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## The majority text debate: A study in New Testament text-critical method

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**THE MAJORITY TEXT DEBATE:  
A STUDY IN NEW TESTAMENT  
TEXT-CRITICAL  
METHOD**

by

**Clive E. Govier, B.D. (Hons.) Lon.**

A thesis submitted in fulfilment of the requirements  
for the degree of Master of Arts  
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## USE OF THESIS

The Use of Thesis statement is not included in this version of the thesis.

### Abstract

The aim of textual criticism is to recover the original text of the New Testament. By studying and comparing the many extant manuscripts it is hoped to discover which of them, or the variants they contain, are closest to the original Text.

In choosing between the many variant readings, New Testament scholars developed the method of grouping manuscripts into different forms of text which fit the pattern of their variants. In contrast to this approach, J. W. Burgon propounded a method later identified as "The Majority Text" approach. This focuses on the Byzantine textual tradition, and assumes that its numerical preponderance is *prima facie* evidence of a superior text.

With the lapse of time, and due to the results of the many studies made of newly discovered papyri, there is growing uncertainty as to the value of the traditional groupings of manuscripts. Both current research and contemporary methods of criticism may indicate that F. J. A. Hort's description of the Byzantine text (*Majority text*), as late, inferior, and recensional, needs to be reevaluated.

There is a loss of methodological consensus; differing ways have emerged of estimating the many variant readings of the New Testament. This depends on whether the critic relies on the supposed history of the text, or prefers to focus on stylistic and philological issues. The need is to find a text-critical method acceptable to all.

Recent debate between scholars advocating different approaches to textual criticism has addressed several key theoretical issues, whose outcome determines whether the *Majority text* method is a viable alternative to other approaches.

This study responds to the recommendation of Kurt Aland (1987) that interested students should test the *Majority text* method, by considering several texts from the Gospels which are relegated to the critical apparatus of the Greek New Testament [UBS<sup>4</sup>]. This is done by employing Burgon's "Seven Notes of Truth", and the results are compared with Aland's conclusions, as well as with the conclusions of other critics who follow similar or varying methods.

Not surprisingly it was found that, of all the verses examined on the basis of the *Majority text* method, the textual decisions were markedly different from those made by Aland and the UBS editors. In contrast, the *Majority text* conclusions for half of the verses considered were in agreement with those reached by the more radical approach of G. D. Kilpatrick who was willing to evaluate some Byzantine variants as good readings.

The differing approaches indicate that New Testament textual criticism is at a methodological impasse. It is hoped that a clearer understanding of the history of the text will provide an objective basis for making sound textual choices. This quest must include a more exact method of patristic studies to enable the critic to place the Text more accurately in the context of its time and location.

If a consensus emerged which accepted that Hort's views of the origins of the Byzantine text are no longer tenable, this may encourage scholars to study Burgon's work more closely, and thereby assess the value of the *Majority text* method.

I certify that this thesis does not incorporate without acknowledgment any material previously submitted for a degree or diploma in any institution of higher education; and that to the best of my knowledge and belief it does not contain any material previously published or written by another person except where due reference is made in the text.

Signature . .

Date ..... *April 1996* .....

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## Table of Contents

	Page
Abstract	2
Declaration	4
Acknowledgments	5
CHAPTER	
1. BACKGROUND TO THE STUDY	9
Stages of Textual Criticism: From Origen to Elliott	10
First Period: The Founding Fathers	10
Second Period: Gathering Data	12
Third period: Changing the Greek Text	14
Fourth Period: Towards a Standard Text	16
The historical-documentary method	16
Competing text-types	20
Exploring the genealogical principle	23
The crisis of the criteria	25
Current Uncertainty and its Causes	32
Revival of an Earlier Debate	35
2. THE THEORETICAL ISSUES	42
Hort's Analysis of the Byzantine Text	43
The Status of Text-Types	44
Characteristics of the Byzantine Text	46
Applying the Genealogical Principle	50
Earliest Evidence for the Majority Text	54
Evidence from the Papyri	55
Ante-Nicene Patristic Evidence	59

CHAPTER (continued)	Page
2. The Majority Text versus Internal Evidence	65
Questioning the Criteria	66
Proclivi lectioni praestat ardua	67
Brevior lectio potior	68
Majority text ambivalence	71
Is Conjecture Inevitable?	74
Differing Views of Conjecture	75
The Majority Text View	77
3. UNDERSTANDING THE MAJORITY-TEXT METHOD	85
Explaining the Majority Phenomenon	85
Normal versus Abnormal Transmission	85
The Case for Radical Dislocation	88
Unresolved Differences	91
Rationale for the Majority Text Method	98
4. TESTING THE METHODOLOGY	118
How Majority Decisions are Made	118
Aland's Marginal Readings	119
Matthew 17:21	120
Mark 9:44, 46	134
Mark 11:26	144
Mark 15:28	153
John 5:3b-4	164
Acts 8:37	179

CHAPTER (continued)	Page
5. EVALUATION OF THE MAJORITY TEXT METHOD	193
Burgon and His Successors Compared	193
The Numerical Argument	194
The Evidence of Variety	197
The Argument from Continuity	199
The Age of the Text	200
The Best Manuscripts	202
The Note of Reasonableness	206
Strengths and Weaknesses of the Majority Text Approach	206
The Importance of the Age of a Manuscript	207
The Relationship between Papyri and Text-types	208
The Validity of the Numerical Argument	209
The Normality of Textual Transmission	212
The Use of the Genealogical Principle	213
The Meaning and Significance of Text-types	214
The Special Characteristics of the Byzantine Text	216
Evidence from Versions and Lectionaries	219
The Patristic Use of the Byzantine Text	221
Issues of Internal Evidence	225
The Results of the Majority Text Debate	228
A Way Forward	232
6. REVIEW	235
Background to the Study	235
The Theoretical Issues	239
Understanding the Majority Text Method	242
Testing the Methodology	244
Evaluation and Way Forward	246
BIBLIOGRAPHY	249

## CHAPTER 1

## Background to the Study

The fundamental aim of the discipline of textual criticism is to not merely discover distinctive readings or variations in word-order in a document, but to establish the original text of the New Testament. This aim, as J. Keith Elliott points out (1974, p. 338), is a more important goal than for other ancient literature, because Christianity is based on an historical revelation and forms the basis for Christian doctrine and practice. The process involves a study and comparison of the many manuscripts, versions, lectionaries and patristic quotations of the New Testament, in order to discover their characteristic patterns of similarities and differences in wording. These patterns show how manuscripts interrelate; the results have been classified in terms of their geographical affinities. Internal phenomena relating to the author's characteristic style and vocabulary are also added to the evidence. By such a method it is hoped to discern which words more probably represent the original text of the New Testament. The purpose of this study is to examine the various ways in which textual critics decide between competing variants, with particular reference to recent discussion as to the significance of the *Majority text* approach. The common objective is to understand the history of the transmission of the New Testament text, and to apply the most effective method by which its original wording may be restored.

*Stages of Textual Criticism: From Origen to Elliott*

The method of choosing between the many variant readings of the New Testament has substantially changed from ancient to modern times; the ensuing outline traces the features common to the discipline which characteristically recur through its gradually unfolding history.

*First Period: The Founding Fathers*

Eldon J. Epp describes Origen as the founding father of textual criticism, as he was the first to apply critical canons to the Text (1993, pp. 17-18). A study by Bruce M. Metzger (1963, pp. 80-81) shows that in exegesis Origen noticed the same various readings later discerned by subsequent critics, and referred to the witness of "few", "many", "most", or "almost all" manuscripts. Further, where he disliked the reading reflected in the majority, Origen freely departed from it when historical or intrinsic reasons compelled him to do so. For the same reasons he also rejected the entire manuscript witness at times, in preference for conjectural emendation. Metzger says: "He was an acute observer of textual phenomena but was quite uncritical in his evaluation of their significance" (p. 93). He did not usually state a preference between readings, but simply set them out, hesitating to pass judgment. His decisions were not always based on the study of manuscripts, but on theological or etymological concerns. Sometimes he preferred readings suiting the immediate context, at other times those which harmonised with parallel passages (1963, pp. 78-95).

F. H. A. Scrivener (1894:2, p. 269) shows the criteria Jerome employed 150 years later. Jerome believed the numerical preponderance of manuscripts to be a significant factor in deciding between readings. Epp

(1993, p. 144) points out that his critical canons also included the age and respectability of manuscripts, the study of the immediate context, and the grammatical soundness of each reading.

The textual work of Erasmus forms an essential link, bridging the classical and patristic world with modern scholarship. The establishing of the printed text of Erasmus, later described as the *Received text*, is seen as the first stage commencing the modern period of textual criticism.

Concerning his textual notes, Jerry H. Bentley (1983, pp. 124-173) says: "The Annotations make it clear that Erasmus devoted an enormous amount of time and effort to New Testament research, considered a staggering amount of evidence, and intelligently evaluated it in proper theological and philological context" (1983, p. 19). These Annotations are mysteriously neglected (1983, p. 139). Bentley adds:

In this field . . . Erasmus far outstripped his predecessors in philological and textual scholarship. And in doing so, he furthered the development of the methods, principles and insights that later philologists would use in classical and New Testament studies. (p. 161)

Erasmus decided between the myriad variant readings which he assembled from Greek and Latin Patristic writings on the weight of textual evidence. He discovered their origin through, for example, assimilation, confusion of homonyms, intentional scribal changes, and through theological and other considerations. He was the first scholar to regularly employ the "harder reading" principle (1983, pp. 146-157). His *Annotations* shows he consulted many more than four manuscripts for his first edition of the Greek New Testament.

He collected readings from different parts of Europe, though Edward F. Hills (1956, pp. 198-199) says that Erasmus in establishing the text was guided by "the common faith" rather than by his own literary preferences; these latter he set out in his *Annotations*, accepting the conventional restrictions of the day.

Whereas Robert Stephens' printed text of 1550 put various readings in the margin, not all extant collated readings were listed until 1657, in Brian Walton's Polyglot. This work was spurred on by the recently presented Cod A. to Charles I. Walton appended a critical apparatus, adding fifteen authorities to those contained in Stephens' margin. (Metzger, 1992, p. 107).

#### *Second Period: Gathering Data*

A second stage began with the assembling of the great mass of materials for critical study by John Fell and John Mill. Fell's 1675 New Testament added to the 100 manuscripts appended to the critical apparatus. This helped and gave impetus to Mill's Edition in 1707. Edward Miller (1886, p. 14), a "disciple" and contemporary of J. W. Burgon, says that Mill added some 30,000 readings, and "far excelled all his contemporaries in accuracy of collation and comprehensiveness of method." The classical scholar Richard Bentley supported Mill's work; he determined to publish a Greek and Latin text of the New Testament restored to the state they were in during the fourth century. However the work has never been published, because it was felt he had been too optimistic about the settled state of the text in the fourth century. A. Souter (1954, p. 90) comments: "The impulse he gave to [text critical] studies was such, that but for him there would have been no Lachmann and no Hort".

J. A. Bengel is sometimes described as "the father of textual criticism." In publishing his New Testament of 1734, Bengel was the first to depart in principle from the *Received text* (Miller, 1886, p.16). "He was the first editor to introduce the principle that authorities must be classified and weighed, not counted" (Souter, 1954, p. 90). This principle led him to evaluate the oldest manuscripts as being generally the best. Bengel was also the first to classify manuscripts into "companies, families, tribes, nations" (Metzger, 1992, p. 112). By this means he distinguished between Asiatic and African manuscripts. Epp (1993, pp. 147-149) sets out Bengel's method in detail. The notes of antiquity, variety of evidence, and number of witnesses are cited; scribal interference within the immediate context is also emphasised as a useful test of a variant, and as helping to explain how a corrupt reading arose. Kurt Aland (1987, p. 11) says of Bengel: "To him is due the laurel for the eighteenth century", rather than to his contemporary J. J. Griesbach.

When publishing his 1751 Edition of the Greek New Testament, J. J. Wettstein set out eighteen critical canons which showed, according to Epp, a more thoughtful approach than was characteristic of his age (1993, p. 150). In conscious opposition to Bengel's view that the more ancient reading is preferable, Wettstein believed that the oldest Greek manuscripts were untrustworthy for having been latinised, that is, corrupted by interpolation from Latin manuscripts. Thus purity for him lay in the later manuscripts. The Greek New Testament editions of C. F. Matthaei in 1782-1806 evidenced him, according to Souter, "a most industrious and accurate collator of manuscripts" (1954, p. 91). Burgon (1883, p. 246) linked his name with Scrivener as "the only two scholars who have collated any considerable number of sacred Codices with the needful amount of accuracy".



J. J. Griesbach (fl. 1774) vies with Bengel as the founding father of current text-critical methods. He was the first to print the Greek text with a critical apparatus unrestricted by any deference to the *Received text*. Bengel saw geography as the key to the groupings of manuscripts, and his successors, J. S. Semler and Griesbach, applied the principle by classifying manuscripts as Alexandrian, Western and Byzantine. Griesbach followed Semler's similar threefold division of manuscripts, according to Metzger (1992, p. 119) and established the principle which treated a variant as authentic where all three streams agreed on it. He made the joint testimony of two independent families always to prevail over the third (Scrivener, 1894:2, pp. 224-225). This had the effect of declaring the Byzantine manuscripts subordinate, inasmuch as the other two families more often than not united with each other, vis-à-vis the *Majority text*.

Thus in the work of these scholars we see the influence of 18th century classical studies. New Testament critics rejected the earlier more general approach to textual characteristics as being unscientific, in favour of grouping manuscripts into different forms of text which ostensibly fit the pattern of their variants. By genealogical linking it was hoped to provide empirical proof for these manuscript groupings.

### *Third Period: Changing the Greek Text*

K. F. Lachmann introduced a third stage of textual criticism in 1831 by printing the Greek text in a modified form. He was the first to break totally with the *Received text*. He tried to complete Richard Bentley's work, that is, with the aim of recovering the text as it existed at the fourth century. In doing this he relied on only two, three, or four of the oldest of the Alexandrian and Western manuscripts, and totally ignored the Byzantine

(Metzger, 1992, pp. 125). He used the principle of the "community of errors" to distinguish further between the text-types. Souter says of Lachmann: "The Introduction [of his Greek/Latin Edition] is comparable to Mill's and Hort's in importance" (1954, p. 92). Lachmann relied entirely on external evidence.

J. M. A. Scholz, a contemporary of Hort, may foreshadow to some extent modern trends, in questioning the Western text as a homogeneous and separate tradition. Thus he modified Griesbach's theory of three recensions, by combining Griesbach's Western text in the Alexandrian stream. In doing this he reverted to Bengel's twofold classification which he had earlier held (Scrivener, 1894:2, p. 229). However, in contrast to most critical scholars, he preferred the Byzantine text; but he applied his view inconsistently.

Constantin von Tischendorf's unparalleled labours led to his published eighth edition of 1872 with its critical apparatus, which is still unsurpassed. He personally discovered 15 uncial manuscripts, including Cod. Aleph (hereafter: Cod.  $\aleph$ ); he used 23 for the first time, collated 13, copied 4, and worked on 30 others (Miller, 1886, pp. 23-24). He made the rule of Griesbach foundational to his method: "the reading which explains all others is most probable." He gave more weight to Cod.  $\aleph$  than many scholars could accept. Metzger (1992, p. 127) notes that Tischendorf's work was characterised by "a somewhat wooden adherence to a number of critical canons, as well as a certain arbitrariness in dealing with problems not covered by the canons."

S. P. Tregelles was contemporary with Tischendorf and edited the Greek text, which was published progressively between 1857 and 1872. He followed in essence Lachmann's plan. Scrivener (1894, pp. 240-241) says that he refused to give any voice to the Byzantine stream, choosing only

uncial manuscripts, except for four cursives. Epp (1993, p. 156) shows how Tregelles supplemented this external criterion by giving a decisive role to internal criteria, but only when the older witnesses disagreed among themselves; he resisted reliance on internal rules because it involved unacceptable conjecture.

#### *Fourth Period: Towards a Standard Text*

##### *The historical-documentary method.*

The increasing sophistication of critical methods became more obvious when another stage in Textual Criticism was reached with the printing of the English Bible on the basis of a revised Greek text. From their *Introduction to the Greek New Testament*, B. F. Westcott and F. J. A. Hort (1881) showed themselves to be on many points disciples of Lachmann (Scrivener, 1894:2, p. 285) and sought thus to prove that the few oldest manuscripts were more trustworthy than the great majority. They accordingly exalted the fourth-century text as original, especially Cod. B. In doing so, they tended to downgrade Codd. A and C, even though they were both nearly as ancient. They posited a Lucianic recension to explain how the Byzantine text became an official ecclesiastical text. F. G. Kenyon (1949, p. 165) says that their theory in *Prolegomena* is "in the direct line of descent from Bengel and Griesbach." Hort classified the manuscripts into four groups: Group ( $\alpha$ ) were the later uncials and the mass of cursives descended from an Antiochian revision, which he called Syrian; the ( $\beta$ ) group were "neutral" manuscripts, particularly Cod. B, which had received no editorial revision, and thus was relatively pure; group ( $\gamma$ ) were manuscripts, named Alexandrian, which had readings akin to "neutral" except that they showed signs of stylistic revision; and finally ( $\delta$ ), a group

of manuscripts with a predominant Latin (Western) attestation, which widely diverged from other families (1949, pp. 166-167). Hort's method relied on the basic principle: "the earliest manuscript contains the best reading." This approach, which E. J. Epp (1992, p. 432) calls the historical-documentary method, is still seen by many as the ideal.

Hort ruled out group ( $\alpha$ )--as did Bengel, Griesbach, Lachmann, Tischendorf, and Tregelles before him--by claiming that no distinctive Byzantine reading occurred in patristic writings before Chrysostom. The ( $\gamma$ ) group showed stylistic revision and thus should be used with caution. The Western group ( $\delta$ ) antedated ( $\beta$ ) inasmuch as Latin fathers from circa AD 150 onwards were characteristically Western; also the Old Syriac had many Western readings. Though of earlier date, Hort felt on the basis of "instinctive preference that the Western showed marks of deliberate and licentious alteration". This left ( $\beta$ ), a group of manuscripts, with Cod. B superior, whose characteristic readings were "almost decisive" when supported by  $\aleph$ , except for a few interpolations which Cod. D happens to omit (Kenyon, 1949, pp. 166-168).

Hort admitted that his first rule in deciding between competing variants was to follow internal rather than external evidence. An instinctive preference had priority, though checked by Bengel's rule: *Difficilior lectio potior*. This involves finding the variant which best explains the others, then making a comparative estimate of manuscripts. This process meant deciding on an external basis which variant is supported by manuscripts which most often offer superior readings elsewhere. This comparative estimate helps to decide readings which otherwise would be left in doubt. Such a procedure necessitates making a group classification of manuscripts, by observing whether a manuscript supports a set of characteristic readings usually found combined together within the text of certain manuscripts. Such a

classification enables descent to be tracked, at least in theory, to a previous generation of manuscripts, that is, it implies descent among the manuscripts of a group from a more remote ancestor (Kenyon, 1949, pp. 165-166).

In contrast to this developed method of Westcott and Hort (1881), J. W. Burgon (1896a, pp. 40-67) propounded a method later to be identified as "*The Majority text*" approach to the textual method of this 19th-century scholar. Burgon's method has an interesting resemblance to that of Jerome; however Jerome does not share the tests of Continuity and Variety which help to make up Burgon's "Seven Notes". Burgon also shares some similar features with Bengel, though Burgon's overall method was very different. He believed that the age of a manuscript was only one external factor to consider, and not the main basis for evaluation. He mostly distrusted the study of intrinsic and transcriptional probabilities which explain what gave rise to variant readings, and so gave no value to internal evidence, as usually understood. As a last resort, he preferred to accept a variant reading upheld by the mass of manuscripts as genuine, regardless of age, than speculate as to how the various readings arose. This method will be considered in more detail later.

Another contemporary of Burgon was F. H. A. Scrivener. In contrast to Burgon, Scrivener (1894, pp. 300-301) looked for agreement between the oldest manuscripts, versions, and Fathers as the key to authenticity. His method was to follow the reading of the oldest uncials, unless they conflicted internally. If the oldest manuscripts conflicted internally, he resorted to the later uncials, and thence to the great mass of cursives usually in support of them. Burgon, however, objected to making the earliest manuscripts oracular, inasmuch as it reduced the critic's resources to about 1 verse in 3 of the Gospels - they being, between them, so textually incomplete.

Hort's work established some clear working principles which, compared with Burgon's approach, were more in line with methods which had long been used in the analysis of classical texts. Hort's method relied on the basic principle: "the earliest manuscript contains the best reading." This approach, the historical-documentary method, is still seen by many as the ideal. The method emphasises external manuscript evidence using the criteria of age and provenance, the quality of scribal habits, and the resultant text contained in the manuscript. However, there are few if any current practitioners of this method because it is so difficult to isolate the earliest form of text.

The work of H. von Soden (1911-1913) is an example of the historical-documentary method, developed independently of Hort, though contemporaneous with him. Von Soden's analysis of text-types is somewhat similar to his predecessors. He assumed there were three, which he named with the abbreviations: I - H - K, that is, the Koine, the Egyptian (Alexandrian), and the Jerusalem text. The latter type was the Western and Caesarean combined. All three he thought were recensions derived from an archetype held by Origen. Thus where all three families agreed on a variant it was almost certainly original. If 2 out of the 3 agree, this is generally enough (Aland, 1987, p. 22). However Kenyon considered his I text was mistakenly described, whilst the K text was overrated. The result, he thought, was a text "not materially different from that of most other modern editors" (Kenyon, 1949, p. 186). According to Epp (1993, p. 212), his groupings have stood the test of analysis over time very well. However the same cannot be said for his I-H-K theory of recensions, which describes how the smaller groups isolated by him fit into text-forms or recensions. Nevertheless "the isolation, homogeneity, and independent existence of most of his small groups . . . have become contributions of abiding value."

The challenges ensuing on the results of Hort's textual studies included the search for the original text behind the great uncials. Pursuant to this, von Soden classified some 1,260 minuscules, out of a resource of 1,350; this was 63% of what is now available for study (1993, p. 212). This massive work forms the basis for all classification of minuscule manuscripts today. Epp agrees that a greater part of Von Soden's monumental and detailed dissection of the Byzantine text has produced permanent results; however his reconstruction of the history of the text has not stood up so well to criticism (1993, p. 39).

Aland (1987, pp. 26-27) believes that among the many manual editions of the 20th century, Von Soden's 1913 Greek New Testament has had the strongest influence, and stands much closer to Tischendorf than to Hort.

*Competing text-types.*

Hort's views were met with both confirmation and opposition. The work of Kirsopp Lake and R. P. Blake (1902) developed the historical-documentary approach, as did the detailed analyses of B. H. Streeter (1924). At the same time the study and evaluation of the Western text brought some opposition to Hort's conclusions which led him to champion the Alexandrian text. The battle over text-types led in turn to a search for a *via media*.

Lake and Blake developed Hort's genealogical method and thereby discovered a new family of manuscripts, the Caesarean text. P<sup>45</sup> was analysed as the leading manuscript with this text-type. Epp (1993, pp. 89-90) describes how Lake and Lake, with others, went on to describe its precise nature as "pre-Caesarean", that is, a text which could be shown to be among the sources of the later Caesarean text.

Running parallel to this, Streeter also used the historical-documentary method to argue for more localised texts. G. D. Kilpatrick (1978, p. 142) explains that Streeter's aim, like that of Lake, was to recover the original text by travelling the high road of recovering the texts of the great provincial churches, then working back to a common original. By isolating smaller manuscript groups and localised texts, there emerged, for example, Family Π and the Ferrar Group; also Family Theta ostensibly represents the old text of Caesarea, after Origen arrived there. Subsequent to the work of Streeter and Lake, the growing view has been that the Caesarean text divided into pre-Caesarean, and a recensional text—the Caesarean proper. Both are seen as self-contained texts. On the basis of extensive collations and analyses, L. W. Hurtado questioned whether the pre-Caesarean is after all a distinct type (Epp, 1993, p. 90). This reinforced Aland's (1965, p. 337) earlier denial of its existence. Quantitative analysis suggests it is a "midway" text between Codd. B and D, where these latter are seen as "competing extremes of a spectrum of texts" (Epp, 1993, p. 92). By these local texts they hoped to work back to a common original which would explain them all. "Development of the genealogical method was distinctive of Streeter's generation", says David C. Parker (1977, p. 153).

Whilst Hort's view of a "neutral" text was being promoted by such analyses, opposition to his textual conclusions was seen from two directions: first, in the work of A. C. Clark who was able to argue the priority of Cod. D and the Western text over the Alexandrian "neutral", by showing from classical analysis of Cicero that Griesbach's canon *brevior lectio potior* was faulty (1977, p. 154). Epp (1993, p. 25) shows that Kenyon rallied to the defence of Hort's "neutral" text even though earlier he had argued on the basis of the mixed character of the Chester Beatty papyri, that there was no evidence in favour of a pure Egyptian text; rather Cod. B's homogeneous



character showed that it was recensional (Fee, 1993, pp. 248-251). During this same "between the wars" period, C. H. Turner (1923) further strengthened doubts as to Hort's conclusions, by a study of the stylistic and philological features of Mark; this aimed to show that safe textual conclusions could be reached without joining battle over competing text-types. In place of Hort's dictum: "Knowledge of documents should precede final judgement upon readings", he substituted: "Knowledge of an author's usage should precede final judgment" (Epp, 1993, pp. 168-169). Turner had been anticipated in this by the work of Hort's contemporary, Bernhard Weiss (fl. 1870). However, writing as an exegete rather than as a critic, Weiss reached the same high estimate as Hort of Codex B and the Alexandrian text. He did so, according to Metzger (1992, pp. 137-138), not in the usual way of first externally grouping manuscripts, but by the study of purely internal features, making no discrimination between manuscript traditions. By thus reaching textual conclusions without reference to recognised text-types, he foreshadowed later textual method.

It is clear from the foregoing developments that although Hort had achieved his aim of dethroning the *Received text*, his attempts to reconstruct the text did not meet with unqualified approval by textual critics.

The gradual accumulation of new papyri brought a shift of understanding and method. This resulted from the work done on several early papyri which have come to light, from among the Martin Bodmer and Chester Beatty collections. These date from the second and third centuries and cover a large part of the New Testament. They have been carefully studied and the results have thrown into question the earlier accepted view of the Alexandrian and Western texts. However the Byzantine text-type which makes up the majority of Greek manuscripts is still seen as a product of later recension.

*Exploring the genealogical principle*

Further opposition to Hort's conclusions came with the sceptical response to Hort's "neutral" text, by Leon Vaganay and E. C. Colwell. They believed that the genealogical method was useless as an instrument to recover the original text, as distinct from recovering the text of 200 AD (Parker, 1977, pp. 151-152). Colwell despaired of the genealogical principle because he saw that all manuscripts contained "mixture", and thus the impassable barrier of the second century remained. According to Parker (1977, p. 152), Colwell then developed the alternative method of "Multiple Readings".

Whilst Colwell was working on the Gospels and Acts, Günther Zuntz affirmed the classical genealogical principle by applying it to the *Corpus Paulinum* (1946), with the aim of recovering the state of the text as at 100 AD. Zuntz accepted the priority of the Alexandrian text and saw it as a 150-year process commencing with Origen who used superior manuscripts not for the purpose of producing merely an edifying revision, but with the critical concern of the Alexandrian school for scientific exactness. Zuntz believed that he was able, by the genealogical method, to separate out the mixture. For example, he found Western readings in non-Western witnesses. Parker (1977, pp. 149-162) explains that Zuntz discovered that seventy "Western" readings were to be found in  $\text{p}^{46}$ ; these readings, though rarely right on their own, were almost always right when allied to an earlier form of the Egyptian text; he termed the latter "proto-Alexandrian" (p. 156), and grouped it with other Egyptian witnesses. Zuntz also found Byzantine readings in "Western" witnesses.

Zuntz describes the achievement of his objective:

We thus begin to discern, beyond the later 'families', the second century reservoir from which derive all those readings, whether right or wrong, which are found in more than one of them. . . . From this 'reservoir'--it is not a 'text'--issued both the remarkably pure 'Alexandrian' stream and the muddy Western tradition. (cited in Parker, p. 156)

Parker (1977, p.157) believes that Zuntz's work avoids Colwell's criticisms inasmuch as Zuntz applied the genealogical principle to manuscript groups, not to individual manuscripts themselves, so that what he discovered were not stemmata but streams. The distinction between stemmata and streams is, however, not obvious. Nevertheless, he claimed to have discovered the second-century source, and considered the "proto-Alexandrian" to be a superior non-neutral text. The work of contemporary scholars, particularly on the Bodmer Papyri, p<sup>66</sup>, p<sup>72</sup>, p<sup>74</sup>, and p<sup>75</sup> have given rise to the same problems that p<sup>45</sup> gave to Zuntz. Thus the studies of K. Aland, A. J. K. Klijn, J. N. Birdsall and others have led to the conclusion that Hort's theory needs serious revision. Also the studies of Metzger, Porter, and Gordon D. Fee have established close links between p<sup>75</sup> and Cod B.

J. H. Petzer (1986) says:

These links have shown the text of B to be much older than the 4th century, at least as old as the first part of the 2nd century. This is the historical evidence which Westcott and Hort needed to prove that the Alexandrian text is older than the Western. (p. 21)

The discovery and study of the many papyri in the twentieth century has led to a noticeable loss of consensus in providing a firm external basis for textual criticism by means of a convincing theory which explains the history of the text. The battle over text-types, and the relevance of the genealogical principle has not served to strengthen the validity of Hort's textual

conclusions, though the latter are still acceptable to many in a modified form. It is difficult to evaluate text-types confidently when, for many, Hort's "neutral" text is no longer convincing, the Caesarean text has all but disintegrated, and the "Western" is said to lack homogeneity. The inferior status of the Byzantine text, however, remains unchanged for most textual critics. Modern textual criticism thus divides on how to weigh the external evidence, and whether there is a theoretical reconstruction which adequately explains the history of the text.

*The crisis of the criteria.*

As the following quotation indicates, the methodological problem is not limited to the difficulties encountered in weighing external evidence; lack of consensus also exists on the question of critical method, that is, how to evaluate evidence internal to the Text itself, in order to decide between variants. Epp (1993) sets out the ongoing dilemma:

Following Westcott-Hort but beginning particularly with C. H. Turner (1923ff.), M.-J. Lagrange (1935), G. D. Kilpatrick (1943ff.), A. F. Klijn (1949), and J. K. Elliott (1972ff.), a new crisis of the criteria became prominent and is very much with us today: a duel between external and internal criteria and the widespread uncertainty as to precisely what kind of compromise ought to or can be worked out between them. (p. 40)

Most critics are confessedly eclectic. The work of Eldon J. Epp (1993) focuses on the question of method. He defines an eclectic as someone who pursues:

a method: (1) that treats each text-critical problem . . . separately . . . , (2) that "chooses" . . . from among the available and recognised text-critical criteria those that presumably are appropriate . . . , and (3) that then applies the selected criteria in such a way as to "pick" or "choose" (ἐκλέγομαι) a reading from one or other MS. (p. 141)

Epp (1993, pp.142-143) goes on to explain that the weakness of the method is clearly seen when a choice must be made between criteria which conflict, both external and internal. Thus it may be impossible to apply the shorter reading if the harder reading which is to be preferred is also, as it happens, the longer reading. Likewise, the earliest manuscript principle may conflict with the "best text" desideratum; for example, a Byzantine reading in an early second century papyrus may easily put in doubt a hitherto preferred "neutral" reading in Cod. B. External evidence may indicate a Western reading which is earlier than an Alexandrian one. According to Hort, internal evidence shows that the Alexandrian text generally gives better readings; nevertheless the principle of genealogy implies that Western readings should be given more credibility. At the end of the day, for Hort the rules of intrinsic and transcriptional probability required that Alexandrian readings be preferred; he acknowledged the problem and capitulated to internal evidence. A third example of the weakness of the eclectic method is when the test of "the oldest and best manuscripts" runs counter to a variant preferred for its "conformity to the author's style."

Critics disagree as to how to make hard choices. Some are radical in approach while others claim to be more reasonable, that is, they try to maintain a balance in using differing kinds of evidence. Thus radical eclectics ignore the competing claims of this or that text-type; this departure stems from the distrust created by study of the papyri towards earlier recensional theories. Consequently, there is a prevailing sense of inability to be able to clearly define the worth of comparative text-types, when deciding between variants as to which is the correct reading. This despair of "the cult of the best manuscript" leads them to rely solely on the evidence of internal criteria. This is the approach of G. D. Kilpatrick (1990) and Elliott (1972). Most scholars however, still believe that the papyri do not touch the

validity of the traditional text-type classification as it applies to the fourth century and afterwards. Thus Fee (1993, p. 127) charges the "thoroughgoing" eclectic critic with a method which, to use Colwell's words, "relegates the manuscripts to the [mere] role of supplier of readings." Elliott's (1978, pp. 97-98) defence to this charge is: "But unless one is able to point to one manuscript as the sole possessor of the monopoly of original readings, one is bound to use manuscripts in this way."

