the previous line containing the phrase βουλημα**ΤΙ ΑΥΤΟΥ** τις (perhaps at the line-end in his *exemplar*); he may have already copied two characters when he realized the visual mistake. The correction was immediately effected by cancelling the two letters with expunging dots and diagonal strokes *plus* (at least) a two-letter space-gap before writing the correct entry. This must have involved momentary pauses between the expunction and the writing of the correct wording, perhaps even involving repeated look at the *exemplar* to ensure the exactness of the correction. Other examples of this sub-type include:

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[111] We see this pattern employed also in Heb 2.4 ($f22^{r}$-l^{03} συνεπιμαρτυ||ρουντ>ε/ο<ς); 12.4b ($f35^{r}$-l^{10} αν||τικατεστη>σ/κ<εν); 1Cor 3.21 ($f41^{r}$-l^{15} καυ>[.]/χ<ασ||θω); 4.5 ($f42^{v}$-l^{06} γε>[.]/ν<η||σεται); 11.27 ($f52^{r}$-l^{03} ος <ε̣/α>ν); 15.24a ($f58^{r}$-l^{16} πα||ραδι>ο̣/δ<ω); 2Cor 8.14 ($f69^{r}$-l^{04} υστερη>κ/μ<α); and Col 3.3 ($f92^{v}$-l^{06} απεθαν>ε/α<τε). See also this pattern in the correction event of Heb 9.14 ($f30^{v}$-l^{15} >π̄/ᾱ̄ ῑ̄ <>ν̄/μ̄<ᾱ̄ [i.e., π̄ ν̄ ᾱ̄ to ᾱ̈ιμ̈ᾱ̈]) in pp. 282-84.

[112] There is one instance of this type that occurred at the middle of a word: 1Cor 8.10 ($f47^{r}$-l^{12}) ειδωλ**ΑΥ**$^{ο>}$_θυτα).

[113] The same leap backward happened in Rom 13.12 ($f17^{r}$-l^{02}); our scribe's eyes jumped backward to the previous line of his manuscript (not his *exemplar*), directly situated above it, where the visual similarity of the verbal endings could be easily misconstrued, i.e., αποβαλ**ΩΜΕΘΑ ΟΥΝ** and ενδυσ**ΩΜΕΘΑ**, hence, the dittographic copying of the conjunction **ΟΥΝ**. The error was rectified by a horizontal line through the word and expunging dots above each letter. See also pp. 278-79.

Heb	3.7 (f23^{r}-l^{12})	της φωνης‖ⲙ̀ⲟ̀ⲩ̀__ⲁⲩⲧⲟⲩ ακουσητε;[114]
	10.22b (f32^{v}-l^{01})	προσερ=]‖χο^{ω}μεθα ⲓ̀ⲅ̀ⲁ̀ⲣ̀__ μετα;
1Cor	8.7 (f47^{r}-l^{01})	εσθιουσιⲛ̀__και);
	15.24b (f58^{r}-l^{17})	$\overline{\text{ⲡⲁⲣ}}$ⲏ__οταν);[115]
Gal	1.14 (f81^{v}-l^{12})	περισσοτερὸⲛ̀__ⲱⲥ);[116]
Rom	9.4? (f12^{v}-l^{15})	υιοθε=‖ⲥⲓⲁⲛ__ και.[117]

D. Tidbits of Interesting (Mis)Corrections

There are a few cases where our scribe, by his correction activities, produced unattested forms. For instance, the definite article in Gal 5.17 (f85^{r}-l^{25})[118] was puzzlingly (wrongly) inserted[119] into the line, i.e., το δε $^{<\text{το}>}$ $\overline{\pi\nu\alpha}$.[120]

Also, his correction in 2Cor 10.3 (f70^{v}-l^{05}) περιπατουτ<ⲁ/^{ϵ}>ς resulted in a nonsense reading[121] because he forgot—after inserting "ϵ" in place of "ⲁ"—to write the orthographically essential "ⲛ" to render the correct reading, περιπατουντες, as supported by the majority of manuscripts.

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[114] This particular instance, admittedly another complex scenario, may yet again provide a glimpse to our scribe's tendency in committing error involving "memory verses". Either our scribe "accidentally" copied μου into his manuscript from rote memory (this is part of an OT quotation) or that our scribe was faced with two contrasting readings in his *exemplar* (ⲙⲟⲩ versus ⲁⲩⲧⲟⲩ) which he only discovered after he had already copied the first one. Since the μου reading is unsupported (αυτου is supported by 𝔓^{13} ℵAB etc) and the other two occurrences of the quotation both have φωνης αυτου (Heb 3.15 and 4.7), the first scenario seems more likely. At any rate, the problem was solved by cancelling μου via expunging dots, a space-gap, and then the writing of αυτου.

[115] This and Phil 1.23 (f87^{r}-l^{14} ε$\dot{\nu}\overline{\chi}\bar{\omega}$ν) are the only two correction instances in the whole of 𝔓^{46} (i.e., extant pages) that involve *nomina sacra* contractions, both *in scribendo* corrections.

[116] *DNTAP*$^{2.2}$, 6, noted that this expunction is "*wahrscheinlich von der 2. Hand*". However, I deem this to be more likely from the first-hand in view of ink colour similarity, thus, corroborating the suggestion that this pattern of correction (expunction + space-gap) is characteristic of the first-hand than anyone else. Sanders and Royse (also *in scribendo*) ascribed this to the first hand as well.

[117] I reckon this particular example only as a possibility as this involves a continuation of an unfinished word from the previous line running through the next line. Usually, our scribe puts a space-gap in these cases; on this, see my analysis on space-gap involving end-line words, pp. 199-200.

[118] Kenyon, *CBBPIII-1936*, 139, noted, "το *add. m.2 per errorem*". However, I am inclined that the corrector in this instance is the first hand himself, perhaps an *in scribendo*, due to similar ink colour and density. It may look like that τ is a bit slanting to the right, but autopsy reveals that there is actually a very small fragment of papyrus strand that is covering some parts of the τ, making it look like slanting.

[119] In fact, the insertion is unwarranted as there are no grammatical errors in the original version at that point.

[120] The corrected ⲧⲟ ⲇⲉ ⲧⲟ ⲡⲛⲁ is a peculiar reading, while the original reading ⲧⲟ ⲇⲉ ⲡⲛⲁ is supported by the manuscript tradition.

[121] 1Cor 7.17 (f45^{r}-l^{17}) εμερι<σ/^{κ}>εν is another correction resulting to a nonsense reading.

IV. CORRECTED TOWARDS THE ALEXANDRIAN TEXT?

Zuntz[122] and Royse[123] have proposed separately that corrections of certain readings in $\mathfrak{P}^{46}$ are in the direction of Western to Alexandrian (Royse further argued that there are no corrections to the reverse direction),[124] and the field seems to have been convinced by it.[125] This is a very interesting proposal and deserves a more dedicated study; as such I shall only raise here a methodological question, for future researchers to ponder.

Whilst there is element of truth to this observation (insofar as few correction events are concerned), this presents a methodological problem, too, primarily because both Zuntz and Royse did not inform us precisely how the ones they identified as corrections "toward the Alexandrian text" (or what Royse describes as corrections "from the Western text") quantitatively fare in comparison with readings where $\mathfrak{P}^{46}$ allied itself with "Western" representatives (i.e., DFG) which were *not* corrected. In short, why would a very small set of corrected passages supported by DFG be classified as "the tendency of correction"[126] if more passages supported by the same were *not* corrected even when they are also situated on the same page where corrections of this type have been undertaken?

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[122] Zuntz, *TEDCP*, 254, "In turning to consider those alterations which bear upon textual variation (as distinct from the removal of mere scribal blunders), we note a momentous characteristics of the tradition represented by P^{46}. The material is not very great, but it is unequivocal. At all stages which the papyrus allows us to recover—and they extend over a whole century—we observe the endeavour to move away from such forms of the texts as are attested, in the extant tradition, by Western witnesses (exclusively or with others) and to replace them by 'Alexandrian', and particularly by B-readings."

[123] Royse, *SH-M*, 243: "…there appear to be no examples at all of correction on the opposite direction, i.e., from the Alexandrian text to the Western text. This is particularly striking result since the direction is based on corrections by the scribe, the second hand, and the third hand, and since $\mathfrak{P}^{46}$ does have many readings that are found in D F G. Thus, the tendency of correction is definitely toward the Alexandrian text."

[124] But see his caveat on p. 243, n209.

[125] For instance, quoting Zuntz, Tasker, "Text of the *Corpus Paulinum*," 190, argued, "The pertinent fact about these corrections is their tendency 'to move away from such forms of the text as are attested by Western witnesses and to replace them by Alexandrian and in particular B readings'."

[126] Zuntz, *TEDCP*, 257-58, cited the following as examples: Heb 5.2; 8.6; 10.1; 12.25; 1Cor 10.2; 12.20. On the other hand, Royse, *SM-H*, 243, listed 15 cases exemplifying this "tendency", undertaken by various hands: Heb 3.7; 5.11; 9.8; 10.2; 11.12; 13.22; 1Cor 6.14 (2*x*) 7.17; 13.5; 2Cor 1.19; 6.14; 7.8; Eph 4.15.

Zuntz stated that corrections of this type are "not very great, but it is unequivocal". But how unequivocal is "unequivocal"? It seems instead that the preponderant non-correction of readings agreeing with DFG (or the "Western" text representatives) strongly counts against such a scenario. All things being equal, therefore, what appears to be *tendential* corrections "toward the Alexandrian text" may after all just be cases of coincidental agreement rather than a reflection of *deliberate* scribal preference toward a particular "texttype". This is further corroborated by the indelible fact that there are as many corrections in $\mathfrak{P}^{46}$ that are unambiguously neither toward the "Alexandrian text" nor the "Western text",[127] which seem to disprove the claim that "*the* tendency of correction is *definitely* toward the Alexandrian text".[128]

CONCLUSION

David Parker noted three broad reasons for corrections in Codex Sinaiticus: 1) to correct the mistakes made by the scribe; 2) to change the presentation; and 3) to change the

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Zuntz's mention of 1Cor 10.2 as an example of this textual correction flow "toward the Alexandrian" is a bit puzzling (if not mistaken). In fact, it is more precisely accurate to say that the corrected reading, i.e., εβαπτισαντο supported by BKLP 056 0142 0150 0151, is neither a correction *toward* "Alexandrian" (B is aligned here with the "Byzantine" KLP) nor it is a correction *from* "Western" (DFG in this instance co-support the reading of ℵACΨ, i.e., εβαπτισθησαν). Similarly, νυν<ι> in Heb 8.6 (28^{v}-l^{19}), supported by ℵAD¹KLPΨ 056 075 0142 0150 0151 0278, cannot be a correction "toward Alexandrian" since the original νυν is both supported by D* and B.

[127] The list includes:

Rom	15.23a ($f19^{r}$-l^{09})	εχ<ε>αι<ν>
Heb	1.1 ($f21^{r}$-l^{05})	τοις πατρασιν<ημων> (*cum* $\mathfrak{P}^{12}$)
	6.1 ($f25^{r}$-l^{19})	φερω<ν/ᴹ>εθα
	10.24a ($f32^{v}$-l^{07})	κατανο<ηc>ωμεν
	10.25b ($f32^{v}$-l^{09})	την ἐπὶ‖συναγωγην
	12.4b ($f35^{r}$-l^{10})	αν=‖τικατεστη>C/K<εν
	12.26 ($f36^{r}$-l^{15})	ϊ̈ η φωνη
	13.5a ($f37^{v}$-l^{06})	αρκουμενο>ι/C<
1Cor	8.7a ($f47^{r}$-l^{01})	εσθιουσιẇ
	10.2b ($f49^{r}$-l^{03})	εβαπτιζø<cA>ντο (*cum* BKLP 056 0142 0150 0151)
2Cor	3.11 ($f63^{v}$-l^{21})	το‖ṯo̤ καταργουμενον
Gal	5.17α ($f85^{r}$-l^{25})	το δε <το> πν̄α̅

[128] Royse, *SH-M*, 243. Emphases added.

text to make it conform to a different text.[129] In the case of $\mathfrak{P}^{46}$, we have demonstrated that #1 is definitely a major reason for correction, whilst #2 is not a concern at all! Some have argued that #3 is also a reason for some of the corrections, but as we have demonstrated in the foregoing, this is not necessarily an airtight argument.

None of those who have made corrections in $\mathfrak{P}^{46}$, including our scribe, made a deliberately systematic correction of our codex—they all made alterations only as they noticed them—in the course of their (public) reading regimen for the later "users" and in the course of their temporal reading for the contemporary correctors. Although far from being perfect, our scribe appears to be the "best corrector" of them all, having undertaken corrections not only *inter scribendum*, but also removing potential mistakes and reversing the potential misfortune right there and then. He also exhibits awareness that his work will be "corrected" by someone else; hence, he conspicuously left a few cases where he seemed to have resigned himself to the fate that "the" corrector will deal with them accordingly! But of course, corrections would have been unnecessary if there were no errors in his codex.

Because of the interplay of many hands, however temporally distant from one another, $\mathfrak{P}^{46}$ becomes an even more interesting manuscript that unveils quite graphically the sociology of ancient book production. Most of those who made corrective annotations were later users of this codex, changing a few passages that struck them as erroneous. A contemporary corrector also left his imprint upon our codex, coming into the scene with a commanding presence. Our scribe made most of the corrections—he knew very well that his codex was not perfect, but he nonetheless attempted his best to come up with a manuscript that is acceptable to its would-be end-users.

[129] Parker, *Codex Sinaiticus*, 85-86.

SECTION THREE
PROFANING THE SACRED?
NOMINA SACRA AND THE SCRIBE OF $\mathfrak{P}^{46}$

INTRODUCTION

Disquisitions on this subject since Ludwig Traube enunciated the term *nomina sacra* (*NS*) more than a century ago have largely been on the genesis of the system.[1] No proposal on its origin has yet commanded general assent; discussions continue. The prevalent view, however, is that there were fifteen words that have been ultimately treated as *NS* across the manuscript tradition, and have come in a three-tiered developmental timeframe. This schema results from comparative analyses of surviving manuscripts using mainly the criterion of *referential (in-)consistency* in the application of the system, i.e., sacral or profane. However, this perspective faces a number of methodological problems, as we shall demonstrate in a while.

[1] Ludwig Traube, *Nomina Sacra: Versuch einer Geschichte der christlichen Kürzung* (München: Beck'sche Verlag., 1907); Paap, *Nomina Sacra*; José O'Callaghan, *Nomina Sacra in Papyris Graecis Saeculi III Neotestamentariis* (Rome: Biblical Institute Press, 1970); Schuyler Brown, "Concerning the Origin of the Nomina Sacra," *Studia Papyrologica* 9 (1970): 7-19; Roberts, *Manuscript, Society and Belief*, 26–48. More recent discussions on the subject include Alan Millard, "Ancient Abbreviations and the *Nomina Sacra,*" in *The Unbroken Reed: Studies in the Culture and Heritage of Ancient Egypt, in Honour of A. F. Shore* (eds. C. Eyre, A. & L. M. Leahy; London: Egypt Exploration Society, 1994), 221-26; C.M. Tuckett, "$\mathfrak{P}^{52}$ and Nomina Sacra," *NTS* 47 (2001): 544-48; Idem, "*Nomina Sacra* in Codex E," *JTS* 57 (2006): 487-99; Comfort, *Encountering the Manuscripts*, 199-253; S.D. Charlesworth, "Consensus Standardization in the Systematic Approach to *nomina sacra* in Second- and Third-Century Gospel Manuscripts," *Aegyptus* 86 (2006): 37–68; Idem. "Indicators of 'Catholicity'," 37-42; Don Barker, "P. Lond. Lit. 207 and the Origin of the Nomina Sacra: A Tentative Proposal," *Studia Humaniora Tartuensia* 8 (2007): 1-14; Kenneth Solomon, "Nomina Sacra: Scribal Practice and Piety in Early Christianity" (unpublished paper presented during the 2008 Midwest Regional Meeting of Evangelical Theological Society); Jane Heath, "*Nomina Sacra* and *Sacra Memoria* before the Monastic Age," *JTS* 61/2 (2010): 516-49. Larry Hurtado's 1998 article, "The Origin of the Nomina Sacra: A Proposal," has been recently updated in his *Earliest Christian Artifacts,* 95-134; for bibliographical purposes, the former provides a rather more extensive list of studies on the subject.

My immediate interest in this section is to provide an exhaustive database regarding $\mathfrak{P}^{46}$ and how its scribe most likely understood of the system,[2] not only at the level of the text of his *exemplar* (textual properties), but also at the aesthetic levels (palaeographical and codicological properties), with the intention of discovering his copying habits with regard to individual words treated as "sacred" in particular, and how his habits can contribute to the discussion of the whole system in general. The strategic importance of this kind of study lies in exploring how our scribe used this convention of contraction, especially his use of contractive forms that differ from the ones more prevalent in other manuscripts.[3] This will also explore whether there is indeed any justifiable ground to the claim that the scribe of $\mathfrak{P}^{46}$ had a difficulty in comprehending the system, as some students of $\mathfrak{P}^{46}$ have proposed.

I. RAISING A METHODOLOGICAL PROBLEM

The prevalent view is that there were fifteen words that have been generally treated as *NS* across manuscript tradition,[4] and of this number four were considered the original "core" (κυριος, θεος, ιησους, and χριστος), then followed by another batch (ανθρωπος, πνευμα, σταυρος), and the rest following at a later period (πατηρ, υιος, σωτηρ, μητηρ,

[2] Paap's list contains only the number of occurrences of the *NS* forms in the manuscript he studied. Whilst this is generally helpful, the help it provides for an exhaustive analysis of the *NS* in a particular manuscript is limited, as he did not identify the actual locations where these *NS* appear; for a critique of this aspect, see E.G. Turner, Review of A.H.R.E. Paap, *Nomina Sacra*, *JEA* 46 (1960): 125. To remedy this lack, I have provided Appendix P, tallying the extant occurrences of all the *nomina sacra* found in $\mathfrak{P}^{46}$ as well as those that have been written in *plene*, to provide a more complete picture of how the system was actually used in our manuscript.

[3] That $\mathfrak{P}^{46}$ is a by-product of a single scribe (see pp. 149-52), in contrast with multiple scribal participation in other manuscripts, e.g., codex ℵ or codex D, provides further justification for this study, not to mention that $\mathfrak{P}^{46}$ is one of the most extensive and earliest witnesses to this practice of contraction.

[4] As to the question why only 15 words, Paap, *Nomina Sacra*, 123, argued, "Again the answer is given by the material itself. The meanings of the *nomina sacra* are clearly connected; they are, so to say, technical terms of Christianity and spring from a common spiritual background. This applies to the original group of 4 or 6 contracted words as well as to the system as a whole. That at all times there have been writers and copyists who were aware of this relation between meaning and contraction, appears from the texts where contracted and full forms are used according to the meaning."

ουρανος, ισραηλ, δαυειδ, ιερουσαλημ). This three-tiered developmental timeframe[5] results from comparative analysis of surviving manuscripts, of which the main criterion used is the (in-)consistency in the application of the system, primarily in terms of referents, i.e., whether sacral or non-sacral/profane.

A number of methodological problems, however, can be raised against this perspective, foremost of which is the fact that the manuscript evidence must represent only a fraction of the actual number of manuscripts produced in antiquities. This raises the issue of manuscript survival and how surviving manuscripts are assessed in view of this. Second, whilst the list of manuscript evidence on which Traube and Paap based their analyses is cumulatively impressively large and instructive for statistical purposes, most of these manuscripts are very fragmentary. Such a fragmentary nature makes their witness very tentative, especially if one notes that earlier more extensive manuscripts present more inconsistencies (both in terms of *reference* and *manner of contraction*) in the application of the system than one might wish to suppose. One can easily be carried away by Paap's statistical presentation, as he has a subtle tendency to make it appear that a particular manuscript supports a particular form of a *nomen sacrum*, even though that particular manuscript only provides a singular instance owing to its fragmentary nature.[6] Accordingly, some have even proposed an origin for the system based on reconstructed portions of particular manuscripts.[7] Third, there is also a problem of failing to discuss instances of textual variations in the manuscript evidence, in terms of the scribe's

[5] See Roberts, *Manuscripts, Society and Belief*, 27; and Hurtado, "The Origin of the *NS*," 655-56.

[6] One only needs to scan through Paap's Chapter Four to make this observation evident.

[7] For instance, Christopher Tuckett, "$\mathfrak{P}^{52}$ and Nomina Sacra," 544-48, argued that $\mathfrak{P}^{52}$ may have contained the unabbreviated Ιησουν and not its *nomen sacrum* form, and therefore casting doubts as to the early existence of the system. However, the fact that the portion at issue is broken makes Tuckett's proposal untenable. For a rebuttal of Tuckett, see Charles Hill, "Did the Scribe of $\mathfrak{P}^{52}$ use the *Nomina Sacra*? Another Look," *NTS* 48 (2002): 587-92; and Larry Hurtado, "$\mathfrak{P}^{52}$ (P. Rylands Gk. 457) and *Nomina Sacra*: Method and Probability," *TB* 54 (2003): 1-14. See also Skeat's own calculation of the eroded portion and his conclusion against Tuckett's proposal in Elliot, *Collected Biblical Writings*, xxiii-xxiv.

original reading and subsequent scribal corrections, and how they affect the analysis and discussion of particular instances of *nomina sacra*. The egregious treatment by Paap and Roberts of Heb 9.14, cited rather often in the literature, is a case in point (see discussion below). Finally, we need to revisit the question of definition, i.e., what makes a word (non-)sacral? This is a fluid criterion, and to a certain extent depends subjectively on the interpretational eloquence of the researcher, rather than what the scribes might have really thought about their use of the system in the manuscripts they produced.[8]

To represent the evidence more objectively, it seems methodologically desirable to study the system using primarily the evidence from the more extensive (earlier) manuscripts,[9] in conjunction with their scribal practices, based on palaeographical, codicological, and textual evidences. Whilst comparative analyses of surviving manuscripts are informative, studies on how more extensive manuscripts used the system[10] will significantly enhance the discussion and rectify previous conclusions (as the case may be) on *NS*, especially as one reckons with the observation that the more extensive the manuscript is, the more referential and formal variations it reflects. In conjunction with this, palaeographical and codicological details, not only textual details,

[8] Here the obvious tendency of Comfort to appeal to exegesis is a case in point. But even Paap, *Nomina Sacra*, 5, was also cognizant of this dilemma when he stated, "Occasionally either meaning (i.e., sacral and profane) could be defended."

[9] For recent works on some of the more extensive manuscripts, see Parker, *Codex Bezae*, 97-106; and Jongkind, *Scribal Habits of Codex Sinaiticus*, 61-84. Some studies on a particular book/s of an extensive manuscript include J. Bruce Prior "The Use and Non-use of Nomina Sacra in the Freer Gospel of Matthew," in *The Freer Biblical Manuscripts: Fresh Studies of an American Treasure Trove* (ed. Larry Hurtado; Atlanta: SBL, 2006), 147-66; Tommy Wasserman, *The Epistle of Jude: Its Text and Transmission* (Coniectanea Biblica, New Testament Series 43; Stockholm: Almqvist & Wiksell, 2006), 45-46 [on 1 Peter and Jude of $\mathfrak{P}^{72}$]; and Peter Head, "The Gospel of Mark in Codex Sinaiticus: Textual and Reception-Critical Considerations," *TC* 13 (2008): 1–38, esp. pp. 15-20.

On the other hand, Charlesworth, "Consensus Standardization in the Systematic Approach to *nomina sacra*," 37-68, collectively analysed the system as they are found in the earliest Gospel papyri. Similarly, Anne Marie Luijendijk investigated the use of *nomina sacra* in documentary papyri from Oxyrhynchus; see *Greetings in the Lord: Early Christians and the Oxyrhynchus Papyri* (Harvard Theological Studies 60; Massachusetts: Harvard Divinity School, 2008), esp. 57-78.

[10] It is in this instance that the studies of Parker (codex Bezae) and Jongkind (codex Sinaiticus) become very instructive.

where *NS* appear should be similarly discussed, in order to get a better assessment of the scribal practice of a particular manuscript; the manner of contraction, placement of the overlines, ink used (to determine whether a reading is original or a correction), including lay-outs and other details indicative of scribal copying habits that can help establish the scribe's familiarity or unfamiliarity with the system. Finally, since variation does not only exist in "referential" terms but also in "formal" terms (i.e., forms of contraction), it is therefore imperative to use both the referents *and* contractive forms as the main criteria in studying the system as they were applied in particular manuscripts.

II. MANNER OF CONTRACTIONS IN $\mathfrak{P}^{46}$

Evidence from the manuscript tradition shows that there are basically two major ways of presenting *NS*: *suspension* (retention of the first two letters and dropping the rest, e.g., $\overline{\text{ιη}}$ (for Ιησους) or *contraction*. There are no suspensions in $\mathfrak{P}^{46}$. The manner of contractions employed by our scribe occurs in varying degrees of sophistication, which can be indicative of the various stages of development that each *nomen sacrum* was going through at the time our manuscript was produced. The more stable abbreviated forms include contractions retaining the (1) first and last letters,[11] (2) first two letters and the last,[12] and (3) first two letters and last two letters.[13] The less stable forms include the retention of the (4) first two letters and the last syllable,[14] and (5) first letter and the last

[11] Thus: $\overline{\text{θς}}$ and its derivatives ($\overline{\text{θυ}}$, $\overline{\text{θω}}$, $\overline{\text{θν}}$, $\overline{\text{θε}}$);
$\overline{\text{κς}}$ and its derivatives ($\overline{\text{κυ}}$, $\overline{\text{κω}}$, $\overline{\text{κν}}$, $\overline{\text{κε}}$);
the two instances of $\overline{\text{πρ}}$;
the two-letter contracted form of $\overline{\text{χς}}$ and its derivatives ($\overline{\text{χυ}}$, $\overline{\text{χω}}$, $\overline{\text{χν}}$); and
the two-letter contraction of $\overline{\text{υς}}$ and its derivative forms ($\overline{\text{υω}}$, $\overline{\text{υν}}$).

[12] Thus: the three-letter contracted form of $\overline{\text{χρς}}$ and its derivatives ($\overline{\text{χρυ}}$, $\overline{\text{χρω}}$, $\overline{\text{χρν}}$);
$\overline{\text{ιης}}$ and its derivatives ($\overline{\text{ιηυ}}$, $\overline{\text{ιην}}$);
$\overline{\text{πνα}}$ and some of its derivatives ($\overline{\text{πνι}}$, $\overline{\text{πνς}}$ [for πνευματος and the adjectival/adverbial derivative forms for πνευματικος/ως); and
the three-letter contraction of $\overline{\text{υις}}$ and its derivatives ($\overline{\text{υιυ}}$, $\overline{\text{υιν}}$).

[13] $\overline{\text{παρι}}$; $\overline{\text{πνων}}$; $\overline{\text{στου}}$, $\overline{\text{στρω}}$; $\overline{\text{ανος}}$, $\overline{\text{ανου}}$, $\overline{\text{ανον}}$, $\overline{\text{ανων}}$.

[14] $\overline{\text{πνκος}}$, $\overline{\text{πνκον}}$; $\overline{\text{στρος}}$, $\overline{\text{στρου}}$.

two letters.[15] Quite irregular forms of contraction[16] also occur and they seem to be specific to particular words, or might have arisen due to accidents in copying.[17]

Of the fifteen proposed "standard" *nomina sacra*, only nine were used in the extant pages of $\mathfrak{P}^{46}$. Occurrences in $\mathfrak{P}^{46}$ of the following words are all *plene*: ουρανος,[18] δαυειδ,[19] ισραηλ,[20] ιερουσαλημ,[21] σωτηρ,[22] and μητηρ.[23] Apart from ισραηλ, the first contracted occurrence of which is in Chester Beatty V (c. 2nd century A.D.), contracted forms of these words first occurred well after the production of $\mathfrak{P}^{46}$,[24] which may lend support to the proposal that these *NS* were integrated much later into the system.