Epp (1993, p. 169) says:

Kilpatrick claimed that his proposals entailed no disparagement of external evidence . . . yet in a real sense external criteria were seriously undermined. . . . How can [these criteria] play a role when "each reading has to be judged on its merits and not on its supports"?

In rejecting such supports, Kilpatrick (cited by Epp, p. 169) said: "We cannot accept or reject textual types or manuscripts as wholes," for that makes each segment merely a collection of variants. Elliott's (1978, pp. 108-110) further rely to Epp's (1993, p. 171) criticism of him for an unhistorical attitude was to defend his position on the basis of two reasons which he finds compelling: (1) Any theory which reconstructs the history of the text is too dubious to be of use, and (2) the radical method forces the critic to helpfully focus on the history of individual variants, to answer the prior question: "Which reading best accounts for the rise of the variants?" This involves the study of the history of doctrine, christological claims, the Arian controversy, and the influence of Atticism. Elliott (1995, pp. 330-331) also defends his apparent ignoring of the textual tradition by emphasising that the bulk of deliberate changes were made prior to the recognition of the canonical status of the New Testament, that is, prior to 200 AD.

Kilpatrick (1990, p.34) appealed, in his time, to H. J. Vogels in making a similar claim. No manuscripts are available for the earliest period; so external evidence is of little relevance.

"Reasoned eclectics" are unhappy with such a pessimistic outcome, where "the cult of the best manuscript gives way to the cult of the best reading". Their use of internal criteria is still marked, but is subject to the claims of clear external evidence, if available. Epp (1993, pp. 169-170) is unhappy with Kilpatrick's (1990) method-axiom: "The decision rests ultimately with the criteria as distinct from the manuscripts" (p. 115). He comments on the effects of the "rigorous" approach on external evidence: "For all practical purposes it has been eliminated from the text-critical decisions on the original text" (p. 170). Epp has similar problems with Elliott's thoroughgoing eclecticism, claiming that "He devotes very little attention to external evidence" (p. 170). No exposition by Elliott mentions or approves an external criterion among a long list of canons.

The distinctive method of the reasoned eclectic is described by Metzger (1992, pp. 207-211), in his *Textual Commentary* (1971, pp. xxiv-xxviii). Michael W. Holmes (1995, p. 344) notes the remarkable degree of consensus among reasoned eclectics as to how this method should operate, whether theoretically or practically. All agree on a method which decides between variants by weighing up in a balanced way the external witness of manuscripts and the textual choices indicated by applying the rules of intrinsic and transcriptional probability. There is an ongoing struggle to achieve a balanced approach. However Epp (1993, pp. 143-144) makes a distinction between eclectics who are "generalists" and others who are "specialists"; the former are those who try to give both external and internal evidence equal consideration; the latter are those who tend to rely on one or other of the two. Epp (1993, p. 143, n. 3) believes that the number of

critics who "apply the appropriate criteria without prejudice as to their relative weight or value" are very few (for example, Vaganay and Birdsall). Most by far value either external criteria above internal, or vice versa. For example, Elliott (1978, pp. 103-104) points out in defence of his own method that Fee allows even a singular reading to be original if the internal evidence is "decisive". Also other critics of the radical method at times print readings with only meagre support, and claim originality for them.

Moreover, Epp found that "a perusal of *A Textual Commentary on the Greek New Testament* by the editors of *UBS<sup>3</sup>* strongly suggests that . . . [the editors have] a predilection for an external principle . . . and most often this means B with  $\aleph$ " (1993, p. 166). "It signifies that [they] . . . are not eclectic generalists after all . . . (but) are to be classed as eclectic specialists, whether on the right wing [using external evidence] . . . or on the left", preferring internal criteria (p. 166). Fee, for example, is on the left of Epp's spectrum in trusting internal evidence above external, and only thereafter "appealing to the relative value of the witnesses" (1993, p. 140). In the ongoing struggle to achieve a balanced approach it is not surprising that a critic is driven to rely on one kind of evidence rather than another. The pattern was set by Hort's method. Of this method, Epp (1993, p. 160-163) says that by a "synergism of external and internal evidence" (p. 160) Hort tried to determine the single best text. But the process of exalting B as best meant rejecting even earlier readings in D. This "synergism . . . breaks down to reveal . . . a genuine polarity of external and internal evidence" (1993, p. 161). Preference for "neutral" over Western "not only violates their genealogical principle . . . but also is a clear capitulation to the primacy of internal evidence" (p. 162).



In the light of controversial discoveries thrown up by study of early papyri, another method expounded more recently is Kurt Aland's "local-genealogical" approach. This is eclecticism by another name. Thus, after marshalling the extant variants vying for originality, Aland, in the introduction to Nestle<sup>26th</sup> Greek New Testament (1979), says that the textual decision must "always then be determined afresh on the basis of external and internal criteria" (p. 5). Aland (1987, p. 275) comes on the right of Epp's specialist spectrum, as shown by his rule three. He calls his method "local" in as much as a manuscript stemma for an entire textual tradition is too difficult to reconstruct, unlike classical texts. How any two manuscripts interrelate is impossible to describe comprehensively. How the individual variants interrelate is, in contrast, more accessible by discovering which variant explains the history of the others. Aland describes the process of isolating all variants for a single text as "the reconstruction of a stemma of readings" (p. 276). Epp explains that Aland's method denies that the genealogical principle is valid, not only for distinguishing manuscripts more loosely grouped as text-types, but also for textual "families". "A family", says Metzger (1992), "is a relatively tightly-knit group" (p. 287). Epp (1993) says: "His 'local-genealogical' approach really seems to represent a rejection of the whole enterprise of grouping New Testament manuscript witnesses for the purpose of tracing the history of the text" (p. 116). It seems ironic, therefore, that in his 12 basic rules for textual criticism, Aland (1987, pp. 275-276) states his belief in the prior value and importance of external evidence. He also carries this through in the way he makes textual decisions for the 15 selected verses omitted from his "Standard Text" (1987, pp. 292-300). Yet he seems in fact to be sceptical as to the superior value of any particular textual tradition. How does this negative approach to text-types help him judge on the basis of external evidence? Such scepticism

may be born out of his distrust of Hort's preference for Cod. B. and his belief that the second and third century papyri cannot be categorised in terms of later text-types. The logical inference of this position is that Aland was left with only internal rules to work with in reaching textual conclusions!

Critics who prefer external evidence have a major problem in knowing "which historical-developmental scheme to adopt as normative" (Epp, 1993, p. 168); should they choose Hort's, or some other reconstructed history? Epp's judgment is:

This inconclusiveness is no fault of the eclectic method, but is rather a weakness--perhaps *the* weakness--of modern NT textual theory. . . . If reasonably confident assertions could be formulated as to precisely how our extant MSS are related to that history of transmission, these differences . . . would disappear. (p. 168)

It can be seen from this summary of the various stages in the history of textual criticism that there have emerged, in the main, three possible bases on which variant readings are evaluated: (1) the historical-documentary method, (2) the ostensibly "reasoned" approach of balancing external and internal factors, and (3) the more extreme method of dispensing with documentary evidence in favour of a free choice between readings. The differing ways of arriving at textual decisions often remove the sense of any certainty among critics that right textual choices have been made. Because it is so difficult to isolate the earliest form of text, most critics content themselves with methods (2) and (3) above, that is, an "eclectic" approach which sees no need for a rigid application of a set of rules which are entirely self-consistent--if such were possible. This means that the textual results emerging from the use of these rules differ markedly from critic to critic, depending on his controlling assumptions.

*Current Uncertainty and its Causes*

Rather than simply rest content with Hortian dogma concerning the nature and lesser importance of the Byzantine text, further research into it is being pursued through the development of Colwell's analysis of "multiple readings" (1969, pp. 26-44). Epp (1993, p. 219) explains the work carried out by Frederick Wisse and Paul McReynolds in 1982, which has focused on nearly fourteen manuscripts of Luke. It is anticipated that by using the Claremont Profile Method, groupings (that is, "clusters of manuscripts") may be established, on a more inductive basis. The aim of this method is to identify the earliest chronological group of manuscripts by a selection of key variants. This group is then linked by a similar analysis of patristic and versional data to see how they interrelate. This method recognises the relevance of number in assessing the value of a witness to the original text, but only within the context of *readings* grouped within a cluster of manuscripts. By a process of quantitative analysis critics are trying to analyse and classify the myriad variants more scientifically. In discussing this method with Epp, Aland (1979) shows his scepticism towards this approach:

Epp believes he can establish a stemma, that is an overall view, of the history of the Text. This is to be achieved by radically restricting the number of manuscripts needing to be taken into consideration to those which contain a small number of significant patterns--whether they be real or whether they be merely hypothetical--distinguishing them from those other manuscripts from which they are thought to be derived. However, if anyone believes that such established "master manuscripts" will be automatically validated by this process, he is making a basic mistake. . . . [If a critic's aim is] to investigate the earlier history . . . in order to fix on "master manuscripts", he dreams a dream that

cannot be fulfilled. The time is irrevocably gone for "leading stars" (B for Westcott-Hort, ⌘ for Tischendorf) to show the scholar a straightforward way of achieving his goal of recovering the original Greek text of the New Testament. So is the naivety which ever believed it was possible. (pp. 10-11, trans.)

Perhaps Aland was indirectly faulting an analytical starting point of this process which assumes the fundamental validity of the traditional description of text-types, grouped more or less along geographical lines. Irrespective of this problem of text-type status, other shortcomings of the method have also been exposed by Bart D. Ehrman (1987).

The loss of consensus as to how to do textual criticism exists because of the uncertain nature and relative value of the differing text-types in the earliest centuries. The value of external evidence is measured largely by the extent to which we have an accurate history of the Text. However there is no unanimity; various explanations are promoted and vie for acceptance.

The current lack of consensus in text-critical method suggests that no examination of text-types as separate entities can lead, as of now, to a categorical affirmation that one type is superior to another in the task of recovering the Original Text (Epp, 1993, p. 87). Whether a critic is more or less sceptical about the validity of classification based on geographical groupings -- for example, "Alexandrian", "Western", "Byzantine", "Caesarean" -- or on supposed editorial revisions, both feel free to choose across these familiar groupings. If these groupings are objectively verifiable, why do radical eclectics ignore them as an essential guide in making their final choice between competing variants? Should we not infer from this conflicting methodology that there is substantial uncertainty as to the comparative worth and relevance of text-types ?

Epp (1993, p. 172) believes the 'eclectic approach' of doing textual criticism, whether reasoned or radical, is only a tentative method, and a temporary expedient. Most textual criticism proceeds on the assumption that ultimately a convincing and detailed history of the transmission of the Text will be recovered. In contrast Holmes (1995) believes that "as long as our subject matter is, to paraphrase Housman, the human mind and its disobedient subjects, the fingers, hopes for a more objective method will remain an impossible dream" (p. 349). Holmes believes that, whether the dream is one day realised or not, the eclectic method of choosing between variants on the basis of internal canons will always be necessary, because it is unlikely that a recovery of the transmission will ever be so detailed and complete as to be able to explain, without the need to apply internal rules, how competing variants arose in the first place. The common aim, however, is always to get closer to the Original.

As long as there remain ambiguities in weighing up external evidence, and in employing uncertain canons of internal evidence, there will be a continuing need to find a text-critical method acceptable to all. It is in the light of this background and atmosphere of uncertainty, that the Majority text method is examined in the belief that Burgon's defence of the "Traditional Text" (Byzantine text) may yet prove to contribute to a breakthrough in text-critical method. Epp (1993, pp. 83-84) has described the current methodological impasse, resulting from a century of study since Hort, as an interlude in the history of the discipline, in the hope that through study of newly-discovered materials a break-through is about to occur. If this happens, it will bring with it greater certainty to the process of deciding which variants are part of the original Text.

*Revival of an Earlier Debate*

During the last 30 years there has been a revival of a debate held in the wake of the 1881 English revision by scholars advocating a method of doing textual criticism which questions the prevailing consensus regarding the Byzantine text. The original debate was between Burgon and C. J. Ellicott; between Scrivener and Westcott and Hort; between Miller and W. Sanday; between H. C. Hoskier and Souter.

In these discussions the results of Hort's work were evaluated, after the *Received text* was "overthrown." Although the focus on the majority of manuscript readings has not received the approval of leading critics, the debate has raised some important issues which need to be addressed.

One important reason for the revival of the earlier debate is the need to re-evaluate textual theory and practice in the light of the present uncertainty resulting from new papyrus discoveries and research. Petzer (1986) comments:

It is clear that they are still trying to prove Westcott and Hort's rejection of the Byzantine text wrong, and rightly so . . . . The papyri have strengthened the arguments against Westcott and Hort somewhat, because of the questions they raised about the relevance of [their] reconstruction of the history of the text. (p. 26)

Study of papyri from the second and third centuries shows that their text, with one or two possible exceptions, bears very little resemblance to the fixed text-types long familiar to the critic. The prevalence of mixture in the papyri shows that all text-types seem to be represented earlier than the recensional activity that was supposed to give rise to them!

This has led some critics to suggest that a fresh re-examination of the evidence for text types needs to be undertaken. Others, however, still

believe study of the papyri confirms the basic soundness of Hort's methods and results.

Another problem motivating the current debate is the way the results of criticism are now inconsistently applied. The more debatable passages are retained within the text of some modern translations for the sake of sentiment rather than from reasoned conviction, as shown, for example, by the caveat in the New International Version, with reference to the longer Marcan ending, and the *pericope de adultera*: "The most reliable early manuscripts do not have [these verses]" (1978: NT, p. 70). This shows the continuing influence of Hortian theory which relies on the readings of  $\aleph B$ . Why should the fact be ignored that the large majority of manuscripts and versions contain the relevant verses? Even though many no longer accept Hort's idea of a "neutral", that is, pure Alexandrian text, nonetheless where there is conflict in the evidence, the Alexandrian reading will usually be relied upon. In the words of Scrivener (1894), this "[made] a clean sweep of all critical materials, Fathers, versions, manuscripts uncial or cursive, comprising about nineteen-twentieths of the whole mass" (p. 288).

Some scholars now feel the need to examine more critically the theoretical reasons underlying this change. They believe that the text witnessed to by the majority of manuscripts will more likely contain the original reading. Pickering (1977, pp. 79-87, 149-159) sets out the arguments for the *Majority text* in the belief that (a) the most frequently used rules of internal evidence, as applied to specific texts, have been too seriously compromised by adverse criticism to be relied upon and (b) that the *Majority text* is more likely on a statistical basis to contain a reading closer to the original Text than any other.

Westcott and Hort (1881) established clear working principles of textual criticism and concluded from them that the text closest to the

Original was to be found in the few early fourth-century uncial manuscripts, rather than in the mass of later ones. *Majority text* advocates now question the gains supposed to have been made by applying classical methods to the New Testament text. They doubt whether Hort was correct in seeing it as an inferior secondary revision. They argue in favour of returning to the *Received text*. They believe it is unreasonable to ignore the numerical weight of the great variety of documentary attestation to a specific textual reading.

Hort's revised Greek Text was embraced by the 1881 revision committee through the very skilful advocacy of Hort who, according to Salmon (1897, pp. 33-34), was so dexterous in argument that he could make the most unlikely reading appear to be original. Burgon (1883, pp. 231, 502-503) shows how it was only with great reluctance that Scrivener acquiesced in Hort's control of the more debatable textual decisions made in the Revision process. For Scrivener (1894) Hort's theories were not only "entirely destitute of historical foundation" (p. 291), but also were based on an arbitrary evaluation of Alexandrian readings as intrinsically superior (pp. 291-292, 296). Burgon (1883, pp. 25-29, 106-107), who is described by K. W. Clark (1950, p. 9) as one of the "great contemporaries" of Tischendorf, shared Scrivener's scepticism of Hort's view. Although he believed minor skilful revision of the *Received text* was needed, Burgon (1896a, pp. 46-47) wanted essentially to retain the *Received text* on the basis that (1) it reflected the majority reading of manuscripts, versions, fathers and lectionaries from the earliest times--which accounts for the later manuscripts having a remarkably uniform character--and, (2) he believed Hort's genealogical theory of manuscripts was an irrelevant device designed to arbitrarily justify why the majority of the evidence had been consigned to oblivion (1883, pp. 253-256). Scrivener believed Burgon was right to assert



the non-neutral and corrupted state of  $\aleph B$  (1894:2, pp. 296-297, n. 1), and that in the absence of some clear external historical evidence, Hort's hypothesis of a Syrian recension was doubtful. Hort's system had "foundations . . . laid on the sandy ground of ingenious conjecture" (p. 285). Scrivener did not accept Burgon's "majority principle", as shown by his practical rule four (p. 301). Rather he assigned "the highest value to those readings which come to us from several remote and independent sources" (p. 301). Burgon agreed with this latter test (1896a, p. 52) except that he gave the Majority principle a value equal to it, because he believed that numerical preponderance of readings was an obvious factor to consider in evaluating their place and importance in the transmission of the Text (pp. 43-49).

Those critics who doubted Hort's new theory of text did not necessarily champion the *Received text* per se, as if all principles of Textual Criticism were suspect. But they believed that Hort had an unreasonable prejudice against it. Dean Burgon, in his attack on the Greek text underlying the English Revised Version, based his objections on the conviction that Codd.  $\aleph B$  are two of the most scandalously corrupt manuscripts in existence. This distrust is shared by Pickering, whose work, and that of his mentor Zane C. Hodges (1968), renewed a debate which was mostly buried with the passing of H. C. Hoskier. This attempt to rehabilitate the *Received text* has been heavily criticised by Fee, among others, in an exchange of views with Pickering and Hodges between 1968-1987. However, not all critics share Fee's (1993, p. 272) conviction that Hort was right to evaluate the Alexandrian text as superior to other text-types, and non-recensional in nature.

Burgon believed Hort's evaluation of textual phenomena lacked objectivity, and that his theory of the text lacked the required independent

historical witness in its support. This assessment of Hort's theory and practice inevitably resulted from a conviction that any method which dismissed 95% of the evidence is patently suspect, where it is based on intuitive preferences for readings which had "the ring of genuineness." He believed that closer study of all the evidence would eventually prove that Hort's judgements on the *Majority text* were incorrect, and that the day would come when the Byzantine tradition would no longer be dismissed as late, edited and recensional, and the *Majority text* would be reinstated.

Miller set forth Burgon's textual method in the years following, but Hort's view on text-types became sufficiently well established for critics to consider the status of the Byzantine text to be a closed matter by the 20th century inter-war period. After a silence of 30-40 years, the debate was effectively renewed, when Zane Hodges and Gordon Fee entered into a discussion whose motive force came partly from a renewed interest in the *Received text* by conservative Christians, in the shape of the King James Version. W. Pickering's work has provided more substance to the Majority view, though it was severely criticised by Fee. This discussion resulted directly from increasing disparity between the views of critics, stemming from the perplexing nature of the early papyri and the difficulties found in relating the results of papyrus studies to hitherto well established views on text-types.

Scholars who hope to reinstate the Byzantine text usually prefer the term "*the Majority text*." They question the correctness of limiting this type of text to the Greek-speaking eastern area of ancient Christendom. Hodges (1978, pp. 150-152) says it is safe to assume that there was continual intermixture between the textual traditions of all parts of Christendom throughout the middle ages. He believes no extant evidence invites the assumption that the *Majority text* was merely an eastern tradition, in as

much as Greek-speaking refugees from religious persecution took their manuscripts westwards with them to be copied faithfully. To say with Metzger (1992): "By the sixth century [Greek] was scarcely understood beyond the borders of the Byzantine Empire" (p. 292) may be to overlook the fact that some Biblical scribes would have known Greek; in any case they would have copied Greek manuscripts, even if they did not understand the language properly. Hills (1956, pp. 170-172) summarises evidence which shows the distinctive readings of the *Majority text* are well represented outside any one strictly geographical area. Thus, for example, Cod. A was written in Egypt; it has the *Majority text* in the Gospels, and shows the early presence of Burgon's "Traditional Text" there. Likewise the Freer manuscript of the Gospels, Cod W, has the *Majority text* among a selection of other types in the Gospels; it also may have been written in the place of its original ownership, namely, Egypt. Also Metzger (1977, p. 385) affirms the Gothic version has a fourth century Byzantine text.

However Daniel B. Wallace (1995, p. 313, n. 85) says that some *Majority text* advocates are content to see the Byzantine text as having been produced in a corner, for example, W. G. Pierpont and M. A. Robinson, W. F. Wisselink, and Russell P. Hills. Wallace suggests they infer from this that Constantine in effect acted the part of an 'Erasmus'. Hodges' *Majority text* is Griesbach's "Byzantine text", and Burgon's "Traditional Text." These are basically synonymous terms, as is Hort's "Syrian" text. This study uses the terms interchangeably, according to whose views are being represented or discussed.

There are four questions which constitute the main theoretical issues at stake for *Majority text* advocates; these determine their entire approach to textual criticism. They concern the status of the Byzantine text, the evidence--or lack of it--for the belief that the *Majority text* reaches back to the Original, the usefulness of internal rules of evidence, and the question as to what place, if any, conjecture should have in the decision-making process. These are the issues to be examined in the next chapter, the responses to which determine whether the claims of the *Majority text* approach can be seriously considered in the desire to find methodological agreement.

## CHAPTER 2

## The Theoretical Issues

There are four theoretical issues, the responses to which decide whether the *Majority text* approach is valid. They are (1) The status of the Byzantine text: Does Hort's explanation of the main characteristics of the Byzantine text really stand up in the light of current studies? (2) The relationship between the Majority text and the Original: *Majority text* advocates argue that the *Majority text* could be at least as close to the Original text as any other type. (3) The usefulness of internal rules of evidence: Why do *Majority text* advocates question whether the well-established canons of internal evidence are a sound basis for making textual decisions? and (4) the place of conjecture in the decision-making process: Are *Majority text* advocates reasonable in believing that the uniqueness of the New Testament makes it both possible and necessary to find a method which will eliminate the use of conjecture in resolving textual problems? These four areas of debate are crucial in determining whether the claims of the *Majority text* approach can be seriously considered in its attempt to offer an alternative text-critical method.

Several reviews have been undertaken of the *Majority text* debate and these have not been generally favourable. Nevertheless, whatever the critic may think of Hodges' method, the debate of the last thirty years provides a convenient vantage-point from which to re-evaluate the arguments for the secondary value of the Byzantine text of the New Testament. It also provides an interesting and convenient context within which to examine the current state of text-critical theories and methods.

*Hort's Analysis of the Byzantine Text*

Does Hort's explanation of the main characteristics of the Byzantine text really stand up in the light of current studies? For the participants in the *Majority text* debate, the first and fundamental question at issue is: Has there been an overreaction to the claims of the *Received text* to be a primary witness within the Greek manuscript tradition? Can it any longer be maintained in the light of current studies by textual critics of the second and third-century papyri that Hort's analysis of the Byzantine tradition is unquestionably correct? Hort went to immense trouble to 'dethrone' the *Received text*. He did this by delineating the internal characteristics of the Byzantine text as late, inferior and recensional. Recent studies of the papyri, however, indicate that not only may the Western and Alexandrian text-types be traceable back to the second century but the Byzantine also. Did Hort really prove that the *Majority text* cannot be found in the ante-Nicene church fathers, or was it merely assumed as a necessary inference from other evidence now put in doubt by the papyri?

These questions, once buried, may now be raised again in so far as some textual scholars in the field are themselves either asking similar questions, or doing textual criticism in a way which suggests the questions should be asked. It is also increasingly recognised that it was wrong for Hort to make his proof of the inferiority of the Byzantine text depend on the genealogical principle.

If the questions above are answerable in the affirmative, then it is worth discussing whether the *Majority text* may be closer to the original New Testament text than any critical revision. If the Byzantine text can be traced to the earliest centuries, then it vies with the other two textual

streams for the same claim to being a non-recensional witness to the first copies.

### *The Status of Text-Types*

Matters of external evidence are, theoretically, of prior importance. First, there is a lack of consensus as to meaning and significance of the various text-types for textual criticism. Metzger (1992) distinguishes a text-type from a "family" of manuscripts. Whereas the latter denotes a more tightly knit group, a text-type is defined as "a more broadly-based form of text that evolved as it was copied and quoted in a particular geographical area of the early Christian world" (p. 287).

Manuscripts have long been classified along geographical lines as Alexandrian, Western and Byzantine (Majority). Reasoned eclectics are mostly committed to these categories, though some ask for fresh reassurance as to their accuracy. Radical eclectics question the relevance of text-types. *Majority text* advocates, Pickering (1977, p. 110) for example, even doubt their existence, and wish the term could be retired.

The familiar groupings remain axiomatic for many, for example, Metzger, Aland, and Fee. The Alands (1987, pp. 67-71) state a clear but limited commitment to text-types:

Only the Alexandrian text, the Koine text, and the D text are incontestably verified. . . . [Whatever may be proposed about] the so-called Western, Caesarean, and Jerusalem text-types is purely theoretical, based on uncertain foundations and often completely in the clouds. (p. 67)

Metzger (1992, p. 179) believes text-type classification is necessary to accurately assess a very large number of manuscripts and other witnesses. Thus although the geographically-oriented terminology has been modified to some extent, the concept holds.

Fee (1993, pp. 7-8) says that:

Although there is general agreement that making such groups is both a possible and necessary task the significance of such groupings remains contested. It is surely dubious procedure to accept or reject a reading solely because it is found in a certain text-type; on the other hand such grouping . . . greatly reduces the work of sifting a multiplicity of MSS. (p. 8)

Not all critics are happy, however, with this supposed consensus. Thus Leon Vaganay and C-B. Amphoux (1986/1992, p. 70) ask that text-types be established afresh, and the radical eclectic Kilpatrick (1978) doubts their relevance in making textual decisions:

Even today terms like Alexandrian, Western, and Byzantine are current in the text-books, though they do not appear to help us much toward the solution of our problem [of recovering the original text]. If we have labelled a reading as Alexandrian or Western or Syrian, have we really discovered thereby that it is any more likely to be original? (p. 144)

Elliott (1978) makes a similar point. He acknowledges there is a history to trace but no reconstruction so far attempted is significant for textual decisions because "the ability to trace such a history is doubted by so many critics nowadays"(p. 108). The use of Byzantine readings by Kilpatrick and Elliott does not sit easily with the view that the Byzantine text is inferior, late and recensional. Few, if any, Byzantine readings can be shown to have arisen after the second century. How does the presence of them altogether after the fourth century as a text-type, prove the non-existence of the Byzantine text-type before this time? Regardless of whether a papyrus is ever discovered which shows all the Byzantine peculiarities together, some scholars seriously doubt whether Hort's description of Byzantine origins is



still valid (Holmes, 1995, p. 351). In this case, the way is more open to suggest that the *Majority text* may reach back to the Original.

For Pickering (1977, pp. 54-57), contemporary studies point to the non-existence of firm text-types, and he refers to them, accordingly, within inverted commas. If the lack of a convincing history of the Text leaves the truth or relevance of text-types in doubt, then this leaves the field open for *Majority text* advocates to offer a method of choosing between variants which does not depend on the superiority of their supposed characteristics.

#### *Characteristics of the Byzantine Text*

Can it any longer be maintained in the light of current studies by textual critics of the second and third century papyri that Hort's analysis of the Byzantine tradition is unquestionably correct? *Majority text* advocates believe that Hort unfairly stigmatised the Byzantine text--his analysis of its special characteristics is essentially faulty. Thus Hodges (1968, pp. 31-34) questions the accuracy of Hort's description of Byzantine peculiarities, a view that is shared by Van Bruggen (1976, pp. 30-35), and Pickering (1977, pp. 59-92). Hort (1881) described the *Majority text* in three ways: He believed it was the result of an editorial process which (1) produced "conflate" readings (p. 106), (2) harmonised parallel accounts in the interests of a fuller text, and (3) deliberately smoothed out roughness (pp. 134-135).

On the first point, Colwell (1947, p. 118) objected to the generalisations implicit in "conflates", in as much as all texts involve mixture. Van Bruggen (1976, p. 32) believes that the very basis of Hort's proof is defective in as much as he gives only eight examples of conflation, which all come from two Gospels. Not only are there few others to draw on, but "conflations" are not exclusive to the text-type; they occur in Cod.

B. Metzger (1971, p. 620), for example, explains Col 1:12 in this way. Van Bruggen (1976, p. 32) says, in agreement with Kilpatrick, that "many of these shorter readings can equally well be described as reduction-readings with regard to the longer and original reading." Wallace (1983) makes a similar point in commending the usefulness of Hodges and Farstad's (1982) *The Greek New Testament according to the Majority Text*: "A perusal of almost any page of text will reveal that . . . the alleged 'conflations' of the Byzantine text-type do not always hold up: quite frequently these manuscripts have a *shorter* reading than that found in Egypt" (p. 120).

As to the second perceived characteristic of the Byzantine text, Kilpatrick (1965, p. 37) questioned harmonisation as a Byzantine peculiarity because he saw this tendency at work in other text-types also. In similar vein Van Bruggen (1976, p. 33) says that it depends on which angle the reader is coming from as to whether a reading is convincingly explained as harmonisation. For example, a reading may seem to be assimilated from a parallel source, yet the third synoptic parallel still deviates--even though the assimilation supposedly occurred after the Four Gospel Canon was well established. A reading may seem to be borrowed from another Gospel yet it is out of tune with another statement in its own wider context. Van Bruggen believes that when the overall context is kept in mind it can be readily seen that examples of this arbitrary process are innumerable. Besides, he says, if this was a deliberate editorial policy, why did they pursue it so sparingly? The obvious purpose of dealing this way with parallel passages was to eliminate apparent contradictions in the face of criticism of the Gospels by the fourth century Neoplatonic school, yet the problem of apparent contradictions in the Byzantine text remains just as strongly, as can be seen in our modern text editions. Defence of the

Gospel harmony was both needed and given in Augustine's day, yet it was not achieved by forcing an artificial consistency with the use of redaction methods. Van Bruggen (1976, p. 30) refers to Hills' work on harmonisations in the Caesarean text of Mark, which illustrates how one can "prove" a non-Byzantine text-type is also characterised by the same harmonising, conflating and other supposed peculiarities of the *Majority text*. Peculiarities are distinctive and general if they characterise the whole text; however Hort (1881) himself admitted that the evidence of harmonisation and assimilating interpolations in the Byzantine text are "fortunately capricious and incomplete" (p. 135). Van Bruggen (1976, pp. 30-35) again points out that Metzger (1971, pp. 47-48, 680, 684-686) explains non-Byzantine readings in Cod. B as resulting from the tendencies of scribes to assimilate and simplify the text. Thus this characteristic is not peculiar to the Byzantine text-type either! W. F. Wisselink published a study on harmonisation in 1989, which shows that the Alexandrian text manifests this trait as much as the Byzantine. Wallace (1995, p. 305) faults Wisselink's method by saying he has attributed to the Alexandrian text-type as a whole what is characteristic of merely individual manuscripts within it.

As to the *Majority text* being a fuller text, Van Bruggen believes the reproach of "completeness" is undeserved, because it shares this reputation with the Western text, compared to which it is at many points shorter. Neither does it have several *Received text* readings which might be expected, for example, 1 John 5:7-8, and Acts 9:5b-6a. "Thus these differences cannot be mentioned as typifying characteristics" (1977, p. 35).

Wallace (1989) comments on the results of testing the "longer reading" rule in studying Hodges' and Farstad's *Greek New Testament*:

One would expect [the Byzantine text] to . . . have adapted and adopted earlier traditions. But of the 6,577 differences between the *Majority Text* and the critical texts, in only 1,589 places is the *Majority Text* longer than the critical. This is less than one-fourth of the total differences. . . . Further, the *Majority Text* is sometimes shorter than the critical text. (p. 277)

As to the third characteristic mentioned, roughness and difficult expressions are supposedly smoothed out of the text. Yet Kilpatrick (1965, p. 205) shows this resulted not from fourth-century revision, but has its source in a second-century practice of eliminating Semitisms, improving poor Greek, and "Atticising" here and there. He also believed the Semitic expressions of the Byzantine text with its smoother Greek style were in many cases part of the original text. If this is true, Van Bruggen (1976, p. 34) believes the Byzantine text is better understood as linguistic restoration rather than as a result of editorial freedom. Kilpatrick's studies show that "one cannot speak of a typical secondary character of the Byzantine text as far as the language is concerned." Van Bruggen believes he has made his case: "[Its supposed] secondary character rests on the suggestive force of selected illustrations, but is contrary to the facts as a whole. What is advanced as 'typifying' is not distinctive and is not general" (1976, p. 35). He questions whether Hort's preference for a New Testament "more fitted for cursory perusal or recitation than for repeated and diligent study" (Hort, 1881, p.135) should determine whether we believe the Original had to have characteristics naturally more admired by a philologist and textual critic, than by the common reader.

Arguing from these criticisms, *Majority text* advocates like Van Bruggen and Pickering (1977, pp. 58-62, 86) feel confident to assert that these characteristics are part of the original text. It seems the characteristics of the Byzantine text may not be so peculiar after all.

### *Applying the Genealogical Principle*

In the past an appeal to the numerical weight of the Byzantine text was ruled out by the genealogical principle, as laid down by Hort (1881, p. 57). This assumes that a family of causal relationships must be traced between manuscripts before the numerical factor can have any possible relevance to a textual decision (Colwell, 1947, p. 111). Souter (1913) explains this principle as the one "scientific" argument that Westcott and Hort offered to make the majority approach invalid:

The old unscientific method of Textual Criticism was to construct the text from the consensus of the majority of witnesses. What nineteen out of twenty witnesses read must be right against that which is read by the twentieth. This erroneous method of counting is corrected by the application of the principle of genealogy of manuscripts. (p. 104)

The genealogical principle was brought in to explain why numerical preponderance is not in itself evidence for superiority of text. Pickering (1977, p. 46) relies on studies by Colwell and Kirsopp Lake which question the validity of the genealogical principle.

Thus Colwell cautioned:

Since [I pointed out the limitations in Hort's use of genealogy] many others have assented to this criticism, and the building of family trees is only rarely attempted. . . . [However] Hort has put genealogical blinders on our eyes. (*quoted* in Pickering, 1977, p. 47).

Studies by Lake, Blake, and Silva New on the manuscripts of Mt. Sinai, Patmos and Jerusalem showed that, of the many Byzantine manuscripts found and studied there, hardly any had been copied directly from one another. Almost no evidence suggested a direct genealogical relationship between manuscripts. Pickering quotes the authors' conclusions in a statement which, if true, is fatal to all effective genealogical study: "It is hard to resist the conclusion that the scribes usually destroyed their exemplars when they had copied the sacred texts" (1977, p. 52). Thus Colwell (1935, pp. 212-213) declared that the complexity of Byzantine manuscripts, and the lack of evidence for a close genealogical relationship between manuscripts within this broad stream rules out the idea of a gradual editorial process which led to a uniform medieval text. In 1969 Colwell seemingly did a volte-face in advocating "Hort-Redivivus;" yet Hort's conclusions about the Alexandrian tradition were based not on genealogy but on internal criteria, and these do not affect Colwell's earlier scepticism towards the genealogical principle. Presumably he would still affirm his earlier statements. Thus the genealogical principle should not be used in an a priori fashion to rule out any text or textual tradition, as Hort did in the case of the Byzantine manuscripts. Its validity and appeal should be restricted to textual situations where causal links are painstakingly proved, that is, in an a posteriori manner.

There is general agreement among eclectic critics that Colwell's evaluation of the genealogical principle was correct. Thus, Kilpatrick (1965, pp. 136-153) points out that Hort never applied the principle in the way that classical scholars did; this he believes is necessary if the threefold classification is to convince us that text-type evaluation is more than hypothesis. It has been shown, he says, that a few manuscripts have

genealogical relations which can be traced, for example, families 1 and 13.

Thus:

There are manuscripts [in the New Testament] which are related among themselves in such a way that their relationship can be expressed in terms of a family tree. . . . The majority . . . are in no such condition. . . . Much less can any genealogical tree be constructed to cover the New Testament manuscripts as a whole. Consequently rigorous arguments based on . . . the imprecise grouping of manuscripts in local texts or text-types . . . cannot be employed in this way. (p. 143)

Epp's (1993) comments sum up why the whole notion of genealogy is of doubtful relevance:

The older simplistic genealogical approach (stemma and archetypes) has been abandoned almost entirely by NT textual critics (except in connection with small "families" of MSS) because it is both inapplicable to the massive and disparate NT data and ineffectual in tracing sure developmental lines through MSS with such complex mixture as those of the NT textual tradition (quoting Colwell and Kenyon). (p. 143)

If this is true, it is surprising that Metzger (1992) still feels able to dismiss Burgon's *Majority Text* approach on the basis that he was "apparently unable to comprehend . . . the force of the genealogical method, by which the later, conflated text is demonstrated to be secondary and corrupt". (p. 136)

Pickering believes that one of the important reasons why the genealogical approach is too difficult to apply to the New Testament is the presence in the text of many intentional and religiously motivated scribal errors; he thus believes the numerical factor has been unfairly excluded by the principle (p. 43). However, *Majority text* advocates are not agreed on the relevance of the genealogy principle to the text. Thus, Hodges attempts

to apply stemmatics to parts of his text (1982, pp. xii, xxv). Indeed, he believes the *Majority text* lends itself to this method:

Most modern textual critics have despaired of the possibility . . . . Nevertheless this method remains the only logical one . . . . In fact, the major impediment to this method in modern criticism has been the failure to recognize the claims of the Majority Text. Any text-form with exceedingly large numbers of extant representatives is very likely to be the result of a long transmissional chain. All genealogical reconstruction should take this factor into account. (p.xii)

Hurtado (1984) is sceptical of Hodges' and Farstad's commitment to genealogy as a basis for providing final decisions on readings, in so far as they have applied the principle so sparingly to the Text. Fee (1983, p. 112) criticises their genealogical work on the Apocalypse, in that they wrongly extrapolate from the data discovered there, that is, they fail to realise they are arguing from within a tradition which contains unique features. This, Fee believes, makes the application of their discoveries invalid outside The Apocalypse. The same, he says, is true for their treatment of the *pericope de adultera*. According to Wallace (1989a, p. 287), the attempt to apply the genealogical principle is abortive for the method, since the results of doing so have an undermining effect on the numerical principle. This is because, in the process of choosing between more than one variant within the Majority tradition, the editors not infrequently chose a reading supported by only a minority of manuscripts within it. M. Silva (1983, p. 187) believes also that, by this means, Hortian principles get in through the back door--which probably invalidates the editors' approach. Besides, says Wallace (1989a, p. 288), Hodges' approach to textual criticism is ostensibly based only on external evidence, in the attempt to avoid Hort's allegedly subjective and intuitive preferences.



However, Hodges cannot apply the genealogical method to the text without employing some form of internal criteria by which to determine what Hort (1881:2, p. 32) described as "morally certain or at least strongly preferred readings."

Fee (1978c, p. 155) agrees the genealogical argument *is* defective but shows (1993, pp. 192-195) that this fact does not necessarily touch Hort's analysis of the Byzantine text-type as late and recensional, because his evaluation was based not on genealogical claims as such, but on the internal criteria he employed

By way of summary, the work of Kilpatrick and Van Bruggen, for example, tends to show that Hort's analysis of the Byzantine tradition may not be correct. *Majority text* advocates believe that Hort unfairly stigmatised the Byzantine text--his analysis of its special characteristics is essentially faulty. Neither can the genealogical argument be pleaded as relevant to the status of the Byzantine text. If the latter were allowed to vie with the other text-types in a claim to originality, then the numerical preponderance of the *Majority text* may be seen to have more significance.

#### *Earliest Evidence for The Majority Text*

The second question which tests the status of the *Majority text* is the possibility that it reaches back to the autographs. How does the *Majority text* advocate respond to the assertion that his preferred text did not appear until the fifth century? On what basis does he argue that his preferred text could be at least as close to the Original text as any other type? He believes there is clear supportive evidence for this from the results of contemporary papyrus studies; he also believes that the

acknowledged presence of Byzantine readings in the writings of church fathers before the fourth century is evidence that the *Majority text* reached back to the second century.

*Evidence from the Papyri*

The direction of the evidence from recently studied papyri suggests a much more fluid state for the text of the second and third centuries than had before been anticipated. Epp (1993, pp. 38-39) stresses the need to discover answers which explain how the earliest papyri relate to the later text-types. He acknowledges that though most of the great figures in the past had a theory of the text, we are today without one; this is because none of the theories we inherited can be integrated with the findings of the papyri, and these papyri reflect varying textual complexions within each manuscript. Thus:

When . . . much earlier MSS . . . [earlier than the cornerstone MSS of each text-type] began to appear . . . we began to recognise the anachronism of placing these earlier MSS into groups whose nature had been determined on the basis only of the complexion of later MSS. (p. 37)

The need for solutions to this problem is demonstrated by the differing explanations as to how the textual characteristics of the earliest papyri should be described, that is, whether they can be described in terms of existing text-types, and thus be integrated with the rest of the later manuscript tradition. Thus Aland concludes that text-types do not apply to the earliest extant manuscripts, whereas Fee, Epp, and others believe that they they *are* present in embryo in the second century.

The textual characteristics of third century papyri seem to confuse the scholar's understanding of text-types. This is because studies show that a mixture of text-types existed in the second and third centuries before the

editorial activity which was thought to have produced them. Aland's response to the presence of "mixture" in the papyri was initially to question the very existence of text-types (1965, pp. 334-337), at least as they apply to the second and third centuries. Both Colwell (1969, pp. 156-157), and Epp (1993, pp. 93-94) deny that such a conclusion is necessary. Epp (1993, p. 119) expresses astonishment that K. Aland went for the simple solution. Aland remained unimpressed by the anticipated merits of quantitative analysis as a route to confirming the established outlines of text-types. Such confirmation would enable the critic to use them more accurately in making textual decisions. Epp quotes Aland as recommending the making of textual choices by simple papyrus readings, that is, as a decision-making process untrammelled by all talk of text-types:

If this 'early history of the text' is visible anywhere, it is directly and immediately [visible] only in the nearly forty papyri and uncials from the time up to the third/fourth century. Here it [the early history of the text] can be studied in the original [!]; all other efforts must remain reconstructed theories. (pp. 118-119)

Aland is not denying the reality of later text-types; but like the radical eclectic, he may just as well have done so, in as much as he makes them irrelevant for textual criticism.

Pickering (1977, pp. 55-56) avers that much textual work on the papyri indicates that there were no clear textual streams in the earliest centuries. To prove this he quotes studies in the papyri by Epp (p<sup>45</sup>, St. Mark 6-9), A. F. J. Klijn (p<sup>45</sup>, p<sup>66</sup>, p<sup>75</sup>, St. John 10-11), and Fee (p<sup>66</sup>, St. John 1-14). He believes that these studies each indicate that the papyri have a wide and complex textual colouring which defies any attempt to prove beyond reasonable doubt that they should be mainly assigned to one

text-type rather than another. Pickering quotes Birdsall's comments as implying the same thing::

In these third-century manuscripts, whose evidence takes us back into the mid-second century at least, we find no pristine purity, no unsullied ancestors of Vaticanus, but marred and fallen representatives of the original text. Features of all the main texts isolated by Hort or von Soden are here found--very differently mingled in p<sup>66</sup>, p<sup>45,63</sup>. (p. 56)

In contrast, Epp (1993) himself is more confident that the earlier papyri should be grouped in traditional categories:

Several early papyri draw to themselves other later MSS and form three reasonably separate constellations with similar textual characteristics . . . each with roots in the earliest period. . . . [These] also constitute three distinguishable "text-types" as early as the second century. (pp. 118-119)

Fee (1993, pp. 247-251, 272) is also much less sceptical than Aland of the traditional framework received from Hort, both as it applies to the Byzantine and Alexandrian text-types. Although not accepting Hort's method of reasoning in all particulars, he is in effect a champion of his main textual conclusions. Up to 1956 the mixed character of the papyri showed no evidence of a pure Egyptian text. Kenyon (1949, pp. 248-249) believed the Chester Beatty papyri indicated that Cod. B by its homogeneous character was recensional. Thus Hort was wrong to denote it as a "neutral" text (Fee, 1993, pp. 250-251). Although Cod. B was thus demoted, it came out of the third century, and was based on good Alexandrian manuscripts. In contrast to Kenyon, Fee believes he has successfully proved from a study of p<sup>75</sup> that Cod. B is not at all a late third century recension; rather the text of B existed in the second century "across two textual histories both in its main features and in most of its particulars "

(1993, p. 256). Of the three major families of texts generally accepted, Fee (1993, pp. 7-8) describes the Alexandrian as being composed of  $\text{p}^{66}$ ,  $\text{p}^{75}$ ,  $\text{p}^{46}$ ,  $\text{p}^{72}$ , Cod. B and Origen. Fee believes that "the combined study of  $\text{p}^{75}$ ,  $\text{p}^{46}$ ,  $\text{p}^{72}$ , and Origen has placed this text in all of its particulars squarely in the second century". There is, he says, no evidence of recension, that is, of a carefully edited or created text at Alexandria, whether in the second or fourth centuries.  $\text{p}^{75}$ , and B are a "relatively pure" form of preservation of a "relatively pure" line of descent from the original text" (1993, p. 272). This leaves Hort's view intact--he anticipated an Alexandrian 'neutral' prototype of Cod. B. Fee believes he has found it in  $\text{p}^{75}$ ; it is "proto-Alexandrian"; it is a carefully preserved tradition; it is a careful copy, not an edited revision (p. 272).

Hort believed he had proved that the Byzantine text-type did not emerge before the fourth century. However H. A. Sturz (1984, p. 240) attempted to show from the papyri that the Byzantine text-type is indeed present in the third century. As early as 1952 Colwell had said: "Most of [the Byzantine text] readings existed in the second century" (quoted in Pickering, 1977, p. 76). Fee (1985, pp. 239-242) was unconvinced by Sturz' reasoning, and offered three criticisms: (1) Many of Sturz' "Byzantine" readings are not in fact peculiar to that text-type, because they are present in other streams; (2) a reading from the Byzantine text-type found in an early papyrus does not in itself prove the text-type existed--other characteristic features must also be present in the same manuscript, and (3) no distinction was made between variants which are significant for determining manuscript relationships, and other readings. However, Elliott (1986) felt that Sturz' work *had* successfully "rehabilitated the Byzantine text" (p. 282).

Majority text advocates argue from the unusual results of second-third century studies for the non-existence of text-types (Pickering, 1977, p. 55). This suggests that the *Majority text* could be at least as close to the Original text as any other type. Aland's more sceptical approach to such categorisation makes Pickering's (1977, p. 57) proposal more credible, namely, that critics should abstain from allotting papyri to text-types until relationships between the later manuscripts have been empirically plotted. Until such time, the later manuscripts should be treated as individuals rather than being lumped into the Byzantine basket. Until the history of the text can be convincingly shown, the weight of a manuscript cannot be evaluated. Until individual manuscripts can be weighed properly, the numerical aspect should assume more significance in deciding which variant reading is correct (p. 130).

#### *Ante-Nicene Patristic Evidence*

The *Majority text* advocate also believes that the acknowledged presence of Byzantine readings in the writings of Church Fathers before the fourth century is evidence that the *Majority text* reached back to the second century, that is, to the earliest period of manuscript history. This was a key argument of Hort (1881, pp. 114-115) for the inferiority of the *Majority text*, namely, it appears to be absent from the writings of Greek Fathers. Hort (1881, p. 112) says we have clear patristic material only during the period 170-250 AD, and thus the evidence is restricted mainly to Irenaeus and Hippolytus in the West, and Clement and Origen in the East--especially Origen.

The evidence available to date is interpreted in one of two ways: Either the main characteristics of the later Byzantine text-type are believed to be absent from pre-Nicene writings--even though various isolated

Byzantine readings may be found among them--or, alternatively, the Byzantine text-type, even if it could be shown to be present in those writers, still bears no witness to the early presence of the Majority text, for we may safely assume that later editors assimilated Byzantine readings into the Fathers' texts to conform with ecclesiastical usage. The first interpretation will be tested by further patristic studies. The second interpretation depends on whether Hort's use of internal canons can be so trusted that we should affirm their implications as certainly as if we had independent historical evidence for them, that is, independent of inferences drawn from the text. The inference is that a fourth century ecclesiastical revision was undertaken, whether by Lucian or some other editor.

The paucity of patristic material during the second century is particularly relevant to the status of the Byzantine text, as Van Bruggen (1976, pp. 22-23) points out. The limitation means we are quite unaware of what the text of Antioch looked like in this period. From the silence we may infer that the *Majority text* suddenly emerged, but arguments from silence can be presumptuous. Where are the contemporaries of Origen or Tertullian in Antioch to show us the textual colour of their New Testament? Western Fathers and Alexandrian writers used a non-Byzantine text. This indicates two possibilities: (1) The writers preferred the local text current in their time and region, rather than the *Majority text*, and (2) the *Majority text* was not used by them because it was not in the majority in their location. We have, at present, no way of knowing whether they lived at a time and in a region where the textual tradition was at its best, or, alternatively, whether it was a tradition disturbed by all kinds of influences during the second century. Van Bruggen urges caution in a situation where difficult data can often be interpreted in various ways. Metzger's (1972, pp. 387-395) review of Boismard's studies on St. John suggest that a separate

Church Father text-type can be made out for the fourth Gospel. Van Bruggen illustrates his point from Baarda's study of Aphrahat's use of St. John; the latter interpreted Aphrahat as siding with the Egyptian text rather than the Byzantine. Yet when comparing the use of these two texts, Van Bruggen says the Byzantine text does not come out unfavourably: "A relevant variation occurs only in seven instances in the passages discussed" (1976, p. 23). If we assume on the basis of a few Fathers in three or four regions that the Byzantine text was unknown before AD 325, we encounter an even greater difficulty than the ambiguity of the evidence, and that is its sudden appearance on the scene:

How can this text . . . suddenly be known, for example, in the writings of Eustathius of Antioch (beginning fourth century), and in the writings of the Syrian Aphrahat? How can this text then be found in a section of Chrysostom's works as the known text? [If one says,] this now proves that this Byzantine text was made at the time of Nicaea [then]. . . how did it manage to spread so quickly? Through what influence? And why are there no indications, in the writings of the fourth century, that the writers were aware that they were introducing a newer text? (p. 24)

Thus Van Bruggen suggests a more plausible reconstruction of the history, as found in the law of antecedence. If fourth century writers used the Byzantine text as a normal text, then they did not regard it, he says, as "new." It is thus more likely that it was handed on from a previous age. After all, Antioch has, historically, far more significance for textual transmission than either Alexandria or Rome. It was the first Gentile Church, the hub of Apostolic activity outside Israel. It would not be surprising if it had a remarkable archive of early copies of Gospels and Letters. The fact that Antioch before the fourth century is a blank spot on the text-historical map should make us aware that our description of the textual history of the earliest centuries might conceivably be different if we



restricted ourselves to the data we actually possess, rather than reaching conclusions based, in part, on intelligent conjecture.

Earlier *Majority text* advocates like Miller believed they had proved the presence of the traditional text in the ante-Nicene writings. Pickering (1977, pp. 64-77), apparently unaware of the changing method of studying the Fathers, relies on Miller (1886, pp. 53-54) and Burgon (1896a, pp. 94-122), whose patristic studies tested Hort's view that no ante-Nicene father used the Byzantine text. After examining 76 patristic sources between 100-400 AD, Miller (1896a, pp. ix-x) firmly concluded that the "Traditional Text" (a term approximately equivalent to "Byzantine") predominated in this early period. F. G. Kenyon earlier this century (1901, pp. 277-278) criticised Miller's work from the Hortian standpoint, that is, he believed that the fourth century recensional nature of the Byzantine text did not square with Miller's findings. It was thus logical to infer that all readings peculiar to the Byzantine text, which occur in the ante-Nicene fathers, must have been placed there by later editorial assimilation (1901, p. 276). Fee (1993, pp. 201-202), whom Holmes (1983) describes as "among the most active and significant researchers in the area of patristic citations" (p. 16), objects to Pickering's reasoning on two counts: (1) He is confusing readings, which are undoubtedly present in ante-Nicene patristic writing, with those readings which, when combined together characterise the text-type, and (2) Miller's quotations of patristic references to the New Testament falsely assume the general trustworthiness of their transmission.

Burgon and Miller (1896a) had said:

Too much variation . . . of readings meets us in the works of the several Fathers, for the existence of any doubt that in most cases we have the words, though perhaps not the spelling, as they originally issued from the author's pen. Variant readings of quotations occurring in the different editions of the Fathers are found, according to my

experience, much less frequently than might have been supposed. (p. 98)

Pickering relies on Kenyon's (1901) statement that when critical editions of the Fathers have made allowance for assimilation to the Byzantine text "the errors arising from this source would hardly affect the general result to any serious extent" (p. 276). However, Fee apparently disagrees with the implications of Kenyon's statement. He emphasises that it is crucial to be sure that a Father's work is faithfully transmitted--crucial both to the specific question as to what text-type is reflected in it, and to the more general question of the theoretical soundness of the text. Fee (1995) reflects Hort's views in evaluating the importance of patristic evidence:

When properly evaluated . . . patristic evidence is of *primary* importance, for both of *the major tasks* of New Testament textual criticism, that is, the recovery of the original text and reconstructing its history. . . Unfortunately . . . the data . . . have not always been used circumspectly, thus often resulting in skewed . . . information or conclusions. (p. 191) (italics mine)

Fee (1993, p. 202) explains that reliance on either Burgon or Miller's work is hopelessly inadequate, in that they failed to recognise, for example, whether the Father was quoting one Gospel rather than another, when examining synoptic parallels. Miller (1896a, pp. ix-x) had examined 30 test passages where variations between manuscripts are more substantial and obvious. He found that the ante-Nicene fathers quoted the Traditional Text against other variations, that is, the Western or Neutral, with a ratio of at least 3:2 in favour of the *Majority text*. Fee (1993, p. 203) demolishes the credibility of these studies by referring to his enquiry into one of the 30: Matthew 9:13. Miller had cited seven Fathers who supported the Byzantine reading for this verse. However Fee found only one of them in certain support; none of the others had made it clear whether Matthew was

being quoted. It would be helpful to know whether such serious defects in Miller's work, in relation to a small sample, are characteristic of inattention to detail in the rest of the evidence for the "Traditional Text".

Critics in the Hortian tradition do not see the need for such caution in using the argument from silence; they anticipate that both past and future patristic studies will be seen to justify it. The first appearance of the *Majority text* as a text-type is said to be in the homilies of Chrysostom (Hort, 1881, p. 91). Pickering (1977, p. 63) challenged this latter identification by referring to the results of Geerling's and New's collation of Chrysostom's text. The authors said: "No known manuscript of Mark has the text found in Chrysostom's homilies, or anything approaching it. And probably no text which existed in the fourth century came much nearer to it" (p. 135). Fee (1995, pp. 197-200) reexamined the findings of Geerlings and New on Chrysostom and found that "[their] analysis, which has frequently been quoted or referred to, has proved to be quite inaccurate and misleading" (p. 197). Fee also says that studies were pursued up to 1970 with the general aim of placing a Father's text in the history of transmission, by using evidence presented in the form of lists of variants from the *Received text*. Now, with the advent of the quantitative method, percentages of agreements are established between the Father's text and other manuscripts which are representative of the main textual "families" or streams. Percentages are based on readings where at least two of the manuscripts used in the collation agree in variation against the rest. If the *Majority text* is inferior and secondary, those readings with characteristics peculiar to it are by definition late readings, and thus if they occur in early Fathers they must have been assimilated to the text by later scribes, as Kenyon had said. Pickering (1977, pp. 72-74) pleads, however, that before it is affirmed that no ante-Nicene Father quotes the Byzantine text, it must

be clearly *demonstrated* that later copyists altered the earlier Father's wording to make it conform to the revision. However, Pickering (1977, p. 171, n. 122) is looking for reasons from patristic evidence to rehabilitate the *Majority text*. He compares the treatment received by patristic texts with the way  $f^1$  and  $f^{13}$  were distinguished in the process of describing the Caesarean text-type. In order to define the Caesarean text, it was assumed that the Byzantine text is late and recensional; readings which differed from the Byzantine were made characteristic of all the manuscripts within the group, even if the variant occurred in only one manuscript. Pickering (1977, pp. 72-73) believes there is unfair bias at work here. As applied to patristics, if editorial assimilation to the later text should first be demonstrated, not merely assumed, then the patristic writings in the earliest centuries of manuscript transmission become evidence for the presence of the Byzantine text before the fourth century. Whether it be assumed or not, Fee (1993, p. 202) is convinced from his own patristic studies, and that of others, that the combined characteristics of the Byzantine text do not occur in the early Fathers.

### *The Majority Text versus Internal Evidence*

The third question posed by recent discussion and debate is: Why do *Majority text* advocates question whether the canons of internal evidence are a sound basis for making textual decisions? Is there an alternative to the eclectic method which makes the well-established rules of internal evidence redundant? Thus the *Majority text* debate is not only about the significance of external evidence; the place of internal evidence is also critical to the whole discussion.

There is among critics a prevailing consensus on this matter, that is, both radical and reasoned eclectics rely heavily on the rules of internal evidence; this is true even when, as not infrequently happens, their textual conclusions differ.

### *Questioning the Criteria*

*Majority text* advocates do not accept long-established rules of internal evidence. There are two main criteria on which the others depend: "Choose the reading which fits the context", and, "Choose the reading which explains the origin of the other reading". The second of these rules is clarified by a further guideline: *proclivi lectioni praestat ardua*--the harder reading is preferable. Colwell (cited by Hodges, 1968, p. 35) said of these two rules: "As a matter of fact these two standard criteria for the appraisal of the internal evidence of readings can easily cancel each other out and leave the scholar free to choose in terms of his own prejudgments". Hodges explains why he questions the application of the main criteria normally used to determine an original reading. He considers it does little more than provide opportunity to express subjective and uncertain opinion. G. Salmon's (1897) comment about Hort's defence of his revised Greek Text reflects the problem associated with these internal rules. It is a sad fact that, in the absence of a history of the Text convincing enough to take us beyond theory, all textual traditions must inevitably be evaluated on the basis of internal evidence. Salmon said:

That which gained Hort so many adherents had some adverse influence with myself--I mean his extreme cleverness as an advocate; for I have felt as if there were no reading so improbable that he could not give good reasons for thinking it to be the only genuine. (pp. 33-34)

Salmon's comment would still have been relevant, had Hort chosen to champion, for example, Cod. D, rather than Cod. B. Hodges (1968, pp. 35-36) believes the rules too easily conflict and appeals to Colwell to justify his reservations:

Indeed, it is Colwell who has effectively pointed out that the generalizations which scholars have been making for so long about scribal habits are based upon a quite inadequate induction of the evidence. He calls for a fresh and comprehensive description of these. But if this is needed then it is also clear that we must reconsider nearly all the judgments previously based on individual readings on the basis of the alleged tendencies of scribes. (p. 36)

Thus where the harder reading is normally to be preferred, this may at the same time be the longest reading, which is normally to be rejected.

Vaganay (1986/1992, p. 79-81) also questioned the efficacy of both the "harder reading" rule and the "shorter reading" rule; scribal habits can be contradictory. Thus, according to A. C. Clark (cited in Pickering, 1977, p. 80) while the scribe's wish to ensure that nothing be lost from the text led him often to interpolate, yet still the characteristic carelessness of scribes may have made them even more prone to omit than to interpolate.

*Proclivi lectioni praestat ardua.*

As for the "harder reading" rule, Metzger (1992) explains why scribes were inclined to introduce many intentional changes into the text: "It is apparent from even a casual examination of a critical apparatus that scribes, offended by real or imagined errors of spelling, grammar, and historical fact, deliberately introduced changes into what they were transcribing" (p. 196).

Pickering believes this fact vitiates the entire rule:

The amply documented fact that numerous people in the second century made deliberate changes in the text, whether for doctrinal or other reasons, introduces an unpredictable variable which invalidates this canon. . . . We have no way of knowing what factors influenced the originator of a variant . . . or whether the result would appear to us to be "harder" or "easier". (p. 84)

*Brevior lectio potior.*

Another rule much used since Hort is *brevior lectio potior*--the shorter reading is better. This also is currently under more serious scrutiny. Kilpatrick (1978) shows how Hort was not completely enslaved to Cod. B in choosing the shorter reading:

The maxim *lectio brevior potior* delivered Hort, on occasion, from idolatry, but is it true? . . . Can we see any reason, apart from repetition and tradition, why it should be right or wrong? We can produce reasons for thinking sometimes that the longer text is right and sometimes that the shorter text is right, but that will not demonstrate our maxim. (p.140)

Pickering (1977, pp. 82-83) describes scribal habits by quoting Colwell (1965, pp. 376-377) at length to disprove Hort's (1881, p. 235) assertion that corruptions by interpolation are many times more numerous than corruptions by omission. Colwell summarised his findings as they applied to the scribes of p<sup>66</sup>, p<sup>75</sup> and p<sup>45</sup>, as follows:

p<sup>66</sup> has 54 leaps forward, and 22 backward; 18 of the forward leaps are haplography. p<sup>75</sup> has 27 leaps forward, and 10 backward. p<sup>45</sup> has 16 leaps forward and 2 backward. From this it is clear that the scribe looking for his lost place looked ahead three times as often as he looked back. In other words, the loss of position usually resulted in a loss of text, an *omission*". (pp. 376-377) (italics mine)

Elliott (1972) sets out the implications of the tendency of scribes to omit:

The rational critic should accept the originality of a longer rather than a shorter reading other things being equal (e.g. if the style and language of the longer reading are consistent with the author's usage) on the assumption that to omit is common and accidental when copying, whereas to add to a text being copied demands a conscious mental activity. . . . As well as accidental errors explicable on palaeographical grounds, scribes often deliberately omitted material which was against their theological position. (p. 342)

Fee (1993, pp. 194-6) believes Pickering makes too much of doctrinal motives in explaining textual changes. The latter's dismissal of internal canons stems from his assumption that deliberate changes to the text were always made for dogmatic reasons, that is, with a theological motive. It does not occur to him that the myriad trivial changes resulted from the scribe's inherent inclination to alter the text in the interests of clarifying the meaning. Fee, however, sets out reasons for saying that the vast majority of changes were made, not to bolster orthodox teachings derived from the Text, but simply to clarify what is already there: "For the early Christians, it was precisely because the meaning was so important that they exercised a certain amount of freedom in making that meaning clear" (p. 195). Thus two forces were at work: first a tendency to shorten the text through carelessness or from doctrinal motives; secondly a tendency to lengthen the text to clarify meaning. Fee says the latter tendency was the more usual cause of corruption:

Thus the canon of "the shorter reading", though less useful than others, simply means that in most cases of deliberate variation scribes were more likely to have added words (pronouns, conjunctions, etc.) than they were to have deleted them. The canon of "the more difficult reading" means that since a copyist changed the text one way or the other, the change usually was made toward a more "readable," or clearer, understanding of the text. (p. 196)



None of these issues feature in Burgon's writings, for he believed textual criticism was a handmaid of theology rather than an independent discipline. Though by no means unacquainted with the very many "trivial" changes in the text, he was far more concerned with omissions which had doctrinal significance, especially those which tended to dilute the New Testament witness to orthodox Christology. For example, he saw the effects of the fourth-century Arian controversy at work motivating the many omissions of Hort's Neutral text, whereby Trinitarian doctrine was emaciated (1896a, pp. 159-165).  $\alpha$ B were seen as the products of semi-Arian or homoean teaching intimately linked with Alexandrian philosophy. The Arian controversy was played out during AD 318-381 with Constantine under Arian influence, and this coincided in mid-century with the appearance of  $\alpha$ B. Orthodoxy emerged at end of the fourth century under Chrysostom which coincided with the triumph of the traditional text. Burgon saw Origen as responsible for the shorter text, but whether or not B should be directly linked with Origen was not the crucial point. Burgon believed that Greek philosophical principles had taken the edge off Biblical doctrines, and that Origen and  $\alpha$ B shared a sceptical character in several areas. Thus (1896a, pp. 287-291), there was a tendency to soften passages which taught the true deity of Jesus Christ, and to omit passages which taught the everlasting punishment of the wicked. Besides Burgon suspected omission as sceptical--sceptical, that is, of the supernaturalism assumed by the Biblical writers. He saw some omissions as "evincing a 'philosophical' obtuseness to tender passages", like the agony of Gethsemane and the Crucifixion, and "the mutilation of the Lord's prayer" (1896a, p. 290). Burgon also anticipated Kilpatrick's detection of the many second century Atticisms contained in  $\alpha$ B. He believed that the literary tastes of classical scholars schooled in "Thucydidean compactness,

condensed and well-pruned" were too fastidious to serve as a standard by which to measure a literature designed to appeal to people whose habitual life-style was very ordinary, or even to scholars exercised in a wider range of literature. Thus the philosophical preference of the Alexandrian school led them to omit where an excuse provided. He also believed the orthodox were just as capable of changing the text from a pious motive, by "the insertion, suppression or substitution of a few words in any place from which danger was apprehended" (1896b, p. 197). Correspondingly, those who draw their inspiration from Burgon see the Traditional text as championing Trinitarian theology as a definitive, historic Christian statement.

*Majority text ambivalence.*

The misgivings of *Majority text* advocates about internal rules naturally and inevitably result from their unwillingness to ignore the numerical weight of the great variety of documentary attestation to a specific textual reading. By using the "shorter reading" rule, Hort characterised the "fuller" Byzantine text as recensional. It was thus by the use of these rules that Hort concluded the secondary nature of the Byzantine text. However by employing the same methods, Kilpatrick reached different textual decisions which implicitly challenged some of Hort's conclusions (1965, pp. 190-193). This provides another important reason for Hodges' (1970) misgivings; he was no doubt referring to himself when he said: "To anyone schooled in the standard handbooks of textual criticism, it may come as a shock, for example, to find Kilpatrick defending so-called Byzantine "conflate" readings as original!" (p. 36). Nevertheless, in his acceptance of the status quo, Fee (1993, p. 187) continues in the conviction that no historical phenomena suggest anything but the inferiority

of the Byzantine text. Rather than arguing every important detail of external evidence in response to Hodges and Pickering, Fee (1993, pp. 195-201) relies on the logical and persuasive force of applying the rules of internal evidence, believing that following the method will inevitably justify confidence in the effectiveness of this way of dealing with the text.