It is not infrequent in the literature to portray somewhat suspiciously the scribe of $\mathfrak{P}^{46}$ in the way he handled the system, ranging from his idiosyncrasies or inability to make sense of certain abbreviation. For instance, Haines-Eitzen commented, "When we look closely at the use of the *nomina sacra*, we find, in fact, that scribes during the second

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[15] $\overline{\pi\eta\rho}$, $\overline{\pi\rho\iota}$, $\overline{\pi\rho\alpha}$.

[16] This includes the retention of the
initial + medial + final letters: $\overline{\pi\rho\varsigma}$
first 2 letters + last three letters: $\overline{\alpha\nu o\iota\varsigma}$
first 2 letters + medial letter + 3 final letters: $\overline{\pi\nu\iota\kappa o\nu}$
first 2 letters + medial letter + final letter: $\overline{\sigma\tau\rho\nu}$
first 3 letters + medial letter + final 2 letters: $\overline{\varepsilon\sigma\tau\rho\alpha\nu}$, $\overline{\varepsilon\sigma\tau\rho\theta\eta}$, $\overline{\varepsilon\sigma\tau\rho\alpha\iota}$
first 3 letters + medial letter + final 3 letters: $\varepsilon\overline{\sigma\tau\rho\nu}[\overline{o\nu}]$ (= εσταυρωμενον)
combining preposition + first 2 letters of the root word + medial letter + final 2 letters: συν$\overline{\varepsilon\sigma\tau\rho\alpha\iota}$; and
first 3 letters and final 2 letters: $\overline{\varepsilon\sigma\tau\alpha\nu}$

[17] The contraction ανα$\overline{\sigma\tau\rho\varepsilon\varsigma}$ (combining preposition + first 2 letters of the root word + medial letter + final 2 letters) might have been derived from ανασταυρουντες rather than ανασταυρουντας; see Kenyon, *CBBPIII-1936*, 29.

[18] Ουρανου (Heb 11.12; 1Cor 15.47; 2Cor 12.2); ουρανων (Heb 7.26; 12.5; Eph 4.10); ουρανω (1Cor 8.5); ουρανοις (Heb 8.1; 9.23; 12.23; 2Cor 5.1; Eph 3.15; 6.9; Col 1.20); ουρανον (Rom 10.6; Heb 9.24; 12.26; Col 1.23). Paap, *Nomina Sacra*, 8, mistakenly reported that there is an instance of the nominative plural ουρανοι.

[19] Rom 11.9; Heb 4.7; 11.32.

[20] Rom 9.6a, b, 27a; 10.19, 21; 11.25, 26; Heb 11.22; 1Cor 10.18; 2Cor 3.7; Eph 2.12; Gal 6.16; Phil 3.5.

[21] Ιερουσαλημ (Rom 15.19, 25, 26, 31; Heb 12.22; 1Cor 16.3; Gal 4.25, 26); ιερουσαλυμα (Gal 1.17, 18; 2.1).

[22] Σωτηρ (Eph 5.23); σωτηρα (Phil 3.20?); σωτηριας (Heb 2.10; 5.9; 6.9; 2Cor 1.6; Eph 1.13) σωτηριαν (Rom 10.1; Heb 1.14; 9.28; 11.7; 2Cor 7.10; Phil 1.19); σωτηριου (adj., Eph 6.17).

[23] Μητηρ (Gal 4.26); μητρος (Gal 1.15); μητερα (Rom 16.13; Eph 5.31; 6.2).

[24] $\overline{o\upsilon\nu o\nu}$ in P. 11778 (A.D. 220±); $\overline{\delta\delta}$ in Acta Pauli (A.D. 300±); $\overline{\iota\lambda\eta\mu}$ in Washington Freer MS V (III-IV A.D.); $\overline{\sigma\rho\varsigma}$ in P. Berol. 13415 (IV A.D.); $\overline{\mu\eta\rho}$ in P. Leipzig-LXX (after A.D. 338).

and third centuries are quite idiosyncratic in their application of the *nomina sacra.* Whilst they appear to be aware of a tradition of treating divine names and words in a special way, they do not exhibit standardized and uniform contractions. It is precisely in the earliest period of text transmission that such *inconsistency* in the treatment of the *nomina sacra* is evident."[25] Arguing that these inconsistencies and idiosyncrasies were due to the "private scribal network" who also copied Christian manuscripts (or what she calls scribal "multifunctionality"), she cited a few[26] examples from $\mathfrak{P}^{46}$ and then concluded, "If $\mathfrak{P}^{45}$ demonstrates some inconsistency internally and exhibits some unique *nomina sacra*, with $\mathfrak{P}^{46}$ these traits are found far more frequently... That such inconsistency and idiosyncracy is found among our earliest manuscripts is significant, for it points toward a mode of transmission in which standardization and uniformity was not in existence."[27]

James Royse, on a similar tack, cast doubts on the scribe's comprehension of the whole system. Whilst he was only suspicious of the scribe's comprehension ability in his 1981 dissertation,[28] he nonetheless exuded more confidence in his massive monograph, concluding that his few examples[29] "*confirm* that the scribe (of $\mathfrak{P}^{46}$) has difficulty understanding the abbreviations for *nomina sacra* that stood in his *Vorlage*, and accordingly *often introduces an impossible form*".[30] Royse further stated, "The scribe makes

[25] Haines-Eitzen, *Guardians of the Letters*, 92.

[26] Haines-Eitzen's statement that "(T)he number of *nomina sacra* preserved in $\mathfrak{P}^{46}$ that are unique to this manuscript is *quite large*" (*Guardians of the Letters*, 93; emphases added) is statistically misleading, as we shall demonstrate below.

[27] Haines-Eitzen, *Guardians of the Letters*, 93; see also her reiteration of this point in "The Social History of Early Christian Scribes," in *TNTCR*[2], 479-96, pp. 490-91. Whilst I concur with her general conclusion, I nonetheless disagree with her use and appraisal of $\mathfrak{P}^{46}$ to make her point, as the evidences taken as a whole rather than piecemeal prove otherwise.

[28] Royse, *SH-D*, 248, "The scribe appears to have difficulty understanding the abbreviations for nomina sacra which stood in his *Vorlage*, and accordingly often introduces an impossible form".

[29] It is very important to underscore at this juncture that Royse's generalization stems from analysing only a few examples of *nomina sacra* in $\mathfrak{P}^{46}$, particularly those that are "singular readings"; see our discussion on πνευμα.

[30] Royse, *SH-M*, 259. Emphasis added.

a number of errors that result in nonsense... *Many* of these seem to arise from his faulty understanding of what he is copying, resulting in a *high density* of nonsense in context readings. In particular, he rather *often* errs when he encounters abbreviations of *nomina sacra*."[31]

Whether comments like this are sustainable or not can only be satisfactorily answered if all the evidence is taken as a whole rather than selectively cited, especially if one is to comment on our scribe's practice with regard to his use of the system.

III. THE OVERLINE IN $\mathfrak{P}^{46}$

Apart from contraction, as is equally conventional across the manuscript tradition, a superscript line (a.k.a. supralinear line, overbar, crossbar, etc; henceforth, overline) is *also* placed over the abbreviated word treated as *nomen sacrum*. Studies of *NS* normally make passing comments only on this aspect of the system. But in my opinion, any study on this subject should accord similar importance to this denotative mark as it is equally an *essential component* of the whole system; i.e., despite a very few instances of questionable application of this mark,[32] the system historically developed *with* and not apart from this convention. In fact, across the manuscript tradition, words that have been treated as *nomina sacra* have always been marked *with* the overline. Paap put it best when he said that the overline "serves to focus the attention on the sacral meaning of a word rather than its written form."[33] The practical value of such a device is inescapable, for in manuscripts wherein the characters are written in *scriptio*

[31] Royse, *SH-M*, 358. Emphasis added.

[32] Apart from the overlined $\overline{\text{αιμα}}$ in Heb 9.14 of $\mathfrak{P}^{46}$, Paap, *Nomina Sacra*, 28, 114, 124, also noted the overlines on some fully written out words (*plene*) in other manuscripts, e.g., $\overline{\text{πνευματος}}$, $\overline{\text{αγιου}}$, $\overline{\text{σαρξ}}$, amongst others.

[33] Paap, *Nomina Sacra*, 124. Hurtado, *Earliest Christian Artifacts*, 112, proposed a further importance of the overline, arguing that the overline "maybe a clue to the origin of the *nomina sacra*".

continua contracted letters can be easily confused for something else.[34] Hence, the use of overline to indicate a *nomen sacrum*, as an additional aid, puts the readers (and scribes) on their guard.[35] For this reason, it is indispensable to look at how the scribe of $\mathfrak{P}^{46}$ dealt with this equally essential component of the system.

Overlines in $\mathfrak{P}^{46}$ normally cover all the letters (or more precisely portions of the letters) in the contraction, but sometimes, especially for 3-letter or more contractive forms, the first letter is not included (Fig. 4-3.1). Despite this, this partial exclusion to a large extent does not affect the visual impact of the signal; the overlined contraction can still be easily distinguished as a *nomen sacrum*, e.g., against the line-end final ν or other abbreviations.[36]



Figure 4-3.1
F29^{r}-I^{19}, showing the overline on the last two letters only of the *nomen sacrum* ΠΝϹ.

Not infrequently the overline extends beyond the end of the compendium to accommodate a space sufficient for at least one more character. Note, however, that despite the fact that $\mathfrak{P}^{46}$ is *scriptio continua* there is a conscious effort on the part of the scribe, with a very few exceptions, to leave a space immediately after writing a *nomen*

[34] Roberts, *Manuscripts, Society and Belief*, 26, is not incorrect when he stated that the presence of the overline on the contracted characters is "a warning that the word cannot be pronounced as written".

[35] This is not to suggest that the use of the superscript line in *NS* is chronologically after the convention of contraction (as suggested by Paap, *Nomina Sacra*, 2, 124); I merely intend to emphasize the practical value of this device.

[36] I must here note that three pagination numbers were unusually marked with supralinear strokes: f35^{r} $\overline{\text{ΣΘ}}$ (page 69), f46^{v} $\overline{\text{Η}}$ (page 90), and f52^{r} $\overline{\text{ΡΛ}}$ (page 101). However, these are excluded in our analysis since they belong to another hand and not from our scribe.

sacrum.[37] Hence, where the supralinear line is extended it is almost always the case that there is a vacant space covered by the extended overline (although there are some exemptions to this, as shown in Figs. 4-3.2 and 4-3.3). This clearly indicates that our scribe added the overline immediately after copying the contraction, rather than completing the whole line first and then returning to it to put the overline.



Figure 4-3.2
F19^{r}-I^{02}, showing the overline covering a portion of the preceding letter



Figure 4-3.3
F37^{r}-I^{09}, showing the extended overline covering portions of the first letter of the following word.

In the rare cases of longer contractions, specifically involving compound words with the root σταυρος and its derivative participial form, the scribal pattern is to put the overline on the contracted root form only and leave the combining preposition without the overline, as in Fig. 4-3.4 for the words ανα$\overline{\text{στρες}}$ (=ανασταυρουντες) and συν$\overline{\text{εστραι}}$ (=συνεσταυρωμαι). In this way, only the roots seem to have been reverently treated and not the combining prepositions (i.e., prefixes).

[37] See our discussion in pp. 182-83.

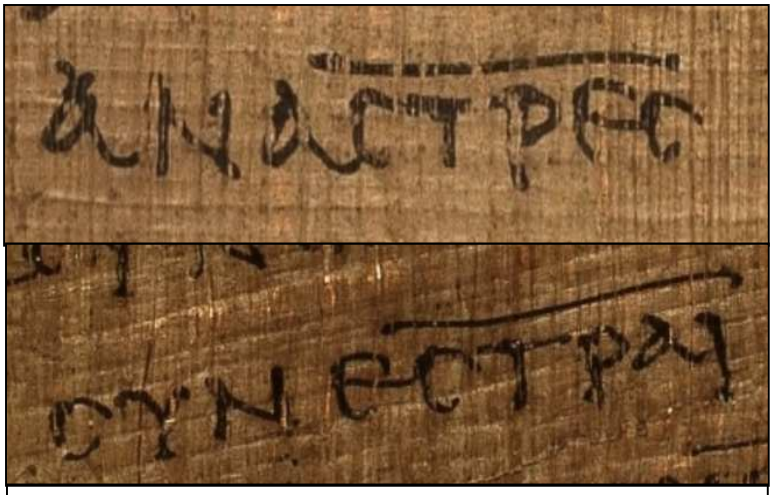


Figure 4-3.4
Compound *nomina sacra*

An interesting form is 1Cor 1.23 where εσ̲τ̲ρ̣ν̣[ο̲ν̲[38] ιουδαιοις] seems to be the most likely reading. The overline is unusually long and can cover at least 6-7 characters (see Fig. 4-3.5), making Kenyon's εσ̲τ̲ν̲ ϊ[ουδαιοις][39] unlikely. *DNTAP*[2.1]'s[40] εσ̲τ̲ρ̣[ω̲μ̲ε̲νο̲ν̲] is equally unlikely since the total number of characters on the line would be comparatively smaller (only 23; previous 10 lines has an average of 26-29).[41]

Figure 4-3.5 F39[v]-I[24], showing the extended overline which can accommodate at least 2-3 characters more.



[38] This form finds support in P. Oxy. III, 406 (A.D. III) where it reads ε̲σ̲τ̲ρ̲ν̲ο̲ς for the participial εσταυρομενος.

[39] Kenyon may have been influenced by the seemingly "double dots" above the broken portion and may have mistaken it to be the diaeresis for the initial *iota* of ιουδιαοις. However, these cannot be the diaeresis since the second "dot" is actually the shadow of the tip-end of a papyrus strip.

[40] *DNTAP*[2.1], 160; this is also the reading of VMR- Muenster.

[41] Comfort and Barrett, *Text of the Earliest*, 251, and Jaroš, *Das Neue Testament*, 1452, both read ε̲σ̲τ̲ρ̣ο̣ν̣ [ιουδαι||ιος].

What the foregoing discussions clearly suggest is that for the scribe of $\mathfrak{P}^{46}$ the overline is an integral part of the *NS* system, and therefore has carefully employed it whenever contractions have been undertaken.[42]

IV. THE EVIDENCE

Because Traube's seminal work on this phenomenon was prior to the discovery and publication of $\mathfrak{P}^{46}$, it is difficult to discern its immediate direct implications for the scribal studies of $\mathfrak{P}^{46}$. But when Paap published his supplementary volume to Traube's, the witness of $\mathfrak{P}^{46}$ figured prominently in his detailed discussion of each of the 15 words treated as *NS*. However, since it is very likely that he isolated the *NS* of $\mathfrak{P}^{46}$ from Kenyon's transcription, statistical differences between his and my own analyses will eventually become apparent.[43] Therefore, a few caveats are in order. First, the data presented here only pertain to the extant portions of the papyrus, where the reading can be clearly established. Second, and as an inevitable consequence of the first, reconstructed readings are excluded in the statistics. The only exemptions here are if a reasonable portion of the contracted word has been preserved and the reading can be justifiably established. Third, since I am primarily interested in the habits of the scribe, I only included the first-hand reading, rather than the corrected reading by a later hand. Having said that, we are now ready to proceed with the evidence proper on how the scribe of $\mathfrak{P}^{46}$ used the intricate system of *nomina sacra*. Each *nomen sacrum* is presented according to their degree of

[42] As we noted in pp. 281-82, the lone instance where our scribe left a contraction without an overline is 1Cor 1.2. ($f38^{v}$-l^{22}). Apart from this, the scribe was very consistent in the employment of the overline throughout the codex where a *nomen sacrum* appears.

[43] The fact that Paap's analysis relied mainly on Kenyon's transcription presents the need to itemise the actual occurrences of the *NS* in $\mathfrak{P}^{46}$, since the mis-transcriptions in Kenyon might have also been carried over by Paap into his analysis. For instance, Paap may have included 1Cor 1.31 in his list of contracted $\overline{\theta\upsilon}$ since this is Kenyon's transcription. However, a closer look at the plate shows that the correct transcription should have been $\overline{\kappa\omega}$.

referential and formal stability, with the first ones as more stable and the latter ones as less stable.

1. **ΘΕΟC**

CONTRACTION PROFILE

SACRAL		NON-SACRAL	
Nomina Sacra	***Plene***	***Nomina Sacra***	***Plene***
359	0	1	3 (plurals)

All the extant instances of the singular θεος and its cognate cases are contracted, with appropriate overlines above them, and the 2-letter contraction has been applied with exceptional regularity throughout (360/360). The manner of contraction is also very stable, retaining only the first and the last letters, depending on the grammatical cases. Except in only one instance, all refer either to the first or second member of the divine Trinity. Phil 3.19, the lone anomaly with a profane referent ("their *god* is their belly"), might have been a simple case of oversight,[44] or an indication of "universal contraction". That is, at this stage of manuscript transmission history there is already a general recognition that all occurrences of the singular θεος had to be treated, at the first instance (unless otherwise clearly having a profane referent), as *nomen sacrum*, and that our scribe also undoubtedly exhibited such recognition.[45] This is further corroborated by the observation that in $\mathfrak{P}^{46}$ the plural form is always

[44] Amongst the NT papyri with the Philippian text, only $\mathfrak{P}^{46}$ is extant at this point. (Interestingly, amongst the earliest majuscules, codices ℵ, A, and B have the contracted form here also). However, the other occurrence of a non-sacral context, 2Cor 4.4 ("the *god* of this world"), is not extant in $\mathfrak{P}^{46}$. At any rate, despite this lone anomaly, the scribe still exhibits a very high level of consciousness as to the reverential value attached to this word.

[45] That our scribe's *exemplar* already reflects universal contraction for θεος is not contrary to this observation.

written out in *plene* (θεοι [1Cor 8.5a ("so-called *gods*"), 5b ("many *gods*")]; and θεοις [Gal 4.8 ("not *gods*")]),[46] presumably influenced by their monotheistic concept of God.

A hint as to the widespread recognition of the convention can also be derived from the way a corrector of this manuscript executed his task. For instance, in Heb 9.20, the subject ο θεος[47] within an OT quotation is only *implicit* in the text of $\mathfrak{P}^{46}$, hence, a contemporary corrector[48] inserted the "missing" subject in its contracted form with the overline, i.e., ο $\overline{\theta\varsigma}$, which points to the fact that even in instances of corrections the "rules" still apply to legitimate *NS* words.

2. ΚΥΡΙΟC

CONTRACTION PROFILE

SACRAL		NON-SACRAL	
Nomina Sacra	***Plene***	***Nomina Sacra***	***Plene***
169	0	1	4 (plurals)

Arguing for catholicity in the early gospel manuscripts, Scott Charlesworth asserted that in the earlier era of gospel manuscript production the question of whether to contract or not to contract κυριος "presented an interpretive problem".[49] Whether this is sustainable or not,[50] this clearly does not hold true for our codex.

All extant occurrences of the singular κυριος and its cognate cases are consistently contracted (170/170), with corresponding overlines, after the 2-letter

[46] None of the papyri with 1Cor and Gal texts are extant in these three instances. But in codices ℵ, A, and B, all these occurrences of the plural (where $\mathfrak{P}^{46}$ is extant) are also written in *plene*, further suggesting the same recognition of the use of the system in regard to non-sacral referents.

[47] Although in Alfred Rahlfs (ed.), *Septuaginta* (Editio altera/Revised and corrected edition by Robert Hanhart; German Bible Society, 2006), the subject is κυριος and not θεος.

[48] The corrector is definitely not the first hand; the heavy black ink used rather points to our M^2 corrector.

[49] Scott Charlesworth, "Indicators of 'Catholicity' in Early Gospel Manuscripts," in *ETNT*, 37-48, p.39.

[50] That ancient scribes applied "interpretive judgment" in whether or not to employ contraction is not limited to κυριος but to all the *NS* words For a critique of Charlesworth's proposal, see Kim Haines-Eitzen, "The Social History of Early Christian Scribes," 479-95, p. 491, where she described Charlesworth's proposal as "especially forced".

format.[51] There seems to be a conscious effort on the part of the scribe to strictly confine the sacral designation to Jesus Christ or the Divine Name,[52] since except for Rom 14.4,[53] all extant occurrences have sacral referents, i.e., mostly Jesus is the referent but not infrequently the "Lord" within OT quotations. Conversely, the plural forms are all fully written out (κυριοι [1Cor 8.5; Eph 6.9]; κυριοις [Eph 6.5; Col 3.22]). Additionally, three singular vocatives (κυριε) appear in the Pauline Epistles and Hebrews, all within OT quotations, but only two are extant in $\mathfrak{P}^{46}$ (Rom 10.16 and Heb 1.10); both are rightly contracted, contextually referring to the "Lord" (Heb. יְהוָה).

Interestingly, how our scribe understood the distinction between the "Lordship" of Jesus as opposed to other "lords" (or "masters") is best illustrated in Eph 6.8-9 where he deliberately reflected the distinction by contracting κυριος when reference is made of Jesus Christ and retaining the *plene* form when referent is non-sacral (Fig. 4-3.6).

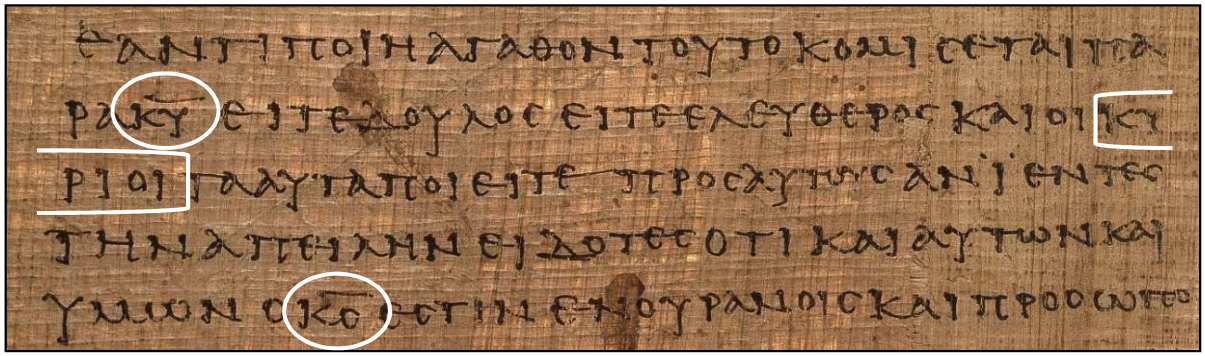


Fig. 4-3.6 F8o^{v}-II$^{01\text{-}05}$, showing the two *nomen sacrum* forms for the singular whilst the plural is in *plene.*

[51] Contraction of κυριος is almost always to the 2-letter format across the manuscript tradition. In fact, in Paap's list (*Nomina Sacra*, 23, 79) only MS # 79 (=P. Rain. II. 59) reflects 3-letter contractions (first two letters + final letter), i.e., $\overline{\text{κρς}}$ (3x) and $\overline{\text{κρυ}}$ (once).

[52] Also noted by Paap, *Nomina Sacra*, 101.

[53] Others have argued that the passage can be interpreted both ways, as can be seen in modern English translations such as Contemporary English Version, Good News Bible, or the New English Bible to name a few. Comfort, *Encountering the Manuscripts*, 216, following similar tact, argued that "the scribe of $\mathfrak{P}^{46}$ made both occurrences of *kurios* nomina sacra, thereby indicating his interpretation that the passage speaks about every Christian's relationship to their Lord".

The same careful distinction is also reflected in Eph 6.4-5, wherein biological fathers, in verse 4, are admonished to bring up their children according to the discipline and instruction of the "*Lord*" (contracted), and then slaves are addressed in the following verse to be obedient to their "earthly masters" (uncontracted); see Fig. 4-3.7.

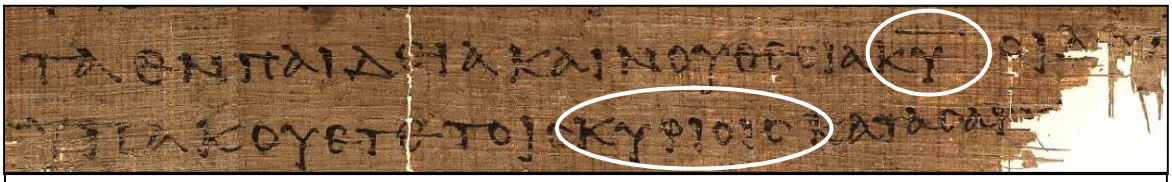


Figure 4-3.7
F8o^{r}-ll$^{25-26}$, showing the *nomen sacrum* for the singular and the plural written out in *plene.*

3. ΙΗCOΥC

CONTRACTION PROFILE

SACRAL		NON-SACRAL	
Nomina Sacra	***Plene***	***Nomina Sacra***	***Plene***
107	0	2	0

Like κυριος, abbreviation for Ιησους is consistently applied, using the 3-letter format; the 2-letter suspension (ιη), used in a few contemporary manuscripts,[54] was never employed in 𝔓^{46}. Except in two instances,[55] all have Jesus Christ as the referent. Paap noted that ιης (χρς) in Heb 13.21 is dative in function despite its nominative form.[56] This is only partly true; it is more likely that in context it has a genitival function, hence, consequently stimulating scribal correction toward the genitive, i.e., ιηυ χρυ.[57]

[54] For an example of this suspended ιη for Jesus, see Bell and Skeat, *Fragments of Unknown Gospel*, 3. Other manuscripts noted by Paap include 𝔓^{45} + P. Vindob Gr. 31974, P. Dura 10, P. Oxy. 17.2070, P. Oxy. 8. 1079, and P. Oxy. 10.122.

[55] Col 4.11 ("*Jesus* the one called Justus") and Heb 4.8 (referring to "*Joshua*"). Surprisingly, Hurtado, *The Earliest Christian Artifacts*, 129, erroneously reported that these (as well as 2Cor 11.4 ["another *Jesus*"]) were written *plene* in 𝔓^{46}.

[56] Paap, *Nomina Sacra*, 9.

[57] Although the corrector (our M^{2}) also missed rectifying the immediately following relative pronoun ω to the genitive ου.

Viewed against the backdrop of other surviving papyri witnessing to the text of Pauline Epistles and Hebrews, $\mathfrak{P}^{46}$ shares only with two other papyri ($\mathfrak{P}^{30}$ and $\mathfrak{P}^{65}$ [both from the third century]) in equally exhibiting preference for the 3-letter compendium. In contrast, the majority of them prefer the shorter form, although they mostly represent a production timeframe after the 3rd century.[58] However, outside the orbit of the Pauline Epistles and Hebrews, other contemporary papyri with extant instances of Ιησους exhibit either the 3-letter contraction strictly (e.g., $\mathfrak{P}^{38}$ [3rd, 3/3]) or the 2-letter (e.g., $\mathfrak{P}^{66}$ [c. 200, 157/157]) or combination of both forms (e.g., $\mathfrak{P}^{45}$ [early 3rd, 36 (2-letter) and 2 (3-letter)]). This shows that when $\mathfrak{P}^{46}$ was produced both contractive forms were already in currency, and since either form does not diminish the message the word at issue intends to convey the choice of a contraction form in a manuscript may have been left variably to the preference of the scribe.

As in θεος and κυριος, our scribe also exhibits a deliberate effort to contract Ιησους every time it occurs (109/109). His *exemplar* might have contained this pattern already which he willingly adopted. This becomes evident when we consider that despite the fact that the non-sacral referents of the two exemptions are easily distinguishable in context, the scribe did not write them in *plene.* This practice of contraction is not without historical significance for the development of the system insofar as the name "Jesus" is concerned.

In the case of Heb 4.8, where the reference is clearly to the OT "Joshua" than to "Jesus", Peter Head suggested that the contraction of ιησους to its usual *nomen sacrum* form might indicate that the users of $\mathfrak{P}^{46}$ were "interpreting the passage in terms of

[58] $\mathfrak{P}^{11}$ (4th century), $\mathfrak{P}^{13}$ (3rd-4th), $\mathfrak{P}^{16}$ (3rd-4th), $\mathfrak{P}^{49}$ (3rd), $\mathfrak{P}^{51}$ (c. 400), $\mathfrak{P}^{61}$ (c. 700), $\mathfrak{P}^{68}$ (7th), and $\mathfrak{P}^{79}$ (7th).