*Majority text* advocates tend thus to follow Burgon in their distrust of the rules of internal evidence. This is not true, however, of Hodges and Farstad, as shown by the explanation of their guiding principles in their introduction to *The Greek Text New Testament according to the Majority Text*. Moises Silva (1983, pp. 186-187) points out that their principles do not square with their earlier criticism of the "harder reading" rule. Thus, where there is substantial division of readings within the *Majority text* tradition and these readings rival each other in terms of von Soden's estimate of "good Byzantine readings", Hodges and Farstad (1982) say: "Rival readings [are to be] weighed . . . with regard to intrinsic and transcriptional probabilities" (p. xxii). Fee (1978a, p. 111) faults them for not taking a thoroughgoing attitude to internal canons; it seems they are willing to use them, but only as a last resort where the history of the text is notoriously complex, as in, for example, John 7:53-8:11. Thus Hodges and Farstad say: "Excellent reasons almost always can be given for the superiority of majority readings over their rivals" (1982, p. xi), yet at the same time they criticise the use of internal canons as "unduly subjective"--this Fee finds to be an almost incredible tour de force.

Miller (1886, pp. 120-121) believed external evidence far outweighs any other tests, and at first saw no inconsistency with his preference for the Traditional text in following the seven internal canons set out by Scrivener (1894:2, pp. 247-256) as useful. Scrivener affirmed: (1) Bengel's "harder reading", (2) Griesbach's "shorter reading", (3) the reading which explains

the origin of the variation (Tischendorf), and (4) the reading which conforms to the author's style; (5) that the variant should be evaluated in the light of "the special genius and usage of each authority; for example, always suspect the omissions of B, the carelessness of A, and the interpolations of D" (1894, p. 121); (6) that the transcriptional probabilities relating to grammatical changes, for example, itacisms, be noted; and finally (7) that nonsense readings which injure meaning and construction be rejected. Miller agrees with Scrivener in rejecting Griesbach's rule, namely, that "suspicion must ever rest upon such readings as make especially for orthodoxy" (1886, p. 121).

Modern *Majority text* advocates like Hodges and Farstad are thus seen to be identified with Miller's earlier willingness to use the internal canons. Burgon himself seems to give no quarter to it, which is an attitude which Pickering receives from him. The results of the work are correspondingly different. Thus, on the one hand, Hodges and Farstad employ internal rules when confronted with choices within the *Majority text*, and as a result sometimes choose minority readings. On the other hand, Pickering (cited in Wallace, 1995, p. 315) is reluctant to follow suit, and so finds it too difficult a task to affirm Hodges' and Farstad's choice between split readings. At this point, Pickering would seem to be furthest removed from accepted text-critical methods. However, Wallace seems to be overstating the implications of this when he says: "[*Majority text* advocates] make no large-scale effort to interact with the intrinsic and transcriptional evidence. *This . . . is a tacit admission that the traditional text is indefensible on internal grounds*" (1995, p. 315). (italics mine)

*Is Conjecture Inevitable?*

The fourth concern for *Majority text* advocates relates to conjectural emendation as a way of resolving textual problems. It is acknowledged generally that there is an increasing tendency for critics to use conjectural emendation. This tendency is shown by a survey of several places in the *UBS Textual Commentary* (cited by Holmes, 1995, pp. 348-349). Colwell (1965, pp. 371-372) admits that often a conjectural element has to be allowed in making textual decisions. He defines "conjecture" so widely as to apply it to all textual choices based on internal evidence, even those which appeal to some manuscript support:

[Hort's] prudent rejection of almost all readings which have no manuscript support has given the words "conjectural emendation" a meaning too narrow to be realistic. . . . We need to recognise that the editing of an eclectic text rests upon conjectures. If these conjectures are to be soundly based, they must rest upon transcriptional probability as well as intrinsic probability. (p. 371)

This definition of conjecture seems to assume that any textual decision based on the superiority of manuscripts is suspect, because we are still working in the realm of "the theory of the text." If this theoretical status is accepted, it follows that basing textual conclusions on the supposed superiority of one or more manuscripts falls within a dictionary definition of conjecture, namely, "to arrive at a conclusion from incomplete evidence."

The presence of conjecture is shown in the different decisions reached by the radical and the reasoned eclectic, when applying the rules of internal evidence to the same text. Westcott and Hort in their Text found 60 passages, each of which they felt contained an error older than any extant witness--thus necessitating the use of conjecture to remove them. Fee (1993, p. 191) comments on the settled opposition shown by Elliott

and Kilpatrick to many of the reasoned conclusions reflected in the *UBS<sup>3</sup> Greek New Testament* texts. Metzger (1992, pp. 184-185) also notes that the critical apparatus of Nestle's 24th Edition of the Greek New Testament includes about 200 conjectures from various sources.

### *Differing Views of Conjecture*

Fee (1993, p. 191) does not agree that textual decisions based on a balanced use of both internal and external evidence contain a conjectural element. He could, however, be interpreted as defining certainty in such a way as to leave the scholar equally free to either reject or accept conjectural emendation, as a way of dealing with the Text:

The immense amount of material available to NT textual critics . . . is . . . their good fortune, because with such an abundance of material one can be reasonably certain that the original text is to be found somewhere in it. [Thus] they scarcely ever need to resort to textual emendation, though the possibility must always be kept open that the very first copy of the original MS, from which all others derived, had some uncorrected errors. (p.6)

Holmes (1995, pp. 347-348) comes out unashamedly for conjectural emendation on the grounds that, (1) it will always be the only way forward--contrary to Epp's view that reasoned eclecticism is at best a temporary approach--, and (2) it is presumptuous to assume that the original must in every case have survived somewhere among the extant manuscript testimony. Holmes compares the historical-documentary method with the classical approach to textual criticism and shows that the latter follows two main stages in searching for the original. First the *Recensio* stage often reduces the evidence to two or more manuscripts or archetypes. At the next *Selectio* stage a choice is made between the variants. Sometimes a third and fourth stage is necessary. Thus

*Examinatio* may need to test the earliest discernible stage of the textual tradition. This third process may detect an unsound linkage in the tradition which suggests that no variant is a likely candidate for the original.

*Divinatio* may be a needed final stage where competing conjectural proposals are evaluated. This whole process assumes that the aim of reaching the autograph may prove to be a simplistic objective. "It implies some sort of 'fixed target'" (1995, p. 353). But what if, says Holmes, there were two editions from St. Mark's pen, or several original copies of Ephesians, just as there may have been two editions of Acts? Holmes believes New Testament critics should follow this classical model. With such a possibility, he says that to stop short of conjectural emendation "amounts to a squandering of resources, a neglect of evidence entrusted to us by the accidents of history" (p. 348). Thus, in many cases, recovering the original will never be confidently achieved. However close we might get to it, documentary evidence will never deliver certainty. Holmes quotes Hort to say "only intrinsic probability is concerned with absolute originality" (p. 348). Thus he believes external evidence should be weighed up in the same way, that is, by acknowledging "the accidents of history that could, if properly used, enable us to penetrate beyond the limits of the extant tradition", by "[detecting] the presence of some primitive corruption antecedent to all extant witnesses, and [recognising] in these cases the need for emendation." (pp. 348-349)

*The Majority Text View*

The *Majority text* advocate agrees with F. F. Bruce (1963) who said:

It is doubtful whether there is any reading in the New Testament which requires it to be conjecturally emended. The wealth of attestation is such that the true reading is almost invariably bound to be preserved by at least one of the thousands of witnesses. (pp. 179-180)

There are three main reasons why the *Majority text* advocate rejects conjecture: (1) He believes the vast amount of critical resources are sufficient to give an externally based evidence for making decisions which are certain; (2) the providential preservation of the text ensures certainty, and (3) human nature needs certainty as a motivational force in making moral choices.

On the first point, the *Majority text* approach believes the eclectic method will never provide certainty, whilst on the other hand an almost total reliance on external factors will provide it; that is, reliance on factors external to the literary context of a variant. Following J. W. Burgon's "seven notes" (1896a), *Majority text* advocates tend to distrust the established rules of intrinsic and transcriptional probability used to explain what gave rise to variant readings. They also suspect the value of probability in most textual judgments which are based on internal evidence and eclectic reasoning. All talk of conjectural emendation is "anathema" to the *Majority text* advocate for whom theory by very definition moves in the realm of uncertainty, and all claims to be able to discern where editing or reconstruction of the text has occurred are uncertain.

The *Majority text* advocate also rejects conjecture in the belief that (2) the providential preservation of the text ensures certainty, that is, the uniqueness of the New Testament makes it both possible and necessary to



find a way to eliminate every conjectural element (Pickering, 1977, pp. 25-26). His objection is theologically based, that is, the commitment to the Biblical concept of "the authority of Scripture" carries with it, for him, the implied corollary of the verbal inerrancy of the autographs. If such an a priori assumption be valid, it should be possible to be certain of the original reading in all or most cases. The concept of providential preservation is naturally more relevant for someone who adheres to such an a priori when coming to the Text. Kilpatrick (1990, p. 99) comments: "If such were the case, we might wonder why this Providence has not exerted itself a little further to ensure that at each point of variation the original reading would be manifest and immediately demonstrable." Burgon (1896a, pp. 11-12) had an answer to this; he believed it is unreasonable to expect that copyists should have been protected against the risk of error, in every minute detail, by a "perpetual miracle." However, Hort's revised text suggested an opposite extreme, namely, that very little, if any, providential preservation was in evidence to protect statements which had substantial doctrinal significance integral to them. For Burgon (1896a) championing one text-tradition as "neutral", that is, as free of editorial influence, meant:

that at the end of 1800 years 995 copies out of every thousand . . . will prove untrustworthy; and [assuming Hort's reconstructed text,] that . . . at the end of 1800 years much of the text of the Gospel had in point of fact to be picked . . . out of a waste-paper basket in the convent of St. Catherine. (p. 12)

Some *Majority text* reasoning bases its approach on the Protestant doctrine of Scriptural preservation, for example, "the Word of God . . . kept pure in all ages" (Westminster Confession, chap. 1:8). Wallace (1992, pp. 41-43) sees this as "entirely wrong-headed" (p. 41) for several reasons: First, its youthfulness is not in its favour, that is, it was not a doctrine of the

ancient church. Second, Biblical texts are quoted as supposed proof that the *ipsissima verba* of Scripture would be preserved. Contrary to *Majority text* reasoning, Wallace believes these texts suggest no such idea; rather, they should be interpreted as statements which offer sanction to moral laws which have an absolute claim. Alternatively they bear a prophetic character, inasmuch as they ostensibly guarantee the future fulfilment of every promissory statement. Third, it is sufficient to speak of God's providential care of the text being evidenced through the history of the Church, without having to give the idea dogmatic status; such a step is unnecessary considering the great quantity of manuscripts at the critic's disposal, and the remarkably close proximity of some of them to the originals.

The third reason explaining his rejection of conjectural emendation is the *Majority text* advocate's desire for certainty. More than anything else the conjectural element explains his dissatisfaction with the reliance on internal evidence in reaching textual decisions. Even theologically conservative writers like D. A. Carson (1979) and P. McReynolds (1974, p. 481) believe the desire for certainty in making textual decisions is unrealistic. Wallace (1995, p.300-301) illustrates the desire for certainty in the case of the textual critic E. F. Hills who, notwithstanding scholarly recognition for his text-critical analyses, nevertheless reverted to the traditional certainties of his youth by consciously embracing the *Received text*. Wallace (1992, pp. 36-37) feels it is quite wrong to equate certainty with truth:

Truth is objective reality; certainty is the level of subjective apprehension of something perceived to be true. . . . It is easy to confuse the fact of this reality with how one knows what it is. . . . Indeed people with deep religious conviction are very often quite certain about an untruth. . . .

At bottom, this quest for certainty, though often masquerading as a legitimate epistemological enquiry, is really a presuppositional stance, rooted in a psychological insecurity. (p. 38)

However, this quest for certainty, or as Wallace puts it, for "simple answers to the complex questions of life" (1992, p. 38) is no side-show in the debate over methodology. Either we seek the wording of the original Text where we can confidently say of a text: "This is it", or we reconcile ourselves to the "impossible dream" (Aland, 1979, p. 11)), or to the "retreating mirage of the 'original text'" (K. W. Clark, 1966, p. 11). Wallace's distinction between certainty and truth tends to establish a sceptical epistemology, in as much as most knowledge which has only "probability" status is still evaluated by us as psychologically certain before we are willing to act on it, that is, treat it as useful knowledge. On the other hand, Wallace (1992) is trying to be consistent with the need to treat both Testaments even-handedly:

In many places, all the extant Hebrew manuscripts (as well as versions) are so corrupt that scholars have been forced to emend the text on the basis of mere conjecture. Significantly, many such conjectures (but not all) have been vindicated by the discovery of the Dead Sea Scrolls. (pp. 40-41)

There seems to be a double-minded attitude to the text-critical enterprise, where two presuppositions, each contrary to the other, serve simultaneously to guide the critical task. These are: (1) that the goal of text-critical analysis is to achieve textual choices which are certain, that is, more than merely probable, and yet (2) that the goal of certainty in textual criticism is neither attainable nor necessary. The *Majority text* approach may itself not produce the required certainty any more than any other text-method; however it is possibly more appealing in its approach to the task,

in that it attempts to be consistent with the presupposition that certainty is required in an assertion, fact or idea before it may be considered to be fully useful or objectively true.

It is difficult to evaluate *Majority text* method, as its proponents, Pickering, for example, have hitherto given so few textual examples. Almost the entire focus is on the value of external evidence as a way of eliminating the ambiguities of subjective reasoning. However Wallace (1995, pp. 314-315) emphasises that present day *Majority text* approaches still do not eliminate the subjective element involved in making textual choices. This he (1995, pp. 306-307) believes is evidenced by the way the *Majority text* principle is used ambiguously by the differing viewpoints within the *Majority text* position. This ambiguity is illustrated in three ways: First, Wallace appeals to Hodges' and Farstad's attempts to apply stemmatics to the Text--this leads them to opt for some minority readings within the Majority tradition. Secondly, he sees Burgon as, in effect, failing to use the sevenfold method he propounded for determining the Text. [Thus Wallace believes that if a variant had numerical proponderance in its favour, the other six Notes are not taken seriously--Acts 8:37 is a good example to test this criticism, but Burgon did not deal with the text in his writings.] A third example of ambiguity in applying the Majority principle is seen in the writings of the Dutch scholars Van Bruggen and Wisselink, neither of whom apply the Majority principle exclusively in deciding between variants. This is because they allow for Byzantine harmonisations and corruptions.

In comment on the first of these three points, Hodges and Farstad may be seen as not abandoning the *Majority text* principle, if the minority reading they adopt in the process of stemmatic reconstruction deserves special status for being preserved in that special stream. They would

doubtless claim it does deserve special favour. Hodges' (1982) first principle is: "Any reading overwhelmingly attested by the manuscript tradition is more likely to be original than its rival(s)" (p. xi). This principle is so worded that it allows for minority readings, in effect; thus a minority reading within the Majority tradition may still have far more manuscript support than a reading outside it. To weigh the *Majority text* reading as "more likely" is effectively to acknowledge that other factors need to be considered besides numerical preponderance. On Wallace's second point, Pickering (1977, pp. 129-138) is careful to avoid raising the numerical principle to an absolute by expounding the seven-fold method Burgon believed should be used, to determine the identity of the Text. Thus if there exists the threefold cord of number, variety, and continuity, the identity of the text is secure. If however one of the three is missing, other factors like the antiquity and weight of the manuscripts will tip the scales toward one variation rather than another. As is shown below, Acts 8:37, on this basis, may belong in the Text after all. It is not supported by most manuscripts, but the other "notes of truth" may be in its favour. Doubtless, conservative restraint made Burgon a little wary of committing himself one way or the other.

The desire to follow Burgon's Majority approach is intimately linked with the goal of effectively reinstating the *Received text*, as being no longer Hort's "villainous . . . [and] vile *Textus Receptus* leaning entirely on late MSS" (quoted by Pickering, p. 31). D. D. Shields (1985, pp. 80-89, 132-137) shows the impetus for its rehabilitation came from Burgon, and has been felt since the mid-1950's at several levels: a popular defence, a scholarly defence, and now specifically the *Majority text* approach. Burgon, says Wallace (1995, p. 299), failed to distance himself sufficiently from the *Received text* by clarifying his views on texts which have long

been among the touchstones of New Testament criticism, for example, the *Comma Johanneum*, Acts 8:37, and Revelation 22: 16-21. However, to be fair to Hodges (1978, pp. 145-146) who began the current debate over the *Majority text*, he avers that he is willing to follow the evidence on the unresolved issues wherever it may lead, that is, whether it leads to reinstating the *Received text* or not.

This chapter has outlined the four theoretical issues in the field of textual criticism which remain controversial matters for debate. When the current *Majority text* approach is related to the greater depth and textual skill evidenced in Burgon's writings, it can more easily be seen as an alternative method of textual criticism available--a method which takes these problems seriously. The *Majority text* advocate would summarise his premises as follows: (1) The latest evidence indicates that Hort's analysis of the Byzantine Text is faulty. (2) Work on the earlier papyri tends to show that the main features of the *Majority text* did not emerge through the centuries gradually but were present from the first. (3) Recent discussions of the various [internal] criteria show increasing scepticism and ever-decreasing certainty in making textual decisions--the inconsistent way they are applied makes their canonical status undeserved. (4) It is not possible with established methodology to eliminate an element of conjecture from textual decisions. But, they claim, the uniqueness of the New Testament makes this an intolerable situation.

Is it true that the textual critic should take the *Majority text* more seriously than it was earlier thought? The answer to this question partly depends on whether a *Majority text* advocate possesses an alternative and viable critical method of doing textual criticism which, while taking into account valid criticisms of established method, still shows that its own textual decisions make sense. The *Majority text* advocate claims that he

does have such a method, one proffered by Burgon--albeit with great passion--in response to Hort. By examining concrete textual examples it may be possible to establish whether, in its practical outworking, the *Majority text* approach is credible. This question will be examined in the next two chapters.

## CHAPTER 3

## Understanding the Majority Text Method

*Explaining the Majority Phenomenon*

The discussion of the previous chapter shows how and why it is possible for the *Majority text* approach to be re-considered. After a lapse of years during which any questioning of the secondary recensional nature of the Byzantine text was dismissed, textual criticism has moved into an era of great uncertainty, particularly in weighing up matters of external evidence. The whole notion of "best text" is seen as difficult to establish, regardless of the particular text-type under discussion. This bolsters the confidence of the Majority text advocate who senses that he may after all have something valuable to offer, particularly for those who feel dissatisfaction with conjecture as a way of resolving textual issues. The notion of "best text" and the use of conjecture were considered in chapter 2. There is a third reason for the *Majority text* approach. This relates to the felt need to explain the majority phenomenon. The discussion between Hodges and Fee, as subsequently taken up by Pickering, and critiqued by Wallace, focuses on the nature of the history of transmission of the text.

*Normal versus Abnormal Transmission*

First, the *Majority text* method attempts to provide a convincing historical reason for the numerical preponderance of one type of text. Discussion revolves around the question as to whether this preponderance is explained as the outworking of a normal transmission, that is, a healthy transmission free from serious corruption, vis-à-vis an abnormal one. Hort



(1881, p. 45) had admitted a theoretical presumption which is perhaps a crucial argument for *Majority text* advocates, namely, that the text which has been copied more continuously and consistently than any other has a better claim to represent the original. This is a guiding principle for Hodges. Holmes (1983) quotes Hodges' way of developing this point:

Under the normal circumstances the older a text is than its rivals, the greater are its chances to survive in a plurality or a majority of the texts extant at any subsequent period. But the *oldest* text of all is the autograph. Thus it ought to be taken for granted that, barring some radical dislocation in the history of transmission, a majority of texts will be far more likely to represent correctly the character of the original than a small minority of texts. This is especially true when the ratio is an overwhelming 8:2. Under any reasonably normal transmissional conditions, it would be for all practical purposes quite impossible for a later text-form to secure so one-sided a preponderance of extant witnesses. (p. 15)

Pickering (1977) attempts to describe simply how normal transmission occurred:

Already by the year 100 . . . there was a swelling stream of faithfully executed copies emanating *from the holders of the Autographs* to the rest of the Christian world. . . . The producers of copies would know that the true wording could be verified, which would discourage them from taking liberties with the text. . . . I see no reason to suppose the situation was much different by the year 200.

With an ever-increasing demand . . . and with the potential for verifying copies by having recourse to the centers still possessing the Autographs, the early textual situation was presumably highly favourable to the wide dissemination of MSS in close agreement with the original text. By the early years of the second century . . . the logical consequence [was] that *the form of text they embodied would early become entrenched* throughout the area of their influence.

[Thus] a basic trend was established at the very beginning . . . that would continue inexorably until the advent of a printed N.T. text. . . . The probabilities against a competing text form ever achieving a majority attestation would be prohibitive no matter how many generations of MSS there might be. (pp.105-106) (*italics mine*)

Contrary to the generally accepted view, both Hodges and Pickering believe no historical grounds exist to suggest there has been a radical dislocation in the history of the transmission of the text. Pickering (1977, pp. 104-107) identifies the Aegean area as the home of two-thirds of the New Testament autographs, and believes that the geographical area, later termed Byzantine was far better placed to check proliferating copies against the Original, even as late as 200 AD, than Alexandria for example, which did not receive a single original manuscript of the New Testament. Given this distinct advantage, Pickering believes it is fair to presume that "the early textual situation was highly favourable to the wide dissemination of manuscripts in close agreement with the original text" (p. 106). Thus he says:

A basic trend was established at the very beginning--a trend that would continue inexorably . . . because, given a normal process of transmission, the science of statistical probability demonstrates that a text form in such circumstances could scarcely be dislodged from its dominant position . . . . It would take an extraordinary upheaval in the transmissional history to give currency to an aberrant text form. (pp. 106-107)

Pickering (pp. 88-89) cites Kenyon, Colwell, F. C. Grant, and Jacob Geerlings, who all found difficulty with Hort's view that the Byzantine text was the product of a deliberate revision.

Birdsall (1970) candidly admits that this is:

. . . one of the major problems of the historian of the New Testament text. . . . The origin of the Byzantine text is [not] known. . . . [It] is frequently ascribed to Lucian of Antioch, and the ascription is turned to fact by frequent repetition . . . but . . . there is no direct evidence of any philological work by him upon the New Testament text. (p. 320)

It is by no means clear just how crucial the numerical factor is for a *Majority text* discussion on a given variant; however, the principle is clear. This principle establishes the continuity of the current discussion with Burgon. Hodges quotes his brother's mathematical work to justify the relevance of the statistical argument (1978a, p. 148). However T. J. Ralston (1994) has recently sought to show through computer analysis that this method of using textual data in exploring manuscript traditions is faulty.

#### *The Case for Radical Dislocation*

Fee (1993, pp. 183-188) says that the way Hodges and Pickering argue their case for "normal transmission" amounts to a "rather total illogic" (p. 185, n. 6); he offers a way of explaining how one text-type could become predominant. A satisfactory explanation for the *Majority text*, he says, is found in three factors that converged between the fourth and seventh centuries: (1) The trained scribe emerged, for example, in Alexandria, whose more disciplined approach would begin to "freeze" (1978a, p. 26) the forces making for diversity in the text; (2) the lessening amount of new variation in text would be proportionate to the growing awareness of "canon" among copyists, and would increase scribal concern for accuracy by cross-referencing readings with other manuscripts,

and (3) the making of copies was motivated by the need to study rather than distribute them, which had the effect of focusing the text within a narrow Greek-speaking geographical area.

Metzger (1992, p. 291) cites several upheavals which show the transmission was not "normal." Some examples of human interference which radically dislocated textual transmission are: the Imperial persecutions; the destruction of libraries on a large scale; the Islamic conquests which caused copying to cease, for example, in Alexandria and Caesarea; and the decline in the use of Greek in the Western Church.

The idea of radical dislocation could perhaps work in favour of the *Majority text* in the belief that it reaches back to the second century. Because the destruction of manuscripts occurred on such a vast scale through the great persecutions, for example, this might help to explain why there are so few manuscripts of the Majority type from this early period. It is unlikely that one text could gain a dominant position through a natural process of development, in the face of traumatic historical events like the great persecutions and the destruction of manuscripts--even entire libraries. Such events would radically dislocate the textual transmission and work against the *Majority text*, stopping it from gaining dominance. The extant evidence gives the impression that the *Majority text* did not gain its dominant position until the early medieval period. However, rather than this being a necessary inference, it may instead be a mere assumption, made through lack of any available evidence to suggest otherwise.

Fee (1993, p. 185) finds Hodges' reasoning illogical because, he says, the further removed a manuscript is from the autograph, the more it will reflect the errors made during the history of its transmission. He compares manuscript history to that of human genealogy (p. 185, n.6).

Thus the multiple effects of producing a new copy, by using two manuscripts reflecting different text-types, may be compared to the physiognomous changes where a couple from two distinct and widely-separated ethnic and geographical backgrounds marry; this leads to new and quite unpredictable features, whose origins may not thereafter be easily traced. Thus, for example, if the results of Alexandrian editing of a passage are transmitted en bloc to another manuscript hitherto uninfluenced by it, the potential for error is greatly increased, and even more so with the further passage of time. In contrast, Hodges' (1978a, p. 152) approach is probably explained as an unwillingness to see the relevance of family patterns, or text-types, for the first two centuries. Thus, assuming that the *Majority text* may reach back to the early second century, it could not have resulted from the cross-fertilisation between patterns of variation.

Both Metzger (1992, p. 212) and Fee still affirm a deliberate revision by Lucian or his associates. Fee (1993, p. 8) believes most textual critics agree with this. Kenyon (1901, pp. 277-278) suggested that it was the result of a gradual and deliberate process over time. Fee (1993, p. 187) describes the process in three stages: The "full-fledged form" appeared by AD 350; it evolved from an earlier form, and finally came to full flower in the eighth and ninth century uncials. Parvis (1952, p. 172) quotes the view of F. Pack who traced the beginning of such a process to Origen, and believed it was completed by Chrysostom.

T. J. Ralston (1992) explains the effect of this process:

Hodges' statistical model which lies at the heart of the *Majority Text* theory demands that a text-type becomes less homogeneous over time as the cumulative effect of scribal errors and emendations are transmitted in subsequent generations of manuscripts. . . . However, the case is reversed for the Byzantine manuscripts, which grow more homogeneous over time, denying Hodges' statistical presupposition. (pp. 133-134)

Wallace (1995, pp. 310-313) summarises three reasons to explain why the *Majority text* cannot claim to represent, or even be close to, the Original: (1) The *Majority text* did not fully develop its typical characteristics until the ninth century, and did not exist at all in the first four centuries; thus we find no Byzantine text-form in the papyri; (2) most of the Versions, for example the Coptic, Ethiopic, Latin, and Syriac do not reflect the Byzantine text--even if one of these versions did reflect it, this would still not mean that the versions in general supported the *Majority text*; (3) the ante-Nicene fathers did not use the *Majority text*. Metzger (1992, p. 279) emphasises Wallace's third point (dealing with patristic evidence) in affirming that there has been a radical dislocation in transmission. Writers like Origen and Jerome explicitly refer to readings which were familiar to them in many manuscripts then extant, but which are now not available; though widely known these readings are now found in few witnesses or in none. "Such a situation rules out any attempt to settle questions of text by statistical means" (p. 292).

### *Unresolved Differences*

Hodges' (1978a, pp. 148-152) response to Fee's way of describing how the *Majority text* emerged, was to ask for documentation to support these assertions (p. 148), as he believes a growing uniformity would

demand more, rather than less, intense communication and distribution of texts between copying centres. We look in vain, he says, for any convincing explanation as to how ecclesiastical forces could so effectively work against the earlier and great diversity to produce a uniform text. He (pp. 151-152) quotes Birdsall's studies on Photius, Patriarch of Constantinople, which show he did not use the official, or standard text in his writings. But it is inconceivable, Hodges believes, that a ninth-century teacher exercising broad and deep scholarship in the Imperial University (Birdsall, 1974, p. 779) should not have used the Byzantine text at the height of its acceptance, if such an edited text existed. Hodges (1978a, p. 152) suspects from this that "the concept of a standardised Eastern text is an historical fiction." Hodges (1968/1970) believes that, in the absence of historical evidence independent of the Text itself, the idea of a deliberate editorial recension is inherently improbable.

The Majority text, it must be remembered, is relatively uniform in its general character with comparatively low amounts of variation between its major representatives. No one has yet explained how a long, slow process spread out over many centuries as well as over a wide geographical area, and involving a multitude of copyists . . . could achieve this widespread uniformity out of the diversity presented by the earlier forms of text. . . . An unguided process achieving relative stability and uniformity in the diversified textual, historical, and cultural circumstances in which the New Testament was copied, imposes impossible strains on our imagination." (pp. 33-34)

Burton (1883, pp. 292-296) rejected Hort's theory of a Lucianic recension for lack of concrete evidence: "It is simply incredible that an incident of such magnitude and interest would leave no trace of itself in history" (p. 293). At the same time he saw the crucial importance of the theory to any text-critical approach which presupposed the inferiority of the Byzantine text.

Contrary to Metzger's view, Hodges (1978a, pp. 150-152) believes, as did Burgon, that the transmission of Greek manuscripts was not so abnormal as to restrict the copying of them to Constantinople during the Byzantine period. He believes the survival of bilingual Graeco-Latin codices from the Middle Ages suggests that an interest in Greek continued among scholars; consequently text-types other than the Western were copied and in circulation there, as manuscripts moved back and forth during the period.

Hort's argument from genealogy is no longer an issue, yet its influence lingers on. Miller (1886, pp. 47-49) believed that Hort should not have made genealogy the groundwork of an argument. To him, applying such a figure to manuscripts suggested a false analogy, in as much as distinct human generations gradually come and go over time, whereas "generations [of manuscripts] might be generated as fast as the pens of scribes would admit" (p. 48).

Hodges (1968/1970, pp. 31-34) relies on the fact that no satisfactory explanation is available to account for the rise, apparent uniformity, and dominance of the *Majority text*. Thus in the absence of historical evidence to the contrary, we may assume it represents simply the continuous transmission of the original text from the very first. "All minority text-forms are, on this view merely divergent offshoots of the broad stream of transmission whose source is the autographs themselves" (p. 34). Burgon (1883) had an answer to the question: "Why then is the *Majority text* completely unknown in the ante-Nicene Fathers, and why are there no manuscripts from the earliest period which contain this Text?" Referring to Hort's preference for the Alexandrian text, he said: "Had B and  $\alpha$  been copies of average purity, they must long since have shared the inevitable fate of books which are freely *used* and highly prized; namely,



they would have fallen into decadence and disappeared from sight" (p. 319). Constant handling and unremitting use of *Majority text* manuscripts resulted in loss of evidence as they perished. Pickering (1977, pp. 123-124) cites a scholarly explanation for the silence of extant witnesses to the *Majority text* in the earliest period--it resulted from the scribal habit of tearing up the exemplar, particularly once it became practically useless. Fuller (1970, p. 7) develops this theme by saying that those that survived, which were "offshoots" of the broad stream, were those the Greek church put back permanently on the shelf for being faulty. Wallace (1995) believes this explanation is nullified by its implications:

If the Byzantine MSS wore out, what is to explain how they became a majority from the ninth century on? On *Majority text* reckoning, the real majority should never be found as an extant majority. Further, what is to explain their complete nonexistence before the late fourth century? Are we to suppose that every single "good" NT somehow wasted away--that no historical accident could have preserved even one from the first 350 years? . . . [This] stretches the credibility of the theory far beyond the breaking point. (pp.311-312)

Hurtado (1984, p. 162) is at a loss to know how the survival of a majority text indicates anything more than its popularity as a text preferred by scribes or readers.

An argument from silence is very prominent in all criticisms of the *Majority text*. This is shown in the way Wallace (1995, p. 311) offers his reasons why the *Majority text* cannot be even close to the original. Thus, he says, the discovery of almost 100 New Testament papyri over the last century, none of whose earliest examples reflects the homogeneous text characteristic of later Byzantine manuscripts, is sufficient evidence to prove the Byzantine text is late and recensional. Similarly, Fee (1978a, pp. 27-29) believes it is fatal to Hodges' view that there are no examples of the

Majority text-type in the early papyri. He says he has shown (vide 1993, p. 256, 301-334) from the relationship of Cod B with  $\text{p}^{75}$  that they share an Alexandrian text-type which is non-recensional, and which reaches back to an archetype in the early or mid-second century. Hodges in reply (1978a, pp. 153-154, 1978b, pp. 162-163) discusses the results of studies done on the relationship of Cod. A with Cod. Petropolitanus P. These lead to the inference that both texts derive from a common ancestor at least coeval with  $\text{aB}$ ; furthermore that the ninth century P was adjudged closer to the text of the ancestor than the fifth century Cod. A. Hodges uses this example to appeal to the fact that all textual scholars regularly infer from critical data that early non-extant archetypes must be postulated for later manuscripts. "What we maintain is that the extant evidence for the majority text demands a very extended transmissional history, and therefore its existence long before any of its surviving representatives were written must be assumed" (p. 162).