'Jesus' rather than 'Joshua'".[59] On the other hand, Hurtado argued that this contraction "has to be seen in the context of early Christian belief that, as a divinely ordained prophetic move, this figure had been given the name of God's Son".[60] Whilst these proposals are both plausible, it seems likely though, in view of the fact that all the occurrences of ιησους have been contracted throughout regardless of their referents, that the contraction of "Joshua" to $\overline{\text{ιης}}$[61] was directly more in line with our scribe's over-all practice of contracting ιησους at the first instance than a product of his own interpretative decision.[62] This observation lends further credence to the kind of stability this particular *nomen sacrum* has already attained by the time our manuscript was produced. Hence, it is not unreasonable to concur with Roberts when he asserted that due to the prevalence of writing ιησους as a *nomen sacrum* during the second half of the 2nd century it became "second nature to the scribe(s)" to contract ιησους every time it occurs.[63]

4. ΧΡΙCΤΟC

CONTRACTION PROFILE

SACRAL		NON-SACRAL	
Nomina Sacra	***Plene***	***Nomina Sacra***	***Plene***
252	0	0	0

[59] Peter Head, "'If Jesus had given them rest' Heb 4.8 in $\mathfrak{P}^{46}$," in http://evangelicaltextualcriticism.blogspot.com/2010/06/if-jesus-had-given-them-rest-heb-48-in.html; (site accessed 15 Nov 2010); see also similar suggestion by Comfort, *Encountering the Manuscripts*, 222.

[60] Hurtado, *Earliest Christian Artifacts*, 126, n94.

[61] Other relatively early MSS reflecting this compendium here include $\mathfrak{P}^{13}$, Sinaiticus, Vaticanus, etc. Note, however, that Sinaiticus and Vaticanus reflect the *plene* in Col. 4.11; only Hebrews is extant in $\mathfrak{P}^{13}$.

[62] Of course, it is also possible that the universal contraction is *exemplaric*; even then that possibility does not cancel out this observation.

[63] Roberts, *Manuscript, Society and Belief*, 37. Decades earlier, this practice had been observed already in the Chester Beatty biblical papyri by Frederic Kenyon, "*Nomina Sacra* in the Chester Beatty Papyri," *Aegyptus* 13/1 (1933): 5-10, p.6, "This (contraction of Joshua) is only conceivable after the contraction of the *nomen sacrum* Ἰησους had become so well established that it was natural for a scribe to use it even when the name was not that of Jesus Christ but of Joshua, and it is surprising to find this extension of the use so early as the 2nd century."

All extant occurrences of χριστος and its cognate cases are contracted with corresponding overlines above them, and all refer to Jesus Christ. That all the occurrences are contracted is not surprising, since there is nothing in the Pauline Epistles and Hebrews where the messianic title χριστος was ever used in reference to entities other than Jesus. However, it is noteworthy that both the 3-letter and 2-letter contractions for χριστος appear in $\mathfrak{P}^{46}$; there are no instances of *christogram.* In all the grammatical cases, the 3-letter form has higher usage frequency than the 2-letter (202 > 50).[64] Except in 1Thess (due to its highly fragmentary state), both forms of abbreviation appear in all books, but always dominated by the 3-letter form (the only exception is in 2Cor where the dative $\overline{\chi\omega}$ was used more than its 3-letter counterpart [5 > 1]), especially in 1Cor where the discrepancy is at the highest (48 vs 7). In fact, the genitive $\overline{\chi\upsilon}$ was used only once in this book as against 24 for $\overline{\chi\rho\upsilon}$.

TABLE 4-C1
ⲭⲥ-ⲭⲣⲥ Distribution Table Per Book

	$\overline{\chi\varsigma}$	$\overline{\chi\rho\varsigma}$	$\overline{\chi\upsilon}$	$\overline{\chi\rho\upsilon}$	$\overline{\chi\omega}$	$\overline{\chi\rho\omega}$	$\overline{\chi\nu}$	$\overline{\chi\rho\nu}$	2-Letter Totals per Book	3-Letter Totals per Book
ROM	2	4	1	9	1	9	4	2	8	24
HEB	1	8	0	3	0	0	0	0	1	11
1COR	3	12	1	24	2	9	1	3	7	48
2COR	0	2	6	22	5	1	0	4	11	29
EPH	1	3	2	16	1	11	1	3	5	33
GAL	2	3	5	12	1	4	1	3	9	22
PHIL	2	2	2	10	2	7	1	0	7	19
COL	1	1	0	10	1	3	0	2	2	16
1THESS	0	0	0	0	0	0	0	0	0	0
GRAND TOTALS	**12**	**35**	**17**	**106**	**13**	**44**	**8**	**17**	**50**	**202**

[64] Paap, *Nomina Sacra*, 9, 94, noted 247 instances only, with the 3-letter dominating (199>48). On the other hand, Comfort, *Encountering the Manuscripts*, 223, noted 242 cases only, with the 3-letter also dominating (196>46). We have no way of checking their figures since they did not provide any catalogue of occurrences.

The extent of seemingly indiscriminate formal variation is best illustrated by looking at how contractions were configured in terms of distance from one form to another. We can do this on two levels: 1) between similar grammatical cases (e.g., nominative vs nominative), and 2) between different forms (e.g., 2-letter nominative vs 3-letter form in any case).

On the first level, between similar grammatical forms, there are formal variations in six folios where the distance between two abbreviations involves at least 6-15 lines.[65] Visually, within that distance the variation would have already been easily spotted. But it is even easier within 1-5 lines difference, which is exactly the case in 12 folios.[66] In f83^{v}, involving the accusative χριστον in Gal 3.27a and 27b, both forms were used even though they are only one line apart, separated by only one word!

On the second level, between different grammatical forms, we see formal variations happening within distances as close as two lines[67] and single lines.[68] F83^{v},

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[65] These folios include:
F89^{r} Phil 3.12 ($\overline{\chi\upsilon}$) and 18 ($\overline{\chi\rho\upsilon}$); 13 lines apart;
F64^{v} 2Cor 4.6 ($\overline{\chi\rho\upsilon}$) and 10 ($\overline{\chi\upsilon}$); 11 lines apart;
F87^{v} Phil 2.1 ($\overline{\chi\omega}$) and 5 ($\overline{\chi\rho\omega}$); 10 lines apart;
F83^{v} Gal 3.22 ($\overline{\chi\upsilon}$) and 26 ($\overline{\chi\rho\upsilon}$); 7 lines apart;
F86^{v} Phil 1.6 ($\overline{\chi\upsilon}$) and 8 ($\overline{\chi\rho\upsilon}$); 7 lines apart; and
F13^{r} Rom 10.6, 7 ($\overline{\chi\nu}$) and 9 ($\overline{\chi\rho\nu}$); 5-6 lines apart.

[66] Thus:
F51^{v} 1Cor 11.1 ($\overline{\chi\rho\upsilon}$) and 3 ($\overline{\chi\upsilon}$); 5 lines apart;
F78^{r} Eph 4.12 ($\overline{\chi\rho\upsilon}$) and 13 ($\overline{\chi\upsilon}$); 5 lines apart;
F57^{v} 1Cor 15.13, 14 ($\overline{\chi\varsigma}$) and 12 ($\overline{\chi\rho\varsigma}$); 4-5 lines apart;
F82^{v} Gal 2.20 ($\overline{\chi\varsigma}$) and 21 ($\overline{\chi\rho\varsigma}$); 4 lines apart;
F76^{r} Eph 2.6 ($\overline{\chi\rho\omega}$) and 7 ($\overline{\chi\omega}$); 3 lines apart;
F87^{r} Phil 1.20 ($\overline{\chi\varsigma}$) and 21 ($\overline{\chi\rho\varsigma}$); 2 lines apart;
F79^{v} Eph 5.20 ($\overline{\chi\upsilon}$) and 21 ($\overline{\chi\rho\upsilon}$); 2 lines apart;
F58^{r} 1Cor 1.18 ($\overline{\chi\omega}$) and 19 ($\overline{\chi\rho\omega}$); 2 lines apart;
F92^{v} Col 3.1 ($\overline{\chi\omega}$) and 3 ($\overline{\chi\rho\omega}$); 2 lines apart;
F82^{v} Gal 2.16a ($\overline{\chi\rho\upsilon}$) and 16b ($\overline{\chi\upsilon}$); 2 lines apart but of the same verse;
F87^{r} Phil 1.23a (first hand $\overline{\chi\omega}$) and 23b ($\overline{\chi\rho\omega}$); 2 lines apart but of the same verse; and
F83^{v} Gal 3.27a ($\overline{\chi\rho\nu}$) and 27b ($\overline{\chi\nu}$); 1 line apart, same verse.

[67] Phil 3.7 ($\overline{\chi\nu}$) and 8 ($\overline{\chi\rho\upsilon}$); Gal 3.27 ($\overline{\chi\nu}$) and 28 ($\overline{\chi\rho\upsilon}$).

[68] Some instances include,
F66^{r} 2Cor 5.16 $\overline{\chi\rho\nu}$ and 17 $\overline{\chi\omega}$;
F82^{v} Gal 2.16 ($\overline{\chi\rho\nu}$) and ($\overline{\chi\upsilon}$); 6 words apart;

involving Gal 3.26, 27, is interesting as the forms $\overline{\chi\rho\upsilon}$ and $\overline{\chi\nu}$ are almost directly parallel! But even more interesting is f82^{v} (Gal 2.17) where the forms $\overline{\chi\rho\omega}$ and $\overline{\chi\varsigma}$ are within the same line, separated only by six words!

The picture becomes more complex when one discovers that the name ιησους (which frequently appears in tandem with χριστος) was uniformly contracted to the 3-letter format throughout our codex. When we scan through the combination, however, we do get a glimpse of the scribe's apparent predilection for the 3-letter format, for the ratio is decidedly lopsided in favour of the longer form. That is, when χριστος precedes Ιησους, the scribe preferred χριστος in the 3-letter format 33 times out of 38 extant occurrences.[69] Yet even when Ιησους comes first, the 3-letter format is still the preferred contraction form, 40 times out of 43.[70]

On the basis of the synchronic occurrences of both forms, Sanders contested Kenyon's early third century dating,[71] arguing that the 3-letter format is the earlier form which was then indiscriminately used with the 2-letter form in the fourth century until the former faded practically out of use in the fifth century.[72] Such a suggestion, however, seems untenable in view of the fact that the 3-letter form has survived in other contemporary or slightly later manuscripts, either strictly in one

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F82^{v} Gal 2.20 ($\overline{\chi\varsigma}$) and ($\overline{\chi\rho\upsilon}$); 14 words apart;

F83^{v} Gal 3.26 ($\overline{\chi\rho\upsilon}$) and 27b ($\overline{\chi\nu}$); 7 words apart, almost directly parallel to each other.

[69] The five in 2-letter format include 1Cor 1.30 ($\overline{\chi\omega}$ $\overline{\iota\eta\upsilon}$); Eph 2.7 ($\overline{\chi\omega}$ ι$\overline{\eta}$[υ]); Gal 3.14 ($\overline{\chi\omega}$ $\overline{\iota\eta\upsilon}$); Phil 1.6 ($\overline{\chi\upsilon}$ $\overline{\iota\eta\upsilon}$); and 3.12 ($\overline{\chi\upsilon}$ $\overline{\iota\eta\upsilon}$).

[70] Only 2Cor 4.10 ($\overline{\iota\eta\upsilon}$ $\overline{\chi\upsilon}$), Gal 3.22 ($\overline{\iota\eta\upsilon}$ $\overline{\chi\upsilon}$) and Eph 5.20 ($\overline{\kappa\upsilon}$ ημων $\overline{\iota\eta\upsilon}$ $\overline{\chi\upsilon}$ τω $\overline{\pi\rho\iota}$ και $\overline{\theta\omega}$) have the 2-letter format.

[71] It is noteworthy that even in matters of manuscript dating, the *NS* system is also appealed to in support of a particular date. For instance, T.C. Skeat, as quoted by Metzger, *Text of the New Testament*, 265-66, lent support to Kenyon's proposed early third century dating by arguing that "Moreover, $\mathfrak{P}^{46}$ uses an extensive and well developed system of *nomina sacra*, which it is difficult to believe can have existed, not merely in A.D. 80, but presumably in one of its ancestors." Skeat was reacting to Young Kyu Kim, "Palaeographical Dating of $\mathfrak{P}^{46}$," 257, who argued for 80 A.D. date contending that the *NS* system (amongst others) was already in use in contemporary and even earlier documents as that of $\mathfrak{P}^{46}$. For a critique of both positions, see Griffin, "The Palaeographical Dating of $\mathfrak{P}^{46}$"; and Pickering, "Dating of the Chester Beatty-Michigan Codex," 216-27. See also our discussion on the date of $\mathfrak{P}^{46}$ in pp. 138-48.

[72] Sanders, *TCPC*, 16.

form (e.g., $\mathfrak{P}^{38}$ [3rd-4th], $\mathfrak{P}^{72}$ [3rd-4th],[73] $\mathfrak{P}^{78}$ [3rd-4th], D [5th],[74] 0232 [4th-5th?], etc) or in combination with the 2-letter contraction (e.g., ℵ [4th],[75] W [4th-5th],[76] etc).

More specifically, viewed against the backdrop of other surviving papyri witnessing to the texts of Pauline Epistles and Hebrews,[77] the presence of both contractive forms in $\mathfrak{P}^{46}$ is shared by no other manuscript. The surviving papyri either strictly follow the 2-letter[78] or the 3-letter format.[79] Of course, no definitive conclusions can be derived from this observation as it represents only a fraction of the total manuscript evidence. Be that as it may, this serves to highlight the point that the use of both forms in $\mathfrak{P}^{46}$ is suggestive of a kind of "formal" transitory stage in treating the word χριστος in the development of the system of *nomina sacra*. During this stage of transition the reflected forms of contraction at various instances seem to be more of an expression of scribal preference than a deficiency in the system. But what we see unfolding in $\mathfrak{P}^{46}$ is not a totally pattern-less expression of scribal proclivity. Whilst it is admittedly difficult to establish the contraction pattern when χριστος is standing alone, we have seen that when used in tandem with Ιησους the pattern of contraction becomes more predictable.

5. ϹΤΑΥΡΟϹ

CONTRACTION PROFILE

SACRAL		NON-SACRAL	
Nomina Sacra	***Plene***	***Nomina Sacra***	***Plene***
18	1	0	0

[73] For $\mathfrak{P}^{72}$ and $\mathfrak{P}^{78}$, see their transcriptions in Tommy Wasserman, *Epistle of Jude*, n.p.
[74] See Parker, *Codex Bezae*, 100.
[75] See Jongkind, *Scribal Habits,* 64.
[76] See Paap, *Nomina Sacra*, 49, 51.
[77] Data derived mainly from *DNTAP*[2.1-2].
[78] For instance, $\mathfrak{P}^{11}$ (4th century), $\mathfrak{P}^{13}$ (3rd-4th), $\mathfrak{P}^{15}$ (3rd), $\mathfrak{P}^{16}$ (3rd-4th), $\mathfrak{P}^{27}$ (3rd), $\mathfrak{P}^{31}$ (7th), $\mathfrak{P}^{32}$ (c.200), $\mathfrak{P}^{34}$ (7th), $\mathfrak{P}^{49}$ (3rd), $\mathfrak{P}^{61}$ (c.700), $\mathfrak{P}^{68}$ (7th), $\mathfrak{P}^{79}$ (7th), and $\mathfrak{P}^{94}$ (5th-6th).
[79] For instance, $\mathfrak{P}^{51}$ (c.400), $\mathfrak{P}^{65}$ (3rd), and $\mathfrak{P}^{92}$ (3rd-4th).

$\mathfrak{P}^{46}$ provides the most extensive contraction samples for σταυρος/ω across the earliest manuscript tradition. Impressively, except in Rom 6.6 ([συνεσταυ]ρωθη),[80] all 18 extant occurrences of the noun and its verbal derivatives are contracted. There are no instances where the *staurogram* was used.[81] In fact, it is likely that the scribe was unacquainted with the *staurogram* since in two instances the *rho* was dropped in the contraction (Col 1.20 $\overline{\sigma\tau\upsilon}$ "blood of his *Cross*"; Gal 5.24 $\overline{\varepsilon\sigma\tau\alpha\nu}$ "*crucified* the flesh"). The abbreviation is generally consistent retaining the σ, τ, and ρ, changing affixes according to grammatical conjugations.

In 13 instances the reference is explicitly to the "*cross* of Christ"[82] or to acts pertaining to Christ's *crucifixion*, hence, with sacral implications.[83] In five instances, whilst the referents are not necessarily non-sacral, the contractions were situated in sort of negative contexts: Heb 6.6 "they are *crucifying* [$\alpha\nu\alpha\overline{\sigma\tau\rho\varepsilon\varsigma}$] the Son of God"; Gal 5.11 "stumbling block of the *Cross* [$\overline{\sigma\tau\rho\upsilon}$]"; Phil 3.18 "enemies of the *Cross* [$\overline{\sigma\tau\rho\upsilon}$]"; 1Cor 1.13 "was Paul *crucified* ([$\overline{\varepsilon\sigma\tau\rho}$]θη)"; Gal 5.24 "*crucified* [$\overline{\varepsilon\sigma\tau\alpha\nu}$] the flesh".

There are three instances of the compound but only two are contracted, and as noted above compounds are contracted only in the root leaving the affixes

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[80] Comfort, *Encountering the Manuscripts*, 249, claimed that "… the scribe of $\mathfrak{P}^{46}$ *always* made (*staurōmai*) a nomen sacrum". (Emphasis added). However, Rom 6.6 disproves this claim, where despite being lacunose at this portion of the page (fo8^{r}-l^{06}) it is most likely that the aorist passive was written *plene* on the line, i.e., [ανθρωπος συνεσταυ]ρωθη ϊνα καταρ||. Had it been written as a *nomen sacrum* the line would be comparatively shorter than the rest on the page. Surprisingly, in his transcription (with Barrett), Comfort transcribed the verb in *plene*; see Comfort and Barrett, *Text of the Earliest NT*, 209.

[81] Paap, *Nomina Sacra*, 98, listed at least five manuscripts exhibiting the *tau-rho* compendium. For a more extensive recent discussion on this subject, see Larry Hurtado, "The Staurogram in Early Christian Manuscripts: The Earliest Visual Reference to the Crucified Jesus?" in *New Testament Manuscripts: Their Text and Their World* (ed. T.J. Kraus and T. Nicklas; *TENTS* 2; Boston/Leiden: Brill, 2006), 207-26; also, for the few examples of *staurogram* in the text and *coronis* of ℵ, see Jongkind, *Scribal Habits of Codex Sinaiticus*, 67, n26.

[82] 1Cor 1.17 ("*cross* [$\overline{\sigma\tau\rho\varsigma}$] of Christ"), 18 ("*cross* [$\overline{\sigma\tau\rho\upsilon}$]"); Eph 2.16 ("through the *cross* [$\overline{\sigma\tau\rho\upsilon}$]"); Gal 6.12 ("to the *cross* of Christ"), 14a ("to the *cross* of our Lord"); Col 1.20 ("by the blood of his *cross* [$\overline{\sigma\tau\upsilon}$]"); 2.14 ("he [God] nailed it to the *cross* [$\overline{\sigma\tau\rho\omega}$]").

[83] Heb 12.2 ("Jesus endured the *cross* [$\overline{\sigma\tau\rho\nu}$]"); 1Cor 2.8 ("they would not have *crucified* [$\overline{\varepsilon\sigma\tau\rho\alpha\nu}$] the Lord of Glory"); Eph 1.23 ("we preach Christ *crucified* $\overline{\varepsilon\sigma\tau\rho\alpha}$[$\overline{\nu\sigma\nu}$]"); Gal 2.19 ("I have been *crucified* [$\sigma\upsilon\nu\overline{\sigma\tau\rho\alpha\iota}$] with Christ"); 6.14b ("to me the world has been *crucified* [$\overline{\varepsilon\sigma\tau\rho\alpha\iota}$]"); Phil 2.8 ("death on the *cross* [$\overline{\sigma\tau\rho\upsilon}$]").

unchanged, with the corresponding overlines placed above the contracted root only[84] (Gal 2.19 συνε̅σ̅τ̅ραι = συνεσταυρωμαι and Heb 6.6 αναα̅σ̅τ̅ρες = αναστανρουντες).[85] In this regard, it appears that this is another of $\mathfrak{P}^{46}$'s idiosyncrasies.

6. ΥΙΟC

CONTRACTION PROFILE

SACRAL		NON-SACRAL	
Nomina Sacra	***Plene***	***Nomina Sacra***	***Plene***
21	2	0	21

The number of contracted υιος is comparatively smaller than χριστος. But it is nonetheless worth noting since the earliest surviving occurrence of this word as a *nomen sacrum* is in $\mathfrak{P}^{46}$.[86] Paap noted that with regards to the papyrus witnesses he examined, υιος is "seldom contracted".[87] However, this is not the case with $\mathfrak{P}^{46}$.

Υιος has contracted forms only in the singular, explicitly or implicitly referring mostly to Jesus Christ;[88] but not all the singulars are contracted.[89] Plurals are always fully written out.[90] As with χριστος, contraction pattern for υιος vary, with the 3-letter format dominating the shorter form (15 > 6). In Hebrews, where most of the contraction occurrences are extant at ten, the pattern is split (5 vs 5); the only other instance we see the 2-letter format is in Gal. 1.16.[91]

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[84] Also noted by Comfort, *Encountering the Manuscripts*, 249.

[85] Interestingly, in ℵ, A, and B both συνεσταυρωμαι and ανασταυρουντ(ε/α)ς were written in *plene.*

[86] Paap, *Nomina Sacra*, 119.

[87] Paap, *Nomina Sacra*, 110.

[88] Υιος (Heb 1.5a; 2.6; 3.6; 5.5; 2Cor 1.19), υιου (Rom 8.29, 32; 1Cor 1.9; 2Cor 4.11; Eph 4.13), υιω (Heb 7.3), and υιον (Heb 1.5b, 8; 4.14; 6.6; 10.29; Gal 1.6; 4.4; 1Thes 1.10). In context, υ̅ι̅ς̅ in Gal 4.7a, b refers to the Christian believers.

[89] Υιος (Heb 11.24; Gal 4.30);υιου (Phil 4.30);υιω (Heb 1.2);υιον (Heb 7.28); and υιε (Heb 12.5).

[90] Υιοι (Rom 9.26; Heb 12.8; Gal 3.7, 26; 4.6); υιων (Rom 8.19; 9.27; Heb 7.5; 11.21, 22); υιοις (Heb 12.5a; Eph 2.2; 3.5); and υιους (2Cor 3.7; 6.18; Eph 5.6; Gal 4.22).

[91] The three other contractions in Galatians all have the 3-letter format: υ̅ι̅ν̅ (4.4) and υ̅ι̅ς̅ (7a, 7b).

The six extant occurrences of the enigmatic title υιος θεου are contracted, all with Jesus Christ as referent.[92] Two cases referring to the Christian believer's divine sonship and his special relationship with God the Father are also contracted.[93]

The messianic title υιος ανθρωπου is a matter of interest. It occurred only once (Heb 2.6) in our codex and υιος is accordingly contracted. In its present context the title is found within an OT quotation (Psa. 8.5-7), the original context of which understandably does not directly refer to Jesus Christ, but to human beings in general. However, in consonance with the overall exegetical practice of the author of Hebrews, this OT quotation was also re-appropriated christologically in this instance, to highlight the supremacy of Jesus over the created order. Hence, we see contraction effected by our scribe and some others.[94]

At first glance, it is very tempting to propose that occurrences of the word within an OT direct quotation influenced the scribe to effect contraction.[95] However, Rom 9.27; 2Cor 6.18; and Heb 12.5b disprove this, as these are direct OT quotations but were in *plene.*

The scribe has 21 instances written out in full involving both the singular and plural forms that clearly have non-sacral referents.[96] Conversely, in two instances contractions should have been made for their sacral meaning, but were not. Heb 1.2, a

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[92] Heb 4.14; 6.6; 7.3; 10.29; 2Cor 1.19; Eph 4.13.

[93] Gal 4.7a and b refer to a Christian believer as "son of God" and "heir(s) of God" respectively.

[94] No other papyri with Hebrews text survived at this point but we see codex ℵ also contracting the υιος here (i.e., $\overline{\upsilon\varsigma}$ ανθρωπου). On the other hand, codex A did not contract υιος but did so for ανθρωπου (i.e., υιος $\overline{\alpha\nu\upsilon}$), whilst codex B copied both in *plene* (i.e., υιος ανθρωπου).

[95] So is Heb 1.5a and 5.5 ("You are my Son"); and 2.6 ("what is the son of man"); as well as Heb 1.5b and 8, referring to Jesus Christ.

[96] Υιος (Heb 11.24 ["*son* of Pharaoh's daughter"]; Gal 4.30["*son* of the slave"]); υιοι (Rom 9.26 ["*sons* of the Living God"]; Heb 12.8 ["you are not *sons*"]; 3.7 ("*sons* of Abraham"], 26 ["*sons* of God"]; 4.6 ["*sons* {of God}"]); υιου (Gal 4.30 ["*son* of a free woman"]); υιων (Rom 8.19 ["*sons* of God"]; 9.27 ["*sons* of Israel"]; Heb 7.5 ["*sons* of Levi"]; 11.21 ["*sons* of Joseph"], 22 ["*sons* of Israel"]); υιοις (Heb 12.5 ["addressed you as *sons*"]; Eph 2.2 ["*son* of disobedience"]; 3.5 ["*sons* of men"]; υιους (2Cor 3.7 ["*sons* of Israel"]; 6.18 ["*sons* and daughters"]; Eph 5.6 ("*sons* of disobedience"); Gal 4.22 ["Abraham had two *sons*"]); υιε (Heb 12.5 ("my *son*").

coded reference to Jesus, and Heb 7.28, referring obviously to Jesus, should have been contracted for their sacral connotations but both were fully written out.

What the foregoing underlines is the instability in the use of the system as to this word, both in referential and formal terms, when our scribe produced this manuscript. Whilst most of his contractions point to Jesus Christ as referent, they do not exclusively pertain to Jesus as the "Son [of God]"; the believer's divine sonship is in focus at times.

7. ΠΑΤΗΡ

CONTRACTION PROFILE

SACRAL		NON-SACRAL	
Nomina Sacra	***Plene***	***Nomina Sacra***	***Plene***
12	12	3	16

The use of πατηρ in its *nomen sacrum* form in $\mathfrak{P}^{46}$ reflects a still tentative stage. Though it is hard (if not impossible) to satisfactorily prove, it is likely that the scribe's *exemplar* might have written this word all in *plene* which the scribe then intermittently attempted to fit into the system. The irregularity of the manner of contraction and the seemingly indiscriminate choice of which instances to contract may help make the point. In fact, in a couple of instances the shorter form ($\overline{\pi\rho}$) was used, both with sacral referent (1Cor 8.6 and Gal 4.6).[97] Paap lends support to this by describing this 2-letter contraction as a situation of "initial uncertainty", where a particular contraction was still making its way.[98]

Out of the extant thirty two occurrences of the singular, only fourteen have been contracted,[99] and of this number three have strikingly non-sacral referents.[100]

[97] We also see this 2-letter contraction in $\mathfrak{P}^{5}$ (2 sacral) and $\mathfrak{P}^{45}$ (5 sacral, 2 non-sacral); like $\mathfrak{P}^{46}$ these manuscripts also used the 3-letter format.

[98] Paap, *Nomina Sacra*, 125, n5.