The *Majority text* advocate thus hopes to provide a convincing historical reason for the numerical preponderance of one type of text. Hodges' explanation of the majority phenomenon focuses on Hort's admission that the text which has been copied more continuously and consistently than any other has a better claim to represent the original. Hort (p. 45) added a caveat that "the smallest tangible evidence of other kinds" must be heeded which weighs against this presumption. But Hodges remains unmoved by the silence of the earliest papyri, in the belief that silence does not constitute the "tangible" evidence that weighs against Hort's presumption.

There is general rejection by eclectic critics of Hodges' and Pickering's arguments for the *Majority text*. Even an ultra-conservative like T. P. Letis (1987, pp. 12-13), who asserts the superiority of the *Received*

*text* on a theological basis, that is, a belief in the providential preservation of the best text, emphasises as a fatal weakness the "inability [of the Majority text advocate] to substantiate the majority principle." He sees Elliott's welcome of the new Greek *Majority text* New Testament as a "latitudinal approach" (p. 13), serving to promote the *Majority text* not because its readings may bring us nearer to the Original, but as a foil not only to the *Nestle/UBS* "standard" monopoly-text, but also to Fee's "triumphalism" in lauding reasoned eclecticicism as the currently reigning method.

*Majority text* advocates use two basic arguments: first, that on a statistical basis the *Majority text* is more likely to contain a reading closer to the original Text than any other, and second, that the main rules of internal evidence are not sufficiently self-evident or reliable to be used as sound guides to the correct reading. Pickering (1977, pp. 91-92) stresses that the relevance of the first argument, namely, that the *Majority text* is closer to the original than any critical revision, partly depends on whether or not it is admitted that we have, as yet, discovered the proper history of the New Testament text.

Wallace (1992, pp. 29-30) faults the use of the numerical factor in evaluating the text, on the assumption that *Majority text* advocates are interested in numbering only Greek manuscripts. He thinks they are inconsistent in failing to number manuscripts of other versions whose textual colour may be at variance with the Byzantine manuscripts, for example, the Old Latin tradition. However Burgon (1896a) clarifies this point in replying to those who use a well-known maxim in criticising the *Majority text* method, namely, "witnesses are to be weighed, not counted."

He says:

It assumes that the 'witnesses' we possess,-- meaning thereby every single Codex, Version, Father--, (1) are capable of being weighed : and (2) that every individual Critic is competent to weigh them : neither of which propositions is true. . . . The undeniable fact is overlooked that 'number' is the most ordinary ingredient of weight and . . . is an element which even */sic/* cannot be cast away. (p. 43)

This comment has immediate relevance as it aptly describes the difficulties currently experienced in providing a sound historical base to New Testament text-critical method. Apart from this, it shows the wide range of evidence to which the numerical factor should be applied. Having recounted the various ways in which countless numbers of manuscripts have perished, Burgon (1896a) lists the then-known available resources:

Nevertheless 63 Uncials, 737 Cursives and 414 lectionaries are known to survive of the Gospels alone. Add the various Versions, and the mass of quotations by Ecclesiastical writers, and it will at once be evident what materials exist to constitute a Majority which shall outnumber by many times the Minority, and also that Number has been ordained to be a factor which cannot be left out of the calculation. (p. 45)

Clearly then, more than Greek manuscripts are in view. All the evidence must be considered under this head. Burgon believed the multitude of the evidence shown by the *Majority text* for the authentic reading of disputed passages gives substance to his view that: "Number . . . constitutes Weight . . . not of course absolutely, as being the sole Test, but *caeteris paribus*, and in its own place and proportion" (*ibid.*, p. 44).

*Rationale for the Majority Text Method*

Wallace (1995) explains the several arguments which constitute Burgon's approach, on the basis of a belief in verbal-plenary inspiration:

(1) a theological a priori that God has preserved the text--and that such a preserved text has been accessible to the church in every age; (2) an assumption that heretics have, on a large scale, corrupted the text; (3) an argument from statistical probability related to the corollary of accessibility (viz., that the majority is more likely to contain the original wording); and (4) a pronouncement that all early Byzantine MSS must have worn out. As well, a fifth point is inferred from these four: arguments based on internal evidence (e.g., canons such as preference for the harder and shorter readings) are invalid since determination of the text is based on the "objective" evidence of quantity of MSS. (p. 299)

Fee has criticised Burgon's method as a virtually total reliance on external factors. He sees it as no more than an invitation to count manuscripts as the one way of evaluating readings. Wallace (1992) shares Fee's view that the *Majority text* method is "in essence purely external (i.e. counting manuscripts)" (p. 37). Wallace (1989, pp. 279-282) joins Fee and others in exposing flaws in Hodges' reasoning from the majority phenomenon.

However, the method deserves more than a dismissive comment about a method which merely "counts noses". If the *Majority text* advocate applies the seven notes faithfully to a variant, it seems an insult to Burgon's intelligence to describe his method as one which depends mainly on statistics. Also Burgon's work should invite more than cursory attention if K. W. Clark (1950) was correct in describing him as one of the "great contemporaries" of Tischendorf (p. 9).

Rather than dismiss the method, it is worthwhile to examine the nature and implications of Burgon's seven notes, as outlined by Pickering (1977, pp.129-137) who attempted to faithfully describe and develop them a little, though only in theory, it would seem. A study of Burgon (1896a) shows that he linked three of the seven notes as especially inter-dependent; these were, number, variety and continuity. They were to act as "a threefold cord not quickly broken." He also acknowledged that the age of a manuscript is an important consideration not to be overlooked. However he felt that too much trust was placed on the readings of the then five ancient uncials, simply by virtue of their great age (p. 29). The advice may still be wise today, *mutatis mutandis*.

The first argument named above, that the *Majority text* is closer to the original than any critical revision depends on Pickering's (1977, p. 91) belief that we have not yet discovered the proper history of the New Testament text. As we are unable to weigh the external evidence properly, it is wise to assume that the true reading will be contained somewhere within the manuscript tradition which has been better preserved through the centuries than any other (pp. 106-107). It is better to accept a variant reading upheld by the mass of manuscripts as genuine (p. 130), regardless of age, rather than wrestle with textual decisions resulting from doubtful accounts of how the various readings arose.

The *Majority text* method in its modern form is very different from the more sophisticated ways of studying variation units within the text. It is thus regrettable that *Majority text* advocates have not provided sufficient examples of how they apply the method to specific texts. The guidelines however are clear enough in the written analyses of Burgon, who provided the learning and impetus for the method.

Burgon's (1896a, p. 29) seven "Notes of Truth," to test the worth of a variant reading, are explained by Pickering (p. 129), as an alternative way to judge the worth of a variant. They are listed as follows:

1. Antiquity, or Primitiveness.
2. Consent of Witness, or Number.
3. Variety of Evidence, or Catholicity.
4. Continuity, or Unbroken Tradition.
5. Respectability of Witnesses, or Weight.
6. Evidence of the Entire Passage, or Context.
7. Internal Considerations, or Reasonableness.

Thus the age of a manuscript is only one important factor to consider, and not the main basis of evaluation. The variant should occur early, and there should be continuity; every period of church history should show its use; the variant should show up throughout Christendom, whether in manuscripts, fathers, versions or lectionaries; the manuscript witness should be "respectable," that is, generally reliable; the witness should be "credible," that is, its textual context not suspiciously confused with conflicting variants; the numerical preponderance of manuscripts witnessing to the variant should be as "full" as possible; finally, the reading claimed as original should be "logically possible," that is, not grammatically or scientifically unsound.

A discussion of Burgon's Seven Notes follows.

1. Age: As to which centuries were most important for determining the Text, Burgon (1896a, p.42) said: "If I must assign a definite period . . . [I mean] the first six or seven centuries." He believed that chronology had a wide application in evaluating the comparative worth of manuscripts:

[As] a general rule, only, a single early Uncial possesses more authority than a single later Uncial or Cursive, and a still earlier Version or Quotation by a Father must be placed before the reading of the early Uncial. (p. 41)

"The more ancient testimony is probably the better testimony" (p. 40) but "not by any means always so"--hence the need for other tests. Antiquity as a principle must mean "the greater age of the earlier copies, Versions, and Fathers," although, as one note of truth "it cannot be said to cover the whole ground" (p. 43). Miller's work (p. 42) also quotes the evaluation of others in attempting to define "Antiquity" in terms of the best cut-off point.

Burgon (1896a, pp. 29, 40) believed that the major corruptions to the Text occurred during the second century, that is, within the first 50-100 years. Therefore "the earliest manuscript" principle is not the key, but the "best attestation," that is, not the five oldest Uncials  $\alpha$ ABCD together, or this or that version, but "the body of ancient authorities" (p. 31) or at least a majority of them. The key for Burgon is "the entire mass of ancient witnesses," not "a fragment . . . arbitrarily broken off" (p. 31).

H. A. Sturz (1984, pp. 67-70) shows that where several later independent manuscripts agree, it is rightly assumed that they point to a common source older than themselves. Pickering (1977, pp. 129-130) believes that inference is usually unnecessary, that is, wide late attestation usually has explicit early attestation. Looking for the oldest reading is also important in cases where other older variants compete for acceptance. So Burgon's other six "Notes" are essential in deciding between variants.

Burgon could not see why, because a manuscript is an uncial, it is by definition superior to a cursive (1896a, p. 36). There is no available proof that the agreement of the five oldest Uncials guarantees an authentic reading. He thus enquired for the verdict given by the main body of the



copies, which is generally unequivocal. Where doubt exists, he examined separate witnesses to see what "they may singly add to the weight of the vote already tendered" (pp. 37-38). He thought that when the uncials agreed with the main body of cursives, they are more likely to be right by 100 to 1, "because . . . they embody the virtual decision of the whole Church" (p. 38). This brings in again Hort's "ring of genuineness" argument, but at a collective unconscious, or corporate level.

Burton's high esteem for the cursives was linked with his conviction that they were not all descended from the uncials. In contrast to Hort's approach, he deduced from laborious collation that  $\alpha$ ABCD were "as a rule . . . discordant in their judgments" (p. 37), that is, discordant among themselves, unless uniting against the majority. Putting the same point slightly differently, when the majority principle is followed, it can be seen that  $\alpha$ B are usually wrong, and B is often wrong as against  $\alpha$ . When all five uncials agree against the majority, as they often do, all are still in error; that is, if the cursives and later uncials are practically unanimous, it is unsafe to assume that "a veto can rest with such unstable and discordant authorities" (pp. 37-38).

The Majority principle however directly challenges as unproved the assumption that an earlier manuscript will usually transmit a superior text. The theory of genealogy was designed to establish this assumption as a reasonable one, but it has been implemented with very little success, unless the ostensible link Fee (1993, p. 272) has drawn between B and  $p^{66}$  and  $p^{75}$  is an exception. Using the principle: "Identity of readings implies identity of origin", Hort concluded  $\alpha$ B were derived from a common original much older than themselves "the date of which cannot be later than the early part of the second century, and may well be yet earlier" (*quoted in* Miller, 1886, p. 40). Hort deciphered a common original in the

resemblances of manuscripts, and, discarding individual traces of corruption, he inferred the purity of their text. Miller objected to this line of reasoning by questioning the presumption that  $\alpha B$  were in existence by the early second century, in as much as "generations [of manuscripts] might be propagated as fast as the pens of scribes would admit" (1886, p. 48), especially after the wholesale destruction of manuscripts during the reigns of Diocletian and Galerius. However some scholars now assert the presence of a proto-Alexandrian text in  $p^{45}$ ,  $p^{46}$ ,  $p^{66}$ , and  $p^{75}$ , although Pickering (1977, pp. 55-57) supplies reasons why the papyri should not be associated with any particular text-type.

2. **Number:** If a reading is attested by a majority of independent witnesses, it is likely to be genuine: if by only a few witnesses, it is unlikely to be genuine. The greater the majority, the more nearly is the reading taken to be accurate. Unanimous attestation provides the desired certainty (Pickering, 1977, p. 130). Burgon (1896a, p. 44) however, was far too aware of the several kinds of evidence to suggest that readings should be decided by counting as the supreme test.

Burgon (1896a, pp. 43-44) appeals to the analogy of the courtroom, consonant with describing manuscripts as "witnesses" to statements or events, the truth or falsehood of which significantly affect the readers' quality of life. Thus where facts are in dispute, and nine witnesses in court independently unite against one dissenting voice, does the jury ignore the numerical factor in weighing up issues of credibility? Not surprisingly, Burgon was unable to evacuate New Testament textual criticism of all theological content. Thus he evaluated Majority readings as God's way of affirming, though in a general sense only, the integrity of the *deposit* as hard fact (p. 44).

He thus related the numerical factor to other relevant factors, for example, the different times and circumstances when copying took place, and the different kinds of evidence in support. Where two readings competed with each other with a difference of 1500 to 10, the ratio for Burgon spoke for itself eloquently (pp. 46-47).

The genealogical principle was raised to nullify the numerical argument; it was thought to give substance to the presumption that the *Majority text* manuscripts descended from a common archetype (not the Original!), thus ruling out the possibility of drawing any positive inferences from numerical weight. However the *Majority text* advocate believes there are good grounds for believing the Byzantine manuscripts have been shown to be individual witnesses in their own generation. Thus Pickering (1977, p. 52) cites the studies of Lake, Blake, and Silva New which provide supportive grounds for their attitude to the *Majority text*. The objection to maintaining Hort's assumption that the Byzantine manuscripts are a product of recensional editing is that their genealogical history has not yet been plotted to show whether the large number of readings--readings peculiar to themselves as a group--finds its source in the Original itself, or in a recensional archetype. Until such empirical proof is demonstrated, it is right to assume that Burgon's statement applies to the Byzantine manuscripts at *every* historical stage of their transmission: "every one of them represents a manuscript, or a pedigree of manuscripts older than itself; and it is but fair to suppose that it exercises such representation with tolerable accuracy" (1896a, pp. 46-47). It is assumed that this text reaches back to the autographs in the belief that no evidence has yet been produced, for example by study of the earliest papyri, which clearly invalidates the assumption. Thus as part of the weighing process, manuscripts must also be counted.

Miller (1886) points out that, in a ratio of 9 Byzantine manuscripts to 1 Alexandrian, if the critic says that the *Majority text* has only the value of one manuscript, he in effect "disregards the presumption that a larger number of descendants came from a larger number of ancestors, and that the Majority may be only thrust back from one generation to a previous one" (p. 48). Thus if you trace the argument back to the fourth century, it is right to assume that the *Majority text* of the 20th was also in the majority in the fourth century, unless there is proof positive against it. The comparative paucity of manuscript remains is not itself an argument against it. If there are only a few witnesses in favour of a variant, it is unlikely to be genuine. The greater the majority, the more nearly certain the reading. Unanimous attestation implies originality. Ancestors are assumed to be independent until proved otherwise, for example, by community of error. Thus the five oldest Uncials  $\alpha$ ABCD (1896a, p. 43) appeal by virtue of their great age, yet on repeated study Burgon was very impressed by their *concordia discors*. Pickering's confidence in his scepticism towards Hort's claims for the superiority of  $\alpha$ B also arises out of serious doubt whether the internal canons can reliably show whether a variant reading is true or false. The majority of manuscripts do have characteristic readings in common. But the radical method of Kilpatrick and Elliott, and its results to date, suggests that a "community of error" has to be proved again, that is, it has to be shown that Hort is right--Byzantine readings are in fact inferior. Until then, the majority of manuscripts may be accepted as "independent witnesses" in an important sense. Burgon (1896a) said of these manuscripts:

Hardly any have been copied from any of the rest. On the contrary, they are discovered to differ among themselves in countless unimportant particulars; and every here and there single copies exhibit idiosyncrasies which are altogether

startling and extraordinary. There has therefore demonstrably been no collusion--no assimilation to an arbitrary standard,--no wholesale fraud. It is certain that everyone of them represents a MS., or a pedigree of MSS., older than itself; and it is but fair to suppose that it exercises such representation with tolerable accuracy. (pp. 46-47)

When another quotation of Burgon (1883) is compared with the previous one, it can be seen how crucial it is to consider the comparative weight of the Alexandrian and Byzantine text-types, in determining the true reading:

*ⲛABCD are five of the most scandalously corrupt copies extant :--exhibit the most shamefully mutilated texts which are anywhere to be met with :--have become by whatever process (for their history is wholly unknown) the depositories of the largest amount of fabricated readings, ancient blunders and intentional perversions of Truth,--which are discoverable in any known copies of the Word of God. (p. 16)*

3. **Variety:** A variant competing for originality should be known in many geographical areas; it should be attested to by different kinds of witnesses, speaking different languages: Greek manuscripts, versions, Fathers, and lectionaries. Of these four latter sources, at least two of them must provide evidence of a variant, before Burgon (1896a, pp. 50,57-58) allows it to be witnessed to by variety. Two misconceptions are prevalent, which affect the way in which Majority text method is evaluated: (1) That the method invites the critic to do textual criticism by simply counting manuscripts, and that (2) as the Byzantine text was produced in a geographical corner, it is unworthy of critical attention equal to or over-and-above other text-types.

Burgon argues against these misconceptions, as well as further explaining the importance of his second "note":

Variety distinguishing witnesses massed together must needs constitute a most powerful argument for believing such Evidence to be true. Witnesses of different kinds; from different countries; speaking different tongues:--witnesses who can never have met, and between whom it is incredible that there should exist collusion of any kind:--such witnesses deserve to be listened to most respectfully. Indeed when witnesses of so varied a sort agree in large numbers, they must needs be accounted worthy of even implicit confidence. . . .

Variety it is which imparts virtue to mere Number, prevents the witness-box from being filled with packed deponents, ensures genuine testimony. False witness is thus detected and condemned, because it agrees not with the rest. Variety is the consent of independent witnesses.

It is precisely this consideration which constrains us to pay supreme attention to the combined testimony of the Uncials and of the whole body of the Cursive Copies. They are (a) dotted over at least 1000 years: (b) they evidently belong to so many divers countries,--Greece, Constantinople, Asia Minor, Palestine, Syria, Alexandria, and other parts of Africa, not to say Sicily, Southern Italy, Gaul, England, and Ireland: (c) they exhibit so many strange characteristics and peculiar sympathies: (d) they so clearly represent countless families of MSS., being in no single instance absolutely identical in their text, and certainly not being copies of any other Codex in existence,--that their unanimous decision I hold to be an absolutely irrefragable evidence of the Truth. (pp. 50-51)

It is usually assumed that, because the textual evidence Burgon is referring to is written in Greek, the *Majority text* relates only to Eastern Christendom, where Greek was spoken throughout the Middle Ages--hence the term "Byzantine" for the *Majority text*. Secondly it is generally asserted that very few of the Versions reflect this text. It would thus be interesting to discover on what basis Burgon felt able to claim that the *Majority text* is

more than merely provincial, but relates to every significant area of the then-civilised world.

4. **Continuity:** The previous two Notes provide, for Burgon, catholicity of place (variety) and of people (number). To these he added a Catholicity of time (1896a, p. 59), that is, a variant cannot lay claim to originality unless it has shown traces of its existence throughout Church history. There should not be "a chasm [in the evidence ] of greater or less breadth of years" (p. 59). The three notes of variety, number and continuity are the most important, and interdependent. Where all three agree, practical certainty is assured in the making of textual choices. Nevertheless "concerning the seven Notes of Truth the student can never afford entirely to lose sight of any of them" (p. 67).

Burgon is not more specific on the degree of continuity to be expected, but presumably at least every century during the period AD 100 to 1500 should have left traces of a competing variant for it to be possibly genuine. The evidence must not, for example, die out in the fifth century, or commence in the twelfth. There is a double-presumption at work here, which justifies, but only if correct, this "note of truth:" (1) There is such a vast amount and variety of kinds of evidence, that it is very unlikely that an original reading would fail to show its presence at every stage of the Tradition (p. 44), and (2) it is consistent with the divine inspiration of Scripture to expect that the complete original Text has been available to the Church at every point in the historical process, unless the presumed intentions of its ostensible Author are to be defeated.

The first presumption above seems to depend on the second for its validity. Burgon thus relies unashamedly on a theological presupposition in pursuing textual criticism (1896a, pp. 9, 11-12). Presuppositional reasoning is however characteristic of every intellectual discipline and

recognises that the reasoning process per se depends on first principles. (First principles are premises which, although they are essential to the particular discipline, can only be accepted as either self-evident, or rejected as irrelevant and unprovable.) The presupposition of the providential preservation of the text implies that writers and critics through the ages have generally been guided by God to sanction the faithful copy and disallow the spurious (1896a, p. 12). This for Burgon did not eliminate the need for textual criticism; however in advocating a presuppositional framework different from that assumed when studying other classical texts, it did alter the critical method and its ensuing results. With the advent of the printed text, Erasmus' textual choices were determined by the belief that "the descent of the text [had been] evidently guarded with jealous care . . . that it rests mainly upon much the widest testimony; and that where any part of it conflicts with the fullest evidence attainable . . . [it] calls for correction" (p. 15). However, Burgon believed that, as copyists had not been protected against the risk of error, there were no grounds for believing the *Received text* was the subject of a perpetual miracle --thus, "it calls for skilful revision in every part" (p. 15).

Through this test Burgon was affirming that the "ring of genuineness", in relation to inspired Scripture, was best vouched for by many rather than by few. Miller (1886, p. 63) agreed with his "mentor" (Burgon, 1896a, p. 11) that New Testament textual criticism should be based on the fact that sacred text deserved to be approached within a distinct presuppositional framework, and that the Biblical promise, for example, to guide the Church "into all truth" (St. John 16:13), was made to a collective or corporate body, not to any one individual. If this special orientation of what Miller termed "the rival school of high textualists" (1886, p. 63) is specious and invalid, then it seems the argument from



continuity is not particularly helpful. Contrariwise, if it is a presupposition implicitly required by the special nature of the text, then it usefully serves as a significant strand of a threefold cord. Pickering (1977, p. 134) follows through on the implications of Burgon's view that the Byzantine text was no mere provincial text. Thus where there is wide variety and continuity, this gives powerful argument for the independence of their supporters. If there is substantial continuity, then independence is much more likely, in contrast to readings which appear as little eddies in the later Byzantine stream. This approach also assumes that no proof has been shown that the Text was ever subjected to a process such as would produce a designed uniformity of text.

The threefold witness of number, variety and continuity is available to determine most textual choices. Where their joint witness is not clear, other factors must help decide which is the correct reading.

5. **Weight:** The previous three "Notes" serve to determine whether the external witness of manuscripts is independently based, or whether collusion has been at work between them or not, the latter euphemistically described as a recensional process. This fifth note judges the internal credibility of a witness by its own performance. Looking not at the readings but at the witness itself, how often does it go astray? In collating the ancient five uncials **aABCD** Burgon (1896a, pp. 17, 81, 88) noticed and spoke emphatically of the *concordia discors* between them. He enumerates the frequent transpositions, substitutions and peculiar readings of Codd. A, C and D which indicated error by design not by accident, "the result of arbitrary and reckless *Recension*" (1883, pp. 248-249). Likewise H. C. Hoskier (cited in Pickering, p. 51) shows that Cod. B and **a** disagree with each other more than 3000 times in the Gospels, "which number does

not include minor errors such as spelling, nor variants between certain synonyms which might be due to 'provincial exchange'."

Studies of the papyri also provide many examples of scribal irreverence towards the text. Thus Colwell (1965, pp. 374-376, 387) shows the oldest manuscript of John, that is,  $\rho^{66}$ , has multi-nonsense readings from careless scribal work and sloppy editorialising. Pickering (1977, p. 119) also quotes Zuntz' analysis of  $\rho^{46}$ . Although Zuntz was happy with the Hortian flavour he found there, he nevertheless adjudged it "by no means a good manuscript," that is, it was plentiful in scribal blunders, omissions and additions. The scribe was "careless and dull", and "did his work very badly." Burgon (1896a, p. 58) would have said of such papyri, in spite of their great age: "If they go wrong continually, their character must be low. They are governed in this respect by the rules which hold good in life."

Burgon (1896a, pp. 33-35) believed it is unwise to prefer the witness of two manuscripts--he had  $\alpha B$  in mind--"standing apart in every page so seriously that it is easier to find two consecutive verses in which they differ than two consecutive verses in which they entirely agree" (p. 33). This he saw was against an agreement of 990 out of 1000 copies, of every date from the fifth century to the 14th century, in every part of Christendom. Secondly, he believed that the witness of the five oldest uncials is so internally inconsistent as to show not one text but fragments of many, and that their priority in age is thus no evidence that the earliest manuscripts should be preferred above others. A third reason Burgon rejected the "neutrality" of Cod B. was his belief that it is easier for an editorial scribe to produce a shorter text by omission than a longer one by interpolation, that is, he would change *lectio brevior* to *lectio longior*, if rules were reliable instruments for doing textual criticism.

His fourth reason for being sceptical of B was his conviction that the evidence of antiquity when joined in agreement with variety constituted textual certainty. In other words, when a majority of manuscripts and versions and Fathers from the first five centuries all agree, this is decisive. Too often Cod. B parts company with this united evidence.

For the *Majority text* advocate, then, weighing up a manuscript is not achieved by relying on internal canons, for these tend to cancel each other out. Nor is it achieved by choosing a "star" manuscript or text-type as intrinsically superior to another, for example, the Alexandrian text-type above the Western, or vice-versa. Nor is the manuscript weighed up by how early it was copied--the earliest papyri provide no formula based on age. Rather he notes that the scribe's attitude to his work reflects also on the reliability of peculiar readings contained in it. Perhaps Colwell's (1965) criticism of the scribe of p<sup>45</sup> may also help to explain the shorter text of Cod. B?:

The scribe of p<sup>45</sup> wielded a sharp axe. The most striking aspect of his style is its conciseness. The dispensable word is dispensed with. He omits adverbs, adjectives, nouns, participles, verbs, personal pronouns--without any compensating habit of addition. He frequently omits phrases and clauses. He prefers the simple to the compound word. In short, he favours brevity. He shortens the text in at least fifty places in *singular readings alone*. But he does *not* drop syllables or letters. His shortened text is readable. (p. 383)

Textual critics are mostly unimpressed by the argument that scribal carelessness constitutes "moral impairment;" they would not infer from such carelessness that it casts suspicion on, for example, singular readings within the manuscript.

6. **Context:** This is a specific and limited application of the previous note. How does the manuscript witness behave in the given vicinity of the text? If the context is very corrupt, then considerable suspicion and reserve attach to it.

A good example of the influence of immediate context as an indicator of the state of the Text, from Burgon's viewpoint, is found in John 13:25. Burgon (1896b, pp. 106-111) argues against Hort's ἀναπεσών by several manuscripts with ⱭAD at their head, in favour of ἐπιπεσών used by a majority of manuscripts. In this passage the writer describes Jesus' favourite disciple leaning back on his, the writer's, chest to gain secret information. Burgon shows the intrinsic probability of ἐπιπεσών and then supports it with analytical detail that shows the oldest uncials are hopelessly at odds with one another in six verses on either side of verse 25.

We would expect the superior age of ⱭABCD to have presented the entire context with tolerable accuracy. The verses, he says, present no special difficulty to a transcriber, yet:

The Codexes in question are found to exhibit at least thirty-five varieties,—for twenty-eight of which (jointly or singly) B is responsible: Ɑ for twenty-two: C for twenty-one: D for nineteen: A for three. It is found that twenty-three words have been added to the text: fifteen substituted: fourteen taken away; and the construction has been four times changed. One case there has been of senseless transposition. Simon the father of Judas, (not Judas the traitor), is declared by ⱭBCD to have been called 'Iscariot'. . . . What are we to think of guides like ⱭBCD, which are proved to be utterly untrustworthy? (pp. 110-111)

In contrast, Burgon finds from the context that the *Majority text* is to be preferred:

Every delicate discriminating touch in this sublime picture is faithfully retained throughout by the cursive copies in the proportion of about eighty to one. The great bulk of the MSS., as usual, uncial and cursive alike, establish the undoubted text of the Evangelist. (p. 107)

Pickering (1977, p. 136) also illustrates this "note" with reference to Hort's famous "Western non-interpolations" in Luke 22-24. Burgon (1883, p. 78) took exception to describing many of the omissions of Cod. D in this way. Hort rejected many of the Byzantine readings which he saw as additions to the Text, even though Cod. D had not added them, which would have been characteristic of that manuscript. But D was the only manuscript which had omitted many of them, whilst the others also had very little manuscript support. However to accept these omissions as reflecting the original was to fly in the face of D's known bad character in the opposite direction, namely, a flagrant tendency both to add and omit. Hort himself had said that Cod. D contained "a prodigious amount of error" (1881, p. 149). According to Burgon, when we compare Hort's own revised text with the text of Cod. D for chapters 22-24, we find that D omits 329 words (250 of which are omissions unique to D alone), 173 have been added, 146 substituted, and 243 transposed. Pickering asks, "How can *any* value be given to the testimony of D in these chapters, much less prefer it above the united voice of every other witness?!?!?" (p. 136)

7. Reasonableness: For Burgon (1886) this Note had a very limited application. Grammatical impossibilities must be rejected as inauthentic, and details which are impossible for scientific or geographical reasons. For example, in Luke 23:45, **NB** read τοῦ ἡλίου ἐκλιπόντος in place of καὶ ἐσκοτίσθη ὁ ἥλιος, that is, "the sun having become eclipsed",

is, "the sun having become eclipsed", instead of "darkened". However an eclipse was an impossible occurrence at Passover time, when the moon was full. This illustrates the "very slight exception", applicable only on rare occasions, and having "only subsidiary force". Thus the true reading was to be found in the weight of external evidence. Internal reasons were too often "the product of personal bias, or limited observation: and where one scholar approves, another dogmatically condemns" (1896a, p. 67).

It is a moot point whether the *Majority text* method may be expanded beyond the restricted fence which Burgon put around "reasonableness". Ten years earlier Miller (1886, pp. 120-122) seemed to give a larger place to internal evidence than Burgon's writings suggest are appropriate. Though Miller believed external evidence far outweighs any other tests, he had earlier set out Scrivener's (1894:2, pp. 247-256) seven internal canons, as follows: (1) Bengel's "harder reading" is good; (2) Griesbach's "shorter reading" is good; (3) follow the reading which explains the origin of the variation (Tischendorf), and (4) the reading which conforms to the author's style; (5) evaluate the variant in the light of "the special genius and usage of each authority, for example, always suspect the omissions of B, the carelessness of A, and the interpolations of D" (Miller, 1886, p. 121); (6) note the transcriptional probabilities relating to grammatical changes, for example, itacism. Finally (7), reject nonsense readings which injure meaning and construction. Miller agreed with Scrivener in rejecting Griesbach's rule, namely, that "suspicion must ever rest upon such readings as make especially for orthodoxy" (p. 121).

It is not always clear whether Miller, when representing Burgon posthumously, does so accurately as he himself says: "I was obliged frequently to supply [interpolations] in order to fill up gaps in the several MSS., and in integral portions of the treatise" (1896b, p. vii).

Perhaps Burgon (1896a, p. xi) convinced Miller, in the process of co-working towards the publication of Burgon's views, that he was on the wrong track, that is, that the *Majority text* viewpoint should not accommodate Scrivener's internal canons:

We came together after having worked on independent lines. . . . At first we did not agree thoroughly in opinion, but I found afterwards that he was right and I was wrong. It is a proof of the unifying power of our principles, that as to our system there is now absolutely no difference between us, though on minor points . . . we do not always exactly concur. (p. xi)

A decade after 1886 it thus seems Miller may have been persuaded by Burgon to abandon a definition of Reasonableness which allowed for Scrivener's canons.

If Miller represents Burgon correctly, he spoke, as we have seen, of "a very slight exception" to following the external evidence; but Griesbach's canon was "monstrous" (1896a, p. 66). Nevertheless, the "harder reading" rule has value if not pressed too far (p. 67). Apart from these exceptions, Burgon's complete reliance on external evidence is clear. This helps to explain why the *Majority text* advocate places no real trust in the internal rules widely used for centuries, though Hodges has followed his own internal guidelines at times, in applying stemmatics to a few passages.

In summary, the strength of the seven notes is in their cooperation. The ground rules in Burgon and Miller (p. 67) seem to be: (1) If we had all the evidence in any one of the seven notes, it would convince us of the correct reading. But we don't have complete evidence for any, so no one note is sufficient without corroboration from others; (2) if all seven agree there is complete certainty; (3) if number, variety, and continuity agree there is practical certainty (1896a, pp. 59, 224), that is, there are no grounds for revising the text; (4) if 1 of the 3 is lacking for example,

number, the text should still not be changed, if the result of aligning the other four notes would lead to the same conclusion as is gathered from variety and continuity combined; (5) if the threefold cord is lacking, and the other notes do not aid to determine the text, then the internal considerations outlined by Miller, for example, the "harder reading" (1896a, p. 67) may help in a limited way to determine the true reading.



## CHAPTER 4

## Testing the Methodology

*How Majority Decisions are Made*

Fee's (1978b, pp. 159-160) practical response to Hodges' arguments was to require him to show from the text how disputed verses are to be treated from the *Majority text* standpoint. This approach is taken up by K. Aland (1987, pp. 292-300) in his response to the publication of Hodges' and Farstad's *The Greek New Testament according to the Majority text*. Aland's reasoned eclectic method is applied to 15 New Testament texts which are relegated to the critical apparatus of *UBS<sup>4</sup>*. In this study I have examined the evidence for several of these texts, and have reached conclusions from the *Majority text* standpoint, that is, assuming Burgon's approach to be valid. This means that instead of applying the usual rules of external and internal evidence, Burgon's "Seven Notes" will be used. Such tests emphasise external evidence almost entirely, but without the fundamental principle of the historical-documentary method, namely, "the earliest manuscript likely contains the best reading". The age factor is only one consideration among several, because the age and origin of the text reflected by the manuscript are seen as too uncertain to be determinative of textual choices. The statistical argument is seen as important in so far as the history of the text is obscure; the many causal influences which explain how variants arose is often a matter of conjecture. Where possible the reasoning of *Majority text* critics is used, Burgon or Miller, for example, to show how the various texts have been dealt with in the past.

In the process of reasoning to a conclusion, the textual decisions are compared, on the one hand to those reached by reasoned eclectics, as represented by Aland and by Metzger's *Textual Commentary*. This sets out the decisions of the UBS Editorial Committee, of which Aland was a member. On the other hand, the decisions are also compared, where possible, to those of the "rational" critics Kilpatrick and Elliott.

*Aland's Marginal Readings*

The aim of the analysis is to test the claim of a *Majority text* advocate that he possesses a viable critical method. This procedure gives some vantage point from which either to invalidate the *Majority text* approach or to acknowledge its viability. Aland invites those drawn to the Majority approach to take the opportunity of "forming an independent judgment of them as well as of the newly proclaimed return to the Textus Receptus" (1987, p. 292). The 15 texts are verses chosen from some 50 verses listed by Kilpatrick (1978, p. 137) who compiled them to show how Griesbach treated verses which have been called in question by subsequent editors. They make up a sizeable sample by which to determine the nature of the *Majority text* method. They are taken mostly from the Gospels and Acts. Various grades are given in *A Textual Commentary* by which to judge the degree of certainty that was felt by the editorial committee in reaching their conclusions. They range from grade {A}, which signifies the text as certain, to grade {D}, which shows that there is a very high degree of doubt concerning the reading selected for the text. Of the fifteen texts or passages, five of them were graded {A}. These five have been chosen as the most suitable examples by which to study and compare one textual method with another. Their certainty, from Aland's viewpoint, helps to

highlight contrasting methods and conclusions. The five texts are examined in this study, together with Matthew 17:21, which was graded {B} (some degree of doubt); they are as follows:-

Matt 17:21  
 Mark 9:44 and 46  
 Mark 11:26  
 Mark 15:28  
 John 5:3b-4  
 Acts 8:37

***Matt 17:21.***

( GNS = Scrivener's Text 1881 - Theodore Beza 1598 Textus Receptus [TR] ).

GNS Mat 17:21 τοῦτο δὲ τὸ γένος οὐκ ἐκπορεύεται εἰ μὴ ἐν προσευχῇ καὶ νηστείᾳ.

For omission: (UBS<sup>4</sup>)

(The number after an abbreviation is the century dated, e.g. it<sup>e-6th</sup>)

<b>κ*</b>		4th
<b>B</b>		4th
<b>Θ</b>		9th
<b>minusc</b>	33, 579, 892*,	9th
<b>/</b>	lectionary 253	
<b>L</b>	it	e-6th, fl-8th
<b>S</b>	syr <sup>c,s,pal</sup>	3rd/6th
<b>E</b>	sah	from 3rd
	boh <sup>pt</sup>	from 3rd
<b>Eth</b>	ms: Paris	from 6th
<b>Geo</b>	geo <sup>1A</sup> : 1st of 2 revisions.	
	A = ms of 2nd rev.	from 5th

For Inclusion:

<b>κ<sup>2</sup></b>		from 4th
<b>C</b>		5th
<b>D</b>		5/6th
<b>L</b>		8th
<b>W</b>		5th
<b>Δ</b>		9th
<b>f<sup>1</sup></b>	{1, 118, 131, 209, 1582}	12-14th
<b>f<sup>13</sup></b>	{13, 69, 124, 346, +9}	11-15th
	28, 157, 180, 565, 597, 700, 1006, 1010, 1071, 1241, 1243	
	1292, 1342 1424,	
<b>Byz</b>	[ E F G H O Σ ]	majority of Byz mss
<b>Lect</b>	184, 1514	
<b>L</b>	vg	
<b>L</b>		it a-4, aur-7th, b-5th, c-12/13th, d-5th, f-6th, ff2-5th, g1-8/9th, l-8th, n-5th, q-6/7th, r2-7th, l-8th,
<b>S</b>	(syr <sup>p, h</sup> )	5th/7th
<b>E</b>	cop <sup>bapt</sup> mss	4th
<b>A</b>	arm	5th
<b>Eth<sup>pp</sup></b>	ethiopic (Pell Platt/Praetorius)	6th
<b>G</b>	geo <sup>B</sup>	5th
<b>Slavic</b>		9th
<b>Origen</b>		254
<b>Asterius</b>		post 341 Antioch
<b>Hilary</b>		367

Basil	379
Ambrose	397
Chrysostom	407
Augustine	430
Jerome	420

also, according to Burgon:

Tertullian	3rd
Clement Alex	before 215
Athanasius	373
Juvencus.	4th
Eusebius	Syriac canons 4th
John Dam	before 754

**Number:** The great mass of witnesses are on the side of inclusion.

Burgon (1883, p. 91) asked why verse 21 is expunged from the text "although it is vouched for by every known uncial *but two*, **NB**, every known cursive *but one* (Evan. 33)"? Subsequently von Soden uncovered the Koridethi Gospels in 1906, an ostensibly Caesarean text supporting **NB**. Two more cursives join 33 in adverse witness. A greater number of versions and Fathers also include verse 21; the Old Latin tradition shows massive support for it, including Cod. D, with only two against.

It is difficult to agree with Aland's assertion that there are a "relatively great number of witnesses for the omission" (1987, p. 296). Only three uncials are against the Majority. As many as eight, and possibly 14 Fathers, witness to verse 21. There is an overwhelming number of manuscripts which support its inclusion.

Scrivener's (1875, pp. 128) comment is: "The omission is not imperatively demanded by the state of the evidence."

**Variety:** The Majority reading is well represented in manuscripts, versions, patristic writings and lectionaries.

The Old Latin tradition is particularly full from the fourth century onwards, with but two in disagreement; even Cod. D unites with them. In contrast, the Egyptian tradition is not united with Cod. B against the verse, that is, Cod. A and some Bohairic manuscripts include it. This shows the verse was known in Egypt from the third century onwards.

The Syriac Peshitta is in support, but this is significant only if it is after all non-recensional, together with its Byzantine readings, and thus may be earlier than is now generally thought. Against it are the Curetonian and Sinaitic, which may be almost as early as the Peshitta was once thought to be; it offers a text similar to  $\aleph$ B. Uniting with the Syriac Vulgate are the Armenian, the Slavonic, the Ethiopic, and the Georgian. Of the latter two versions, there is a revised manuscript of the Georgian which omits, and likewise one manuscript of the Ethiopic.

The Versions are thus strongly in support of including verse 21.

The lectionaries are almost unanimous in favour of retaining the verse.

The Fathers knew and used this text in Alexandria, Egypt, Caesarea, Syrian Antioch and Damascus, Constantinople, Poitiers in Gaul, N. Italy, and N. Africa. If Burgon's patristic references are correct, to this wide provincial spread we can add Spain-Juvencus. The only Father who apparently calls verse 21 in question is Eusebius; however even his witness is divided, inasmuch as, according to Burgon, the Greek version of the

Eusebian canons omits verse 21, while the fourth century Syriac canons include it.

The evidence for this verse is a good illustration of the quality of testimony Burgon (1896a) looked for in a variant. It should be attested to in many geographical areas, with different kinds of witnesses speaking different languages from different countries. Such attestation eliminates the possibility of collusion. It is "the consent of independent witnesses" (p. 50). Burgon believed the combined uncials and cursives represented no one geographical area, but came from all over Christendom.

In contrast, the evidence against has few uncials and cursives in support, almost no patristic, and only one lectionary. There is nevertheless somewhat more substantial versional evidence in support of omission, namely, from the Syriac, and the Egyptian manuscripts.

Scrivener believes that the external evidence is on the side of inclusion, but he is nevertheless persuaded on the internal evidence to omit verse 21: "We are attaching great force of internal probabilities when we allow such a scanty roll [of authorities supporting omission] to outweigh the far more numerous and equally varied authorities that uphold the verse" (1875, p. 128).

The *Majority text* has by far the better credentials in terms of the variety of evidence available for inclusion of the verse.

**Continuity:**

Origen		3rd
Clement Alex.,		3rd [Burgon]
Hilary		4th
Basil		4th
Ambrose		4th
Athanasius		4th [Burgon]
Juvenus		4th [Burgon]
C	Cod. Ephraem	5th
D	Cod. Bezae	5/6th
W	Cod. Freerianus	5th
<i>sp,h</i>	Peshitta, Harclean	4-7th
<i>E</i>	cop, boh	4th
<i>L</i>	it <sup>a</sup> ,d,n	4/5th
Chrysostom		5th
Augustine		5th
<i>A arm</i>		5th
<i>G geo</i> <sup>Bmg</sup>	Georgian	5th
<i>EthPP</i>	Ethiopic-Pell Platt	6th
Byz Lectionary witness begins		8th
<i>L</i>	Cod. Regius	8th
565, 892		9th
1079,		10th
700, 1216, 1230,		11th
<i>F</i> <sup>1</sup>	1,118,131,209,1582	12-14th
<i>F</i> <sup>13</sup>	13,69,124,346, +9	11-15th
28		11th
1071,1241,1344,1365,1172		12th



1009,1242,1546	13th	.
2148,2174	14th	
1253	15th	

Verse 21 has substantial support not only in terms of geographical diversity, but also chronologically. Every century up to the advent of printing witnesses to the presence and use of the verse. Such wide variety and continuity gives powerful argument for the independence of their supporters. Where there is substantial continuity, independence is much more likely, *vis-à-vis* a variant present only in a little eddy of a late Byzantine stream.

Number, variety, and continuity unite with evidence that suggests that Matthew 17:21 has an authenticity which is not put seriously in doubt by the small group of Alexandrian manuscripts championed by Hort, with Cod. B as its leading "star". It can be accurately described as a "received text" by the universal Church through the centuries. As corroboration, age weight and context are next considered, together with reasonableness, that is, inferences which may be drawn from the variants themselves, or internal evidence.

**Age:** Matthew 17:21 is witnessed to in the earliest centuries, that is, prior to or contemporaneous with Cod. B, by Origen, Hilary, Basil, and Ambrose. Burgon (1896a, p. 26) would give these patristic references a value equal to the evidence of a fourth century uncial, especially where they are supported by the early Syriac and Latin Versions. From the Majority viewpoint, the verse passes the antiquity test well, for example Codd. A, C, D, W all lend their support in terms of their age, to include the verse.

**Weight:** The external evidence of Cod. B and 8 with their Egyptian satellites is "more than adequate evidence" for Aland to omit verse 21 (1987, p. 296). In contrast, the external evidence is not even mentioned in the *UBS Textual Commentary* (1971, p. 43). Instead Metzger relies on parallel external evidence for Mark 9:29, which includes important representatives of the varying text-types (excluding the Byzantine) which omit *καὶ νηστεία*. These words are then explained as a gloss on Mark's text, which then found its way into Matthew, by a scribe assimilating almost the entire verse 29: *Τούτο τὸ γένος ἐν οὐδενὶ δύναται ἐξελεῖν, εἰ μὴ ἐν προσευχῇ καὶ νηστείᾳ.*

Since Hort's neutral evaluation of Cod. B is now abandoned by many, the external evidence to corroborate the Egyptian text's omission of this verse is lacking from Greek manuscripts, versions, lectionaries, and patristic references. Accordingly, internal evidence becomes the focus in justifying the exclusion of verse 21.

In contrast, the *Majority text* approach has some support from the Western text, and also from Cod. A and Bohairic manuscripts. Assuming that the text-typical status of the Caesarean text is intact, then the manuscripts 565, 700, together with the Armenian and the Georgian versions, unite to include the verse. The Byzantine manuscripts also unite in favour.

**Context:** Burgon's (1896a) treatment of Matthew 17:21 illustrates effectively what he intended by the note of Context, that is, how it serves as a proof or test of a variant. Thus he comments on the evidence for omission of this verse:

It is plain that the stress of the case for rejection, since  $\aleph$  . . . speaks uncertainly, rests such as it is upon B; and that if the evidence of that MS. is found to be unworthy of credit in the whole passage, weak indeed must be the contention which consists mainly of such support. [p. 63)

Using the *Received text* as a reference point for comparison, Burgon notes that verses 19-20, 22-23 show ten variations from the *Majority text*. Of these, only four are supported by Lachmann, Tischendorf, Tregelles, Westcott and Hort; another two were rejected by the 1881 Revisers. Of the remaining four, another two are supported only by  $\aleph$  and D, with the agreement of four or five cursives, whilst the remaining two are supported only by  $\aleph$ , with very few cursives. The cumulative effect of such an analysis for Burgon is the discovery "that the entire passage in B is wrapped in a fog of error" (p. 63). The context of verse 21 shows that Cod. B cannot be trusted to provide a firm direction as to whether the verse should be included or excluded. Better then to rest on "the witness of all the other Uncials, and Cursives, the rest of the Versions, and more than thirteen of the Fathers beginning with Tertullian and Origen" (p. 63).

**Reasonableness:** Aland reasons in a similar way to Metzger in that he sees the words *καὶ νηστεία* as a gloss on Mark's text which, by the assimilation of the entire verse 29 into Matthew, found its way into the first Gospel.

Mark 9:29 reads: Τοῦτο τὸ γένος ἐν οὐδενὶ δύναται ἐξελεῖν, εἰ μὴ ἐν προσευχῇ καὶ νηστείᾳ.

The words δύναιτο ἐξελεῖν were then smoothed out with the scribal interpolation into Matthew, substituting ἐκπορεύετο. Aland (1987, p. 296) also sees further evidence of the secondary character of verse 21, in that a first corrector of **N** replaced ἐκπορεύεται with ἐκβάλλεται, whilst one lectionary replaced it with ἐξέρχεται. However, if doubt had already been placed on the verse by previous editorialising, it is easy to see how a later scribe would feel at liberty to experiment with the Text.

The external evidence for the authenticity of καὶ νηστεία in Mark 9:29 is, from the *Majority text* standpoint, even stronger than the evidence for including Matthew 17:21. Scrivener (1875, p. 136) asserts that the evidence for including καὶ νηστεία in Mark 9:29 is good--for example, there are more manuscripts of the Egyptian Version in its support, in comparison with the evidence for the Matthean verse--and he implies that Mark 9:29 should not be used as a basis for omitting Matthew 17:21. Thus, in neither of the two verses do the references to fasting need to be rejected.

Has assimilation occurred, from Mark to Matthew? This should not be assumed without proof, unless indeed the "fuller" text of secondary Byzantine manuscripts inevitably points to harmonisation as an essential means to achieve it. On the other hand, an Alexandrian editor may have worked on Matthew's Gospel, excising verse 21 through an already acquired preference for a shorter text. If a shortened form of Mark 9:29 was before him, which omitted καὶ νηστεία, he may have assumed that the entire verse 21 of Matthew was earlier interpolated into the Text.

Metzger (1971, p. 101) explains the predominance of support for the longer reading of Mark on historical grounds, namely, the growing emphasis in the early church on the need to fast. Thus the scribe looked for justification for the practice from the teaching of Jesus, and assuming verse 21 is spurious, he willingly provided it. Two rules of internal evidence

happily combine here: Assume the shorter reading, it is said, and this easily explains the existence of the other variants. Conversely, if we assume the reference to fasting is original, no explanation is readily available as to why it was ever omitted. Aland (1987, p. 296) probably sees the omission of any reference to fasting by the Old Syriac and Coptic traditions as fatal to the Majority view, because fasting was especially valued in the Eastern church. Thus all such references would have been carefully guarded and preserved, rather than omitted from the Text.

Scrivener's comment on the reason for the omission of *καὶ νηστεία* in Mark 9:29 denies that scribes felt free to deliberately alter the text. "We cannot deny too earnestly an unjust charge occasionally brought against the copyists . . . that they accommodated the text before them to the ascetic practices of their own times" (1875, p.136). However it is not clear on what basis Scrivener felt so confident of scribal habits in the earliest centuries.

If the omission of *καὶ νηστεία* in Mark 9:29 helped to place doubt on the genuineness of Matthew 17:21, then a reason for the omission of these two words in Mark may in effect help to explain why Matthew's verse was omitted. Cranfield (1959, p. 304) quotes Hauck's view that a scribe may have felt that Mark 2:18ff. was inconsistent with Mark 9:29. Chapter 2:18 shows that Jesus' disciples did not fast; neither did he intend them to fast, so they could fully enjoy his friendship and teaching. Verses 19-20 clarify the position: "As long as they have the bridegroom with them they cannot fast. But the days will come when the bridegroom will be taken away from them, and then they will fast in those days". A later scribe may have omitted Mark 9:29, preferring to keep chapter 2 believable.

The scribe's motive in this case was not to invalidate a growing practice; he knew that Mark 2:20 validated fasting admirably. Rather he tried to bring consistency into the text, on the assumption that, by retaining *καὶ νηστεία*, the reader would inevitably infer that Jesus was inviting his disciples to fast there and then, rather than at some indefinite future time. Cranfield, however, believes that a scribe would have been more interested in providing authority for contemporary church practice than in harmonising Jesus' teaching (p. 304).

On the other hand, assuming the verse is genuine, an opposite motive may have been at work. Thus in comparing the two passages in Mark, a scribe may have lacked trust in the narrative as it stood, and so determined to resolve the apparent problem. Also, he might well have resented the growing regimentation of fast-days in his own time. He may have felt it had more in common with the exhibitionist hypocrisy of Pharisaic legalism, than with the dynamic example of a St. Peter or St. Paul (Acts 10:30; 2 Cor 6:5; 11:27). Jesus indeed opposed and condemned the emphasis on outward observances and advocated that an element of secrecy be observed, as far as possible (Matt 6:17-18). On the other hand, the inward spirit of the Apostles which exemplified the true spirit of fasting was quickly becoming a dim memory in the church's consciousness. The scribe may thus have succumbed to the temptation of reducing the force of Jesus's teaching, in the hope that the end (to bring consistency to the Text) justified the means (omitting on the basis of a conjecture, that *καὶ νηστεία*, in verse 29, was an interpolation into the Text).

Some may argue the case slightly differently, by suggesting that there were scribes who disliked altogether references to fasting, in an age when church leaders expected Christians to fast as a regular duty. Fasting was seen as being too unpleasant a practice, and so a scribe sought reasons

to minimise Jesus' teaching, justifying his attitude by pointing to Mark 2:18ff. as evidence Jesus did not teach the duty of fasting, and that therefore Mk 9:29 and Mat 17:21 were unlikely to be authentic.

In order to support the Alexandrian omission, Cranfield implies that there was little if any place for fasting in Jesus' example and teaching anyway; he mentions the forty days Temptation in the wilderness, as an exception (p. 304)! However it is clear that Jesus both taught and exemplified fasting, as in Matt 6:16-18, and Mark 2:20 : " . . . then they will fast in those days". Matthew 4:2 shows that his entire ministry was overshadowed by the 40-day experience; it involved a trial entirely crucial to the outcome of his mission. Other occasions strongly suggest Jesus practised fasting informally, for example, John 4: 8, 31-34;6:5. Matthew 6:16-18 shows that Jesus expected the disciples to fast, albeit in their time, not in his. Verse 16 reads: "Ὅταν δέ not εἰ δέ. Besides, the Rabbinic teaching concerning fasting was firmly founded on the Old Testament prophetic tradition, for example, Jdg 20:26; Ezr 8:21; Psa 35:13; Isa 58:3; Jer 36:6; Joel 2:12; Zec 8:19. Like so much else in Jesus' confrontation with the Pharisees, what he opposed was not the practice per se, but the self-justifying motive and the inconsistent spirit in which he believed it was pursued. Thus there are validating passages in the Gospels which undergird early church fasts. Vincent Taylor (1966, p. 401) sees the reference to fasting in Act 10:30 and 1 Cor 7:5 as interpolative, contra the *Majority text*, in the light of the Alexandrian omission of καὶ νηστεία in Mark 9;29. If this is so, why then did St. Paul practise fasting, as shown, inter alia, by Acts 13:2, 14:23?

Scrivener follows Hort in omitting verse 21 on the evidence of internal considerations, though he admits they are "far from considerable" (1875, pp. 135-136). He sees the omission as consistent with the rule *lectio*

*brevior*, but he questions both the historical reason for the omission, and the value of the rule, by saying: "It [was] the tendency of most scribes (though certainly not of all) rather to enlarge than to abridge" (1875, p.115); but why it was ever added he seems unable to say.

Summing up the transcriptional probabilities, it is possible that a scribe omitted *καὶ νηστεύει* in Mark 9:29, preferring to keep Mark 2:18ff. believable. His intention was not to oppose the increasing popularity of fasting as a practice, but to bring consistency into the text, which would indirectly strengthen the practice. Rather than assume scribal assimilation from Mark to Matthew, it may be evidence that an Alexandrian editor worked on Matthew's Gospel with an already acquired preference for a shorter text. If a shortened form of Mark 9:29 was before him, which omitted *καὶ νηστεύει*, he may have assumed that the entire verse 21 of Matthew was an interpolation. For these good reasons, then, it is better to rely on the *Majority text* which includes Matthew 17:21.

A summary for the evidence from Burgon's Seven Notes leaves no uncertainty, on the basis of external evidence, that Matthew 17:21 is an authentic verse of Mark's Gospel. As to number, the great mass of witnesses are on the side of inclusion. As to variety, the Majority reading is well represented in every kind of evidence--in manuscripts, in versions, in patristic writings and in the lectionaries. As to continuity, verse 21 has substantial support not only in terms of geographical diversity, but also chronologically. Every century up to the advent of printing witnesses to the presence of this verse. Such wide variety and continuity gives powerful argument for the independence of their supporters. Thus number, variety, and continuity unite as evidence for the authenticity of Matthew 17:21. This should not be seen as seriously threatened by the small group of Alexandrian manuscripts championed by Hort, with Cod. B as its leading



star; it can be accurately described as a "received text" by the Universal church through the centuries. As to weight, the *Majority text* approach has in its favour the support of the "Western" text, some support from Cod. A and the Bohairic manuscripts. The Byzantine manuscripts unite in favour. Thus age, weight, and context add their corroboration to number, variety, and continuity, together with reasonable inferences drawn from internal evidence. Although the latter have been emphasised in discussing verse 21, the inclusion of the verse was decided on external grounds, as is characteristic of the *Majority text* approach. The internal considerations discussed are merely corroborative, the aim being to show that the inclusion of verse 21 is consistent with the significant place given to fasting in both Old and New Testaments.

***Mark 9:44 and 46.***

GNS Mark 9:43-49: καὶ ἐὰν σκανδαλίζη σε ἡ χεὶρ σου, ἀπόκοψον αὐτήν· καλόν σοι ἐστὶ κυλλὸν εἰς τὴν ζωὴν εἰσελθεῖν, ἢ τὰς δύο χεῖρας ἔχοντα ἀπελθεῖν εἰς τὴν γέενναν, εἰς τὸ πῦρ τὸ ἄσβεστον. 44 ὅπου ὁ σκώληξ αὐτῶν οὐ τελευτᾷ, καὶ τὸ πῦρ οὐ σβέννυται. 45 καὶ ἐὰν ὁ πούς σου σκανδαλίζη σε, ἀπόκοψον αὐτόν· καλόν σοι εἰσελθεῖν εἰς τὴν ζωὴν χωλόν, ἢ τοὺς δύο πόδας ἔχοντα βληθῆναι εἰς τὴν γέενναν, εἰς τὸ πῦρ τὸ ἄσβεστον, 46 ὅπου ὁ σκώληξ αὐτῶν οὐ τελευτᾷ, καὶ τὸ πῦρ οὐ σβέννυται. 47 καὶ ἐὰν ὁ ὀφθαλμός σου σκανδαλίζη σε, ἔκβαλε αὐτόν· καλόν σοι ἐστὶ μονόφθαλμον εἰσελθεῖν εἰς τὴν βασιλείαν τοῦ Θεοῦ, ἢ δύο ὀφθαλμοὺς ἔχοντα βληθῆναι εἰς τὴν γέενναν τοῦ πυρός, 48 ὅπου ὁ σκώληξ αὐτῶν οὐ τελευτᾷ, καὶ τὸ πῦρ οὐ σβέννυται. 49 πᾶς γὰρ πυρὶ ἀλισθήσεται, καὶ πᾶσα θυσία ἀλλὶ ἀλισθήσεται.

For Omission of v.44: (UBS<sup>d</sup>)

Ⲛ	Cod Sin	4th
B	Cod Vat	4th
C	Cod Ephr	5th
L	Paris	8th
W	Washington	5th
Δ	St. Gall	9th
Ψ	Athos	8th/9th
f <sup>l</sup> ,		12th,
it <sup>k</sup>		4/5th
syr <sup>s,pal</sup>		3/4th; from 6th
cop <sup>sa,bo,fay</sup>		from 3rd
arm		from 5th
geo		from 5th

Evidence for inclusion of verse 46 is almost identical with that for verse 44, except for the following which also include verse 46: syr<sup>pal</sup>, geo;

**Number:** The great majority of Greek uncials and cursives include both verses 44 and 46.

The witness of the lectionaries is unitedly in favour of the verses.

The evidence of the versions is as follows: The entire Old Latin tradition, except for *k* is in favour of inclusion. The Vulgate has them. The Ethiopic and the Slavonic include them.

The Old Syriac Peshitta is in favour, though the Sinaitic disagrees. Burgon (1896a, p. 292-293) distrusted Cureton's arguments which placed

the Peshitta in the fifth century; Majority advocates argue that it substantially predates the Lewis manuscript, reaching back to the second century; also that the Sinaiticus is a corrupted form of it. [Reasoned eclectics who accept Hort's view that the Byzantine text did not appear before the fourth century cannot accept any date for the Peshitta earlier than this, as the text in certain places is so similar.]

Patristic evidence is sparse, but Irenaeus shows the presence of these verses in the West. The Latin translation, an exceptionally literal translation (Aland, 1987, p. 168), attests their presence early in the second century.

From the Majority viewpoint the overwhelming number of manuscripts favour these verses as genuine.

**Variety:** The majority of manuscripts, the koine, and Cod. D are in favour; but not the Alexandrian and Egyptian (except Cod. A).

The versions in favour: Latin, almost unanimously; Syriac (divided), Ethiopic, Slavonic, and Georgian (v. 46 only). The versions against inclusion: Syriac (divided), Coptic, Georgian (but v. 44 only), and Armenian. Following Burgon's dating and theories of textual origins, the Latin tradition interacted early with the Syriac--he believed the Peshitta to be second century and thus predated the Sinaitic--which preserved the majority reading. The dialectical variety and provenance divides the versional evidence almost equally, both for and against these verses.

The lectionaries unite in favour.

Italy, North Africa and Gaul provide patristic evidence in support of their genuineness. There is no obvious patristic evidence against.

Some evidence from the note of variety is in favour of seeing verses 44 and 46 as possibly genuine.

**Continuity:** Evidence exists in the first millennium, as follows, the

list being in date order:

Irenaeus		2nd
<i>L</i> vg		4th
OL <sup>a</sup>		4th
A	Cod. Alex	5th
D	Cod. Bezae	5th
Chromatius		5th
Aug		5th
syr <sup>p</sup>		5th
OL <sup>d</sup>		5th
OL <sup>e</sup>		5th
OL <sup>ff</sup>		5th
Chromatius		5th
Augustine		5th
Σ		6th
OL <sup>f</sup>		6th
OL <sup>g</sup>		6/7th
Eth <sup>TH</sup>		6th
syr <sup>h</sup>		7th
OL <sup>r</sup>		7th
E		8th
L		8th
OL <sup>l</sup>		8th
O <sup>ar</sup>		9th
Θ	Koridethi	9th
F		9th

G		9th
H		9th
S		9th
1424,		9/10th
700, 1006, 1243		11th
miniscules	f <sup>1</sup> ,	12th
	f <sup>13</sup> ,	13th
OL <sup>c</sup>		12/13th

The body of the cursives unite in favouring inclusion. There is a hiatus in the evidence, in the third century. The exemplars of *Lvg* and OL<sup>a</sup> in all probability go back to the previous century. Otherwise every century has manuscript or other written evidence which shows the presence of the verses. There is some evidence from continuity, variety, and number combined to suggest that these verses are genuine. However, the rest of the seven notes are needed to help determine if this is so.

**Age:** The earliest evidence is Latin, both versional and patristic.

The earliest Greek evidence for inclusion is from Cod. A, other than Cod. D which is closely associated with the Western.

Hills (1956, p. 171) quotes Gregory and Kenyon as claiming that Cod. A, dating from fifth century, was written in Egypt; it has the *Majority text* in the gospels. This shows therefore the early presence of this text in Egypt.

Also the Syriac Peshitta may, after all, be early evidence for the presence of these readings in the second century. This depends on whether the Byzantine text may be considered to be pre-fourth century, contrary to Hort. As to the age of the Peshitta, it was almost universally believed, until

1881, to have originated in the second century. F. C. Burkitt, in 1904, denied it existed before the fifth century, as it showed a close agreement with the Byzantine text. In support of Hort's Lucianic recensional view of the "ecclesiastical text", he believed that Rabbula, Bishop of Edessa from 411-435 AD, first published it (Hills, 1956, p. 172).

Hills (Burgon, 1959, p. 56) believes that Burkitt's theory is now rightly questioned by some, because the Peshitta was the "Received text" of both factions of the Syrian church. The Church however was not divided until Rabbula's time. Because he was a Monophysite, the Nestorian party opposing his view would never have accepted, off the record, an edited text at his hands. Pickering (1977, p. 90) cites A. Voobus, who argued that Rabbula didn't even use the Peshitta in its present form. Why then did it become standard, if not that it was very ancient? Because of its antiquity, both sides felt able to quote it in discussion to prove a point; its great age determined the people's loyalty to it despite their factions. The possibility of a second century date for the Peshitta should be reconsidered (Hills, 1956, p. 174).

In harmony with the *UBS* Committee, Aland sees the early witness of  $\aleph$ B, with the Coptic versions in support, as decisive external evidence for omitting verses 44 and 46 (1987, p. 297). Cod. L characteristically follows Alexandrian readings. The earliest manuscript evidence is thus divided.

Age per se, as a factor, is not determinative of these verses, and the manuscript evidence is not strong in favour of inclusion.

**Weight:** The frequently given reason for rejecting these verses is based largely on preference for the shorter reading. Thus Metzger (1971, p. 102) says: "The words ὅπου ὁ σκώληξ . . . οὐ σβέννυται, which are lacking in important early witnesses (including  $\aleph$  B C W  $it^k$   $syr^s$   $cop^{sa}$ ), were added by copyists from ver. 48." The confidence to state that harmonisation to the immediate context has occurred stems not from any evidence from within the immediate context--for justification of this, see discussion below, under reasonableness--, but from belief that the shorter reading rule is true. Thus, the longer Byzantine text has filled out the text by free assimilation of parallel passages. However, chapter 2 of this study has set out reasons why the Byzantine manuscripts may deserve more credibility.

W is also brought in as significant evidence, in agreement with Cod. B and the Egyptian versions, that these verses should be omitted. However D is in favour. This disparity early in the textual history illustrates well the tension created by conflicting views which arise in comparing the Western and Alexandrian texts for the history of the Text. The Western favours the verses; the Alexandrian excludes them. Scholars agree neither as to how the two traditions inter-relate, nor as to which should be followed, as the primary witness. Thus, according to Hills (1956, p.126), Griesbach, Burkitt, Lake, and Matthew Black believe the Western to be the earliest and thus primary text, while others like Tischendorf, Hort, Weiss, and Metzger believe the Alexandrian to be earlier and purer. It follows (1956, pp. 183-184) that either the Western became refined (corrupted?) by Alexandrian preoccupation with literary style, or the Alexandrian was corrupted by Western interpretative concerns, as shown by its characteristic additions.