[99] The 17 instances written in *plene* include: πατηρ (Rom 8.15; Heb 12.7; 2Cor 1.3a, b; Eph 1.17); πατρος (Rom 9.10; Heb 7.10; 1Cor 5.1; 2Cor 1.2; Gal 1.1, 3); πατρι (Heb 12.9; Col 1.2; 3.17); and πατερα (Heb 1.5;

Conversely, the majority of the uncontracted singulars have very clear sacral referents with the first member of the Godhead as the explicit or implicit referent,[101] and should have been abbreviated at the first instance. A graphic example is Gal 1.1 and 3 where πατρος occurs in the phrase θεου πατρος. The referent here is unambiguously to "God the Father" but only θεου was contracted and πατρος in both instances was written out *plene*; then in verse 4 both words were contracted in the construction του θυ και πρς ημων. One might be tempted to suggest that the presence of the conjunction και caused the difference in form, but checking against Eph 1.2 (where both words were also contracted) or 2Cor 1.2 (where only θεου was contracted) immediately dismisses the suggestion.[102] Perhaps one would alternatively surmise aesthetic reason for non-contraction, i.e., πατρος in both Gal 1.1 and 3 is at line-ends and that our scribe may have attempted in both instances to keep the imaginary right margin, but this is again upset by Eph 1.2 where πρς is also at line-end.

2Cor 1.2-3 and Eph 5.31 further highlight this contraction inconsistency. Whilst the blessing-doxology passage in 2Cor 1.2-3 mentions πατηρ three times, all unmistakably referring to "God the Father", none was ever contracted, and yet we see the clearly non-sacral πατερα contracted in Eph 5.31![103]

2Cor 6.18; Eph 6.2). The 15 singular *nomina sacra* include: 1Cor 8.6; 15.24; 2Cor 11.31; Eph 1.2; 3.14; 4.6; 5.20, 31; 6.23; Gal 1.4; 4.2, 6; Phil 2.11, 22; and 4.20.

[100] Eph 5.31 has to do with the marriage context (groom leaving his *father* and mother); Gal 4.2 in the context of legal adoption ("trustees of the *father*"); and Phil 2.22 in the context of Paul's relationship with Timothy ("as a son with a *father*").

[101] Πατηρ (Rom 8.15 ["Abba *Father*"]; 2Cor 1.3a ["God and *Father*"], 3b ["*Father* of mercies"]; Eph 1.17 ["the *Father* of glory"]); πατρος (2Cor 1.2 ["from God our *Father*"]; Gal 1.1 ["God the *Father*"]; 1.3 ["God the *Father*"]); πατρι (Col 1.12 ["thanking the *Father*"]; 3.17 ["to God (the) *Father*"]; Heb 12.9 ["*Father* of spirits and live"]); πατερα (2Cor 6.18 ["I {God} will be a *father* to you"]; Heb 1.5 ["I {God} will be a father to him"]).

[102] Note that the other occurrence of the phrase in Eph 6.23 is also contracted, i.e., θυ πρς.

[103] Conversely, the related family context in Eph 6.2 ("honour your πατερα and mother"), which is merely eight lines away from 5.31 on the page [f80^{r}-l$^{11\&19}$]), has the uncontracted form.

Πατηρ in Gal 4.6 and Rom 8.15 is an interesting case. Gal 4.6 involves the Aramaic-Greek emphatic vocative "Abba *Father*" which is contracted in this instance, with the 2-letter format (i.e., αββα ο $\overline{\pi\rho}$), but the exactly similar expression in Rom 8.15 is written out *plene*, i.e., [αββ]ᾳ ο πατηρ.

If these instances are any indication, they suggest an attempt on the part of the scribe to put a distinction between sacral and profane with regard to this word, but that attempt proved inconsistent. Nonetheless, to the scribe's credit, all the occurrences of the plural are written out *plene*, and their referents are all non-sacral, pertaining either to Israel's forefathers or to earthly biological fathers.[104]

Another interesting phenomenon occurs in f58^{r}–l^{17} (1Cor 15.24) involving another "irregular" *nomen sacrum*, one which Paap calls "uncommon form".[105] The original reading is $\overline{\pi\alpha\rho\iota}$ but was corrected *in scribendo* by writing a slashing stroke over the *iota*, i.e., $\overline{\pi\alpha\rho\iota}$. But this was unfortunate since it was an "incorrect" alteration.[106]

What we see highlighted yet again with πατηρ is the inconsistency of our scribe's application of the system, both at the levels of reference and of form. But this inconsistency is far from his idiosyncrasy, since similar inconsistency is equally exhibited by some contemporary scribes or slightly later,[107] which well attests to the still developing status of this word as a *nomen sacrum* at the time our codex was produced.

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[104] Rom 9.5 ("the *patriarchs*"); 11.28 ("for the sake of their *fore*[*fathers*]"); 15.8 ("of the *patriarchs*"); Heb 1.1 ("to our *Fathers*"); 3.9 ("your *fathers*"); 8.9 ("with their *fathers*"); 11.23 ("his [Moses'] *parents*"); 12.9 ("we have [earthly] *fathers*"); 1Cor 4.15 ("many *fathers*"); Eph 6.4 ("*Fathers*, do not provoke"); Col 3.21 ("*Fathers*, do not provoke").

[105] Paap, *Nomina Sacra*, 104.

[106] Correction should have been to $\overline{\pi\rho\iota}$ since the contextual syntax necessitates a dative reading, i.e., πατρι; cf. Paap, *Nomina Sacra*, 104, who thinks that the corrected form is a nominative contraction.

[107] For instance, whilst $\mathfrak{P}^{66}$ keeps the 3-letter form for the occurrences of the singular, it is a bit puzzling why the plural πατερες was contracted in John 4.20, $\overline{\pi\rho\varepsilon\varsigma}$. $\mathfrak{P}^{45}$ also both reflects the 2-letter and 3-letter compendia. Furthermore, W (Gospels) generally has the 3-letter form (42 instances [34 sacral vs 8 non-sacral]) but it reflects the 4-letter format in Matt 10.29, 32 for the genitive πατρος (both sacral), forming what looks like the preposition προς except that it has the overline, i.e., $\overline{\pi\rho o\varsigma}$.

8. ανθρωπος

CONTRACTION PROFILE

SACRAL		NON-SACRAL	
Nomina Sacra	***Plene***	***Nomina Sacra***	***Plene***
1	2	10	68

Why this word was subjected to contraction to indicate "sacrality" is a big question mark—the data hardly satisfy this criterion of the system. Due to the preponderant profane references, perhaps the more appropriate appellation for this is *nomen profanum* rather than *nomen sacrum.*[108] Eph 2.15 is the lone instance where the contraction seems to really make sense insofar as the sacral reference is concerned;[109] others, by no stretch of imagination evince any "sacral" implication. In any case, there are 11 instances of contraction[110] in our manuscript, patterned after the "most usual form" (as Paap described it), the 4-letter $\alpha\overline{\nu o\varsigma}$ and its derivative forms.

Interestingly, the overline consistently does not cover the initial *alpha*, but only the last three or four letters.

Both in its singular and plural forms, the *plene* ανθρωπος and its derivatives generally refer to a particular man (e.g., Paul), the human race, or to human nature (e.g., inner self). The two exemptions are not anomalous either, since, even though they both ultimately refer to Jesus (1Cor 15.21b "by one *man*"; 15.47b "the second *man*"), their reference in context is merely implicit, which might have prompted the scribe not to resort to the *nomen sacrum* form.

[108] Paap, *Nomina Sacra*, 105, qualified that his evidence "concerns mainly the profane meaning".

[109] The reference to Jesus Christ as the "new man" ($\kappa o\iota\nu o\nu\ \alpha\overline{\nu o\nu}$) is implicit in this passage as the one who can reconcile the human race back to God (v16).

[110] The list includes $\alpha\overline{\nu o\varsigma}$ (1Cor 2.14; 4.1); $\alpha\overline{\nu o\upsilon}$ (1Cor 2.9; 2.11b); $\alpha\overline{\nu\omega\nu}$ (1Cor 2.11a; 2Cor 8.21; Eph 3.5); $\alpha\overline{\nu o\iota\varsigma}$ (1Cor 3.21; 4.9); and $\alpha\overline{\nu o\nu}$ (1Cor 3.3; Eph 2.15).

Unlike other manuscripts, in $\mathfrak{P}^{46}$ the lone messianic title υιος ανθρωπου in Heb 2.6 is contracted only in the first word but not ανθρωπου.[111]

9. ΠΝΕΥΜΑ

CONTRACTION PROFILE

SACRAL		NON-SACRAL	
Nomina Sacra	***Plene***	***Nomina Sacra***	***Plene***
72	10	30	23

Of all the *NS* in $\mathfrak{P}^{46}$ πνευμα (and its derivative adjectival and adverbial forms) appears to be the hardest to deal with in terms of establishing scribal pattern. Fortunately or unfortunately, major studies on *NS* have not provided any substantial discussion on this word, when compared with the other four words so-called "core". There are 102 instances of contraction and 33 cases of *plene* in $\mathfrak{P}^{46}$, but despite this there is no clear divide between sacral and profane referents, and it is here that discussion should necessarily commence.

In terms of referential contractions, there is an obvious discrepancy in the scribe's choice of which to contract and which to write out in full. In at least eight instances πνευμα should have been contracted since they clearly have sacral referents (i.e., the Holy Spirit)[112] but were not, and yet in 27 cases contraction should have not been effected, as they do not at all refer to the Third member of the Holy Trinity.[113] In view of this,

[111] Comfort, *Encountering the Manuscripts*, 246, wrongly attributed the phrase as contracted.

[112] Rom 8.23 ("first fruit of *Spirit*); 15.13 (*Holy Spirit*); 15.16 (*Holy Spirit*); Heb 9.14 (eternal *Spirit*); 1Cor 2.10a (through the *Spirit*), b (the *Spirit* searches); 2Cor 13.13 (fellowship of the *Spirit*); and Phil 3.3 (in the *Spirit*).

[113] Heb 1.14 ("ministering *spirits* [$\overline{\pi\nu\alpha}$=πνευματα]"); 4.12 ("soul and *spirit* [$\overline{\pi\nu\varsigma}$]"); 1Cor 2.12a ("the *spirit* [$\overline{\pi\nu\alpha}$] of the world"), 14b ("*spiritually* [$\overline{\pi\nu\varsigma}$=πνευματικως] discerned"), 15 ("a *spiritual* [$\overline{\pi\nu\varsigma}$=πνευματικος] man); 3.1 ("*spiritual* [$\overline{\pi\nu\varsigma}$=πενυματικοις] men"); 4.21 ("*spirit* [$\overline{\pi\nu\varsigma}$] of gentleness"); 5.3 ("present in the *spirit* [$\overline{\pi\nu\iota}$]"), 4 ("(Paul's) *spirit* [$\overline{\pi\nu\varsigma}$]"), 5 ("his spirit [$\overline{\pi\nu\alpha}$] may be saved"); 7.34 ("to be holy in body and *spirit* [$\overline{\pi\nu\iota}$]"); 12.10 ("distinguishing between *spirits* [$\overline{\pi\nu\omega\nu}$]"); 14.15 ("pray with my *spirit* [$\overline{\pi\nu\iota}$] and mind"), 16 ("bless with your *spirit* [$\overline{\pi\nu\iota}$]"); 15.46a ("*spiritual* [$\overline{\pi\nu\kappa\omicron\nu}$=πνευματικον]"), 46b ("*spiritual* [$\overline{\pi\nu\kappa\omicron\nu}$=πνευματικον]"); 16.18 ("you refreshed my spirit [$\overline{\pi\nu\alpha}$]"); 2Cor 7.1 ("defilement of the body and *spirit* [$\overline{\pi\nu\iota}$]"); 12.18 ("with the same *spirit* [$\overline{\pi\nu\iota}$]"); Eph 1.17 ("a *spirit* [$\overline{\pi\nu\alpha}$] of wisdom"); 2.2 ("the *spirit* [$\overline{\pi\nu\varsigma}$] at work amongst the children of disobedience"); 4.23 ("renewed in the *spirit* [$\overline{\pi\nu\iota}$]"); Gal 6.1

Comfort suggested that the scribe's *exemplar* did not have the *nomen sacrum* at all and that the scribe exercised "interpretive decisions about each occurrence of *pneuma*... and often he succeeded in doing it, but not always".[114] To further account for this, he then assumed that $\mathfrak{P}^{46}$ was "produced quite early—probably earlier than all the extant manuscripts that show *pneuma*...," when πνευμα as *nomen sacrum* was not yet fully developed.[115] Elsewhere, Comfort similarly asserted that "(t)he copyist of $\mathfrak{P}^{46}$ also exercised some discernment in distinguishing $\overline{\text{ΠΝΑ}}$ from πνευμα",[116] discriminating the former as the divine Spirit from the latter as human spirit.[117]

Comfort's assumptions, however, do not seem to hold water if one carefully analyses the degree of (in-)consistency in the manuscript tradition with extant readings of the contracted πνευμα. One only needs to scour the evidence from Paap's data and other sources[118] to see that, contrary to expectations, there is rampant inconsistency amongst earlier manuscripts in terms of their choices of which instances to contract and which are not. This is especially evident amongst the more extensive manuscripts, where comparison can best objectively be made. For instance, $\mathfrak{P}^{66}$ (c. 200 A.D.) contracts πνευμα

("*spirit* [$\overline{\text{πνι}}$] of gentleness"), 18 ("be with your *spirit* [$\overline{\text{πνς}}$]"); Phil 1.27 ("stand firm in one *spirit* [$\overline{\text{πνι}}$]"); 4.23 ("be with your *spirit* [$\overline{\text{πνς}}$]"); and Col 2.5 ("with you in the *spirit* [$\overline{\text{πνι}}$]").

[114] Comfort, *Encountering the Manuscripts*, 237.

[115] Comfort, *Encountering the Manuscripts*, 238.

[116] Philip Comfort, "Light from the New Testament Papyri concerning the Translation of πνευμα," *TBT* 35/1 (1984): 130-33, p. 132.

[117] Comfort, "Light from NT Papyri," p.132, specifically mentioned 1Cor 2.10-14, as a case in point, where he argues that the scribe of $\mathfrak{P}^{46}$ discerned the divine-human distinction, asserting that "(a)ccording to the (scribe's) interpretation, πνευμα in v.10… may not be the divine Spirit but perhaps the human spirit (or the human spirit under the influence of the divine Spirit)." He arrived at this conclusion because "(i)n verses 11 through 14 he abbreviated πνευμα the remaining five times" (p.132). However, this argument is weakened by the fact that within verses 11-14 our scribe also contracted the first πνευμα in v.12 which clearly does not refer to the Holy Spirit, i.e., "spirit of the world ($\overline{\text{πνα}}$ του κοσμου)"! See also the contracted πνευματος in Eph 2.2 where the reference is again a negative one in context ("the *spirit* [$\overline{\text{πνς}}$] at work amongst those who are children of disobedience").

[118] In checking readings for NT manuscripts, Muenster-INTF's online *New Testament Transcripts* [http://nttranscripts.uni-muenster.de/] has been of great assistance, as well as the more sophisticated *VMR*[2] [http://ntvmr.uni-muenster.de/en_GB/home], with all its accompanying high resolution manuscript images.

19 times,[119] with 11 sacral[120] and eight non-sacral referents.[121] On the other hand, the Washington Freer MS V [Minor Prophets] (III-IV A.D.) has 20 contractions, eight[122] with sacral referents and twelve[123] non-sacral.[124] Whilst the W [Gospels, 032] (IV/V A.D.) has 83 contractions, 41 sacral and 42 non-sacral! Conversely, Paap reported that whilst Mem. Miss. Arch. Franç. Caire IX (V/VI A.D.) does not have any contraction, yet it has 27 instances of the *plene*, six sacral and 21 profane![125] It may be added that B (IV A.D.) did not treat πνευμα as a *nomen sacrum*. Equally noteworthy is the fact that, except in Luke 10.20, $\mathfrak{P}^{75}$ always contracts πνευμα every time it occurs, even when it refers to evil spirits![126] What this underscores is that, unlike the words κυριος, θεος, χριστος, and ιησους, throughout the first five centuries or so of Christian existence, there is apparently no systematic scribal interpretive standardization, contrary to what some wished to project,[127] in terms of referential ascription on πνευμα.

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[119] John 3.8 originally has πνευμα in *plene*, but was later corrected to the *nomen sacrum* $\overline{\text{πνα}}$. The *plene* πνευμα in John 3.6b, however, was left uncorrected, making it the lone instance of a *plene* for this word in the extant pages of $\mathfrak{P}^{66}$.

[120] $\overline{\text{πνα}}$ (John 1.32, 33a; 3.34; 4.24; 7.39b; 14.17, 26); $\overline{\text{πνς}}$ (John 3.6a, 8b; 7.39a); and $\overline{\text{πνι}}$ (John 1.33b).

[121] $\overline{\text{πνα}}$ (John 3.8a; 6.63a, b; 19.30 ($\overline{\text{π}}$[να])); $\overline{\text{πνς}}$ (John 3.5); $\overline{\text{πνι}}$ (John 4.23; 11.33; 13.21).

[122] Micah 2.7; 3.8; Joel 3.1, 2; Haggai 2.5; Zechariah 4.6; 7.12; and Malachi 2.5a.

[123] Jonah 1.4 ("great *wind*"); 4.8 ("east *wind*"); Habakkuk 1.11 ("*wind*"); 2.19 ("*breath*"); Haggai 1.14a ("*spirit* of Zerubbabel"), 14b ("*spirit* of Jesus"), 14c ("*spirit* of the remnants"); Zechariah 5.9 ("*wind*"); 12.10 ("*spirit* of grace"); 13.2 ("*spirit* of uncleanness"); Malachi 2.15b ("your *spirit*"); and 2.16 ("your *spirit*").

[124] See Henry Sanders and Carl Schmidt, *The Minor Prophets in the Freer Collections and the Berlin Fragment of Genesis* (New York: MacMillan, 1927), 51-151; cf. Paap, *Nomina Sacra,* 15-16, where he noted seven-thirteen ratio, in favour of the non-sacral.

[125] Paap, *Nomina Sacra*, 72, 83.

[126] Luke 4.3 ([ακ]αθαρτοι||$\overline{\text{π}}$[$\overline{\text{να}}$]σ[ι]); 6.18 ($\overline{\text{πντων}}$||ακαθαρτ[ων]); 8.29 ($\overline{\text{πνι}}$||τω ακαθαρτων); 9.39 and 42 ($\overline{\text{πνα}}$ and $\overline{\text{πνι}}$ ακαθαρτω); 10.24 (ακαθαρτον $\overline{\text{πνα}}$), 26 (ετερα $\overline{\text{πντα}}$||πονηροτερα); and 13.11 ($\overline{\text{πνα}}$ εχουσα ασθενειας). Additionally, codex Sinaiticus also reflects a contracted πνευμα in Luke 11.24 (f38^{r}, col ^{3}, l^{17}) where the referent is to an "unclean spirit" (το ακαθαρτον $\overline{\text{πνα}}$).

[127] For instance, Comfort, *Encountering the Manuscripts*, 231-32, despite his caveat about the "peculiar inconsistency" in $\mathfrak{P}^{46}$, asserted "When we look at all the manuscripts of the second and third centuries, where the title *pneuma* (Spirit) occurs, it is written as a nomen sacrum… The early scribes *consistently* used the nomen sacrum form for *pneuma* when designating the divine Spirit. (The word *pneuma* was written out only when designating some other kind of spirit, such as evil spirits)." Emphasis added. Accordingly, on the basis of Comfort's statement, Solomon, *Nomina Sacra*, 2, suggested that πνευμα perhaps should be included in the "core group" together with θεος, κυριος, χριστος, and Ιησους.

In terms of formal contraction, all the noun singulars are consistently abbreviated after the 3-letter format (πνα, πνς, and πνι), whilst the extant occurrences of the plural are either a 3-letter (Heb 1.14 πνᾰ) or 4-letter format (1Cor 12.10 πνων). The picture changes when we come to the adjective πνευματικος and the adverb πνευματικως. Πνευματικος and its derivatives have the following contractions: πνκος πνκον, πνικον, and πνς. The contraction πνς, used for the genitive singular πνευματος, was also used apparently for the dative plural πνευματικοις and the adverb πνευματικως, and because of this a few have advanced a rather negative portrayal of its scribe's understanding about the whole system, an issue we shall now discuss.

Citing only eight examples (three of which had to do with the contraction πνς for πνευματικος/ως),[128] Royse doubted our scribe's comprehension of the *NS* abbreviations that were in his *exemplar*, concluding that "the scribe has difficulty understanding the abbreviations for *nomina sacra* that stood in his *Vorlage*, and accordingly *often* introduces an impossible form".[129] However, this seems to me a serious exaggeration (if not misleading), especially if seen against the background of the total number of *NS* produced by the scribe of $\mathfrak{P}^{46}$. The extant *NS* total to 1,058[130] and if indeed the scribe erred in eight instances only, it does not seem fair to portray negatively the scribe's understanding of the system. It is a gross misrepresentation of the facts to put up an argument based only

[128] Royse, *SH-M*, 257-58, cited the following:
1Cor 2.14 (πνευματικως > πνευματος),
3.1 (πνευματικος > πνευματος),
4.21 (πνευματι > πνευματος),
15.24 (πατρι > πατηρ);
Heb 10.10b (ιησου χριστου > ιησους χριστος),
12.14b (τον κυριον > κυριος),
12.24 (ιησου > ιησους χριστος), and
13.21 (ιησου χριστου > ιησους χριστος).

[129] Royse, *SH-M*, 259. Emphasis added.

[130] Breakdown according to word and form is as follows: θεος (360), χριστος (252), κυριος (170), ιησους (109), υιος (21), πνευματος/πνευματικος/πνευματικως (102), σταυρος (18), ανθρωπος (11), and πατηρ (15). For more details, see Appendix P.

on a very small fraction of instances, in this case only 0.75% (not even 1%), of the evidence.[131] Surprisingly, some scholars seem to have followed Royse's proposal wholesale (Haines-Eitzen, for instance).[132] It may be true that our scribe seriously committed copying blunders in many instances, as observed by Zuntz, but it is definitely not in the area of *nomina sacra*.

Statistics aside, Royse's suspicious appraisal becomes more telling when we note that Royse's assessment is at best applicable only to the contraction of the adjective πνευματικος/πνευματικως and not the *whole* system as such. But even this one is open to debate. Furthermore, we must emphasise that the scribe got the contractions for the nominal πνευμα correctly and consistently in all instances where it is contracted. Where we find formal inconsistencies concern only the three instances of the adjectival/adverbial πνευματικος/ως.[133] But that does not mean automatically that the scribe of $\mathfrak{P}^{46}$ "often" introduced "impossible forms" of *nomina sacra* that cast doubts on his comprehension. In fact, it may be argued that across the manuscript tradition there was no standardised form of contraction for this adjective/adverb, and this observation becomes very important if we note that $\mathfrak{P}^{46}$ is the earliest surviving witness of a contracted πνευματικος/ως.

It is possible, if not probable, that at first only the noun πνευμα was contracted, as evidenced by Chester Beatty V (II A.D.) and Rahlfs P. 967 (II-III A.D.),[134] but subsequently the πνευματικος/ως were also contracted and integrated into the system by some scribes,

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[131] It has been suggested that Royse might have been referring only to the *nomina sacra* in $\mathfrak{P}^{46}$ that are "singulars". However, reading Royse's sections on this does not clearly put such a distinction.

[132] See Haines-Eitzen, *Guardians of Letters*, 93.

[133] 1Cor 2.14, 15; 3.1.

[134] Paap, *Nomina Sacra*, 8. $\overline{\pi\nu\alpha}$ is found in 13^{r}-I^{1} (Ezekiel 18.31); for the image, see http://www.uni-koeln.de/phil-fak/ifa/NRWakademie/papyrologie/Ezechiel/bilder/PT13r.jpg (site visited 25 April 2013).

as is already present in $\mathfrak{P}^{46}$,[135] as well as in ℵ, A, W [Gospels], among others.[136] It is also possible that an ancestor of $\mathfrak{P}^{46}$ was one of those who first used the contracted form for πνευματικος/ως. But it is very important to underscore here that even in these early manuscripts, the abbreviation for πνευματικος/ως is *not* uniform, e.g., the single instance in W [Gospels] has $\overline{\text{πνικην}}$ (=πνευματικην),[137] with sacral referent, whilst P. Bouriant has $\overline{\text{πνκος}}$ (=πνευματικος) and $\overline{\text{πνκα}}$ (=πνευματικα**),** without the *iota.*[138] Furthermore, ℵ exhibits contractions with and without the intervening *iota*, and the overline is placed above the first three letters excluding the suffixes.[139] In $\mathfrak{P}^{46}$ the overline almost always covers all the letters, including the corresponding suffixes.

What can we make of this information? How do we account for these referential and formal "idiosyncrasies" (to borrow Haines-Eitzen term) of $\mathfrak{P}^{46}$ in regard to this *nomen sacrum*? A few assumptions can be made at this juncture. First, there seems to be no other satisfactory way to explain this phenomenon than to view this "formal" instability as attesting the stage wherein πνευμα as a *nomen sacrum* was still at its "infancy stage" or what Paap calls "initial situation", when $\mathfrak{P}^{46}$ was produced. The evidence for this as shown above is revealing. Second, and related to the first observation, whilst there seems

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[135] 1Cor 2.13 presents an interesting case. The UBS-NA common text, reflecting majority of the manuscript tradition, reads "πνεύματος, πνευματικοῖς πνευματικὰ". Of the three consecutive cognates, only πνευματος was contracted in $\mathfrak{P}^{46}$, i.e., $\overline{\text{πνς}}$ πνευ||ματικοις πνευματικα, although πνευματικος was contracted in both v14 and v15. There are no surviving NT papyri for this passage to compare $\mathfrak{P}^{46}$, but when compared with the earliest parchment codices an interesting scenario is highlighted. For instance, whilst B did not make any contraction consistent to its overall treatment of πνευμα/πνευματικος/πνευματικως, both ℵ and A contracted the whole phrase ($\overline{\text{ΠΝC}}$ $\overline{\text{ΠΝΙΚΟΙC}}$ $\overline{\text{ΠΝΙΚΑ}}$).

[136] Paap, *Nomina* Sacra, 64, 83, also noted that P. Bouriant 3 (IV-V A.D.) contains five contractions of this adjectival form.

[137] This form occurred only once in the manuscript tradition, as part of the addition in Mark 16.14 read by W only; on this, see Henry Sanders, *The New Testament Manuscripts in the Freer Collection* (London: MacMillan, 1918), 10; and Prior, "Use and Non-use of *Nomina Sacra,*" 155-56.

[138] On this, see Paap, *Nomina Sacra,* 64.

[139] Jongkind, *Scribal Habits of Codex Sinaiticus*, 65, recorded the following contractions: $\overline{\text{ΠΝΙΚΟC}}$, $\overline{\text{ΠΝΙΚΟΝ}}$, $\overline{\text{ΠΝΙΚΗ}}$, $\overline{\text{ΠΝΙΚΗC}}$, $\overline{\text{ΠΝΙΚΟΙ}}$, $\overline{\text{ΠΝΙΚΟΙC}}$, $\overline{\text{ΠΝΙΚΩΝ}}$, $\overline{\text{ΠΝΙΚΑΙC}}$, $\overline{\text{ΠΝΑΚΑ}}$, and $\overline{\text{ΠΝΙΚΩC}}$ for the adverb.

to be a developing preferred form of contraction in the first five centuries of Christian tradition,[140] that perhaps eventually won the day by the mediaeval period, there are equally observable formal divergences that actually developed in some manuscripts. For instance, whilst Chester Beatty V (Aphroditopolis, II A.D.) has $\overline{\pi\nu\alpha}$ for πνευμα, its contractions for πνευματος and πνευματων are different from the other usual forms: $\overline{\pi\tau\varsigma}$ and $\overline{\pi\nu\alpha\tau\omega\nu}$. Πνευματα was contracted to $\overline{\pi\nu\tau\alpha}$ by Michigan P. III (Fayum, ± 250 A.D.) and W [Gospels] (IV/V A.D.), and to $\overline{\pi\nu\alpha\tau\alpha}$ by Berlin LXX Fr. 14/P. 5874 (Fayum, V A.D.) and **א**. In contracting πνευματων to $\overline{\pi\nu\alpha\tau\omega\nu}$, Chester Beatty V is shared by **א**, W [Gospels],[141] and Mem. Miss. Arch. Franç. Caire IX[142] (Panapolis, V/VI A.D.), but to $\overline{\pi\nu\mu\alpha\tau\omega\nu}$ singularly by P. Lit. London 216 (Wadi Sarga, V A.D.).[143] Finally, three manuscripts have contracted πνευματος to $\overline{\pi\nu\sigma\varsigma}$: $\mathfrak{P}^4$ (Coptos, IV A.D.),[144] P. Oxy. XIII 1602 (Oxyrhynchus, IV/V A.D.), and P. Bouriant 3 (Panapolis, IV A.D.).[145] Third, whilst there is common geographical origin for these manuscripts, i.e., Egyptian provenance, the degree of variation amongst them also suggests how scribal practices and preferences intersected with the employment of the system, and cannot be attributed simply to their common textual origins. The scribes were not merely passive copyists of their *exemplars*; at times they inscribed on their manuscripts their own preferences and understanding of what they were copying.