Epp (1993, pp. 162-163) sees this problem as a major cause of the current methodological confusion, and as a serious flaw at work undermining Hort's theory of the Text and maintaining the prevailing uncertainty.

Burgon's view was different again. He believed the Byzantine text as *the Majority text* ascended to the Apostolic era. It was a non-recensional text, but was thereafter corrupted by the specialist concerns shown to be at work in the recensions of both Alexandrian and Western editors.

**Context:** Fee gives the verse 44 ὅπου ὁ σκώληξ αὐτῶν οὐ τελευτᾷ, καὶ τὸ πῦρ οὐ σβέννυται, and its parallel in verse 46, as examples of harmonisation within a single Gospel under the influence of 9:48 (1993, p. 175). Aland (1987, p. 297) sees the inclusion of the verses as the influence of a quasi-liturgical refrain, in harmony with the repeated εἰς τὴν γέενναν which it follows in each case. This argument is strengthened on the concordant view that the Majority reading of verse 47 is likewise wrong: εἰς τὸ πῦρ τὸ ἄσβεστον, after εἰς τὴν γέενναν.

Such reasoning is based on the assumption that the Byzantine manuscript was edited to become a fuller, expanded text, and that brevity as a rule points to the more authentic reading. However assuming the Byzantine text is not recensional, it is easy to see how the Alexandrian editor could have found it irresistible, in following a self-imposed standard of literary taste, to proceed along familiar lines, and trim away repetitive material from the passage which he assumed, by his own stylistic canons, to have been subject to interpolation at an earlier date.

Further suspicion is placed on the witness of NB by its omission in verse 49 of a sentence after πᾶς γὰρ πυρὶ ἀλισθήσεται, that is, καὶ πᾶσα θυσία ἀλλ' ἀλισθήσεται. This latter sentence is witnessed to by most of the Latin manuscripts, including the most reputed critical texts of the Vulgate.



Many, though not all Versions favour it. The great majority of the Greek manuscripts witness in favour of verse 49b. However, in support for Hort's Alexandrian preference, *καὶ πᾶσα θυσία ἄλλι ἀλισθήσεται* is read by *UBS* and Aland as a scribal reminiscence of Leviticus 2:13 which explains the first clause of verse 49: *πᾶς γὰρ πυρὶ ἀλισθήσεται*. No other argument is given for its inclusion. However, Jesus himself could have quoted this saying, rather than the scribe. Seeing it as a scribal addition is an internal argument to support the Alexandrian reading. Aland approvingly restates Hort's view of verse 49, in as much as it supports the argument for the inferiority of the Byzantine text, it being one of Hort's eight confluents by which he sought to prove that the Byzantine text was recensional. However Kilpatrick (1965) felt free to question Hort's analysis of confluents in justifying his preference for *NB*. Kilpatrick does in fact accept Hort's view of verse 49 as conflatory, probably because he can think of no reason which would make it "explicable on other grounds" (p. 34). Yet he sees no problem in accepting readings distinctive of the Syrian text as older than 200 AD, even if the selection of these readings in that text appeared later, through the recension which originated with Lucian of Antioch (1965, pp. 34, 36). Kilpatrick says that any critical apparatus will show that conflation is not peculiar to the Syrian, but is found in other textual traditions (p. 34). Thus it is but a small step from this to questioning whether Hort had really proved, by this key argument, that the Byzantine is a deliberately edited text and posterior to other text-forms.

On the one hand it is clear historically that Alexandria stood in a long literary tradition of scribal editorialising, and that classical taste was in favour of brevity. On the other hand, clear countervailing historical evidence is hard to find that shows that Lucian, or some other individual, set in motion the editorial process which led to a fuller smoother text.

The omission (9:49) καὶ πᾶσα θυσία ἀλλ' ἀλισθήσεται after πᾶς γὰρ πυρὶ ἀλισθήσεται is easily explained as homoioteleuton where the scribe mistook the second ἀλισθήσεται for the first. There is also some similarity between ἄσβεστον and σβέννυται in verses 43 and 44 which could have caused the omission of verse 44, though deliberate editorial revision is much more likely here.

**Reasonableness:** Burgon's (1896, pp. 160-165) theory of the Text attributed the shorter Alexandrian text to Origen and his school whom he believed deliberately edited the text, particularly by omission, to conform it to a semi-Arian tendency. Greek philosophical principles took the edge off many Biblical doctrines and led them to unnecessarily omit, where variant readings in the text made it easy to do so. Among the beliefs of orthodoxy which Origen questioned included the doctrine of Eternal Punishment. Burgon lists Mark 3:29 and Mark 9:44,46 as examples among many passages deliberately omitted by NB for various reasons (1896a, p. 289). Whether or not Origen can be held responsible for the omission of these particular verses, Pickering (1977, pp. 42-44) quotes Colwell and Matthew Black as evidence that Biblical critics are increasingly recognising most variations in the text were deliberately made from theological or similar motives, unlike changes in the Greek and Latin classical texts which are far more often due to accidental error.

The Alexandrian editor probably saw the repetitive material in this passage as clear evidence of interpolation at an earlier date. But if Mark really believed in Gehenna, and believed that the tradition he was passing on was accurate, he would have been willing to record severe repetition by Jesus at this point as a significantly didactic way of combating natural scepticism towards such an unpalatable doctrine as Everlasting Punishment.

The seven notes come together in general support of one another. Number and continuity both suggest that these verses are genuine, but variety less so. The arguments from weight naturally turn on the status of the Byzantine text. If the latter may be reinstated, then the causes given for the disturbed state of the text, in the context of Mark 9, may be seen as corroborating the genuineness of the verses. Sufficient theological reasons have been adduced to explain why they may have been omitted, perhaps even earlier than Origen's day. There is some early evidence in the age of Cod. A to support this conclusion. The genuineness of these verses depends virtually entirely on whether or not the *Majority text* reaches back to the Apostolic era. Conversely, if the Byzantine text is late and recensional, the verses must be rejected.

**Mark 11:26.**

GNS Mar 11:26. εἰ δὲ ὑμεῖς οὐκ ἀφίετε, οὐδὲ ὁ πατὴρ ὑμῶν ὁ ἐν τοῖς οὐρανοῖς ἀφήσει τὰ παραπτώματα ὑμῶν.

For Omission: (UBS<sup>4</sup>)

Ⲙ	Cod. Sin	4th
B	Cod. Vat	4th
L		8th
W	Washington	5th
Δ		9th
Ψ	Athos	8-9th
	157, 205, 565, 597, 700, 892, 1342, 2427,	
L	it	k-5, 1-8th,
S	syr <sup>s</sup> , pal	from 6th/4th
	cop <sup>sah</sup> , boh pt	from 4th Egyptian

<b>A</b>	Armenian	from 5th	
<b>G</b>	Georgian	from 5th	
<u>For Inclusion:</u>			
<b>A</b>	Cod. Alex	5th	
<b>C</b>	Cod. Ephr	5th	later Alexandrian
<b>D</b>		5th	
<b>X</b>		10th	
<b>Θ</b>		9th	Caesarean
<b>0233</b>		8th	
	minuscules f <sup>1</sup> , f <sup>13</sup> ,	12th, 13th	Caesarean
	28, 33, 180, 579, 1006, 1010, 1071, 1241, 1243, 1292, 1424, 1505		
<b>Byz</b>	{ E F G H N Σ }	most Byzantine mss.	
<b>E</b>		8th	
<b>F</b>		9th	
<b>G</b>		9th	
<b>H</b>		9th	
<b>N</b>		6th	
<b>Σ</b>		6th	
<b>Lect</b>	majority agreement of lectionaries with <i>l</i> AD		
<b>L</b>	it	a-4, aur-7, b-5, c-12/13, d-5, f-6th, i-5, ff2-5th, q-6/7, r1-7th	
	vg		
<b>S</b>	syr <sup>p,h</sup> , Harkleian syr,	5th, 7th	
<b>E</b>	cop <sup>bo pt</sup>	from 3rd	

Eth		6th
S slav	Old Church Slavonic	from C9th, Bulgaria
Cyprian		3rd
Speculum		5th Ps-Aug

**Number:** The majority of Greek manuscripts include this verse as genuine. This applies to both uncial and minuscule manuscripts. The great majority of Old Latin manuscripts also witness to the verse. So do the great majority of the lectionaries.

Burgon believed the multitude of the evidence shown by the *Majority text* for the authentic reading of disputed passages gives substance to his view that: "Number . . . constitutes Weight . . . Not of course absolutely, as being the sole Test, but *caeteris paribus*, and in its own place and proportion" (1896a, p. 44). If the numerical aspect is significant, then, verse 26 belongs in the Text.

**Variety:** Most of the "Western" witnesses are for inclusion: Cod. D and the great majority of the Old Latin manuscripts--with two Old Latin against-- the Vulgate, the Western fathers, Cyprian, and Augustine. But the Syriac Sinaiticus omits.

Also some Alexandrian versions are for inclusion, namely, Cod. A, and a few Egyptian Bohairic manuscripts. But most of the Egyptian manuscripts are against.

From a numerical viewpoint, the predominant Egyptian witness favouring omission is counterbalanced by the predominating Western manuscripts inviting inclusion.

The Caesarean text is discordant, that is, Cod. W omits whilst Cod. Θ includes the verse.

The Byzantine text is unitedly in favour. Among versions in harmony with the Byzantine text, and for inclusion, are the Syriac Peshitta.

The Harkleian Syriac, the Ethiopic, and the Slavonic all include. The Armenian, and the Georgian omit.

Most or all of the lectionaries favour inclusion.

In summary, the evidence shows that verse 26 was known over a wide geographic area, in various forms (in Greek manuscripts, versions, lectionaries and church writings). It is clear that all text-types are represented. But as usual,  $\aleph$ B win the day in the *UBS* text. All text-types witness for and against the passage, except the Byzantine. Metzger's (1971) explanation for omitting the verse therefore, in effect emphasises the appeal to the "earliest manuscript" principle, by saying: "its absence from early witnesses that represent all text-types makes it highly probable that the words were inserted by copyists in imitation of Matt 6:15" (p. 110).

However the evidence from variety is on the side of inclusion.

**Continuity:** Evidence exists in the first millennium as follows, the list being in date order:

Cyprian		3rd
<i>E</i>	cop <sup>bo pt</sup>	from 3rd
<i>L</i>	it	4th
OL <sup>i</sup>		4th
	it <sup>a</sup>	4th

<i>L</i> <sup>vg</sup>		4th
<i>OL</i> <sup>b</sup>		5th
<i>S</i>	syr <sup>p</sup>	5th
<i>A</i>	Cod. Alex	5th
<i>C</i>	Ephraem	5th
<i>D</i>	Cod. Bezae	5th
Speculum	{Ps-Aug}	5th
<i>OL</i> <sup>d</sup>		5th
<i>OL</i> <sup>ff</sup>		5th
<i>N</i>		6th
$\Sigma$		6th
<i>OL</i> <sup>f</sup>		6th
<i>Eth</i>		6th
<i>OL</i> <sup>q</sup>		6/7th
<i>syr</i>	Harkleian	7th
<i>OL</i> <sup>r</sup>		7th
<i>OL</i>	aur	7th
0233		8th
<i>E</i>		8th
<i>L</i>		8th
<i>OL</i> <sup>l</sup>		8th
<i>E</i>		8th
<i>F</i>		9th
<i>G</i>		9th

H		9th
Θ	Koridethi	9th
S	Slavonic	9th
1424, 700, 1006, 1243		9th-11th
miniscules	f <sup>1</sup> ,	12th
	f <sup>13</sup> ,	13th
OL <sup>c</sup>		12/13th

The earliest manuscript witnesses are  $\aleph$  and B, which omit the verse, together with the early Egyptian manuscripts. However, the presence of verse 26 in Cyprian and in some Bohairic manuscripts shows it was known contemporaneously with other earlier witnesses to the text. If the allusion to the verse in Tatian is certain--*UBS<sup>4</sup>* excludes it from its critical apparatus-, the inclusion of verse 26 in Mark 11 is known in every century.

Number, variety, and continuity tend to support the inclusion of this verse as part of the authentic gospel of Mark.

**Age:** As to which centuries were most important for determining the Text, Burgon (1896a) said: "If I must assign a definite period . . . [I mean] the first six or seven centuries" (p. 42). He believed that chronology had a wide application in evaluating the comparative worth of manuscripts.

Evidence from the earlier centuries exists which attests to the verse as genuine. The earliest manuscripts do not favour its inclusion but proof, rather than mere presumption, should first be offered that this fact outweighs other aspects, before concluding that the age of a manuscript is an index to the reliability of its readings.



Aland (1987, p. 297) clearly accepts the Alexandrian witness as "excellent" evidence for omission supported as it is by the Coptic syr<sup>sin</sup>. It is an interesting question whether Hort would have felt compelled to argue that a characteristically shorter text--as is illustrated by NB--is by definition superior, had he not *known* that these Egyptian manuscripts were as early as fourth century. If the supposed allusion in Tatian's Diatessaron is uncertain, and the Syriac Peshitta, like the Byzantine manuscripts generally, is later and recensional, then their age is no particular support for verse 26 being original. But if Burgon's use of the age factor is right, all witnesses from the first eight entries under **Continuity** above (that is, Cyprian, many early Old Latin manuscripts, the Vulgate, the Gothic, some Bohairic manuscripts) in favour of verse 26, count for more than the omission in NB. They have as much claim on the Text, in terms of their age, as fourth century Greek manuscripts.

### **Weight:**

This verse is a good example to illustrate Burgon's method of weighing up manuscripts. Favouring the inclusion of verse 26 are three of the oldest uncials: A, C, and D. He was unhappy with their characteristic *concordia discors* when compared against the *Majority text*. Codd. NB on the other hand omit the verse. Burgon's (1896a) estimate of NB was lower still. If age does not indicate weight, then what does? His answer brought in the value of the cursives. He argued, for example, that if a variant is attested by NB, together with most Old Latin manuscripts, plus one or two Fathers, then "there ought to be found at least a fair proportion of the later uncials and the cursive copies to reproduce it" (p. 60). In other words, a reading should be rejected if later uncials are silent, or the main body of cursives are silent, or many Fathers know nothing of the matter. In the case

of verse 26, only a few Latin manuscripts join with  $\aleph B$  in omitting the verse, and patristic evidence, such as it is, favours inclusion. A couple of later uncials do characteristically support  $\aleph B$ . Burgon (1896a, pp. 202, 206) believed that we are more justified in disregarding uncial evidence than cursive. He saw the era of greater perfection, both in manuscript presentation and content, as reached by the seventh and eighth century, beginning with Cod. E. After this point the uncials are generally united, or considerably so, with dissenting readings in L and  $\Delta$  in conspicuous isolation, as in their treatment of verse 26. The text of the later uncials is the text of the majority of all uncials. This same text is similar to that of the majority of the cursives, and the majority of versions, and the majority of the Fathers (p. 206). He thought it was wrong to separate out, for example, families 1 and 13, and then say all other cursives are alike. Whilst there is usually a clear majority of cursives on one side, in respect of important passages, there are still examples of disagreement which show that the cursives as a body descend from a multiplicity of archetypes. This gives them each an independent personality. Were we able to discover them, their genealogical stems might prove to be extremely numerous (p. 201). If any reading from the Byzantine text is by definition suspect, then clearly there can be no confidence in the genuineness of verse 26. Kilpatrick and Elliott however call for a change of attitude, for the reason given above. The verse may be genuine.

**Context:** This is a specific and limited application of the note above on **Weight**. How does a particular manuscript behave in the given vicinity of the text? If the context is very corrupt, then considerable suspicion and reserve attach to it. Naturally, Burgon evaluated the context with the *Received text* as a convenient basis of comparison. This is not in itself an

unsound way of detecting a turbulent context of disparate readings, in as much as the desired result from using such a method is achievable without having to assume the Byzantine text is superior, even though Burgon did define "corruption" by the extent to which a manuscript departed from the majority reading.

Within seven verses prior to verse 26 there are three variants deserving comment in the critical hand-editions, dealing with minor grammatical issues, that is, the tense, mood, person and number of a few verbs, all of which affect the sense a little. However there is no obvious pattern of inconsistency as to which manuscripts diverge from the *Received text*.

**Reasonableness:** The *UBS* explanation for the omission rejects the idea that homoioteleuton explains it. Instead  $\aleph B$  is appealed to: "Its absence from early witnesses that represent all text-types makes it highly probable that the words were inserted by copyists in imitation of Mt 6:15" (Metzger, 1971, p. 110). However, Elliott's (1974, pp. 343, 346) belief that most alterations to the Text were made before 200 AD leads him to ignore the earlier manuscript principle. Following C. H. Turner's analysis of Marcan usage, he is led to deny the relevance of *brevior lectio* to Mark, and, freed from such rules, he is more ready to see the recurrence of τὰ παραπτώματα ὑμῶν at the end of verses 25 and 26 as a sign of scribal omission. Kilpatrick (1990, p. 307) agreed with this probability in a neighbouring context.

The *UBS*<sup>3</sup> Greek edition shows there are many minor variants within verse 26 involving transposition, substitution, and subtraction of article, pronoun, noun and verb, with no substantial change of meaning. Hills (1956, p. 184) refers to the high esteem in which the Alexandrian

catechetical and textual schools were held by some influential church leaders and scholars, which gave the resultant edited text a prestige status. This may have created a climate of uncertainty among scribes when they were invited, by the exemplar they were using, to include the verse. It is thus possible that the doubt placed on the verse, for example, by Alexandrian editing, left them free to ignore the exemplar, or make a more or less arbitrary alteration. This would give to scribes insufficiently aware of the importance of their work, an otherwise elusive sense of dignity to alleviate the drudgery of unremitting mechanical repetition.

In conclusion, the notes of number, variety, and continuity provide strong probability that verse 26 is genuine. As to the age factor, there is some evidence to show that the verse may have been accidentally omitted during the second century. Its status as a Byzantine reading should no longer cast a shadow on its respectability as a distinct witness to the original Text. Homoioteleuton is as likely a reason as assimilation, to explain its omission from some manuscripts. The verse belongs in the context of Mark 11:25 as a natural sequel to the topic of forgiveness already present in the passage.

***Mark 15:28.***

GNS Mark 15:27-29 Καὶ σὺν αὐτῷ σταυροῦσι δύο ληστὰς, ἓνα ἐκ δεξιῶν καὶ ἓνα ἐξ εὐωνύμων αὐτοῦ. **28** καὶ ἐπληρώθη ἡ γραφή ἡ λέγουσα, Καὶ μετὰ ἀνόμων ἐλογίσθη. **29** καὶ οἱ παραπορευόμενοι ἐβλασφήμουν αὐτὸν, κιννοῦντες τὰς κεφαλὰς αὐτῶν, καὶ λέγοντες, Οὐὰ, ὁ καταλύων τὸν ναὸν, καὶ ἐν τρισὶν ἡμέραις οἰκοδομῶν,

GNS Mar 15:28 καὶ ἐπληρώθη ἡ γραφή ἡ λέγουσα, Καὶ μετὰ ἀνόμων ἐλογίσθη.

For Omission (*UBS<sup>4</sup>*)

Ⲙ	Cod Sin	4th
A	Cod Alex	5th
B	Cod Vat	4th
C	Cod Ephr	5th
D	Cod Bezae	5th
Ψ	Athos	8-9th

minusc 157, 2427

Lect most

*L* it<sup>d-5,k-4</sup> 5th, 4th.

*S* syr<sup>s</sup> from 3rd

*E* cop<sup>sah, bo pt</sup> from 3rd

For Inclusion:

L 8th

Δ 9th

Θ 9th

083, 0250 6/7th, 8th.

Byz [ E F G H ]

E 8th

F 9th

G 9th

H 9th

minusc fl, f<sup>13</sup>, 12th, 13th.

28, 33, 180, 205, 565, 579, 597, 700 892, 1006, 1010,

1071, 1241, 1243, 1292, 1342, 1424, 1505.

Lect		184 <sup>1/2</sup> , 211 <sup>1/2</sup> , 292 <sup>2/3</sup> , 384,
l AD		Apostoliki Diakonia, Athens.
<b>L</b>	it	aur-7, c-12/13, f, ff2-5, 1-8, n-5, rl-7th
(Diatess <sup>arm</sup> )	Ephraem	374 or earlier
Vg		from 4th
<b>E</b>	(cop <sup>bo pt</sup> )	from 3rd
<b>S</b>	syr <sup>p, h</sup> , Harkleian	616
	syr <sup>pal</sup>	from 6th
<b>A</b>	Armenian	from 5th
<b>S</b>	Old Church Slavonic	from 9th, Bulgaria
<b>G</b>	Georgian	from 5th
<b>E</b>	Ethiopic	from 6th
Origen <sup>vid</sup>		2/3rd
Eusebius		4th
Jerome		4th

**Number.** The majority of both uncial and minuscule manuscripts include verse 28. This factor is significant only if it is assumed that the majority manuscripts are, individually, independent witnesses. All that is claimed is that they are independent in their own generation.

**Variety.** There is a preponderance of versions in favour of these verses, namely, almost the entire Latin and Syriac traditions, including the Diatessaron, some Coptic manuscripts, the Armenian, the Georgian, and the Slavonic. Thus the majority of manuscripts and versions are in favour.

However only a few lectionaries agree, including the official Greek version.

Some Fathers witness to the verses including, significantly, Origen<sup>vid.</sup> Caesarea is represented also in Eusebius, and Jerome. Thus the witness of many different languages in various locations throughout Christendom is certain. Such widespread witness is a significant external test of the claim by *Majority text* advocates that the majority reading did not come about by scribes conforming to an arbitrary standard. There was no wholesale collusion, for example, within some narrow textual stream of the Greek-speaking East, as some understand the Byzantine manuscripts to be.

On the face of it, the omission of verse 28 from most of the lectionaries suggests the ten words are not original. The lectionaries however are not united in this, nor does the official Lectionary of the Greek Church reject their genuineness. Burgon had a reasonable explanation for the omission: In the Greek Evangelium no. 71 there is among its "singularly minute and full rubrical directions" (1896b, p. 77) an instruction to the public reader to follow during Holy Week, namely, he must stop at verse 27, skip over verse 28, and go on at verse 29. The purpose of this was, presumably, to maintain unchecked the narrative flow of the Passion narrative in order to emphasise plain historical detail. Burgon saw this as concrete evidence of a very ancient lectionary practice which early made its presence felt in the manuscript tradition, particularly in the four oldest codices. He was surprised that even Griesbach was unable to draw the correct inferences available through lectionary study (1896b, p. 78, n. 1). C. Osburn (1995, p. 64) points out that much more research has yet to be done before it is possible to obtain an agreed consensus on places and dates of origin for the lectionaries. Whereas Burgon (1896b, p. 70) believed that their origins go back to the Apostolic era, other scholars are not prepared to admit any date prior to 300 AD because the lectionaries largely reflect, as they see it, the later recensional Byzantine text.

Linked with the foregoing evidence is the inclusion of the verse in the Eusebian canons. Burgon notes that because Eusebius gave the 10 words a section to themselves,  $\frac{2}{8}16$ , he may be taken as having given special sanction to them. They are likewise recognised in the Syriac sectional system,  $\frac{2}{8}60$ , which is quite independent of the former system (1896b, p. 76).

In conclusion, there is a variety of evidence over a wide geographical area which shows that verse 28 was accepted as genuine. To the objection: "But is it credible that on a point like this such authorities as NABCD should all be in error?", Burgon replied: "On the other hand, what is to be thought of the credibility that on a point like this all the ancient versions (except the Sahidic) should have conspired to mislead mankind?" (1896b, p. 77).

**Continuity:** Evidence exists in the first millennium, as follows, the list being in date order:

Diatessaron, arm		2nd or later
<i>E</i> cop <sup>bo pt</sup>		3rd
Origen <sup>vid</sup>		3rd
Eusebius		4th
Jerome		4th
<i>L</i> vg		4th
OL <sup>a</sup>		4th
<i>S</i>	syr peshitta	5th
<i>A</i>	Armenian	5th
A	Cod. Alex	5th
D	Cod. Bezae	5th
OL	ff 2	5th
OL	n	5th



G	Georgian	5th
Syr	pal	6th
OL	f	6th
Eth		6th
syr	h	7th
OL	aur	7th
OL	r	7th
E		8th
L		8th
OL <sup>1</sup>		8th
⊖	Koridethi	9th
F		9th
G		9th
H		9th
S	Slavonic	9th
1424,		9/10th
700, 1006, 1243		11th
miniscules	f <sup>1</sup> ,	12th
	f <sup>13</sup> ,	13th
OL	c	12/13th

The presence of verse 28 in one manuscript tradition or another is evident throughout the age of the Church in every century. It has also been widely received throughout Christendom.

The "continuity" test is analogous to Hort's "ring of genuineness" in favour of Alexandrian readings, that is, it is seen as partial evidence of a reading which has been generally received, by both leaders and rank and file, as a preferred and genuine reading.

Number variety and continuity all bear witness to the genuineness of these verses.

**Age:** Accepting, for the sake of argument, that the Syriac Peshitta is after all a second century manuscript, Burgon answered the question: "Why shouldn't an older Version be more valuable than a later Greek manuscript?" by saying that in terms of its mere antiquity, it *is* of more value. To this he added patristic evidence, if clear; for example, as in the evidence for verse 28, Eusebius and Jerome are earlier than Codd. A or D. Also the earliest Old Latin manuscripts present rival readings, and both are contemporaneous with, and therefore as valuable as B. Some of the Bohairic manuscripts also witness to the place, and they may be as early as the third century AD. Besides, said Miller, the versions are the more valuable in so far as they are invariably translated from more than one exemplar, unlike Greek manuscripts.

But does not the agreement in Mark 15:28 of the five ancient uncials  $\aleph$ ABCD override versional and patristic evidence? Burgon answered this by agreeing that "the oldest reading of all is what we are in search of" (p. 29), but he did not accept that that reading is necessarily and generally found in the oldest manuscript. Thus, antiquity as an important principle does not mean that of a very few manuscripts, as if their age made them oracular (p. 31). Within a very short time between the Original and the first extant manuscript, scribes and students of that period "evinced themselves least careful or accurate . . . in their way of quoting it" (1896a, p. 29). Thus the whole body of manuscripts, including the cursives, must be consulted.

In justification for his choice of the Byzantine reading for verse 28, Kilpatrick (1990, p. 311) quotes Sturz to show that Hort's theory concerning the secondary character of the Syrian text is effectively answered

in the fact that Byzantine readings often go back to the second century at least. Likewise, Elliott quotes Burgon, approvingly it seems, when the latter dismissed Hort's preference for  $\aleph B$  as scathingly as Hort had dismissed the *Received text*. Elliott (1974) adds,

The age of a manuscript should be no guide to the originality of its text. One should not assume that a fourth-century manuscript will be less corrupt than say a twelfth-century one. . . . Some peculiar readings in papyri, for example, are paralleled only in late Byzantine manuscripts. (p. 342)

If the age of a manuscript is exalted as the supreme test of the worth of a variant, then clearly verse 28 cannot be genuine, where the five oldest uncials unite against it. The Majority principle, however, directly confronts such a method by questioning, as unproved, the assumption that an earlier manuscript will usually transmit a superior text. From the Majority viewpoint, then, the lack of early Greek manuscripts attesting to verse 28 is no index of its spuriousness. The lack of papyrus and uncial evidence from the first four centuries is not seen as a bar to its genuineness. The weight of the various manuscripts is an issue complicated by two assumptions: (1) that the earlier the date of a manuscript, the more weight it carries, and (2) that any version which substantially reflects Byzantine influence is thereby shown to be inferior. As the latter tradition is seen as fourth century at the earliest, and recensional, the value of much versional evidence is determined by this perception. From the Majority viewpoint the external evidence is sufficiently weighty to affirm the genuineness of verse 28.

**Weight:** When Aland (1987) says: "the external attestation for omission is clearly superior" (p. 297), he has in mind the agreement, by omission, of the Alexandrian text and Cod. D. Similarly, the *UBS Textual commentary* shows characteristic reliance on external evidence, namely, the

"earliest and best witnesses of the Alexandrian and Western text-types" (p. 119). However Aland seems to imply that the value of Cod. D as a representative of the Western tradition is in doubt (1987, p. 67); the Old Latin is in support of the verse. Verse 28 is thus a good example of the lack of homogeneity in the Western text, in as much as D parts company with many of the Old Latin manuscripts.

Kilpatrick (1990, p. 309) evaluates Hort's belief that external attestation is decisive against verse 28: "However much we respect his achievements, his judgments are open to question" (p. 311). He rejects the  $\aleph$ B preference: "We have incidentally rejected Hort's appeal to a Neutral text. His theory has come increasingly into question, and even the Alands write 'Nun gibt es für das Neue Testament keinen "neutralen" Text (*Der Text des Neuen Testament*, 24)" (1990, p. 311). Similarly, behind Elliott's (1974, p. 344) loss of confidence in Hort's conclusions is his stated appeal to C. H. Turner's (1923) studies on Marcan usage, when he argued against the text of Hort's best manuscripts in favour of the originality of readings found in Western or Byzantine ones.

Clearly much of the previous discussion under the heading of Age bears on the question as to which manuscripts are most credible, and thus could have been included under this heading. This is not to say that the age of a manuscript is the single most important feature which indicates its worth, as the text within it may be younger, or it may be less accurate than an older manuscript.

The radical eclectics have in effect reinstated some Byzantine readings. Therefore the critic's long-ingrained habit of doubting any variant coming out of such a supposedly inferior source, should itself be seriously questioned.

**Context:** There are no suspicious circumstances surrounding verse 28 in the *Majority text*. In relation to Mark 15:43, Burgon (1896a, pp. 182-185) makes much of Cod. D's many omissions, additions, substitutions, transpositions, and other corruptions. Out of 117 words to be transcribed between Mark chap. 15:47-16:7, sixty-seven of them have been affected in some way. He sees this as clear evidence that the age of a manuscript is no clear indication of the value of its text. Nothing in the context suggests that verse 28 did not originally belong there.

**Reasonableness:** The *Textual Commentary* sees verse 28 as assimilating Luke 22:37: GNS: λέγω γὰρ ὑμῖν ὅτι ἔτι τοῦτο τὸ γεγραμμένου δεῖ τελεσθῆναι ἐν ἐμοί, τὸ Καὶ μετὰ ἀνόμων ἐλογίσθη· καὶ γὰρ τὰ περὶ ἐμοῦ τέλος ἔχει.

Scrivener's (1875) explanation of verse 28 as a later insertion is as follows:

The present citation from Isai. liii. 12 has been brought into St. Mark's text from Luke xxii. 37. Appeals to the Old Testament Scriptures are not much in this Evangelist's manner, and the tendency to enlargement from other Gospels would alone render the passage suspicious. Internal considerations . . . are somewhat adverse to it. . . . The united testimony of the five chief Uncials [NABCD] is simply irresistible. (pp. 136-137)

In this, the *UBS Textual Commentary* and Scrivener overlook the possibility that scribal error explains the omission. Thus the *UBS* editors also note the fact that Mark rarely quotes the Old Testament.

It is thus conjectured that verse 28 began life as a marginal gloss from Luke. This had the effect, in the words of Aland, of "converting the prediction Jesus made there into a theological comment by recounting its fulfillment" (1987, p. 297). Added to this, internal features are considered with a view to confirmation of that decision.

Elliott (1974, p. 343) follows Kilpatrick in reinstating Mark 15:28 by appealing to homoioteleuton, that is, where *καί* is used at the beginning of vs. 27, 28, and 29, as well as further within each of those verses. Kilpatrick (1990, pp. 307-311) evaluates Hort's favourite manuscripts and finds them similarly wanting in omitting verse 28 for the same reason. Concerning scribal errors like homoteleuton he says: Cod.  $\aleph$  and B are "not more immune from this kind of error than other manuscripts" (p. 308). Within range of this verse, there are 18 *καί*'s in the space of 15 verses, as well as 6 *καί*'s within verses 27-29. He shows by several examples that, contrary to the *Textual Commentary*, Mark does make apt quotes from the Old Testament and, if verse 28 is genuine, the way he does this is parallel to other examples. Concerning Luke's use of Isaiah 53:12 he says: "We have . . . according to the overwhelming mass of the witnesses, the same quotation at Mk. xv. 28, a reading that is to be found in Eusebius" (1990, p. 91).

Burgon (1896b, pp. 76-77) supplied a reasonable transcriptional explanation for the omission of verse 28 from the earliest manuscripts. It is outlined under **Variety**, above, in relation to the lectionaries. Not that he thought the canons of internal evidence were really obligatory. He believed that it is usually too difficult a task to discover the origins of, and reasons for variants, particularly those contained in the oldest codices.

Summarising the evidence in accordance with Burgon's seven notes: number, variety, and continuity all bear witness to the genuineness of the verse. As to age, the lack of papyri, and uncial evidence from the first four

centuries is not seen as a bar to its genuineness. The weight of the various manuscripts from the Majority viewpoint affirms the genuineness of verse 28. The verse fits easily into the context, and the lectionary evidence suggests an available reason to explain how the words became omitted from the Text.