V. SYNTHESIS: HOW THE SCRIBE OF $\mathfrak{P}^{46}$ EMPLOYED THE SYSTEM

If the criterion for judging a *nomen sacrum* to be early is in the degree of *contraction consistency* vis-à-vis the uncontracted forms, six immediately stand out in $\mathfrak{P}^{46}$:

[140] I must immediately add nonetheless that most of the manuscripts that Paap presented as seemingly exhibiting formal consistency are very fragmentary and most of the times showing only one surviving instance of a contraction; see Paap, *Nomina Sacra*, 82-83.

[141] That is, the addition to Mark 16.14; see Prior, "Use and Non-use of *Nomina Sacra*," 155.

[142] Although it contracted exactly the same word in another instance to $\overline{\pi\nu\upsilon\tau\omega\nu}$.

[143] For the contraction frequency in these manuscripts, see Paap, *Nomina Sacra*, 82-83.

[144] E.g., Luke 1.67.

[145] See Paap *Nomina Sacra*, 82-83.

ΙΗCΟΥC, ΧΡΙCΤΟC, ΘΕΟC, ΚΥΡΙΟC, CΤΑΥΡΟC, and ΠΝΕΥΜΑ, in that particular order (see Tab. 4-C2). If this is any indication, then the prevalent view that there are only four core words is belied in the evidence of $\mathfrak{P}^{46}$, for CΤΑΥΡΟC equally has a very high percentage contraction rate at 95%, even higher than ΠΝΕΥΜΑ (76%). This becomes even more noteworthy as the earliest surviving occurrence of the contracted CΤΑΥΡΟC is in $\mathfrak{P}^{46}$. Conversely, ΥΙΟC, ΠΑΤΗΡ, and ΑΝΘΡΩΠΟC would have been later additions to the system in this diagram.

TABLE 4-C2
***NOMINA SACRA* VS *PLENE* COMPARATIVE TABLE**

	In *NOMINA SACRA*	In *PLENE*	PERCENTAGE OF *NS*
ΙΗCΟΥC	109	0	100
ΧΡΙCΤΟC	252	0	100
ΘΕΟC	360	3	99
ΚΥΡΙΟC	170	4	98
CΤΑΥΡΟC	18	1	95
ΠΝΕΥΜΑ	102	33	76
ΥΙΟC	21	23	48
ΠΑΤΗΡ	15	28	33
ΑΝΘΡΩΠΟC	11	70	13
TOTALS	**1058**	**162**	

If however the criterion is the integrity of contraction in terms of *referential designation*, the scenario changes and slightly becomes complicated. In this case, rating is based on the degree of error in ascribing "sacrality value" to a particular instance of contraction (see Tab. 4-C3, next page). Error is to be derived from the instances of non-contraction although they have sacral references (*plene* sub-column of the sacral heading) and instances of contraction although they have profane referents (*nomina sacra* sub-column of the non-sacral heading). This will also give us a glimpse as to the degree of difficulty in deciding whether or not to contract a word which our scribe had to deal with.

TABLE 4-C3
SACRAL VS NON-SACRAL COMPARATIVE TABLE

A	B	C	D	E	F	G	H
	SACRAL		NON-SACRAL				
	Nomina Sacra	*Plene*	*Nomina Sacra*	*Plene*	Combined total of errors (C + D)	% of combined errors (F / [B+C+D+E] X 100)	Rating
ΧΡΙΣΤΟΣ	252	0	0	0	0	0	1
ΘΕΟΣ	359	0	1	3	1	0 (0.3)	2
ΚΥΡΙΟΣ	169	0	1	4	1	1 (0.6)	3
ΙΗΣΟΥΣ	107	0	2	0	2	2 (1.8)	4
ΣΤΑΥΡΟΣ	18	1	0	0	1	5	5
ΑΝΘΡΩΠΟΣ	1	2	10	68	12	15	6
ΥΙΟΣ	21	8	0	15	8	18	7
ΠΝΕΥΜΑ	72	10	30	23	40	30	8
ΠΑΤΗΡ	11	12	3	16	15	36	9

Looking at Tab. 4-C3, one is immediately impressed by the high degree of consistency in the contraction and non-contraction of ΧΡΙΣΤΟΣ as the percentage error is pegged at 0. But this is technically problematic since unlike other *NS* words there is strictly no instance of ΧΡΙΣΤΟΣ in Pauline Epistles and Hebrews extant in $\mathfrak{P}^{46}$ that has non-sacral referent against which the integrity of contraction can be measured. But what this table shows is that the scribe generally had the most difficulty in deciding whether or not the instances of ΠΑΤΗΡ and of ΠΝΕΥΜΑ have sacral or non-sacral referents, resulting in the highest degrees of percentage errors at 36 and 30, respectively. The scribe also had difficulty in handling ΥΙΟΣ and ΑΝΘΡΩΠΟΣ. He also had difficulty in maintaining consistent contractive forms for ΠΝΕΥΜΑ (and ΣΤΑΥΡΟΣ). Conversely, he had the least difficulty in handling ΘΕΟΣ, ΚΥΡΙΟΣ, ΙΗΣΟΥΣ and ΣΤΑΥΡΟΣ. It is very likely that the singular forms of the first three and every occurrence (except for one) of ΣΤΑΥΡΟΣ are almost always contracted at the first instance. But how do we know in which area did our scribe encounter the most difficulty in terms of contracting or not?

Table 4-C4 shows the breakdown of Tab. 4-C3, in terms of percentage error for both the sacral and non-sacral categories. What it points to is that the scribe

committed more errors in contracting words that have profane referents than in leaving a *NS* word written out *plene*. Against the over-all total of non-sacral instances the scribe committed about 27% error whilst only around 3% error against the over-all total of error in writing words out *plene* even though they have sacral referents. This means that our scribe's difficulty generally was in deciding whether or not a *NS* word is sacral in context; and more often he wrongly ascribed sacrality value. More particularly, he often mis-ascribed sacrality to ΠΝΕΥΜΑ by writing it as a *nomen sacrum* in 57% (30/53 instances), but when compared against ascribing non-sacrality his degree of error is only at 12% (10/82). In view of this, one must be open to the possibility that in his *exemplar* all occurrences of ΠΝΕΥΜΑ were written out in *plene* and in attempting to exercise judgment he succeeded in some but failed in most.

TABLE 4-C4
SACRAL VS NON-SACRAL COMPARATIVE TABLE (BREAKDOWN)

A	B	C	D	E	F	G	H	I	J
	SACRAL					NON-SACRAL			
	Nomina Sacra	*Plene*	% of error (C / [C+B] x 100)	Rate		*Nomina Sacra*	*Plene*	% of error (G/ [G+H] x 100)	Rate
ΧΡΙCΤΟC	252	0	0	1	ΧΡΙCΤΟC	0	0	0	1
ΘΕΟC	359	0	0	1	ΥΙΟC	0	15	0	1
ΚΥΡΙΟC	169	0	0	1	CΤΑΥΡΟC	0	0	0	1
ΙΗCΟΥC	107	0	0	1	ΑΝΘΡΩΠΟC	10	68	13	2
CΤΑΥΡΟC	18	1	5	2	ΠΑΤΗΡ	3	16	16	3
ΠΝΕΥΜΑ	72	10	12	3	ΚΥΡΙΟC	1	4	20	4
ΥΙΟC	21	8	28	4	ΘΕΟC	1	3	25	5
ΠΑΤΗΡ	11	12	52	5	ΠΝΕΥΜΑ	30	23	57	6
ΑΝΘΡΩΠΟC	1	2	67	6	ΙΗCΟΥC	2	0	100	7
	1010	33				47	129		
TOTALS	Formula: C/[B+C] x 100 **33/1,043 = 0.0316 x 100 = 3.16%**					Formula: G/[G+H] x 100 **47/176 = 0.267 x 100 = 26.7%**			

On the other hand, the contraction of ΙΗCΟΥC every time it occurs may be explained by the likely tendency of the scribe to write all (singular) occurrences of ΙΗCΟΥC in *nomen sacrum* form. This is equally true with ΘΕΟC, ΚΥΡΙΟC, and

ΧΡΙCΤΟC, and perhaps even CΤΑΥΡΟC, which is then suggestive that within the circle of our scribe these five words have already attained higher level of referential stability than others. In the case of ΠΑΤΗΡ, the scenario is reversed; the scribe seems to have a tendency to overlook the sacral context of a passage where ΠΑΤΗΡ occurs (52% vs 16% [10 vs 3]) even when contraction is warranted.

TABLE 4-C5
COMPARATIVE TABLE OF CONTRACTIONS ACCORDING TO GRAMMATICAL CATEGORIES

	NOM	GEN	DAT	ACC	RATING
ΘΕΟC	θ̄ς̄	θ̄ῡ	θ̄ω̄	θ̄ν̄	1
ΚΥΡΙΟC	κ̄ς̄	κ̄ῡ	κ̄ω̄	κ̄ν̄	1
ΙΗCΟΥC	ῑηςῑ	ῑηῡ	ῑηω̄	ῑην̄	1
ΑΝΘΡΩΠΟC	ᾱνος̄	ᾱνοῡ		ᾱνον̄	1
ΑΝΘΡΩΠΟΙ (Pl)			ᾱνοις̄		
ΠΑΤΗΡ	π̄η̄ρ̄	π̄ρ̄ς̄	π̄ρῑ	π̄ρᾱ	2
	π̄ρ̄				
ΥΙΟC	ῡις̄	ῡιῡ		ῡιν̄	2
	ῡς̄		ῡω̄	ῡν̄	
ΧΡΙCΤΟC	χ̄ρ̄ς̄	χ̄ρῡ	χ̄ρ̄ω̄	χ̄ρ̄ν̄	2
	χ̄ς̄	χ̄ῡ	χ̄ω̄	χ̄ν̄	
CΤΑΥΡΟC	σ̄τ̄ρ̄ος̄	σ̄τ̄ρ̄οῡ	σ̄τ̄ρ̄ω̄	σ̄τ̄ρ̄ν̄	3
		σ̄τ̄οῡ			
CΤΑΥΡΟΩ	ε̄σ̄τ̄ρ̄αν̄ ε̄σ̄τ̄ρ̄θη̄ ε̄σ̄τ̄ρ̄αι̯ ε̄σ̄τ̄ρ̄ν̄[ον̄] ε̄σ̄τ̄ᾱν̄				
Verbal Compounds	συνε̄σ̄τ̄ρ̄αι̯ αναστ̄ρ̄ες̄				
ΠΝΕΥΜΑ	π̄ν̄ᾱ̄	π̄ν̄ς̄	π̄ν̄ῑ	π̄ν̄ᾱ̄	3
ΠΝΕΥΜΑ (Pl)	π̄ν̄ᾱ̄	π̄ν̄ω̄ν̄			
ΠΝΕΥΜΑΤΙΚΟC	π̄ν̄κ̄ος̄			π̄ν̄κ̄ον̄	
				π̄ν̄ικ̄ον̄	
	π̄ν̄ς̄				
ΠΝΕΥΜΑΤΙΚΟΙ (Pl)			π̄ν̄ς̄		
ΠΝΕΥΜΑΤΙΚΩC	π̄ν̄ς̄				

Finally, if the criterion is in terms of *formal consistency* (i.e., manner of abbreviation), we can detect at least three groups of contraction, in varying degrees of sophistication (Tab. 4-C5). The first group consists of ΘΕΟC, ΚΥΡΙΟC, ΙΗCΟΥC, and ΑΝΘΡΩΠΟC. Regardless of the number of letters retained in the contraction, members of this group have been consistently contracted throughout. The second group

interchangeably uses two manners of contraction—the 2-letter or 3-letter contractions. This includes ΠΑΤΗΡ, ΥΙΟC, and ΧΡΙCΤΟC. The last group is the most inconsistent and basically has to do with CΤΑΥΡΟC (including its verbal forms) and ΠΝΕΥΜΑ (including the adjectival and adverbial forms). It is in this group that scribal pattern is the hardest to establish, and analysis should make rooms for the possibility of error that contributed to divergent contractive forms.

VI. OTHER INTERESTING DETAILS: *Nomina Sacra* and Aesthetics

Are there indications that our scribe was concerned about the *visual* presentation of the system in his codex or was he concerned only about the *textual* consistency of the system?

Our scribe exhibited some ease in the presentation lay-out of the *NS*. There seems to be no general rule (or restrictions perhaps) as regard the placement of the *NS* on the lines. In fact, there are *NS* written on the actual κολλήσεις (e.g., Fig. 4-3.8). Furthermore, the scribe showed no attempt to avoid (unless extremely inevitable) writing the *NS* on κολλήσεις, which may suggest that our scribe did not consider κολλήσεις as "physical defects" in the manuscript that might otherwise contravene the ascription of sacredness upon the contracted words.[146]

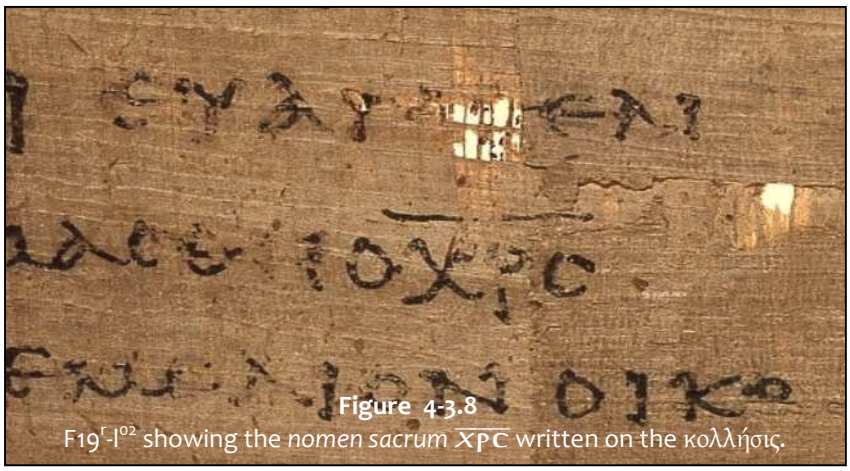


Figure 4-3.8
F19^{r}-l^{02} showing the *nomen sacrum* $\overline{\text{ΧΡC}}$ written on the κολλήσις.

[146] Folios with *nomina sacra* on actual joins include f19^{r}-l^{02} $\overline{\chi\rho\varsigma}$; f20^{r}-l^{07} $\overline{\chi\nu}$; f29^{r}-l^{19} $\overline{\pi\nu\varsigma}$; f35^{v}-l^{14} $\overline{\kappa\upsilon}$; f44^{r}-l^{07} $\overline{\chi\rho\upsilon}$; f52^{r}-l^{06} $\overline{\kappa\upsilon}$; f62^{r}-l^{19} $\overline{\theta\nu}$; f72^{r}-l^{05} $\overline{\chi\rho\upsilon}$; f73^{r}-l^{05} $\overline{\chi\omega}$; f74^{r}-l^{01} $\overline{\pi\nu\iota}$; f77^{r}-l^{24} $\overline{\theta\omega}$; f86^{r}-l^{06} $\overline{\iota\eta\upsilon}$ and l^{19} $\overline{\pi\nu\varsigma}$; and f90^{r}-l^{18} $\overline{\iota\eta\upsilon}$.

Since at least 1-6 lines have been varyingly lost at the bottom portion of a given page of our codex, we cannot determine for certain whether our scribe ended a page with a *nomen sacrum* (or perhaps a run over to the next line). But in the following folios, our scribe commenced a new page with a *nomen sacrum*: f25^{v} (ιηνν), f35^{r} (ιηνν), and f74^{r} (πνι).

In terms of lay-out, our scribe seems to prefer keeping a *nomen sacrum* at line-ends rather than at line-beginning. In 128 cases *NS* were written at line-ends, whereas 62 only at line-beginnings, as shown in the following table.

	θς	χρς	κς	ιης	υς	πνα	στρος	πρς	ανος	TOTALS
Line beginning	17	16	7	9	1	8	1	1	2	**62**
Line ending	33	38	20	12	1	17	3	1	3	**128**

In instances where two *NS* words are combined together, the scribe seems to have a tendency to keep the combined words together in one line. For instance, the combination of χριστος and ιησους are kept together on the same line in 22 instances, either at line ends or at line beginnings.[147] On the other hand, the same combination runs onto the following line in only eight instances.[148] The combination θεος and πατρος when not occurring in the middle of the line are always kept together at line-ends.[149] However, the combination κυριος and ιησους, without any intervening pronoun,[150] is laid out in a less stringent manner; it is either kept on one line (e.g., 1Cor 11.23 κς ιης||) or broken and runs onto the next line, (e.g., 2Cor κυ||[ιηυ]; 11.31 κυ||[ιηυ]). It must be noted, however, that despite this seeming lay-out preference, the scribe was also very careful not to overcrowd the line and has, in most cases, kept the left-side text margin in proper alignment.

[147] Thus, Rom 5.21; 13.14; 15.16; Heb 10.10; 1Cor 1.1, 10; 8.6; 15.57; 2Cor 1.2, 3; 8.9; Eph 5.20; Gal 2.4; 4.14; 6.18; Phil 1.6, 11; 2.11; 3.3, 8, 20; and Col 2.6.

[148] Thus, Heb 13.21; 1Cor 1.7, 9; Eph 1.1; 6.23; Gal 2.16; 3.28; and Phil 4.19.

[149] Thus, Eph 1.2; Gal 1.1; and Col 3.17.

[150] The possessive pronoun is present in Rom 16.20 and 1Cor 1.8.

CONCLUSION

I end this rather long chapter on the textual aspect of $\mathfrak{P}^{46}$ with an investigation of the scribe's use of the *nomina sacra*—an artefactual evidence highlighting few select words deemed to be ascribed with reverence across the manuscript tradition. This is an appropriate ending to this thesis not only because the system's presence in $\mathfrak{P}^{46}$ shows that our scribe is within the same league as other scribes but also because it reveals some of his copying idiosyncrasies.[151]

When the scribe who produced this manuscript inscribed his text onto his codex, he was already acquainted with nine words treated with a certain level of reverence and contracted in particular forms; but he showed no acquaintance of six (or more?) words that were also treated as *NS* in other (later) manuscripts. This may lend support to the earlier formulation and circulation of some contracted words within the Christian literary circle at the time. That is, some of these *NS* were already well-advanced in development and a sense of stability had been reached for them, both in terms of referents *and* forms of contraction; others were still in the process of refinement.

But whilst the system was already in place, the scribe was not reluctant to impose his own preferences, especially in the manner of visual presentation of his manuscript. Palaeographical analysis shows that whilst there are varieties in formal contractions, his use of the superscript line is nonetheless very impressively consistent. There is also an establishable preference whether to begin or end a line with a *nomen sacrum*, both as an individual word or in combination with other *nomina sacra*. We have seen also that the

[151] Of the *NS* in Codex Bezae, Parker, *Codex Bezae*, 106, noted, "… a study which seeks too much information from the *nomina sacra* is of questionable value… But they do betray something of a manuscript's antecedents and of its scribe's own habits." Similarly, Jongkind, *Scribal Habits of Codex Sinaiticus*, 83, noted, "… the use of *nomina sacra* in *Sinaiticus* is not determined solely by reverence. Often *nomina sacra* are employed with very mundane referents…"

scribe's tendency to wrongly ascribe sacrality value upon a particular *NS* word may be explained by his difficulty in deciding whether a word is non-sacral or not in context, which opens up the possibility that some *NS* words (perhaps ΠΝΕΥΜΑ and ΠΑΤΗΡ) were all written out in *plene* in his exemplar, and exercising his interpretive judgment, he then attempted to reflect them in contracted forms he thought most appropriate on a given context (albeit unsuccessfully).

Finally, there is conspicuous inconsistency in the application of this system in $\mathfrak{P}^{46}$, both in terms of reference and form. Some scholars weighted this negatively against the scribe of $\mathfrak{P}^{46}$. However, we have attempted to dispel this impression from the standpoint of the over-all evidence on how the scribe used and understood the system, rather than taking particularly selective examples. In fact, inconsistency in application is a characteristic shared by the more extensive ancient manuscripts, and they perhaps provide the more objective evidence as to the actual use of the system in the earlier stages of the history of manuscript transmission. And here a point must be raised with regard to the need to revisit the methodology of both Traube and Paap in terms of assessing the extant evidence of the manuscripts being studied. My own proposal is to study the system using primarily the more extensive manuscripts and see how these inconsistencies validate or rectify earlier conclusions about the system.

CONCLUSION
SCRIBAL HABITS OF $\mathfrak{P}^{46}$

I introduced this research project using the metaphor of story-telling, with the main scribe of $\mathfrak{P}^{46}$ as its "story-teller" and me as an "intent listener-recorder", in the hope of profiling a more complete portrait about his habits. To this end, I have argued throughout that we need to frame our discussion of "scribal habits" in a broader perspective if we are to fully appreciate the sociology of book production in antiquities, particularly in the Christian context. A *text-focused* methodology, while a valid approach, to locate scribal habits, can only profile a scribe to a certain extent. Unfortunately, that profile would be an incomplete one. The *integrative* methodology[1] we adopted in this thesis attempts to fill in that hiatus.

A detailed and comprehensive profile of the scribal habits of $\mathfrak{P}^{46}$ is, therefore, a justified goal. But the exhaustive investigation of the various raw data must come first, and that is what this thesis has taken up in the foregoing pages: the compilation of previous data, collection of new (undocumented) information, and providing fresh analyses from these combined data, insofar as the formal and conventional features from every page of the 86 extant leaves are concerned.

This thesis is unusual among other scribal studies in that it focuses not solely on the *text* of $\mathfrak{P}^{46}$ (which has mostly been the main methodological source for profiling

[1] Integrative method involves the deployment and integration of papyrology, codicology, palaeography, and textual criticism to locate scribal habits; for its description, see pp. 56-58.

scribal habits), but on all the derivable evidence of scribal activities and participation, encompassing pre-copying and post-copying stages. The contributions that these results can make to our understanding of the production, use, and aesthetics of the ancient books are not only impressive, but more importantly, they broaden our appreciation of ancient scribal culture and how they eventually affected the transmission of the text of the New Testament in general and of the Pauline Letters in particular. Although the extant leaves are imperfect, yet it is often possible to reconstruct a great many details of our papyrus codex, many of which were mentioned here for the first time. The recovery of these data and the analyses of the conjunction of the material (*physical features*), the format (*paratextual features*), and the content (*textual features*) yield a great deal of graphic details about the relationship between our scribe and his papyrus. These suggest, in turn, new questions - and some (provisional?) answers - about the ways in which the scribe of $\mathfrak{P}^{46}$ was very much similar *and* yet also distinctive from his colleagues in the profession.

OUR SCRIBE AND THE PHYSICAL ASPECTS OF $\mathfrak{P}^{46}$

Produced between 175-225 A.D.,[2] $\mathfrak{P}^{46}$ is thus far, the oldest surviving *and* most extensive manuscript witnessing to the text of the *corpus Paulinum*. Salient points flow from this fact alone. But $\mathfrak{P}^{46}$ is not only a manuscript containing the texts of the Pauline Letters; it is first and foremost a manuscript with material components: $\mathfrak{P}^{46}$ is a papyrus manuscript; formatted into a codex; gathered in a single-quire; made of 51 sheets, taken from two different rolls; projecting a tall rectangular shape when folded; and measuring 32 (B) x 28 (H) cm (or about 12 ¾ x 11 inches).[3] More importantly, $\mathfrak{P}^{46}$ is a manuscript with a number of observable manufacturing blemishes. These pre-copying "defects" are, of course, regrettable, but they also helped us know more about our scribe. By looking intently at

[2] See p. 148.
[3] These features are discussed in pp. 60-118.

these material blemishes, we have established that our scribe exerted evident concern for the text by avoiding pre-copying defects in his codex that may discourage the inscription of an intelligible text onto it.

The κολλήσεις in $\mathfrak{P}^{46}$ exhibit a *right-over-left* pattern of pasting[4]—a pasting direction that is supposed to be against the "rule". But this pattern does not reflect our scribe's ineptness or naiveté; it reveals his skilful mastery of his craft instead. Hence, we questioned the proposed rationale for assuming that pasting direction is "always left sheet over the right" because of the *flow of writing*. $\mathfrak{P}^{46}$ disproves this assumption. As such, papyrologists may now need to review the supposition that *pasting direction* is intrinsically connected with the *quality of writing*.

Only 43 sheets are extant, but by examining closely its codicological features, we have established that our codex was made up originally of 51 sheets (or 204 pages).[5] Two of the eight missing sheets contained Rom 6.15-8.14 (now f09-f10) and 1Thess 2.3-5.4 (now f95-f96). We know that the first part of the six other missing sheets contained the front cover and Rom 1.1-5.16 whilst the second part must have contained 2Thess 1.1-3.18, the back cover, and an intervening seven pages, which might have been left blank or filled in with some text. The scribe's line-input per page increased from 26-29 in the first half of the codex to 28-32 in the second half. However, this increase is not *programmatic* in terms of character-input per page, and does not prove that the scribe was compressing his text-input with an *intention* of including all the texts of the Pastorals and Philemon; the seven pages are simply not enough for all these texts. If they have been left blank by the scribe, it is not against the known scribal practice.

[4] This fact and the discussions that come with it are discussed in pp. 119-36.

[5] For this finding and its implications for the canonical discussion of the Pastorals, see pp. 204-35.

OUR SCRIBE AND THE PARATEXTUAL ASPECTS OF $\mathfrak{P}^{46}$

$\mathfrak{P}^{46}$ was produced by one scribe only, who was generally consistent in the sequential execution of strokes and characteristic features that formed the individual letters.[6] The letters are generally upright without any discernible slant; noticeably "serified" at the top and the base; and usually keeping the upper notional line more than the lower line. Some letters are given to angularity; some are with hooks at the upper terminations; and some form occasional ligatures. These are letter formations characteristic of the work of an experienced scribe.

Like any experienced copyists of his time, our scribe conformed to the long-honoured standards of his trade, religiously employing widely observed literary devices:[7] ornamented *titloi*, *paragraphoi*, margination, columniation, line fillers, (sporadic) punctuations, diaereses on initial ι and υ, breathing marks, among others. He was familiar also with the convention of *nomina sacra* and other abbreviation devices. He set his texts in *scriptio continua*, but space-intervals signalling some forms of structures (grammatical and aesthetic) remain visually obvious—the use of this device must have been a standard practice in the trade, too, although there are instances when our scribe's own copying habits stood out.[8] Cumulatively, the use of these devices betrays a scribe who knew his job, was committed to it, and played by its rules.

From the pages of his codex we detected also the intervention of other hands: pagination numbers located at the upper centre part of the page; στιχος notations at the end of each epistle; and *prescriptive* and *alternative* corrections, set either intralinearly or supralinearly. Some pages are filled with reading marks using various ink colours,

[6] On the writing style of our scribe, see pp. 149-63.
[7] For these writing devices, see pp. 169-81.
[8] On this, see pp. 182-202.

different from our scribe's, set to mark "reading units". These are annotations made by the later users of our codex to help them in their reading requirements, strongly suggesting the social milieu where our codex might be located after it left the hands of our scribe—a context where portions from our codex were read out publicly.

OUR SCRIBE AND THE TEXTUAL ASPECTS OF $\mathfrak{P}^{46}$

When discovered, $\mathfrak{P}^{46}$ evinced *confirmatory* significance for textual criticism, exhibiting a text that generally affirms the existing text of the New Testament. However, its text and the way it was inscribed also exhibit interesting, at times idiosyncratic, variant readings. Perhaps most of these variants are not the best of candidates to be the *Ausgangstext* but we have shown nonetheless that these variations, whether from its scribe or from the transmitted text of its *exemplar*, are an excellent resource for framing the sociology of ancient scribal trade using the very lenses its scribe used whilst producing his codex.

One of the first things we can derive from this study is that our scribe "failed" to make an accurate copy of the text that lay before him, for one reason or another.[9] Although this sounds a negative verdict, it in fact positively puts our scribe in the same league with the rest of his colleagues in the trade; our extant manuscripts, especially those of his contemporaries, also exhibit such "failings and shortcomings". But most of the errors of our ancient scribes were unintentional and involuntary—their divided concentration failed them most often—and therefore these errors must be appreciated in light of the exacting nature of copying by hand. In fact, the advent of computer technology has not entirely obliterated similar errors.[10]

[9] On the *exemplar*, including its generally good quality and its "defects", see pp. 237-38; 248-66.

[10] A ready example would be the typographical errors in more recent books produced with high-quality desk-top publishing softwares. While electronic typesetting increases the level of speed and the desired lay-out, the quality of its fidelity to the source text is only as good as the input of the assigned proof-reader/s.

Looking at textual variants within their "material" context, we have demonstrated that some of these can be attributed clearly to the *exemplar*'s own "defects" and idiosyncrasies, which our scribe had to grapple with. Some other variants may be explained as *character-generated*, either present already in the *exemplar* or were accidentally created by our scribe. Locating where these variations occur on the lines also enriches the database of our scribe's copying habits. Accordingly, we have shown that our scribe also committed "non-textual errors"—errors that emphasise the human face of textual transmission.

Apart from our scribe, there are at least more than four other hands who left their imprints on our codex, known through the various corrections they made, whether *prescriptive* or *alternative*.[11] One of them was a contemporary of our scribe who "supervised" his work; the rest were later "users" of our codex. But it was our scribe effected most of the corrections, many of them he made while he was still in the act of copying (*in scribendo* corrections), indicating his consciousness about the demanding responsibilities that come with his job as a copyist. Notably, his corrections also show his demonstrable concern for aesthetics, carefully keeping his manuscript tidy and deliberately avoiding making messy alterations. Accordingly, we argued that there is very little evidence to suggest that the sensible corrections in $\mathfrak{P}^{46}$ are in the direction of the "Alexandrian" text.

The presence of *nomina sacra* in $\mathfrak{P}^{46}$ further highlights the observation that our scribe is in the same league as others, but the manner these *nomina sacra* were used equally reveals some of his copying idiosyncrasies.[12] We proposed that this system seems best pursued through the witness of the more extensive manuscripts, of which $\mathfrak{P}^{46}$ is one of the best representatives, if we are to reconstruct the early stages of the system's

[11] On the corrections in $\mathfrak{P}^{46}$, see pp. 290-322.

[12] The presence of *nomina sacra* and the manner our scribe used them are discussed in pp. 323-66.

history and development. When our scribe produced his manuscript, he was already acquainted with nine *nomina sacra* but showed no acquaintance of six (or more?) words that were also treated as *nomina sacra* in other manuscripts. He exhibited various levels of (in)consistency in contracting *nomina sacra* and in ascribing sacrality-value upon them. This is indicative that some of these *nomina sacra* were already well-advanced in development and already attained a sense of stability, both in terms of referents *and* forms of contraction; others were still in the process of refinement. But whilst the system was already in place, our scribe was not reluctant to impose his own preferences, especially in the manner these *nomina sacra* were presented visually in his manuscript.

ON SCRIBAL STUDIES AND METHODOLOGIES

If further progress is to happen in the field of scribal studies, especially as it relates to formulating an accurate and encompassing history of New Testament textual transmission, it is necessary for scholars and students alike to recognize that the pursuit for the most viable method of isolating scribal habits is still an on-going quest. While we have seen how this sub-discipline has leaped forward in the last fifty years, witnessing the birth of at least two major approaches (i.e., the Epp model and the Colwell-Royse model), we still need to pursue other alternative approaches that can further enhance our appreciation for this still unchartered "frontier" of the world of textual studies.

Concomitant to this recognition is the clarion call for a more careful (precise?) use of the term "scribal habits".[13] What we have done in this thesis has been to highlight this call—the call to have a more integrative approach in studying manuscripts and the traces left by those who produced them and those who eventually used them. These are very strong words, but I am afraid that unless we have fully looked at manuscripts as "socio-

[13] On this, see pp. 50-51.

cultural artefacts" also and not just as textual containers, we are then missing a very important historical component in our textual studies. In this sense, this thesis is a methodological attempt to bridge the gap between the *physical*, the *paratextual*, and the *textual* analyses in isolating the scribal habits of $\mathfrak{P}^{46}$.

DIRECTIONS FOR FUTURE RESEARCH

The time is now ripe to undertake more in-depth researches on the scribes of particular manuscripts—especially extensive manuscripts that connect the 2nd-3rd centuries and the fourth century onwards. We are now in a better position to "animate" these manuscripts—bringing back life to their scribes who "created" them—for the necessary tools and resources are now readily available.[14]

This thesis is a foundational work in the sense that using an *integrative* methodology to locate scribal habits has no particular precedence, and in the sense that the results are fundamental for future researches along this line. The strength of this thesis lies in the comprehensiveness, depth, and thoroughness of the collection and analyses of the data. Those who want detailed facts about material, paratextual, and textual features of a particular (extensive) manuscript will benefit from this study. Admittedly, the comprehensive details, including the ones hitherto undocumented, presented herein may not be to everyone's satisfaction, but the level of details is necessary so as to construct a fuller profile of our scribe. The result is, I believe, a reasonably secure evidentiary basis from which other studies can be based on. My hope is that others will engage this method, still at its developmental stage, whether to

[14] The on-going digitization of ancient manuscripts and making them freely available on-line to generate cooperation among individuals and institutions in fully facilitating exhaustive analyses of these manuscripts provide particular advantage for works such as what we did in this thesis.

challenge its methodological viability or to employ it in locating the scribal habits of other equally extensive manuscripts.

I must admit that identifying scribal habits of a particular manuscript offers no immediate implications for text-editing purposes, except for an indirect contribution where our ability to isolate variations that are borne out of the copying habits of particular scribes may help in forming value judgment as to which reading might be the best candidate to be regarded as the *Ausgangstext*. Even then the prevailing preference for a printed edition, either for its sentimental appeal or pragmatic effects, will continue to present a hurdle in designing an *apparatus criticus* that is exhaustive and inclusive of all the textual *and* physical idiosyncrasies of the manuscripts cited. Of course, the advent of the computing and electronic age is a very promising platform for this. In any case, more systematic works need to be done on other manuscripts from this methodological perspective, in order to appreciate the full scope of the interplay of the physical, paratextual, and textual features of the early witnesses to the New Testament text, and how every scribe of each manuscript laboured, under all conceivable conditions, in order to transmit the "Book" closest to the hearts of the early Christians because it was their "Scriptures". In this regard, we need to restore the "human face" in our studies of ancient manuscripts.

Let me end this thesis with a quote from a man who was alongside with me throughout this research journey, the man who challenged the way I think about "manuscripts and their texts":

> Without the study of manuscripts as physical entities, textual criticism can become a discipline out of touch with reality, dealing in variants with neither historical context nor manuscript tradition. If the examination of manuscripts were to cease, the *apparatus critici* would no longer possess meaning.[15]

[15] David Parker, "The Majuscule Manuscripts of the New Testament," in *TNTCR*[1], 22-42; repr. in *Manuscripts, Text, Theology*, 33-53, p. 52.

WORKS CITED

Aland, Barbara. "Sind Schreiber früher Neutestamentlicher Handschriften Interpreten des Textes?" Pages 114-22 in *Transmission and Reception: New Testament Text-Critical and Exegetical Studies.* Edited by J.W. Childers and D.C. Parker. Texts and Studies Third Series, Vol. 4. Piscataway, New Jersey: Gorgias Press, 2006.

_____. "The Significance of the Chester Beatty Papyri in Early Church History." Pages 108-21 in *Earliest Gospels.* Journal for the Study of New Testament Supplement Series 258. Edited by Charles Horton. London: T & T Clark International, 2004.

Aland, Barbara and Kurt Aland. *The Text of the New Testament: An Introduction to the Critical Editions and to the Theory and Practice of Modern Textual Criticism.* Revised and enlarged edition. Translated by Errol Rhodes. Grand Rapids, Michigan: Eerdmans, 1989.

Aland, Barbara and Jöel Delobel, eds. *New Testament Textual Criticism, Exegesis, and Early Church History: A Discussion of Methods.* Kampen: Pharos, 1994.

Aland, Barbara, Kurt Aland, Johannes Karavidopolous, Carlo M. Martini, and Bruce M. Metzger, eds. *Nestle-Aland Novum Testamentum Graece.* 27th Edition. Stuttgart: United Bible Societies, 1998 (*korrigierter Druck*).

_____. eds. *The Greek New Testament.* Fourth Edition. Stuttgart: United Bible Societies, 1994 (2nd print).

Aland, Kurt. "Der Schluß und die ursprüngliche Gestalt des Römerbriefes." Pages 284-301 in *Neutestamentliche Entwürfe.* Edited by Kurt Aland. München: Kaiser, 1979.

_____. *Repertorium der griechischen christlichen Papyri I: Biblische Papyri.* Patristische Texte und Studien 18. Berlin/New York: Walter de Gruyter, 1976.

_____. "The Greek New Testament: Its Present and Future Editions." *Journal of Biblical Literature* 87 (June 1968): 179-86.

Anderson, Charles. "The Epistle to the Hebrews and the Pauline Letter Collection." *Harvard Theological Review* 59 (Oct 1966): 429-38.

Anonymous. *Christianity in Egypt Project of the McQuarie University*. <http://www.acrc.mq.edu.au/PCE/docs/pceconspectus.pdf>.

Anonymous. "Rare Bible Manuscripts come to America." *The Science News Letter* 25/683 (May 12, 1934): 298.

Baarda, T. A. Hilhorst, G.P. Luttikhuizen, and A.S. van der Woude, eds. *Text and Testimony: Essays in honour of A.F.J. Klijn.* Kampen: J.H. Kok, 1988.

Bagnall, Roger S., *Early Christian Books in Egypt.* Princeton: Princeton University Press, 2009.

_____, ed. *The Oxford Handbook of Papyrology.* Oxford: Oxford University Press, 2009.

Bammel, Ernst and C.F.D. Moule, eds. *Jesus and the Politics of His Day.* Cambridge: Cambridge University Press, 1984.
Barker, Don. "P. Lond. Lit. 207 and the Origin of the Nomina Sacra: A Tentative Proposal." *Studia Humaniora Tartuensia* 8 (2007): 1-14.
_____. "The Dating of New Testament Papyri." *New Testament Studies* 57 (2011): 571-82.
Barrett, Charles Kingsley. *The Pastoral Epistles in the New English Bible; with Introduction and Commentary.* Oxford: Clarendon Press, 1963.
_____. *A Commentary on the First Epistle to the Corinthians: Black's New Testament Commentary.* London: Black, 1968.
Beare, Frank. "The Text of the Epistle to the Hebrews in $\mathfrak{P}^{46}$." *Journal of Biblical Literature* 63/4 (1944): 379-96.
Bell, Harold Idris. "Fragments of an Unknown Gospel." *British Museum Quarterly* 9/3 (Feb 1935): 71-73.
_____. "Early Codices from Egypt." *The Library* N.S. X (1904): 307ff.
Bell, Harold Idris and Theodore Cressey Skeat, *Fragments of an Unknown Gospel and Other Early Christian Papyri.* London: British Museum, 1935.
Benoit, P. "Le Codex Paulien Chester Beatty." *Reveu Biblique* 46 (1937): 58-82.
Bierbrier, Morris Leonard, ed. *Papyrus: Structure and Usage—British Museum Occasional Papers* 60. London: British Museum, 1986.
Binski, Paul and Patrick Zutshi. *Western Illuminated Manuscripts: A Catalogue of the Collection in Cambridge University Library; with the collaboration of Stella Panayotova.* Cambridge: Cambridge University Press, 2011.
Bischoff, Bernhard. *Latin Palaeography: Antiquity and the Middle Ages.* English edition. Translated by Dáibhí Ó Cróinín and David Ganz. Cambridge: Cambridge University Press, 1990.
Black, David Alan, ed. *Rethinking New Testament Textual Criticism.* Grand Rapids, Michigan: Baker, 2002.
_____, ed. *Scribes and Scriptures: New Testament Essays in Honour of John Harold Greenlee.* Winona Lake: Eisenbrauns, 1992.
Bonner, Campbell, ed. *A Papyrus Codex of the Shepherd of Hermas.* University of Michigan Studies-Humanistic Series XXII. Ann Arbor: University of Michigan Press, 1934.
_____. "New Biblical Papyri at the University of Michigan." *Harvard Theological Review* 25/2 (Apr 1932): 205-06.
Brown, Schuyler. "Concerning the Origin of the Nomina Sacra." *Studia Papyrologica* 9 (1970): 7-19.
Bruce, Frederick Fyvie. "Textual Problem in the Epistle to the Hebrews." Pages 27-39 in *Scribes and Scriptures: New Testament Essays in Honour of John Harold Greenlee.* Edited by David Alan Black. Winona Lake: Eisenbrauns, 1992.
_____. "The Chester Beatty Papyri." *The Harvester* 11 (1934): 163-64.
Buck, Charles. "The Early Order of the Pauline Corpus." *Journal of Biblical Literature* 68/4 (Dec 1949): 351-57.
Bülow-Jacobsen, Adam. "'Magna in latitudine earum differentia' (Pliny, NH XIII, 78)." *Zeitschrift für Papyrologie und Epigraphik* 60 (1985): 273-74.
_____. "Writing Materials in the Ancient World." Pages 3-29 in *The Oxford Handbook of Papyrology.* Edited by Roger S. Bagnall. Oxford: Oxford University Press, 2009.

Burkitt, Francis Crawford. "The Chester Beatty Papyri." *Journal of Theological Studies* 34 (1933): 363-68.

Carlson, Stephen C. "The Text of Galatians and Its History." PhD Dissertation, Duke University, 2012.

Cavallo, Guglielmo. "Γραμματα Αλεξανδρινα." *Jahrbuch der Österreichischen Byzantinistik* 24 (1975): 23-54.

_____. "Greek and Latin Writing in the Papyri." Pages 101-48 in *The Oxford Handbook of Papyrology*. Edited by Roger S. Bagnall. Oxford: Oxford University Press, 2009.

Cavallo, Guglielmo and Herwig Maehler, ed. *Hellenistic Bookhands.* Berlin/New York: Walter de Gruyter, 2008.

Chapa, Juan. "The Early Text of John." Pages 140-56 in *The Early Text of the New Testament.* Edited by Charles E. Hill and Michael J. Kruger. Oxford: Oxford University Press, 2012.

Charles, *Literal Translation of the Original Greek New Testament—*$\mathfrak{P}^{46}$. <http://chrles. multiply.com/photos/album/82/Bible_Papyrus_p46>. Accessed 1 February 2010.

Charlesworth, Scott. "Consensus Standardization in the Systematic Approach to *nomina sacra* in Second- and Third-Century Gospel Manuscripts." *Aegyptus* 86 (2006): 37–68.

_____. "Indicators of 'Catholicity' in Early Gospel Manuscripts." Pages 37-48 in *Early Text of the New Testament*. Edited by Charles Hill and Michael Kruger. Oxford: Oxford University Press, 2012.

_____. "T.C. Skeat, $\mathfrak{P}^{64+67}$ and $\mathfrak{P}^{4}$, and the Problem of Fiber Orientation in Codicological Reconstruction." *New Testament Studies* 53 (2007): 582–604.

Childers, Jeffrey and Curt Niccum. "'Anti-Feminist' Tendency in the 'Western' Text of Acts?" Pages 469-92 in *Essays on Women in Earliest Christianity.* Volume I. Edited by Carroll Osburn. MO: College Press, 1993.

Childers, J.W. and D.C. Parker, eds. *Transmission and Reception: New Testament Text-Critical and Exegetical Studies.* Texts and Studies Third Series, Volume 4. Piscataway, New Jersey: Gorgias Press, 2006.

Clark, Kenneth. "Manuscripts belong to Archaeology." *Bulletin of the American Schools of Oriental Research* 122 (April 1951): 7-9.

_____. "Note on the Beatty-Michigan Pauline Papyrus." *Journal of Biblical Literature* 55 (March 1936): 83-84.

_____. "The Theological Relevance of Textual Variation in Current Criticism of the Greek New Testament." *Journal of Biblical Literature* 85 (1966): 1-16.

Clivaz, Claire. "A New New Testament Papyrus: $\mathfrak{P}^{116}$." *Early Christianity* 1 (2010): 158-62.

Coles, R.A., M. Manfredi, P.J. Sijpesteijn, and A.S. Brown, eds. *The Harris Papyri.* Volume 2. The Netherlands: Terra, 1985.

Collins, Raymond, F. "The Case of a Wandering Doxology: Rom 16,25-27." Pages 293-303 in *New Testament Textual Criticism and Exegesis: A Festschrift for J. Delobel.* Bibliotheca Ephemeridum Theologicarum Lovaniensium CLXI. Edited by A. Denaux. Leuven: Leuven University Press/Peeters, 2002.

Colwell, Ernest Cadman. Review of Henry Sanders, *A Third Century Papyrus Codex. Journal of Religion* 16/1 (Jan 1936): 96-98.

_____. Review of Henry Sanders, *A Third Century Papyrus Codex*. *Classical Philology* 32/4 (Oct 1937): 385-87.

_____. "Scribal Habits in Early Papyri: A Study in the Corruption of the Text." Pages 370-89 in *The Bible in Modern Scholarship: Papers Read at the 100th Meeting of SBL*. Edited by Philip Hyatt. Tennessee: Abingdon Press, 1965. Reprinted as "Method in Evaluating Scribal Habits: A Study of $\mathfrak{P}^{45}$, $\mathfrak{P}^{66}$, $\mathfrak{P}^{75}$." Reprinted, pages 106-24 in *Studies in Methodology in Textual Criticism of the New Testament.*

_____, ed. *Studies in Methodology in Textual Criticism of the New Testament.* New Testament Studies and Tools 9. Leiden: Brill, 1969.

_____. "The Origin of Texttypes of New Testament Manuscripts". Pages 128-38 in *Early Christian Origins: Studies in Honor of Harold R. Willoughby.* Edited by Allen Wikgren. Chicago: Quadrangle Books, 1961. Reprinted as "Methods in Establishing the Nature of Text-Types of New Testament Manuscripts". Reprinted, pages 45-55 in *Studies in Methodology in Textual Criticism of the New Testament.* New Testament Studies and Tools 9. Leiden: Brill, 1969

Colwell, Ernest Cadman and Ernst Tune. "Method in Classifying and Evaluating Variant Readings." Pages 96-105 in *Studies in Methodology in Textual Criticism of the New Testament.* New Testament Studies and Tools 9. Leiden: Brill, 1969.

Comfort, Philip W. *Encountering the Manuscripts: An Introduction to New Testament Palaeography and Textual Criticism.* B & H Academic, 2005.

_____. "Light from the New Testament Papyri Concerning the Translation of πνευμα." *The Bible Translator* 35 (1984): 130-33.

_____. *New Testament Text and Translation Commentary: Commentary on the Variant Readings of the Ancient New Testament Manuscripts and how they relate to the major English Translations.* Carol Stream, Illinois: Tyndale House, 2008.

Comfort, Philip W. and David P. Barrett, eds. *The Text of the Earliest New Testament Greek Manuscripts: A Corrected, Enlarged Edition of The Complete Text of the Earliest New Testament Manuscripts*. Wheaton: TyndaleHouse, 2001.

Conrad, Edgar W. and Edward G., eds. *Perspectives on Language and Text: Essays and Poems in Honor of Francis I. Andersen's Sixtieth Birthday.* Winona Lake: Eisenbrauns, 1987.

Crisp, Simon. "$\mathfrak{P}^{46}$ and Good as New: The Bearing of Manuscript Evidence on Decisions about Canon." Pages 115-23 in *Canon and Modern Bible Translation in Interconfessional Perspective.* Edited by Lenart J. de Regt. Istanbul: United Bible Societies, 2006.

Cronert, Wilhelm. *Memoria graeca Herculanensis.* Lipsiae: B.G. Teuber, 1903.

Dain, Alphonse. *Les Manuscrits.* Collection d'études anciennes. 3rd edition. Paris: Les Belles Lettres, 1975.

de Bruyn, Theodore. "Papyri, Parchments, Ostraca and Tablets written with Biblical Texts in Greek and Used as Amulets: A Preliminary List." Pages 145-89 in *Early Christian Manuscripts: Examples of Applied Method and Approach.* Texts and Editions for New Testament Study 5. Edited by Thomas J. Kraus and Tobias Nicklas. Leiden: Brill, 2010.

de Regt, Lenart J., ed. *Canon and Modern Bible Translation in Interconfessional Perspective.* Istanbul: United Bible Societies, 2006.

Denaux, Adelbert (ed.). *New Testament Textual Criticism and Exegesis: A Festschrift for Jöel Delobel.* Bibliotheca Ephemeridum Theologicarum Lovaniensium CLXI. Leuven: Leuven University Press/Peeters, 2002.

Dibelius, Martin and Hans Conzelmann. *Die Pastoralbriefe.* 4th edition. Tübingen: Mohr Siebeck, 1966.

Dickey, Eleanor. "The Greek and Latin Languages in the Papyri." Pages 149-69 in *The Oxford Handbook of Papyrology.* Edited by Roger S. Bagnall. Oxford: Oxford University Press, 2009.

Dimarogonas, Andrew. "Pliny the Elder on the Making of Papyrus Paper." *Classical Quarterly* 45/2 (1995): 588-90.

Duff, Jeremy. "$\mathfrak{P}^{46}$ and the Pastorals: A Misleading Consensus?" *New Testament Studies* 44 (1998): 578-90.

Dwight, Edgar. An *English Translation of* $\mathfrak{P}^{46}$. http://www.cbl.ie/getdoc/c6212daf-30bd-4c1a-9e85-2b09839d39d0/1.aspx. Accessed 12 December 2009.

Dykstra, Tom. "A Review of David Trobisch and David Parker on the Origin of the New Testament, the Historical Jesus, and How Manuscripts can Reveal what Texts Conceal," *Journal of Orthodox Center for the Advancement of Biblical Studies* 2/1 (2009). http://ocabs.org/journal/index.php/jocabs/article/viewFile/41/16. Accessed 30 Aug 2012.

Ebojo, Edgar. "Scribal Tendencies in the Singular Readings of $\mathfrak{P}^{46}$." M.Theol. Thesis, Trinity Theological College, Singapore, 2006.

_____. "How Persuasive is the 'Persuasive Words of Human Wisdom'? The Shortest Reading in 1 Corinthians 2.4." *The Bible Translator Technical Papers* 60 (2009): 1-33.

Ehrman, Bart. "Textual Criticism of the New Testament." Pages 127-45 in *Hearing the New Testament: Strategies for Interpretation.* Edited by Joel B. Green. Grand Rapids, Michigan/Carlisle: Eerdmans/Paternoster, 1995.

_____. *The Orthodox Corruption of Scripture: The Effect of Early Christological Controversies on the Text of the NT.* Oxford: Oxford University Press, 1993; *Updated edition with a New Afterword.* Oxford: Oxford University Press, 2011.

_____. *Misquoting Jesus: The Story Behind Who Changed the Bible and Why.* San Francisco: HarperSanFrancisco, 2005.

Ehrman, Bart and Michael Holmes, eds. *The Text of the New Testament in Contemporary Research: Essays on the* Status Quaestionis*, Essays in Honour of Bruce M. Metzger*. Studies and Documents 46. Grand Rapids, Michigan: Eerdmans, 1995.

_____. *The Text of the New Testament in Contemporary Research: Essays on the* Status Quaestionis, *Second Edition. New Testament Tools, Studies and Documents* 42. Leiden/Boston: Brill, 2013.

Elliott, James Keith, ed. *Collected Biblical Writings of T.C. Skeat.* Supplement to *Novum Testamentum* 113. London/Boston: Brill, 2004.

_____. "Recent Trends in the Textual Criticism of the New Testament: A New Millennium, A New Beginning?" *BABELAO* 1 (2012): 117-36.

_____. Review of Klaus Junack, E. Güting, U. Nimtz, and Klaus Witte's *Das Neue Testament auf Papyrus, II Die Paulinischen Briefe, Teil I: Rom., I Kor., II Kor. Novum Testamentum* 31/4 (Oct 1989): 381-83.

_____. Review of Klaus Wachtel and Klaus Witte, *Das Neue Testament auf Papyrus II Die Paulinischen Briefe Teil 2. Novum Testamentum* 37/3 (July 1995): 302-03.

_____. "Singular Readings in the Gospel Text of $\mathfrak{P}^{45}$." Pages 122-31 in *The Earliest Gospels: The Origins and Transmission of the Earliest Christian Gospel – The Contribution of the Chester Beatty Gospel Codex* $\mathfrak{P}^{45}$. Journal for the Study of the New Testament Supplement Series 258. Edited by Charles Horton. London: T & T Clark International, 2004.

_____, ed. *Studies in NT Language and Text: Essays in Honor of G. D. Kilpatrick on the occasion of his 65th Birthday. Novum Testamentum* Supplement Series 44. Leiden: Brill, 1978.

_____. *The Greek Text of the Epistles to Timothy and Titus.* Studies and Documents 36. Edited by Jacob Geerlings. Salt Lake: University of Utah Press, 1968.

Ellingworth, Paul. *The Epistle to the Hebrews: Commentary on the Greek Text,* New International Greek Testament Commentary. Grand Rapids, Michigan/Carlisle: Eerdmans/ Paternoster, 1993.

Epp, Eldon Jay. "Issues in the Interrelation of New Testament Textual Criticism and Canon." Pages 485-515 in *The Canon Debate: On the Origins and Formation of the Bible.* Edited by Lee Martin McDonald and James A. Sanders. Peabody: Hendrickson, 2002. Reprinted, pages 595-639 in *Perspectives on New Testament Textual Criticism.*

_____. "Issues in New Testament Textual Criticism: Moving from Nineteenth Century to the Twenty-First Century." Pages 641-97 in *Rethinking New Testament Textual Criticism.* Edited by David Alan Black. Grand Rapids, Michigan: Baker Academic, 2002. Reprinted, pages 641-97 in *Perspectives on New Testament Textual Criticism.*

_____. "Minor Textual Variants in Romans 16:7." Pages 123-41 in *Transmission and Reception: New Testament and Exegetical Studies.* Text and Studies Third Series, Volume 4. Edited by J.W. Childers and D.C. Parker; Piscataway, NJ: Gorgias Press, 2006.

_____, ed. *Perspectives on New Testament Textual Criticism: Collected Essays, 1962-2004.* Supplements to *Novum Testamentum* 116. Leiden/Boston: Brill, 2006.

_____. "Text-Critical, Exegetical, and Socio-Cultural Factors affecting the Junia /Junias Variation in Romans 16,7." Pages 227-91 in *New Testament Textual Criticism and Exegesis: A Festschrift for J. Delobel.* Bibliotheca Ephemeridum Theologicarum Lovaniensium CLXI. Edited by A. Denaux. Leuven: Leuven University Press/Peeters, 2002.

_____. "Textual Criticism in the Exegesis of the New Testament, with an Excursus on Canon." Pages 45-97 in *Handbook to Exegesis of the New Testament.* New Testament Tools and Studies 25. Edited by Stanley E. Porter. Leiden: Brill, 1997. Reprinted, pages 461-96 in *Perspectives on NT Textual Criticism: Collected Essays, 1962-2004.* Supplements to *Novum Testamentum* 116. Leiden/Boston: Brill, 2006.

_____. "Textual Criticism". Pages 75-126 in *The New Testament and Its Modern Interpreters.* (Edited by E.J. Epp and G.W. MacRae. Philadelphia: Fortress, 1989. Reprinted as "Decision Points in Past, Present, and Future New Testament Textual Criticism." Pages 227-84 in *Perspectives on New Testament Textual Criticism: Collected Essays, 1962-2004.* Supplements to *Novum Testamentum* 116. Leiden/Boston: Brill, 2006.

_____. "The Papyrus Manuscripts of the New Testament." In *Text of the New Testament in Contemporary Research: Essays on the* Status Quaestionis. *New Testament Tools-Studies and Documents* 46. Edited by Bart D. Ehrman and Michael W. Holmes. Grand Rapids, Michigan: Eerdmans, 2013.

_____. "The Significance of the Papyri for Determining the Nature of the New Testament Text in the Second Century: A Dynamic View of Textual Transmission." Pages 71-103 in *Gospel Traditions in the Second Century.* Studies in Christianity and Judaism in Antiquity 3. Edited by W.L. Petersen. Notre Dame, IN: University of Notre Dame Press, 1989. Reprinted, pages 274-97 in Epp and Fee, *Studies in the Theory and Method of New Testament Textual Criticism.* Studies and Documents 45. Grand Rapids, Michigan: Eerdmans, 1993.

_____. *The Theological Tendency of Codex Bezae Cantabrigensis in Acts.* Society of New Testament Studies Monograph Series 3. Cambridge: Cambridge University Press, 1966.

_____. "Traditional 'Canons' of New Testament Textual Criticism: Their Value, Validity, and Viability—or Lack Thereof." Pages 79-127 in *The Textual History of the Greek New Testament: Changing Views in Contemporary Research.* Edited by Klaus Wachtel and Michael W. Holmes. Text-Critical Studies 8. Atlanta: Society of Biblical Literature, 2011.

Epp, Eldon Jay and Gordon D. Fee, eds. *New Testament Textual Criticism, Significance for Exegesis: Essays in Honour of Bruce Metzger.* Oxford: Clarendon, 1981.

_____. *Studies in the Theory and Method of New Testament Textual Criticism.* Studies and Documents 45. Grand Rapids, Michigan: Eerdmans, 1993.

Epp, Eldon Jay and G.W. MacRae, eds. *The New Testament and Its Modern Interpreters.* Philadelphia: Fortress, 1989.

Eshbaugh, Howard. "Theological Variants in the Western Text of the Pauline Corpus." Ph.D. Dissertation, Case Western Reserve University, 1975.

_____. "Textual Variants and Theology: A Study of the Galatians Text in $\mathfrak{P}^{46}$." *Journal for the Study of the New Testament* 3 (1979): 60-72. Reprinted, pages 81-91 in *New Testament Text and Language: A Sheffield Reader.* Biblical Seminar 4. Edited by Stanley Porter and Craig Evans. Sheffield: Academic Press, 1997.

Evans, Craig and H. Daniel Zacharias, eds. *Jewish and Christian Scripture as Artifact and Canon.* London/NY: T&T Clark, 2009.

Eyre, C.J., M.A. Leahy, and L.M. Leahy, eds. *The Unbroken Reed: Studies in the Culture and Heritage of Ancient Egypt, in Honour of A. F. Shore.* London: Egypt Exploration Society, 1994.

Fee, Gordon. "Corrections in Papyrus Bodmer II and Early Textual Transmission." *New Testament Studies* 7 (1965): 247-57.

_____. "Rigorous or Reasoned Eclecticism – Which?" Pages 174-97 in *Studies in New Testament Language and Text*. Supplements to *Novum Testamentum* 44. Edited by James Keith Elliott. Leiden: Brill, 1976. Reprinted, pages 124-40 in *Studies in the Theory and Methods of NT Textual Criticism*.

_____. *The First Epistle to the Corinthians, New International Commentary of the New Testament.* Grand Rapids, Michigan: Eerdmans, 1987.

_____. "The Significance of Papyrus Bodmer II and Papyrus Bodmer XIV-XV for Methodology in New Testament Textual Criticism." PhD Dissertation, University of Southern California, 1966.

_____. "$\mathfrak{P}^{75}$, $\mathfrak{P}^{66}$, and Origen: The Myth of Early Textual Recension in Alexandria". Pages 247-73 in *Studies in the Theory and Method of New Testament Textual Criticism.* Edited by Eldon Jay Epp and Gordon Fee. Grand Rapids, Michigan: Eerdmans, 1993.

Finegan, Jack. *Encountering New Testament Manuscripts: A Working Introduction to Textual Criticism.* London: SPCK, 1974.

_____. "The Original Form of the Pauline Canon." *Harvard Theological Review* 49/2 (Apr 1956): 85-103.

Finney, Timothy J. "A Proposed Reconstruction of Hebrews 7:28a in $\mathfrak{P}^{46}$." *New Testament Studies* 40 (1994): 472-73.

Flink, Timo. *Textual Dilemma: Studies in the Second-Century Text of the New Testament.* University of Joensuu Publications in Theology 21. Joensuu: University of Joensuu, 2009.

Gallo, Italo. *Greek and Latin Papyrology: Classical Handbook 1.* Translated by Maria Rosaria Falivene and Jennifer March. London: University of London, 1986.

Gamble, Harry. *Books and Readers in the Early Church: A History of Early Christian Texts.* New Haven/London: Yale University Press, 1995.

_____. *The Textual History of the Letter to the Romans: A Study in Textual and Literary Criticism*. Studies and Documents 42. Edited by Irving Alan Sparks. Grand Rapids, Michigan: Eerdmans, 1977.

Gerstinger, Hans. "Ein Fragment des Chester Beatty-Evangelienkodex in der Papyrussammlung der Nationalbibliothek in Wien (Pap. graec. Vindob. 31974)." *Aegyptus* 13/1 (1933): 67-72.

Giversen, Søren. "The Pauline Epistles on Papyrus." Pages 211-12 in *Die Paulinische Literatur und Theologie/The Pauline Literature and Theology.* Edited by Sigfred Pedersen; Göttingen: Vandenhoech & Ruprecht, 1980.

Goodspeed, Edgar. *Introduction to the New Testament.* Chicago: University of Chicago Press, 1937.

_____. "The World's Oldest Bible." *Journal of Biblical Literature* 54/2 (1935): 126.

Grafton, Anthony and Megan Williams. *Christianity and the Transformation of the Book: Origin, Eusebius, and the Library of Caesarea.* Cambridge, Massachusetts/London: Belknap Press of the Harvard University Press, 2006.

Grant, Robert. *A Historical Introduction to the New Testament.* London: Collins, 1963.

Graux, Charles. "Nouvelles recherches sur la stichométrie." *Revue de philologie de littérature et d'histoire anciennes* 2/2 (Apr 1878): 97-143.

Grenfell, Bernard and Arthur Hunt. *Amherst Papyri. Part I*. London: Henry Frowde/ Oxford University Press Warehouse, 1900.

Griffin, Bruce. "The Palaeographical Dating of $\mathfrak{P}^{46}$." Paper presented at the SBL Annual Meeting-New Testament Textual Criticism Section, 1997.
Güting, Eberhard. "The Methodological Contribution of Günther Zuntz to the Text of Hebrews." *Novum Testamentum* 48/4 (2006): 359-78.
Guthrie, Donald. *The Pastoral Epistles.* Tyndale New Testament Commentary. London: Tyndale Press, 1957.
Haines-Eitzen, Kim. *Guardians of Letters: Literacy, Power, and the Transmitters of Early Christian Literature.* Oxford: Oxford University Press, 2000.
_____. "The Social History of Early Christian Scribes." Pages 479-96 in *The Text of the New Testament in Contemporary Research: Essays on the Status Quaestionis.* 2nd edition. Edited by Bart Ehrman and Michael Holmes. Leiden: Brill, 2013.
Hanson, R.P.C. "The Ideology of Codex Bezae in Acts." *New Testament Studies* 14 (1967-68): 282-86.
Harris, John Rendel. "Stichometry Part I." *American Journal of Philology* 4/2 (1883): 133-57.
_____. "Stichometry Part II." *American Journal of Philology* 4/3 (1883): 309-31.
_____. "Was the Diatesseron Anti-Judaic?" *Harvard Theological Review* 18 (1925): 103-09.
Harris, William. "Why did the Codex Supplant the Book-Roll?" Pages 71-85 in *Renaissance Society and Culture: Essays in Honor of Eugene F. Rice, Jr.* Edited by John Monfasani and Ronald G. Musto. New York: Italica Press, 1991.
Hatch, William H.P. "The Position of Hebrews in the Canon of the New Testament." *Harvard Theological Review* 29/2 (Apr 1936): 133-51.
Head, Peter, M. "Christology and Textual Transmission: Reverential Alterations in the Synoptic Gospels." *Novum Testamentum* 35 (1993): 105-29.
_____. "'If Jesus had given them rest' Heb 4:8 in $\mathfrak{P}^{46}$." <http://evangelicaltextualcriticism.blogspot.com/2010/06/if-jesus-had-given-them-rest-heb-48-in.html>. Accessed 15 November 2010.
_____. "Observations of Early Papyri of the Synoptic Gospels, especially on the Scribal Habits." *Biblica* 71 (1990): 240-47.
_____. "Scribal Behavior and Theological Tendencies in Singular Readings in Papyrus Bodmer II ($\mathfrak{P}^{66}$)." Pages 55-74 in *Textual Variation: Theological and Social Tendencies? Papers from the Fifth Birmingham Colloquium on the Textual Criticism of the New Testament.* Text and Studies: Third Series, 5. Edited by Hugh A.G. Houghton and David C. Parker. New Jersey: Gorgias Press, 2008.
_____. "Significant Spaces in $\mathfrak{P}^{46}$". <http://evangelicaltextualcriticism.blogspot.com/2009/08/significant-spaces-in-p46.html>. Accessed 23 July 2011.
_____. "The Date of the Magdalene Papyrus of Matthew (P. Magd. Gk. 17 = $\mathfrak{P}^{64}$): A Response to C. P. Thiede." *Tyndale Bulletin* 46 (1995): 251–85.
_____. "The Gospel of Mark in Codex Sinaiticus: Textual and Reception-Critical Considerations." *Textual Criticism Journal* 13 (2008): 1–38.
_____. "The Habits of NT Copyists: Singular Readings in the Early Fragmentary Papyri of John." *Biblica* 85 (2004): 399-408.
Heath, Jane. "*Nomina Sacra* and *Sacra Memoria* before the Monastic Age." *Journal of Theological Studies* 61/2 (2010): 516-49.

Heide, K. Martin. "Assessing the Stability of the Transmitted Texts of the New Testament and the *Shepherd of Hermas*." Pages 125-59 in *The Reliability of the New Testament*. Edited by Robert B. Stewart. Minneapolis: Fortress Press, 2011.

Hendriks, I.H.M., "Pliny, Naturalis Historia XIII, 74-82 and the Manufacture of Papyrus." *Zeitschrift für Papyrologie und Epigraphik* 37 (1980): 121-36.

Hepper F.N. and T. Reynolds. "Papyrus and the Adhesive Properties of its Cell Sap in relation to Paper-making." *Journal of Egyptian Archaeology* 53 (1967): 156-57.

Héring, Jean. *The First Epistle of Saint Paul to the Corinthians.* Translated by A.W. Heathcote and P.J. Allcock. London: Epworth, 1962.

Hernández, Juan Jr. *Scribal Habits and Theological Influences in the Apocalypse.* Wissenschaftliche Untersuchungen zum Neuen Testament 2. Reihe 218. Tübingen: Mohr Siebeck, 2006.

_____. "Scribal Tendencies in the Apocalypse: Starting the Conversation." Pages 248-60 in *Jewish and Christian Scripture as Artifact and Canon.* Edited by Craig Evans and H. Daniel Zacharias. London/NY: T&T Clark, 2009.

Hill, Charles. "Did the Scribe of $\mathfrak{P}^{52}$ use the Nomina Sacra? Another Look." *New Testament Studies* 48 (2002): 587-92.

Hill, Charles E. and Michael J. Kruger, eds. *The Early Text of the New Testament.* Oxford: Oxford University Press, 2012.

_____. "Introduction: In Search of the Earliest Text of the New Testament." Pages 1-19 in *The Early Text of the New Testament.* Edited by Charles E. Hills and Michael J. Kruger; Oxford: Oxford University Press, 2012.

Hillard, T.W., R.A. Kearsley, C.E.V. Nixon and A.M. Nobbs, eds. *Ancient History in a Modern University, Volume 2: Early Christianity, Late Antiquity and Beyond.* New South Wales/Grand Rapids, Michigan: Macquarie University Press/Eerdmans, 1998.

Holmes, Michael. "Text and Transmission in the Second Century." Pages 61-79 in *The Reliability of the New Testament: Bart D. Ehrman & Daniel C. Wallace in Dialogue.* Edited by Robert B. Stewart. Minneapolis: Fortress Press, 2011.

_____ (ed.). *The Greek New Testament: SBL Edition.* Atlanta/Bellingham: Society of Biblical Literature and Logos Bible Software, 2010.

_____. "The Text of $\mathfrak{P}^{46}$: Evidence of the Earliest 'Commentary' on Romans." Pages 189-206 in *New Testament Manuscripts: Their Texts and Their World.* Edited by Thomas J. Kraus and Nicklas Tobias. Text and Editions for New Testament Study 2; Leiden/Boston: Brill, 2006.

_____. "The Text of the Epistles Sixty Years After: An Assessment of Günther Zuntz's Contributions to Text-Critical Methodology and History". Pages 89-113 in *Transmission and Reception: New Testament and Exegetical Studies.* Text and Studies 3rd Series, Volume 4. Edited by J.W. Childers and D.C. Parker; Piscataway, NJ: Gorgias Press, 2006.

Hort, Fenton John Anthony. *The New Testament in Original Greek: Introduction.* Cambridge and London: MacMillan and Co., 1881.

Horton, Charles. "'It was all a great adventure': Alfred Chester Beatty & the Formation of his Library." *History Ireland* 8/2 (2000): 37-42.

_____. "The Chester Beatty Biblical Papyri: A Find of the Greatest Importance." Pages 149-60 in *The Earliest Gospels: The Origins and Transmission of the Earliest Christian Gospel – The Contribution of the Chester Beatty Gospel Codex* $\mathfrak{P}^{45}$. Journal for the Study of the New Testament Supplement Series 258. Edited by Charles Horton. London: T & T Clark International, 2004.

_____, ed. *The Earliest Gospels: The Origins and Transmission of the Earliest Christian Gospel—The Contribution of the Chester Beatty Gospel Codex* $\mathfrak{P}^{45}$. Journal for the Study of the New Testament Supplement Series 258. London: T & T Clark International, 2004.

Hoskier, Herman. "A Study of the Chester Beatty Codex of the Pauline Epistles." *Journal of Theological Studies* 38 (1937): 148-63.

_____. *Appendix to an Article on the Chester-Beatty Papyrus of the Pauline Epistles known as* $\mathfrak{P}^{46}$. Oxford: Oxford University Press, 1937.

_____. $\mathfrak{P}^{46}$*: Addenda et corrigenda.* Oxford: Oxford University Press, 1937.

_____. *A Commentary on the Various Readings in the Text of the Epistle to the Hebrews in the Chester-Beatty Papyrus* $\mathfrak{P}^{46}$ *(circa 200 A.D.).* London: Bernard Quaritch, 1938.

Houghton, Hugh Alexander Gervase. "Recent Developments in New Testament Textual Criticism." *Early* Christianity 2.2 (2011): 245-68.

Houghton, Hugh Alexander Gervase and David C. Parker, eds. *Textual Variation: Theological and Social Tendencies? Papers from the Fifth Birmingham Colloquium on the Textual Criticism of the New Testament.* Text and Studies, Third Series, Volume 6. New Jersey: Gorgias Press, 2008.

Hurtado, Larry. "$\mathfrak{P}^{52}$ (P. Rylands Gk. 457) and Nomina Sacra: Method and Probability." *Tyndale Bulletin* 54 (2003): 1-14.

_____. "Developments and Directions in NT Textual Criticism." Pages 26-48 in *Studies in the Early Text of the Gospels and Acts: The Papers of the First Birmingham Colloquium on the Textual Criticism of the NT.* Edited by David Taylor. Atlanta: SBL, 1999.

_____. "Early Christian Manuscripts as Artifacts." Pages 66-81 in *Jewish and Christian Scripture as Artifact and Canon.* Edited by Craig A. Evans and H. Daniel Zacharias. Library of Second Temple Studies 13. London/NY: T & T Clark, 2009.

_____. "Manuscripts and the Sociology of Early Christian Readings." Pages 49-62 in *The Early Text of the New Testament.* Edited by Charles E. Hill and Michael J. Kruger. Oxford: Oxford University Press, 2012.

_____. Review of Roger Bagnall, *Early Christian Books in Egypt*. *Review of Biblical Literature* 2010. <http://www.bookreviews.org/pdf/7289_7933.pdf>. Accessed 15 May 2011.

_____. *Text-Critical Methodology and the Pre-Caesarean Text: Codex W in the Gospel of Mark.* Studies and Documents 43. Grand Rapids, Michigan: Eerdmans, 1981.

_____. "The Doxology at the End of Romans." Pages 185-99 in *New Testament Textual Criticism, Significance for Exegesis: Essays in Honour of Bruce Metzger.* Edited by Eldon J. Epp and Gordon D. Fee. Oxford: Clarendon, 1981.

_____. *The Earliest Christian Artifacts: Manuscripts and Christian Origins.* Grand Rapids, MI/Cambridge: Eerdmans, 2006.

_____. "The Earliest Evidence of an Emerging Christian Material and Visual Culture: The Codex, The *Nomina Sacra* and the Staurogram." Pages 271-88 in *Text and Artifact in the Religions of Mediterranean Antiquity: Essays in Honour of Peter Richardson.* Edited by Stephen G. Wilson and Michael Desjardins. Studies in Christianity and Judaism/Études sur le Christianisme et le Judaïsme. Waterloo: Wilfrid Laurier University Press, 2000.

_____, ed. *The Freer Biblical Manuscripts: Fresh Studies of an American Treasure Trove.* Atlanta: Society of Biblical Literature, 2006.

_____. "The Origin of the Nomina Sacra: A Proposal." *Journal of Biblical Literature* 117 (1998): 655-73.

_____. "The Staurogram in Early Christian Manuscripts: The Earliest Visual Reference to the Crucified Jesus?" Pages 207-26 in *New Testament Manuscripts: Their Text and Their World.* Edited by Thomas J. Kraus and Tobias Nicklas. Boston/Leiden: Brill, 2006.

Hyatt, Philip, ed. *The Bible in Modern Scholarship.* Nashville: Abingdon, 1965.

Jang, Min Seok. "A Reconsideration of the Date of Papyrus 46." PhD Dissertation, New Orleans Baptist Theological Seminary, 2010.

Jaroš, Carl. *Das Neue Testament nach den ältesten Griechischen Handschriften die handschriftliche griechische Überlieferung des Neuen Testaments vor Codex Sinaiticus und Codex Vaticanus.* Echter Verlag, Wien und Würzburg, 2006.

Jeremias, Joachim. *Die Briefe und Titus. Der Brief an die Hebräer.* Neue Testament Deutsch 9: Vandenhoeck & Ruprecht: Göttingen, 1934.

Jongkind, Dirk. *Scribal Habits of Codex Sinaiticus.* Text and Studies Third Series, Volume 5. Piscataway, New Jersey: Gorgias Press, 2007.

_____. "Singular Readings in Sinaiticus: The Possible, The Impossible, and the Nature of Copying." Pages 35-54 in *Textual Variation: Theological and Social Tendencies? Papers from the Fifth Birmingham Colloquium on the Textual Criticism of the New Testament.* Text and Studies: Third Series, Volume 6. Edited by Hugh A.G. Houghton and David C. Parker. New Jersey: Gorgias Press, 2008.

Johnson, William A. *Bookrolls and Scribes in Oxyrhynchus*. Toronto: University of Toronto Press, 2004.

_____. "Pliny the Elder and Standardized Roll Heights in the Manufacture of Papyrus." *Classical Philology* 88/1 (1993): 46-50.

_____. "The Ancient Book." Pages 256-81 in *The Oxford Handbook of Papyrology.* Edited by Roger S. Bagnall. Oxford: Oxford University Press, 2009.

_____. "The Function of the Paragraphus in Greek Prose Texts." *Zeitschrift für Papyrologie und Epigraphik* 100 (1994): 65-68.

Judge, Edwin A. "The Magical Use of Scripture in the Papyri." Pages 339-49 in *Perspectives on Language and Text: Essays and Poems in Honor of Francis I. Andersen's Sixtieth Birthday.* Edited by E.W. Conrad and E.G. Newing. Winona Lake: Eisenbrauns, 1987.

Junack, Klaus. "Abschreibpraktiken und Schreibergewohnheiten in ihrer Auswirkung auf die Textüberlieferung." Pages 277-95 in *New Testament Textual Criticism.* Edited by E.J. Epp and G.D. Fee; Oxford: Clarendon Press, 1981.

Junack, Klaus, E. Güting, U. Nimtz, and Klaus Witte, *Das Neue Testament auf Papyrus, II Die Paulinischen Briefe, Teil I: Rom., I Kor., II Kor.* Arbeiten zur Neutestamentlichen Textforschung 12; Berlin and New York: Walter de Gruyter, 1989.

Kannaday, Wayne. *Apologetic Discourse and the Scribal Tradition: Evidence of the Influence of Apologetic Interests on the Text of the Canonical Gospels.* Text-Critical Studies 5. Edited by James Adair. Atlanta: Society of Biblical Literature, 2004.

_____. "'Are Your Intentions Honorable?' Apologetic Interests and the Scribal Revision of Jesus in the Canonical Gospels." *TC: A Journal of Biblical Textual Criticism* <http://rosetta.reltech.org/TC/v11/Kannaday2006.html>. Accessed 25 February 2010.

Kelly, J.N.D. *Commentary on the Pastoral Epistles.* London: A. & C. Black, 1963.

Kenyon, Frederic G. *Books and Readers in Ancient Greece and Rome.* 2nd edition. Oxford: Clarendon Press, 1951.

_____. "*Nomina Sacra* in the Chester Beatty papyri." *Aegyptus* 13/1 (1933): 5-10.

_____. *Our Bible and the Ancient Manuscripts.* Revised by A. W. Adams. London: Eyre & Spottiswoode, 1958.

_____. *Recent Developments in the Textual Criticism of the Greek Bible, The Schweich Lectures of the British Academy, 1932.* London: British Academy, 1933.

_____. Review of Henry A. Sanders, *A Third-Century Papyrus Codex of Saint Paul. American Journal of Philology* 57/1 (1936): 91-95.

_____. *The Bible and Modern Scholarship.* London: John Murray, 1948.

_____. *The Chester Beatty Biblical Papyri: Descriptions and Texts of the Twelve Manuscripts on Papyrus of the Greek Bible, Fasciculus I: General Introduction, with plates.* London: Emery Walker, 1933.

_____. *The Chester Beatty Biblical Papyri: Descriptions and Texts of the Twelve Manuscripts on Papyrus of the Greek Bible, Fasciculus II: Gospels and Acts, Texts.* London: Emery Walker, 1933.

_____. *The Chester Beatty Biblical Papyri: Descriptions and Texts of the Twelve Manuscripts on Papyrus of the Greek Bible, Fasciculus III, Pauline Epistles and Revelation, Text.* London: Emery Walker, 1934.

_____. *The Chester Beatty Biblical Papyri: Descriptions and Texts of the Twelve Manuscripts on Papyrus of the Greek Bible, Fasciculus III, Supplement, Pauline Epistles, Text.* London: Emery Walker, 1936.

_____. *The Chester Beatty Biblical Papyri: Descriptions and Texts of the Twelve Manuscripts on Papyrus of the Greek Bible, Fasciculus III, Supplement, Pauline Epistles, Plates.* London: Emery Walker, 1937.

_____. *The Palaeography of the Greek Papyri.* Oxford: Clarendon Press, 1899.

_____. "The Text of the Bible: A New Discovery—More Papyri from Egypt." *The London Times* (19 Nov 1931): 13-14.

Kilpatrick, George D. "The Chester Beatty Papyrus $\mathfrak{P}^{46}$ and Hebrews xi.4." *Journal of Theological Studies* 62 (1941): 68-69.

Kim, Young Kyu. "Paleographical Dating of $\mathfrak{P}^{46}$ to the Later First Century." *Biblica* 69 (1988): 248-57.

Kleberg, Tönnes. *Buchandel und Verlagswesen in der Antike.* Darmstadt: Wissenchaftliche Buchgesselshaft, 1967.
Klijn, Albertus Frederik Johannes. "Papyrus Bodmer II (John i-xiv) and the Text of Egypt." *New Testament Studies* 3 (1956-57): 327-34.
Kloha, Jeffrey. "A Textual Commentary on Paul's First Epistle to the Corinthians, volumes 1 and 4." PhD Dissertation, University of Leeds, 2006.
Knox, John. *Marcion and the New Testament.* Chicago: University of Chicago Press, 1942.
Koester, Craig. *Hebrews--The Anchor Bible Commentary: A New Translation with Introduction and Commentary.* New York: Double Day, 2001.
Korpel, Marjo Christina Annette and Josef M. Oesch, eds. *Lay-out Markers in Biblical Manuscripts and Ugaritic Inscriptions.* Pericope 5. The Netherlands: Royal Van Gorcum, 2005.
Köster, Helmut. *Einführung in das Neue Testament.* Berlin: Walter de Gruyter, 1980.
Kraus, Thomas J. *Ad Fontes: Original Manuscripts and Their Significance for Studying Early Christianity—Selected Essays.* Text and Editions for New Testament Study 3. Leiden/Boston, 2007.
Kraus, Thomas J. and Tobias Nicklas, eds. *Early Christian Manuscripts: Examples of Applied Method and Approach.* Texts and Editions for New Testament Study 5. Leiden: Brill, 2010.
_____, eds. *New Testament Manuscripts: Their Text and Their World.* Texts and Editions for New Testament Study 2. Boston/Leiden: Brill, 2006.
_____. "The World of New Testament Manuscripts: 'Every Manuscript Tells a Story'." Pages 1-12 in *New Testament Manuscripts: Their Texts and their World.* Text and Editions for New Testament Study 2. Edited by Thomas J. Kraus and Tobias Nicklas. Leiden/Boston: Brill, 2006.
Kurek-Chomycz, Dominika A. "Is There an 'Anti-Priscan' Tendency in the Manuscripts? Some Textual Problems with Prisca and Aquila." *Journal of Biblical Literature* 125 (2006): 107-28.
Lake, Kirsopp. Review of Frederic Kenyon, *Chester Beatty Biblical Papyri-1936*. *Journal of Biblical Literature* 55/3 (Sept 1936): 244.
_____. Review of H.J.M. Milne and T.C. Skeat, *Scribe and Correctors of Codex Sinaiticus. Classical Philology* 37/1 (1942): 91-96.
_____. *The Influence of Textual Criticism on the Exegesis of the New Testament.* Oxford: Oxford University Press, 1904.
_____. *The Text of the New Testament.* 6th edition. London: S. New, 1933.
Lampe, Peter. "Zur Textgeschichte des Römerbriefes." *Novum Testamentum* 27 (1985): 273-77.
Leuven Database of Ancient Books (LDAB) <http://www.trismegistos.org/ldab/index.php>.
Lewis, Naphtali. *Papyrus in Classical Antiquity.* Oxford: Oxford University Press, 1974.
Lietzmann, Hans. "Zur Würdigung des Chester-Beatty-Papyrus der Paulusbriefe." *Sitzungsberichte der Preussischen Akademie der Wissenschaften. Philosophisch-historische Klasse. Abhandlungen XXV* (1934): 774-82. Reprinted, pages 170-79 in *Kleine Schriften. II: Studien zum Neuen Testament. Texte und Untersuchungen zur Geschichte der altchristlichen Literatur*. 68. Band = Reihe 5. Band 1. Berlin: Akademie-Verlag, 1958.

Luijendijk, Anne Marie. *Greetings in the Lord: Early Christians and the Oxyrhynchus Papyri.* Harvard Theological Studies 60. Massachusetts: Harvard Divinity School, 2008.

MacKenzie, Sheldon. "The Latin Column in Codex Bezae." *Journal for the Study of the New Testament* 6 (1980): 58-76.

Malherbe, Abraham J. *Social Aspects of Early Christianity.* Baton Rouge: Louisiana State University Press, 1977.

Markham, Robert. "The Critical Apparatus: A Symposium on the Bible Societies' Greek New Testament." *The Bible Translator* 18 (1967): 3-11.

Marshall, I. Howard and Philip Towner. *A Critical and Exegetical Commentary on The Pastoral Epistles.* Edinburgh: T & T Clark, 1999.

Martin, Michael Wade. "Defending the 'Western Non-Interpolations': The Case for an Anti-Separationist Tendenz in the Longer Alexandrian Readings." *Journal of Biblical Literature* 124 (2005): 269-94

Martinez, David. "The Papyri and Early Christianity." Pages 590-622 in *The Oxford Handbook of Papyrology.* Edited by Roger Bagnall. Oxford: Oxford University Press, 2009.

McCown, Chester Charlton. "Codex and Roll in the New Testament." *Harvard Theological Review* 34/4 (Oct 1941): 219-49.

_____. "The Earliest Christian Books." *The Biblical Archaeologist* 6/2 (May 1943): 21-31.

McDonald, Lee Martin and Stanley Porter, eds. *Early Christianity and its Sacred Literature.* Peabody: Hendrickson, 2000.

McDonald, Lee Martin and James A. Sanders, eds. *The Canon Debate: On the Origins and Formation of the Bible.* Peabody: Hendrickson, 2002.

McKendrick, Scot and Orlaith O'Sullivan, eds. *The Bible as Book: The Transmission of the Greek Text.* London/Michigan: British Library/The Scriptorium, 2003.

Meeks, Wayne A. *The First Urban Christians: The Social World of the Apostle Paul.* New Haven: Yale University Press, 1983.

Metzger, Bruce Manning. *A Textual Commentary on the Greek New Testament.* 2nd edition. Stuttgart: United Bible Societies, 1994.

_____. "Greek Manuscripts of John's Gospel with 'Hermeneiai'." Pages 162-69 in *Text and Testimony: Essays in honour of A.F.J. Klijn.* Edited by T. Baarda, A. Hilhorst, G.P. Luttikhuizen, and A.S. van der Woude. Kampen: J.H. Kok, 1988.

_____. *Manuscripts of the Greek Bible: An Introduction to Greek Palaeography.* Oxford: Oxford University Press, 1981.

_____. "Recently Published Papyri of the New Testament." *The Biblical Archaeologist* 10/2 (May 1947): 25-44.

_____. *The Text of the New Testament: Its Transmission, Corruption, and Restoration.* 3rd edition. Oxford: Oxford University Press, 1993.

_____. "The Future of New Testament Textual Studies." Pages 201-08 in *The Bible as Book: The Transmission of the Greek Text.* Edited by Scot McKendrick and Orlaith O'Sullivan; London/Michigan: British Library/The Scriptorium, 2003.

Metzger, Bruce Manning and Bart Ehrman. *Text of the New Testament: Its Transmission, Corruption, and Restoration.* 4th revised edition. Oxford/New York: Oxford University Press, 2005.

Millard, Alan. "Ancient Abbreviations and the *Nomina Sacra.*" Pages 221-26 in *The Unbroken Reed: Studies in the Culture and Heritage of Ancient Egypt, in Honour of A. F. Shore.* Edited by C.J. Eyre, M.A. Leahy, and L.M. Leahy. London: Egypt Exploration Society, 1994.

Milne, Herbert John Mansfield and Theodore Cressy Skeat. *Scribes and Correctors of the Codex Sinaiticus.* Oxford: Oxford University Press for the British Library, 1938.

Min, Kyoung Shik. *Die früheste Überlieferung des Matthäusevangeliums (bis zum 3./4. Jh.) Edition und Untersuchung.* Arbeiten zur Neutestamentlichen Textforschung 34. Berlin/New York: de Gruyter, 2005.

Monfasani, John and Ronald G. Musto, eds. *Renaissance Society and Culture: Essays in Honor of Eugene F. Rice, Jr.* New York: Italica Press, 1991.

Moule, Charles Francis Digby. "Some Observations on *Tendenzkritik.*" Pages 91-100 in *Jesus and the Politics of His Day.* Edited by Ernst Bammel and C.F.D. Moule. Cambridge: Cambridge University Press, 1984.

Mowry, Luceta. "The Early Circulation of Paul's Letters." *Journal of Biblical Literature* 63/2 (1944): 73-86.

Needham, Paul. *Twelve Centuries of Bookbindings: 400-1600.* New York/London: Pierpoint Morgan Library/Oxford University Press, 1979.

Nevius, Richard. *Divine Names in the Gospels.* Studies and Documents 30. Salt Lake: University of Utah Press, 1967.

Nicklas, Tobias. "Zur historischen und theologischen Bedeutung der Erforschung neutestamentlicher Textgeschichte." *New Testament Studies* 48 (2002): 145-58.

Nongbri, Brent. "The Use and Abuse of $\mathfrak{P}^{52}$: Papyrological Pitfalls in the Dating of the Fourth Gospel." *Harvard Theological Review* 98 (2005): 23-48.

O'Callaghan, José. *Nomina Sacra in Papyris Graecis Saeculi III Neotestamentariis.* Rome: Biblical Institute Press, 1970.

Omanson, Roger L. *A Textual Guide to the Greek New Testament: An Adaptation of Bruce M. Metzger's Textual Commentary for the Needs of Translators.* Stuttgart: German Bible Society, 2006.

Orsini, Pasquale and Willy Clarysse. "Early New Testament Manuscripts and their Dates: A Critique of Theological Palaeography." *Ephemerides Theologicae Lovanienses* 88/4 (2012): 443-74.

Osburn, Carroll, ed. *Essays on Women in Earliest Christianity. Volume* I. Missouri: College Press, 1993.

Paap, Anton Herman Reiner Everhard. *Nomina Sacra in the Greek Papyri of the First Five Centuries A.D.: The Sources and Some Deductions.* Papyrological Lugduno-Batava 8. Leiden: Brill, 1959.

Pack, Roger Ambrose. *Greek and Latin Literary Texts from Greco-Roman Egypt.* 2nd edition. Ann Arbor: University of Michigan Press, 1965.

Parker, David Charles. "A Dictation Theory of Codex Bezae." *Journal for the Study of the New Testament* 15 (1982): 97-112. Reprinted, pages 5-18 in *Manuscripts, Texts, Theology: Collected Papers.* Edited by D.C. Parker. *Arbeiten zur Neutestamentlichen Textforschung* 40. Berlin/New York: Walter de Gruyter, 2009.

_____. *An Introduction to the New Testament Manuscripts and Their Texts.* Cambridge: Cambridge University Press, 2008.
_____. *Codex Bezae: An Early Christian Manuscript and Its Text.* Cambridge: Cambridge University Press, 1992.
_____. *Codex Sinaiticus: The Story of the World's Oldest Bible.* London/ Massachusetts: British Library/Hendrickson, 2010.
_____. *Manuscripts, Texts, Theology: Collected Papers.* Edited by D.C. Parker. *Arbeiten zur Neutestamentlichen Textforschung* 40. Berlin/New York: Walter de Gruyter, 2009.
_____. "Manuscripts of John's Gospel with *Hermeneiai.*" Pages 46-68 in *Transmission and Reception*, 48-68. Reprinted, pages 121-38 in *David C. Parker: Manuscripts, Texts, Theology: Collected Papers.* Edited by D.C. Parker. *Arbeiten zur Neutestamentlichen Textforschung* 40. Berlin/New York: Walter de Gruyter, 2009.
_____. Review of Philip C. Comfort and David Barrett, The *Text of the Earliest Greek New Testament Manuscripts.*" <http://rosetta.reltech.org/TC/vol04/ComfortBarretted1999rev.html> Accessed 1 March 2010.
_____. Review of Carl Jaroš, *Das Neue Testament nach den ältesten Griechischen Handschriften*". <http://rosetta.reltech.org/TC/vol13/Jaros2008rev.pdf.> Accessed 1 February 2010.
_____. Review of James Royse's *Scribal Habits in Early Greek New Testament Papyri. The Bulletin of American Society of Papyrologists* 46 (2009): 255-58.
_____. "Scribal Tendencies and the Mechanics of Book Production." Pages 173-84 in *Textual Variation*: *Theological and Social Tendencies? Papers from the Fifth Birmingham Colloquium on the Textual Criticism of the New Testament.* Text and Studies: Third Series, 5. Edited by Hugh A.G. Houghton and David C. Parker. New Jersey: Gorgias Press, 2008.
_____. "Was Matthew Written before 50 CE? The Magdalen Papyrus of Matthew." *Expository Times* 107 (1995): 40-43. Reprinted, pages 55-63 in *Manuscripts, Texts, Theology*.
Parsons, Mikeal. "A Christological Tendency in $\mathfrak{P}^{75}$." *Journal of Biblical Literature* 105 (1986): 463-79.
Pickering, Stuart R. "Hebrews 7.28: Priest or High Priest." *New Testament Textual Research Update* 2 (1994): 93.
_____. "The Dating of the Chester Beatty-Michigan Codex of the Pauline Epistles ($\mathfrak{P}^{46}$)." Pages 216-27 in *Ancient History in a Modern University, Volume 2: Early Christianity, Late Antiquity and Beyond.* Edited by T. W. Hillard et al. New South Wales/Grand Rapids, Michigan: Macquarie University Press/Eerdmans, 1998.
Pietersma, Albert. "Chester Beatty Papyri." Pages 901-03 in vol. I of the *Anchor Bible Dictionary.* Edited by David Noel Freedman. Four volumes. New York: Doubleday, 1992.
Pintaudi, Rosario. "N.T. Ad Hebraeos VI, 7-9; 15-17." *Zeitschrift für Papyrologie und Epigraphik* 42 (1981): 42-44.
Porter, Stanley, ed. *Handbook to Exegesis of the New Testament.* New Testament Tools and Studies 25. Leiden: Brill, 1997.

_____. "Pericope Markers in Some Early Greek New Testament Manuscripts." Pages 161-76 in *Lay-out Markers in Biblical Manuscripts and Ugaritic Inscriptions.* Pericope 5. Edited by Marjo Korpel and J.M. Oesch. The Netherlands: Royal Van Gorcum, 2005.

_____, ed. *The Pauline Canon.* Pauline Studies I. Boston/Leiden: Brill, 2004.

_____. "Why so Many Holes in the Papyrological Evidence for the Greek New Testament." Pages 167-86 in *The Bible as Book: The Transmission of the Greek Text.* Edited by Scot McKendrick and Orlaith O'Sullivan. London/Michigan: British Library/The Scriptorium, 2003.

Porter, Stanley and Craig Evans, eds. *New Testament Text and Language: A Sheffield Reader.* Biblical Seminar 4. Sheffield: Sheffield Academic Press, 1997.

Preuschen, Edwin. *Analecta: Kürzere Texte zur Geschichte der Alten Kirche und des Kanons, zusammengestellt von Erwin Preuschen.* Leipzig: Mohr, 1893, 138-40.

Prior, J. Bruce. "The Use and Non-use of Nomina Sacra in the Freer Gospel of Matthew." Pages 146-66 in *The Freer Biblical Manuscripts: Fresh Studies of an American Treasure Trove.* Edited by Larry Hurtado. Atlanta: Society of Biblical Literature, 2006.

Quinn, Jerome. "$\mathfrak{P}^{46}$—The Pauline Canon?" *Catholic Biblical Quarterly* 36 (1974): 379-85.

Rahlfs, Alfred (ed.). *Septuaginta. Editio altera*/Revised and corrected edition by Robert Hanhart. German Bible Society, 2006.

Rice, G.E. "The Alteration of Luke's Tradition by the Textual Variants in Codex Bezae." PhD. Dissertation, Case Western Reserve University, 1974.

Riddle, Donald Wayne. "Textual Criticism as a Historical Discipline." *Anglican Theological Review* 18 (1936): 220-33.

Roberts, Colin Henderson. "An Early Papyrus of the First Gospel." *Harvard Theological Review* 46 (1953): 233-37.

_____. *An Unpublished Fragment of the Fourth Gospel in the John Rylands Library.* Manchester: Manchester University Press, 1935.

_____. *Greek Literary Hands: 350 B.C.--A.D. 400.* Oxford: Clarendon Press, 1956.

_____. *Manuscript, Society and Belief in Early Christian Egypt: The Schweich Lectures of the British Academy, 1977.* London: Oxford University Press for the British Academy, 1979.

_____. Review of Henry Sanders, *A Third Century Papyrus Codex of Saint Paul. The Journal of Egyptian Archaeology* 23 (Jun 1937): 133-34.

_____. "The Christian Book and the Papyri." *Journal of Theological Studies* 50 (July/Oct 1959): 155-68.

Robinson, James M. *The Facsimile Edition of the Nag Hammadi Codices, Introduction.* Leiden: Brill, 1984.

_____."The Future of Papyrus Codicology." Pages 23-70 in *The Future of Coptic Studies.* Edited by R. McLachlan Wilson. Leiden: Brill, 1978.

_____. *The Story of the Bodmer Papyri: From the First Monastery's Library in Upper Egypt to Geneva and Dublin.* Eugene, Oregon: Cascade Books, 2011.

Robinson, Maurice. Review of Philip Comfort and David Barrett, *Text of the Earliest Greek New Testament Manuscripts*. <http://rosetta.reltech.org/TC/vol06/CB2001rev.html> Accessed 1 March 2010.

Royse, James. "Scribal Habits in Early Greek New Testament Papyri." Th.D. Dissertation, Graduate Theological Union, 1981.

_____. *Scribal Habits in Early Greek New Testament Papyri.* New Testament Tools, Studies and Documents 36. Leiden/Boston: Brill, 2008.

_____. "Scribal Tendencies in the Transmission of the New Testament Text." Pages 239-52 in *The Text of the New Testament in Contemporary Research: Essays on the* Status Quaestionis, *A Volume in Honor of Bruce M. Metzger.* Studies and Documents 46. Edited by Bart Ehrman and Michael Holmes. Grand Rapids, Michigan: Eerdmans, 1995.

_____. "Scribal Tendencies in the Transmission of the New Testament Text." Pages 461-78 in *The Text of the New Testament in Contemporary Research: Essays on the* Status Quaestionis. New Testament Tools-Studies and Documents 42. Edited by Bart Ehrman and Michael Holmes. Leiden/Boston: Brill, 2013.

Rutgers, L.V., P.W. van der Horst, H.W. Havelaar, and L.M. Teugels. *The Use of Sacred Books in the Ancient World.* Contributions to Biblical Exegesis and Theology 22. Leuven: Peeters, 1998.

Sanday, William. "The Cheltenham List of the Canonical Books of the New Testament and of the Writing of Cyprian." Pages 217-325 in *Studia Biblica et Ecclesiastica.* Volume III. Oxford: Clarendon Press, 1891.

Sanders, Henry A. *A Third-Century Papyrus Codex of the Epistles of Paul.* University of Michigan Studies, Humanistic Series 38. Ann Arbor: University of Michigan Press, 1935.

_____. Review of H.J.M. Milne and T.C. Skeat, *Scribes and Correctors of Codex Sinaiticus. American Journal of Philology* 60/4 (1939): 486-90.

_____. *The New Testament Manuscripts in the Freer Collection, Part I: The Washington Manuscript of the Four Gospels.* New York: Macmillan, 1912.

_____. *The New Testament Manuscripts in the Freer Collection, Part II.* London: MacMillan, 1918.

Sanders, Henry and Carl Schmidt, eds. *The Minor Prophets in the Freer Collections and the Berlin Fragment of Genesis.* New York: MacMillan, 1927.

Saunders, Ernst. "Studies in Doctrinal Influences on the Byzantine Text of the Gospels." *Journal of Biblical Literature* 71/2 (Jun 1952): 85-92.

Schironi, Francesca. *ΤΟ ΜΕΓΑ ΒΙΒΛΙΟΝ: Book-Ends, End-Titles, and Coronides in Papyri with Hexametric Poetry.* Durham, North Carolina: The American Society of Papyrologists, 2010.

Schmid, Ulrich. "Scribes and Variants—Sociology and Typology." Pages 1-23 in *Textual Variation: Theological and Social Tendencies? Papers from the Fifth Birmingham Colloquium on the Textual Criticism of the New Testament.* Texts and Studies, Third Series 6. Edited by H.A.G. Houghton and D.C. Parker. Piscataway, New Jersey: Gorgias Press, 2008.

Schubart, Wilhelm. *Das Buch bei den Griechen und Römern.* Zweite umgearbeitete Auflage. Berlin und Leipzig: bereinigung Wissenschaftlicher Verleger, Walter de Gruyter & Co., 1921.

_____. *Einführung in die Papyruskunde.* Berlin: Weidmannsche Buchhandlung, 1918.

Silva, Moisés. "Response (to the Essays)." Pages 141-50 in *Rethinking New Testament Textual Criticism*. Edited by David Alan Black. Grand Rapids, Michigan: Baker Academic, 2002.

_____. "The Text of Galatians: Evidence from the Earliest Greek Manuscripts." Pages 18-25 in *Scribes and Scriptures: NT Essays in Honor of J. Harold Greenlee.* Edited by David Alan Black. Wynona Lake, Indiana: Eisenbrauns, 1992.

Skeat, Theodore Cressey. "Did Paul write to the 'Bishops and Deacons' at Philippi? A Note on Philippians 1.1." *Novum Testamentum* 37 (1995): 12-15. Reprinted, pages 258-61 in *Collected Biblical Writings of T.C. Skeat*. Edited by J.K. Elliott. Supplement to *Novum Testamentum* 113. Leiden/Boston: Brill, 2004.

_____. "Early Christian Book Production." Pages 54-79, 512-13 in *The Cambridge History of the Bible;* Volume 2: *The West from the Fathers to the Reformation.* Edited by G.W.H. Lampe. Cambridge, Cambridge University Press, 1969. Reprinted, pages 33-59 in *The Collected Biblical Writings of T.C. Skeat.* Edited by J.K. Elliott. Supplement to *Novum Testamentum* 113. Leiden/Boston: Brill, 2004.

_____. "The Length of the Standard Papyrus Roll and the Cost-Advantage of the Codex." *Zeitschrift für Papyrologie und Epigraphik* 45 (1982): 169-76. Reprinted, pages 65-70 in *The Collected Biblical Writings of T.C. Skeat.* Edited by J.K. Elliott; Supplements to *Novum Testamentum* 113: Leiden/Boston: Brill, 2004.

_____. "The Oldest Manuscript of the Four Gospels?" *New Testament Studies* 43 (1997): 1–34. Reprinted, pages 158-92 in *The Collected Biblical Writings of T.C. Skeat.* Edited by J.K. Elliott; Supplements to *Novum Testamentum* 113: Leiden/Boston: Brill, 2004.

_____. "The Use of Dictation in Ancient Book Production." *Proceedings of the British Academy* 42 (1956): 179-208. Reprinted, pages 1-32 in *The Collected Biblical Writings of T.C. Skeat.* Edited by J.K. Elliott; Supplements to *Novum Testamentum* 113: Leiden/Boston: Brill, 2004.

Solomon, Kenneth. "Nomina Sacra: Scribal Practice and Piety in Early Christianity." Paper presented during the 2008 Midwest Regional Meeting of Evangelical Theological Society, 2008.

Spencer, F.A. Review of Henry Sanders, *A Third Century Papyrus Codex*, and Frederic Kenyon, *The Chester Beatty Biblical Papyri Fasc. III. The Classical Weekly* 30 (Dec 7, 1936): 69-70.

Stanton, Graham. *Jesus and Gospel.* Cambridge: Cambridge University Press, 2004.

Stewart, Robert D., ed. *The Reliability of the New Testament: Bart D. Ehrman & Daniel C. Wallace in Dialogue.* Minneapolis: Fortress Press, 2011.

Swanson, Reuben. *New Testament Greek Manuscripts: Variant Readings arranged in horizontal lines against Codex Vaticanus,* 1 Corinthians. Wheaton/Pasadena: Tyndale House/ William Carey International University Press, 2008.

_____. *New Testament Greek Manuscripts: Variant Readings arranged in horizontal lines against Codex Vaticanus,* 2 Corinthians. Wheaton/Pasadena: Tyndale House/William Carey International University Press, 2008.

_____. *New Testament Greek Manuscripts: Variant Readings arranged in horizontal lines against Codex Vaticanus,* Romans, Galatians. Wheaton/Pasadena: Tyndale House/William Carey International University Press, 2008.

Tait, William John. "Guidelines and Borders in Demotic Papyri." Pages 63-89 in *Papyrus: Structure and Usage—British Museum Occasional Papers* 60. Edited by Morris Leonard Bierbrier. London: British Museum, 1986.

Tasker, Randolph V.G. "The Text of the Corpus Paulinum." *New Testament Studies* 1/3 (1955): 180-91.

Taylor, David G.K, ed. *Studies in the Early Text of the Gospels and Acts: Papers of the First Birmingham Colloquium on the Textual Criticism of the New Testament.* Atlanta: SBL, 1999.

Teeple, Howard Merle and F.A. Walker. "Notes on the Plates in Papyrus Bodmer II". *Journal of Biblical Literature* 78 (1959): 148-52.

Thompson, Edward Maunde. *Introduction to Greek and Latin Palaeography.* Oxford: Clarendon Press, 1912.

Thompson, Herbert. *The Gospel of Saint John according to the Earliest Coptic Manuscripts.* Publications of the Egyptian Research Account and British School of Archaeology in Egypt 36. London: British School of Archaeology in Egypt, 1924.

Traube, Ludwig. *Nomina Sacra: Versuch einer Geschichte der christlichen Kürzung.* München: C.H. Beck'sche Verlagsbuchhandlung, 1907.

Treu, Kurt. "Neue neutestamentliche Fragmente der Berliner Papyrussammlung." *Archiv für Papyrusforschung* 18 (1966): 37-48.

Trobisch, David. *Paul's Letter Collection: Tracing the Origins.* Minneapolis: Fortress Press, 1994.

_____. "Structural Markers in New Testament Manuscripts, with special attention to observations in Codex Boernerianus (G 012) and Papyrus 46 of the Letters of Paul." Pages 177-90 in *Lay-out Markers in Biblical Manuscripts and Ugaritic Tablets.* Edited by Marjo Korpel and J.M. Oesch; Pericope 5; The Netherlands: Royal Van Gorcum, 2005.

_____. *The First Edition of the New Testament.* Oxford: Oxford University Press, 2000.

Tuckett, Christopher M. "$\mathfrak{P}^{52}$ and Nomina Sacra." *New Testament Studies* 47 (2001): 544-48.

_____. "*Nomina Sacra* in Codex E." *Journal of Theological Studies* 57 (2006): 487-99.

Turner, Eric Gardner. *Greek Manuscripts of the Ancient World.* 2nd revised and enlarged edition. Bulletin Supplement 46. Edited by P.J. Parsons. London: University of London-Institute of Classical Studies, 1987.

_____. *Greek Papyri: An Introduction.* New Jersey: Princeton University Press, 1980.

_____. Review of A.H.R.E Paap, *Nomina Sacra in the Greek Papyri of the First Five Centuries A.D. Journal of Egyptian Archaeology* 46 (1960): 125.

_____. *The Terms Recto and Verso: The Anatomy of the Papyrus Roll.* Papyrologica Bruxellensia: Brussels, 1978.

_____. *The Typology of the Early Codex.* Pennsylvania: University of Pennsylvania Press, 1977.

Vaganay, Léon and Christian-Bernard Amphoux. *An Introduction to New Testament Textual Criticism.* Second Revised and updated edition. Revised by Christian-Bernard Amphoux and Jenny Heimerdinger. Translated by Jenny Heimerdinger. Cambridge: Cambridge University Press, 1991.

van Minnen, Peter. "Dating the Oldest New Testament Manuscripts." http://library.duke.edu/rubenstein/scriptorium/papyrus/texts/manuscripts.html. Accessed: 14 May 2010.

Virtual Manuscript Room-Münster. <http://intf.uni-muenster.de/vmr/NTVMR/IndexNTVMR.php>.

Virtual Manuscript Room-Münster. *Transcription of* $\mathfrak{P}^{46}$. <http://intf.unimuenster.de/vmr/NTVMR/viewer/viewerCodex01.php>.

Virtual Manuscript Room-Münster. *Digital Images of* $\mathfrak{P}^{46}$. <http://intf.uni-muenster.de/vmr/NTVMR/viewer/viewerWolkenkratzer01.php>.

van der Horst, P.W. "Sortes: Sacred Books as Instant Oracles in Late Antiquity." Pages 143-73 in *The Use of Sacred Books in the Ancient World.* Edited by L.V. Rutgers, P.W. van der Horst, H.W. Havelaar, and L.M. Teugels. Contributions to Biblical Exegesis and Theology 22. Leuven: Peeters, 1998.

von Harnack, Adolf. "Zur Textkritik und Christologie der Schriften des Johannes". Pages 115-27 in *Studien zur Geschichte des Neuen Testaments und der alten Kirche.* Band I. *Zur neutestamentlichen Textkritik.* Berlin: de Gruyter, 1931.

Wachtel Klaus and Klaus Witte. *Das Neue Testament auf Papyrus, Die Paulinischen Briefe, Teil 2.* Berlin/New York: De Gruyter, 1994.

Wachtel, Klaus and Michael Holmes, eds. *The Textual History of the Greek New Testament: Changing Views in Contemporary Research.* Text-Critical Studies 8; Atlanta: Society of Biblical Literature, 2011.

Wallace, Daniel. Review of Young Kyu Kim's *Paleographical Dating of* $\mathfrak{P}^{46}$ *to the Later First Century*. *Bibliotheca Sacra* 146 (1989): 451-52.

Wasserman, Tommy. "$\mathfrak{P}^{78}$ (*P.Oxy.* XXXIV 2684): The Epistle of Jude on an Amulet?" Pages 137-60 in *New Testament Manuscripts: Their Texts and Their World.* Edited by Thomas J. Kraus and Nicklas Tobias. Text and Editions for New Testament Study 2; Leiden/Boston: Brill, 2006

_____. "The Early Text of Matthew". Pages 83-107 in *The Early Text of the New Testament.* Edited by Charles E. Hill and Michael J. Kruger. Oxford: Oxford University Press, 2012.

_____. *The Epistle of Jude: Its Text and Transmission.* Coniectanea Biblica, New Testament Series 43. Stockholm: Almqvist & Wiksell, 2006.

Wayment, Thomas. "The Scribal Characteristics of the Freer Pauline Codex". Pages 251-62 in *The Freer Biblical Manuscripts: Fresh Studies of an American Treasure Trove.* Edited by Larry Hurtado. Society of Biblical Literature Text-Critical Studies 6. Atlanta: Society of Biblical Literature, 2006.

Wellert, Arie. "The Reconstruction of Papyrus Manufacture: A Preliminary Investigation." *Studies in Conservation* 34/1 (Feb 1989): 1-8.

Wilcken, Ulrich. "The Chester Beatty Biblical Papyrus." *Archiv für Papyrusforschung* 11 (1935): 112-14.

Wikgren, Allen P., ed. *Early Christian Origins: Studies in Honor of Harold R. Willoughby.* Chicago: Quadrangle Books, 1961.

Wilson, Robert McLachlan, ed. *The Future of Coptic Studies. Coptic Studies I.* Leiden: Brill, 1978.

Wilson, Stephen G. and Michael Desjardins, eds. *Text and Artifact in the Religions of Mediterranean Antiquity: Essays in Honour of Peter Richardson.* Studies in Christianity and Judaism/*Études sur le Christianisme et le Judaïsme*. Waterloo: Wilfrid Laurier University Press, 2000.

Witherington III, Ben. "Anti-Feminist Tendencies of the 'Western' Text in Acts." *Journal of Biblical Literature* 103 (1984): 82-84.

Zuntz, Günther. *The Text of the Epistles: A Disquisition upon the Corpus Paulinum: The 1946 Schweich Lectures of the British Academy.* London: Oxford University Press for the British Academy, 1953.