**John 5:3b-4.**

GNS Joh 5:3 ἐν ταύταις κατέκειτο πλῆθος πολὺ τῶν

ἀσθενούντων, τυφλῶν, χωλῶν, ξηρῶν, ἐκδεχομένων τὴν τοῦ ὕδατος κίνησιν.

4: ἄγγελος γὰρ κατὰ καιρὸν κατέβαινεν ἐν τῷ κολυμβήθρα, καὶ ἐτάρασσε τὸ ὕδωρ· ὁ οὖν πρῶτος ἐμβὰς μετὰ τὴν ταραχὴν τοῦ ὕδατος, ὑγιὴς ἐγένετο. ὧ δῆποτε κατείλετο νοσήματι.

**Verse 3b, Variants:**

1. ξηρῶν p<sup>66,75</sup> ⋈ A\* B C\* L T 0141, 157, it<sup>9</sup>  
(syr<sup>c</sup>) cop<sup>sah,pbo,bo pt, cop<sup>ach</sup> 2</sup> Amphilochius.

p <sup>66</sup>		200
p <sup>75</sup>		early 3rd
A*	Cod. Alex	5th
B	Cod. Vat	4th
C*	Cod. Ephr	5th
L		8th
T		5th
0141		10th
33		9th
157		1125 approx

it <sup>a</sup>		6-7
(syr <sup>c</sup> )	cop <sup>sah,pbo,bo pt</sup>	all 3rd
cop <sup>ach 2</sup>	sub achmimic	3rd
Amphilochius.		late 4th

Variant 2: ξηρών, ἑκδεχομένων τὴν τοῦ ὕδατος κίνησιν.

A<sup>c</sup> C<sup>3</sup> (W<sup>supp</sup> ἑκδεχομένοι) Δ Θ Ψ 078,0233 f<sup>1</sup>, f<sup>13</sup>, 28, 33, (180 ἀπεκδεχομένων) 205, 565, 579, 597, 700, 892, 1006, 1010, 1071, 1241, 1243, 1292, 1342, 1424, 1505, Byz { E F G H } Lect (l 1016 ἑκδεχόμενοι) it<sup>c,e,ff2</sup> vg, syr<sup>p</sup>, h, pal cop<sup>bo pt</sup> arm eth<sup>pp</sup> geo slav Chrysostom Cyril<sup>em</sup>.

3. ξηρῶν παραλυτικῶν ἑκδεχομένων τὴν τοῦ ὕδατος κίνησιν

D it<sup>a</sup>, aur, b, d, j, l, vg<sup>ms</sup> it<sup>l</sup> ( it<sup>r l</sup>, omit ξηρων )  
(eth<sup>TH</sup>)

#### Verse 4

Ἄγγελος γὰρ κυρίου κατὰ καιρὸν ἔλουέτο (κατέβαινε), ἐν τῷ κολυμβήθρα, καὶ ἐτάρασσε τὸ ὕδωρ· ὁ οὖν πρῶτος ἐμβὰς μετὰ τὴν ταραχὴν τοῦ ὕδατος, ὑγιής ἐγένετο, οὖν ᾧ δῆποτε κατείχετο νοσήματι.

#### For omission:

p <sup>66</sup> ,		200
p <sup>75</sup>		early 3rd
κ	Cod. Sin	5th
B	Cod. Vat	4th
C*	Cod. Ephr	5th
D	Cod. Bezae	5th
T		5th



<b>W<sup>supp</sup></b>		5th
<b>minuscules</b>		1141-10th
		33 -9th
		157-12th
<b>L</b>		itd -5, f-6th, l-8th, q-6/7,
<b>vg<sup>ww,st</sup></b>	Wordsworth-White, Stuttgart 1592;1969	
<b>S</b>	(syr <sup>c</sup> )	3rd
<b>E</b>	cop <sup>sah,pbo,bo pt,</sup>	all 3rd
cop <sup>ach 2</sup>	sub achmimic	3rd
<b>Amphilochius.</b>		late 4th
<b>G</b>	geo	from 5th
<b>A</b>	arm	from 5th

For inclusion:

(according to A, with many variations in later manuscripts and versions.)

<b>A</b>	Cod. Alex	5th
<b>C<sup>3</sup></b>	Cod. Ephr	5th 2nd corrector later Alexandrian text
(W <sup>supp</sup> ἐκδεχομένον)	Washington	5th later hand (original missing) later Alexandrian
<b>L</b>		8th later Alexandrian
<b>Δ</b>		9th later Alexandrian
<b>Θ</b>		9th
<b>Ψ</b>	Athos	8-9th later Alexandrian
<b>078, 0233</b>		6th, 8th
	f <sup>1</sup> , f 13,	12th, 13th

28, 180, 205, 565, 579, 597, 700, 892,

1006,1010,1071,1241,1243,1292,1342,1424,1505 ,

Byz {E F G H} 8th, 9th

Lect

*L* it a-4, ar-9th, aur-7th, b-5th, c-12/13th,  
ff2-5th, j-6th, r1-7th, l -8th

vg<sup>cl</sup> Clementine Vulgate.

*S* syr<sup>p</sup>. pal 5th; from 6th

cop<sup>bo pt</sup> from 3rd.

Diatess 2nd or later

*E* eth from 500

Slav from 9th

Didymus<sup>dub</sup> 398

Chrysostom 407

Cyril<sup>lem</sup> 386

Tertullian, after 220

Hilary 367

Ambrose. 397

**Number:** The vast majority of manuscripts include verses 3b-4. This includes all known cursives (except those listed under "For Omission") together with Byzantine uncials, particularly Codd. E F G H, and typically represented by Codd. IKMUVΓ (with\* throughout).

Early papyri p<sup>66,75</sup> omit. So also do the early uncials ⋈ B C\* D (together with T and W). Just a few cursives omit 0125, 0141, 18, 33, 134, 157, 314. The following versions omit: it<sup>d,fl,q</sup>, vg<sup>ww,st</sup>, syr<sup>c</sup>, cop<sup>sah,pbo,bo pt, ach 2</sup>, arm, geo. Amphilochius knows nothing of the verse.

With the early Alexandrian manuscripts and D in favour of omission, (although D admits v. 3b) as well as several versions, especially the Coptic, the *Majority text* needs other supports if it is to convince of genuineness.

**Variety:** Greek manuscripts are well represented in the *Majority text*.

As to the versions, most of the Egyptian, the Curetonian Syriac, a minority Old Latin, the Armenian (divided), and the Georgian are against. When Scrivener rejects the passage "in the face of hostile evidence so ancient and varied" (1875, p. 158), it is clear he thinks more of the "ancient" than the "varied", because the evidence for inclusion is even more varied, namely, most Old Latin, most Old Syriac, Coptic (part), Armenian, Ethiopic, and Slavonic versions. Again, with Aland (1987, p. 297) the Egyptian Versions win the day. Several versions of the Diatessaron allude to verse 4, though presumably there is no direct quotation from it, for otherwise the *UBS Textual Commentary* would have included it on the same principles as were used in citing patristic evidence (1994, p. 38\*). However, if these allusions did not bear specific witness to the inclusion of verses 3b-4 in the Diatessaron, it is doubtful that the *UBS* editors would have cited the passage in their 1983 edition (p. 338) as being witnessed to

by the Arabic, Armenian, Italian and Old Dutch translations of Tatian's work.

Patristic evidence is much more for inclusion. Evidence for inclusion comes from the following areas: Constantinople, Egypt, Palestine, Carthage, Gaul, Italy, Syria, Asia Minor, and Bulgaria {against: Constantinople}. Though omitted by *UBS<sup>4</sup>*, according to Burgon and Scrivener several Fathers also bear witness to the place: Burgon cites Nilus (4), Jerome, Cyril<sup>Alex</sup>, Augustine (2), Theodorus Studita; Scrivener cites Theophylact and Euthymius. Whether these latter references can be confirmed or not, there is still a clear preponderance of evidence under this head which offers substantial support for the genuineness of the passage.

**Lectonaries:** Most include.

Variety argues strongly for inclusion, though some of the Versions face both ways. Scrivener says in its favour,

Since [Cod. A and the Latin versions] are not very often found in unison, and together with the Peshitto, opposed to the other primary documents, it is not very rash to say that when such a conjunction does occur, it proves that the reading was early, widely diffused, and extensively received. (1894:2, p. 362)

It is thus difficult to understand why Fee should deny that this evidence is diverse and widespread (1982, pp. 214-215), or what text-critical grounds explain the continuing immoveable opposition to this passage.

**Continuity:** There is evidence that the passage was known from the late second century onwards; every century of the first millennium shows acquaintance with it, as well as the cursive witness of subsequent centuries.

Diatessaron	2nd or later
Tertullian	early 3rd
cop <sup>ach 2</sup>	3rd
cop <sup>boh</sup>	3rd
Hilary	4th
Ambrose.	4th
Didymus <sup>dub</sup>	4th
geo	5th
Chrysostom	5th
Cyri <sup>lem</sup>	5th
A* Cod. Alex	5th
W <sup>supp</sup> Washington	5th
C* Cod. Ephr	5th
T	5th
it <sup>ff</sup>	5th
it <sup>e</sup>	6th
it <sup>j</sup>	6th
E	6th
it <sup>f</sup>	6th
it <sup>q</sup>	6th
it <sup>r</sup>	7th
it <sup>l</sup>	8th
L	8th
Ψ	8-9th
Δ	9th
Θ	9th

The preponderant agreement of the uncials and cursives show the presence of the reading in every century, with several Fathers, especially Tertullian, showing they knew the verses at an early period. It may be assumed the omission occurred in the second century, as did most serious omissions.

Number, variety, and continuity combine to indicate the genuineness of the passage, once it is acknowledged that the age of the manuscript witness is not the main factor to consider. As most of the Greek manuscripts, most of the versions, many Fathers, and most of the lectionaries include verses 3b-4, then the main challenge to these verses rests on internal considerations, not external ones. Nevertheless, for the sake of completeness, considerations of age, textual context and manuscript credibility are considered also.

**Age:** The earliest uncials combine in omitting. This, together with the preference of the *UBS* editors for the Alexandrian stream, explains the omission of verses 3b-4 from the critical text. Aland places heavy reliance on its omission by  $p^{66}$ ,  $p^{75}$ ,  $\aleph B$  and the allied Coptic manuscripts. However the unanimity of four out of the five oldest uncials ( $\aleph BCD$ ) also ties in with a similar disagreement that these four manuscripts have over the name of the pool. It could be that various reasons, as outlined under **Reasonableness** below, caused the scribes of  $p^{66}$ ,  $p^{75}$  and the four uncials to further the doubts fostered by asterisks or obeli which previous scribes had marked against these verses, and so reject them. Because doubts had been raised as to whether this pool could be identified in Jerusalem by the second century, the scribe was under pressure to give it a name which placed its location elsewhere than the traditional site. This would explain why none of the earliest papyri and uncials agree together as

to what name to give the once famous pool of Bethesda. Hodges (1979, p. 36) quotes the studies of Jeremias on the Copper Scroll discovered in Cave III at Qumran which show that the *Majority text* is right here in reading  $\beta\eta\theta\epsilon\sigma\delta\alpha$ .

Several manuscripts of Tatian's Diatessaron refer to verse 4: Diatess<sup>a,e</sup> arm, i, n (*UBS<sup>3</sup>*).

The Coptic<sup>ach 2</sup> in sub achmimic dialect (Thompson) and the Bohairic are both dated from the third century, bearing witness to the inclusion of the passage.

Omission by the papyri may appear to be a weakness. However scribal faithfulness militates against their credibility; see below under *Weight*.

**Weight:** Burgon made much of the chaotic state of the Text, in verses 1, 3, and 4 particularly, as an argument in favour of inclusion (1896a, pp. 82-84).

The unanimity of four out of the five oldest uncials ( $\aleph$ BCD) in rejecting verse 4 ties in with a similar rejection of the traditional reading  $\beta\eta\theta\epsilon\sigma\delta\acute{\alpha}$ .  $p^{66}$  and  $p^{75}$  align themselves with the same readings as  $\aleph$ B. Yet these six manuscripts have no agreement among themselves as to what name to give the pool. Burgon (1896a, p. 83) says: "There is so much discrepancy hereabouts in  $\aleph$  and their two associates (CD) on this occasion, [that] nothing short of unanimity . . . would free their evidence from suspicion." The same discrepancies are clearly seen in the two papyri which follow the general variation  $\beta\eta\theta\sigma\alpha\iota\delta\acute{\alpha}$ . Rather, what we find when comparing them with their satellite uncials and cursives is "hopeless prevarication" (p. 83).

The connection between the distrust of the earliest manuscripts towards verse 4, and the rejection of the Majority reading of  $\beta\eta\theta\epsilon\sigma\delta\acute{\alpha}$  becomes significant when considering the probable cause for the omission of the verse in the first place, for which see below.

*UBS<sup>3</sup>* sets out with care the details of the many small scribal variations in writing verse 4. Scrivener believes that such "extreme variation in the reading . . . so often indicates grounds for suspicion" (1894:2, p. 361). This suggests that the verse forms no part of the original Text. However, such variation may in fact witness to nothing more than that the passage was eliminated from some manuscripts, for whatever reason, fairly early in the second century. This ever after placed suspicion on it, and initiated a controversy in the early church which encouraged scribes to declare "open season" on the verse and suggest other possibilities to the detailed wording, happy in the assurance that it may not be a part of the original Gospel anyway.

Fee (1982, p. 209) believes that the omission in some manuscripts of verse 3b alone, whilst retaining verse 4, is evidence of addition, in as much as there is little to explain why a manuscript should drop verse 3b on its own. The relevant manuscripts (Fee, 1982, p. 203) are as follows:-

A\* L Diatess<sup>l, i, n</sup>.

Thus, if the passage is spurious, the addition of verse 3b as well as  $\pi\alpha\rho\alpha\lambda\upsilon\tau\iota\kappa\omega\upsilon$  could be explained as having been added subsequently to verse 4, to facilitate its connection with verse 3.

However, there are also some manuscripts where only verse 4 is omitted, whilst verse 3b is included. These are:

D W<sup>supp</sup>, 0141, 33, it<sup>d, f, l</sup>, vg<sup>ww</sup>, geo.

If verse 4 is spurious, it is difficult to explain how verse 3b was ever inserted in the absence of verse 4, as there is contextually no need for it. Why, asks



Fee, would anyone "have expunged ἐκδεχομένων τὴν τοῦ ὕδατος κίνησιν?" However, if a scribe was unhappy with the writer's belief in the tradition, he may have arbitrarily removed the offending words, though having insufficient temerity to eliminate verse 4--he may have thought of verse 4 as reported speech which did not necessarily reflect whether the writer believed it or not. Besides, the difficulty in explaining the omission also applies, assuming verse 3b to be genuine: Why was verse 3b ever inserted in the absence of verse 4? Although the clause harmonises with verse 7 there is still, contextually, no need for it. There is in fact no way of satisfying the critic's need for clarification from the narrative as to whether the writer accepted the story's supernaturalist perspective. When considering the true causes of the pool's healing properties, one person's faith is another person's incredulity.

There is thus no way of knowing in what order the material of verses 3b-4 was added or omitted--in effect the arguments cancel each other out. Again, the real grounds for Fee's decision seems to be, not so much contextual, but arising out of his trust particularly in  $\aleph B$ , as early and best.

**Reasonableness:** There are four major questions, from an internal standpoint, for or against the genuineness of John 5:3b-4. These are:

(1) The context of the passage naturally invites an explanation of the reason for the hope of the sick man, as referred to in verse 7. Does verse 4 belong naturally in the narrative, or is it better understood as a gloss to supply what the writer's thoughtlessness failed to provide for his readers?

(2) What historical reasons are there which best explain the verses, whether as something added, or as a genuine part of the story but eliminated over time?

(3) Are there stylistic reasons which suggest the writer of the fourth Gospel could not have written the words?

(4) To what extent does the philosophical base, that is, a pro- or anti-supernaturalist assumption, affect the reasoning of the textual critic in trying to decide the genuineness or otherwise of the verse?

Attention is now given to each of these questions in turn.

(1) Hills (1956, p. 145) quotes Hengstenberg who says: "the words are necessarily required by the connection." Hengstenberg quotes von Hofmann who believed it is highly improbable:

that the narrator who has stated the site of the pool and the number of the porches, should be so sparing of his words precisely with regard to that which it is necessary to know in order to understand the occurrence, and should leave the character of the pool and its healing virtue to be guessed from the complaint of the sick man, which presupposes a knowledge of it. (p. 145)

Considering the universal quality and intended readership of the fourth Gospel, it seems inexcusable that the writer should assume his widely-spread readers knew, or ought to know, exactly what was in the mind of the sick man, that is, why he believed the pool had healing power.

(2) Are there historical indications to show whether it is wisest to explain these verses as something added, or as a genuine part of the story though eliminated over time?

As a reason to explain why verses 3b-4 have been asterisked in several manuscripts, Hodges refers to the way they were used in the early church to justify belief in the supernatural effects of water. Tertullian himself explained his belief in baptismal regeneration by reference to verse 4. Hodges quotes Tertullian at length in order to suggest that the verse "was being employed polemically . . . in a fashion uncongenial to certain early Christian circles" (1979, p. 35). He conjectures that Alexandria's

intellectual atmosphere naturally encouraged a disbelief in the healing effect which ostensibly resulted from an angelic interference with water. Against that, Fee (1982, p. 209) finds the suggestion utterly unconvincing, in so far as both Clement and Origen unequivocally support a Biblical view of angels, which allowed for such interaction. He sees Tertullian as drawing attention not to some people's scepticism about angels per se, but rather to their scepticism as to whether the efficacy of Christian baptism could be explained by angelic involvement.

A more convincing reason for the passage being subsequently omitted from the original Text is given by Hills (1956, p.146). He quotes A. Hilgenfeld and R. Steck to suggest there were doubts in the early church of the second century as to whether the pool even existed. Perhaps it could no longer be identified there. Tertullian explained this as Divine retribution. He believed the curative powers mentioned were withdrawn because of Israel's rejection of Messiah. But it would seem that not everyone was convinced by this explanation. The scribe may have felt that sufficient doubt left him free to indulge in conjectural emendation. Witness the connection between the distrust of the earliest manuscripts towards verse 4 and their rejection of the Majority reading of  $\beta\rho\theta\epsilon\sigma\delta\acute{\alpha}$ . This becomes significant when considering the possibility that the various changes in the name of the pool were an attempt to provide an alternative site; such an attempt would overcome the embarrassment of those who felt that if they were to provide an adequate response to the growing number of marginal asterisks against the passage they must find the true location.

(3) Are there stylistic reasons suggesting the writer of the Fourth Gospel could not have written the words? Hodges (1979, p. 38) appears to accept the view of C. H. Dodd as to the use of independent oral sources by the writer. Dodd tried to isolate a pre-canonical tradition behind the Gospel

which included an account of Jesus the Healer, although he thought this was little used by the Evangelist (cited by Guthrie, 1965, pp. 283-284). Fee (1982, p. 213) goes to great lengths to expose not only several hapax legomena in verses 3b-4, but also several non-Johannine characteristics, used, he says, in a context where a special vocabulary was uncalled for. He believes that the cumulative effect of these characteristics makes the authenticity of verse 4 highly unlikely. Morris (1971) says: "There is no need to deny that (John) made use of sources. . . . But he has so thoroughly made them his own that they cannot now be recovered" (p. 58). However, is it true that a writer can completely conceal his use of special sources? If it is true, how can we be sure he used any? If we assume then that John did use sources, would we not expect there to be an alignment of several unusual words, expressions or constructions to indicate such a usage from time to time? Hodges (1979) is surely right to see behind these words:

a tradition that was handed on from mouth to mouth . . . and which served to explain what transpired there. . . . [The tradition had] a certain verbal sameness . . . which tended to re-occur . . . . [The author] would be strongly inclined to use verbiage he himself had heard at the very scene of the event. (p. 38)

The conjunction within a short space of so many hapax legomena, though unusual, may be evidence of John's use of a special source at that point. Consummate artistry allows him to use language not his own with all the impression that it belongs entirely to him. The actual form of verse 4 is Johannine, as witnessed by the use of οὐν. Concerning this B. F. Westcott (1908/1980) says:

St John does in fact insist more than the other Evangelists upon the connexion of facts . . . . His most characteristic particle in narrative . . . is οὐν and this serves . . . to call attention to a sequence which is real, if not obvious. (p. cvi)

(4) Another question relates to the philosophical issue. To what extent does the pro- or anti-supernaturalist stance of the textual critic affect his reasoning, when he attempts to decide the genuineness or otherwise of the verse?

Aland (1987, p. 298) sees the secondary character of angels stirring water as "obvious" internal evidence. Scrivener (1875, p. 157-158) says the verse "certainly wears the semblance of a gloss." He adds, in agreement with Alford: "[It is] an insertion to complete what the narrative implied with reference to the popular belief." This assumes the tradition linked with the pool, when seen through scientific eyes, is hopelessly self-condemned for its naive perspective. However such an anti-supernaturalist philosophical bias is ideologically laden. If an unnecessary a priori were allowed free rein, it would prejudice the external evidence not only for this passage, but also for texts reporting other incidents in the Gospels, for example, Jesus' temptation in the wilderness, the various healing accounts involving demonic possession, the release of Peter from prison, and the angel moving the stone from Jesus' grave (Brown, n.d.). However, Aland makes it clear (1987, p. 275) that he has already decided the question on the external evidence, as laid down by his rule three; the internal evidence merely confirms the decision. This is in contrast to Fee (1982, p. 213), who finds the internal grounds (that is, the non-Johannine language of verse 4) to be "devastating" evidence for its spuriousness. Fee (p. 212) seems to be overstating the case here; for example, he says that in v. 3b, where the enclosed genitive is used with two definite nouns, this usage is as likely to occur in Johannine Greek, as it is for a proper Bostonian to use a Texan drawl. However such a construction also appears in John 18:10.

The evidence for inclusion is now summarised. First as to number: The vast majority of manuscripts include verses 3b-4; as to variety, Greek

manuscripts are well represented in the *Majority text*; as to continuity, there is clear evidence that the passage was known early; every century of the first millennium shows acquaintance with it, together with the cursive witness of subsequent centuries. Number, variety, and continuity all combine to indicate the genuineness of the passage. Because most of the Greek manuscripts, most of the versions, and most of the lectionaries include verses 3b-4, it follows that the main challenge to these verses rests on internal considerations, not external ones. As to age, the earliest uncials combine in omitting. This, together with the preference of the *UBS* editors for the Alexandrian stream, explains the omission of verses 3b-4 from the critical text. However the unanimity of 4 out of the 5 oldest uncials (NBCD) also ties in with a similar disagreement that these four manuscripts show over the name of the pool. Because doubts had arisen as to whether the pool could be identified in Jerusalem by the second century, the scribe was under great pressure to give it a name which placed its location elsewhere than the traditional site. It could be that, for various reasons, the scribes of <sup>p66</sup>, <sup>p75</sup> and the four uncials, furthered doubts fostered by asterisks or obeli which previous scribes had marked against these verses. As to their context, Burgon made much of the chaotic state of the text in these verses 1, 3, and 4 particularly, as an argument in favour of their genuineness.

***Acts 8:37.***

**Variants:**

1. Omit v.37. *UBS*<sup>4</sup>

2. Act 8:37 εἶπε δὲ αὐτῷ ὁ Φίλιππος. Ἐὰν πιστεύεις ἐξ ὅλης καρδίας σου, ἰσωθήσῃ. ἀποκριθεὶς δὲ εἶπε, Πιστεύω εἰς τὸν Χριστὸν τοῦ υἱοῦ τοῦ Θεοῦ.

3.GNS (TR) Act.8:37 εἶπε δὲ ὁ Φίλιππος, Εἰ πιστεύεις ἐξ ὅλης τῆς καρδίας, ἔξεστιν. ἀποκριθεὶς δὲ εἶπε. Πιστεύω τὸν υἱὸν τοῦ Θεοῦ εἶναι τὸν Ἰησοῦν Χριστόν.

Variant 1, For omission:

p <sup>45</sup>		3rd
p <sup>74</sup>		7th
κ	Cod. Sin	4th
A	Cod. Alex	5th
B	Cod. Vat	4th
C	Cod. Ephr	5th
Ψ	Athos	8-9th
33 <sup>vid</sup>		9th
minusc 81-10,181-11, 614-13, 1175-11,1409-14,2344-11,		
Byz [ L P ] Lect		9th
vg	ww,st	Wordsworth-White, Stuttgart
syr	P	1st 1/2 5th, or 2nd?
cop	sah,boh	from 3rd
eth <sup>pp</sup>		from 500 Pellplatt
Chrysostom		407 Byz
Ambrose		397

Variant 2:

εἶπε δὲ αὐτῷ ὁ Φίλιππος. Ἐὰν πιστεύεις ἐξ ὅλης καρδίας σου σωθήσῃ ἀποκριθεὶς δὲ εἶπε, Πιστεύω εἰς τὸν χριστὸν τὸν υἱὸν τοῦ Θεοῦ."

E	6th
it <sup>e</sup>	6th
Greek ms	8th according to Bede

Variant 3: TR

Acts 8:36... τί κωλύει με βαπτισθῆναι; **37** εἶπε δὲ ὁ Φίλιππος, Εἰ πιστεύεις ἐξ ὅλης τῆς καρδίας, ἔξεστιν· ἀποκριθεὶς δὲ εἶπε, Πιστεύω τὸν υἱὸν τοῦ Θεοῦ εἶναι τὸν Ἰησοῦν Χριστόν. **38** καὶ ἐκέλευσεν στήναι τὸ ἄρμα:

minuscules:	36-12, 307-10, 453-14, 610-12, 945-7/8, 1678-14, 1739-10, 1891-10.
l	lectionaries 592 (16th), 1178 (11th),
l <sup>AD</sup>	Apostoliki Diakonia, Athens.
<b>L</b>	it ar-9, c-12/13, dem-13, gig-13, l-7, p-13, ph-12, ro-10, t-11, w-14
	[it starts at 9th, goes through to 14th]
earliest OL	l-7, ar-9, ro-10
vg <sup>cl</sup>	Clementine lat vg [Pope Clement VIII 1592]
S <sup>syr</sup> h,with*	Harkleian syr. AD 616, reading in text enclosed with * and metobelos



<i>E</i> cop <sup>meg</sup>	Middle Egyptian	from 3rd
<i>A</i>	Armenian	from 5th
Eth <sup>TH</sup>	Ethiopic	from c. 500: takla haymanot
<i>S</i>	Old Church Slavonic	from 9th, Bulgaria.

Irenaeus	2nd
Cypr	258
Ambrosiaster	post 384 anon.
Pacian	ante 392 Bp. Barcelona.
Chromatius	407 Bp. Aquileia, N Italy.
Aug	430
Speculum	5th Ps-Aug.

**Number:** Scrivener mentions that Erasmus could not find it in any Greek manuscript, save one marginal addition: "Hence its authenticity cannot be maintained" (1875, p. 73). The *Received text* reading of verse 37 fails the Majority test in that almost all uncial manuscripts and most cursives omit it; likewise the papyri p<sup>45</sup> and p<sup>74</sup>. The cursive witness for it consists of twenty-six manuscripts beginning in the tenth century (Brown, 1973). Unusually the Byzantine manuscripts agree with the Alexandrian. The general agreement of papyri, uncials and cursives for omission weighs heavily against its genuineness, as each manuscript, even the minuscules, must be seen as independent witnesses (Burgon, 1896a, pp. 46-47).

However the manuscripts should show 100% agreement to provide certainty. *E* (6th) and some of the later cursives (36-12th, 307-10th, 453-14th, 610-12th, 945-7/8th, 1611-12th, 1739-10th, 1891) contain it. Burgon saw *E* as the beginning of a period of greater perfection in text which reached its height in the later middle ages (1896a, p. 203). As the

manuscripts are not unanimous in omitting it, the question must be partly determined by variety and continuity--*quod semper, quod ubique, quod ab omnibus*--the threefold cord not quickly broken for the *Majority text* advocate (1896a, p. 224).

**Variety:** The versions are facing both ways, particularly the Egyptian Coptic, the Syriac, the Ethiopic; for inclusion are most Old Latin manuscripts (many latish),--and this is remarkable bearing in mind how diverse their readings are; so also the Clementine vg, cop<sup>meg</sup>, syr<sup>h</sup>, arm, eth, geo, and Slavonic. For omission are early Egyptian papyri, cop<sup>both</sup>, syr<sup>pal</sup>, p, eth, vg<sup>corr</sup>.

Patristic evidence faces both ways: For omission, according to region, are Constantinople, and N. Italy; for inclusion are Gaul, Carthage, Spain, N. Italy, and Britain.

Lectionary evidence: the bulk favour omission. But some include: 592 (16th), 1178 (11th), including I-AD (Apostoliki Diakonia), the lectionary text of the Greek Church, Athens, 1904. The reading of I-AD makes the lectionary evidence per se equivocal.

By way of interim summary, there is a variety of evidence in both directions; the united witness of Old Latin manuscripts counterbalances the Greek lectionary evidence in favour of omission.

**Continuity:** Evidence exists in every century, as follows, the list being in date order:

1.	Irenaeus	2nd	2.	Tertullian	2-3rd
3.	p <sup>45</sup>	2-3rd	4.	cop meg	3rd
5.	Cyprian	3rd	6.	Pacian	4th
7.	Ambrose	4th	8.	Ambrosiaster	4th
9.	Augustine	5th	10.	Chromatius	5th
11.	Georgian	5th	12.	Armenian	5th
13.	Cod D (hiatus)	5th	14.	Old Lat e	6th
15.	Oecumenius	6th	16.	OL	6th
17.	Ethiopic TH	6th	18.	Syriac h	7th
19.	OL l	7th	20.	P 74	7th
21.	OL r	7-8th	22.	Lat vg A c	7-8th
23.	Cod E	8th	24.	OL ar	9th
25.	Lat Vg	9th	26.	Arabic	8-14th
27.	Slavonic	9th	28.	OL g	9th
29.	Speculum (m)	10th	30.	OL ro	10th
31.	Cursive 1739	10th	32.	107	10th
33.	103	11th	34.	Theophylact	11th
35.	OL t	11th	36.	Cursive 945	11th
37.	13	11th	38.	15	11th
39.	18	11th	40.	100	11th
41.	106	11th	42.	14	11th
43.	25	11th	44.	29	11-12th
45.	323	11-12th	46.	Cursive 105	12th
47.	97	12th	48.	88	12th
49.	OL ph	12th	50.	lectionary 59	12th
51.	Cursive 5	12th	52.	OL p	12th

53.	OL c	12-13th	54.	OL dem	13th
55.	OL gig	13th	56.	Cursive 36	13th
57.	69	13th	58.	1877	14th
59.	629	14th	60.	OL w	14-15th
61.	429	14-15th	62.	630	14th
63.	4mg	15th	64.	27	15th
65.	60	15th	66.	7322	15th
67.	Clement	16th	68.	Lat vg Sixtus	16th

( Brown, op. cit.)

Number clearly suggests omission. Variety is indeterminate.

Continuity favours inclusion. Therefore other notes must decide.

**Age:** Very early patristic testimony in Irenaeus (second century: significant as very early and Greek), and Tertullian; then Cyprian, Ambrose, Augustine. The earliest versional evidence is fifth century Georgian and Armenian. The Old Latin witness for inclusion is strong: it<sup>l</sup>, ar, ro (7th, 9th, 10th), and only <sup>e</sup> (6th) for variant 2. The second century reading provides important early evidence suggesting it may have belonged to the original text of Acts.

**Weight:** "This [note] is concerned with the credibility of a witness judged by its own performance" (Pickering, 1977, p. 135). Thus although P<sup>45</sup> is the earliest manuscript evidence extant (200AD) and in favour of omission, the scribe's work in copying Acts is characterised by intentional omissions in favour of a shorter text (1977, p. 118, n. 13, quoting Colwell). Aland (1987) dismisses the verse: "The external evidence is so weak . . . [What evidence there is] does not give the insertion sufficient

support to qualify it for a claim to originality" (pp. 298-299). For the Majority school however, the Alexandrian manuscripts Cod. B and  $\aleph$  to which Aland tacitly appeals, are witnesses of very low character. The evidence from D is missing. The peculiar readings of Cod. D are usually assessed as an extraordinary collection of interpolations in a manuscript containing "a prodigious amount of error" (Hort, p. 149). Cod. E in support of variant 2 is an important manuscript which may be a witness to its well-established presence, during a second stage of the development of the ecclesiastical text (Burgon, 1896a, p. 203). The question of weight is not directly relevant in deciding on v. 37.

**Context:** This is a specific and limited application of the note of Weight. How does the witness behave in the given vicinity of the text? If the context is very corrupt, then considerable suspicion and reserve attach to it. The only substantial variations within the vicinity are in 7:24, and 7:39, both of which concern additions from  $\delta$  (i.e. Cod. D). It could therefore be argued that verse 37 is part of the series of interpolations from  $\delta$  omitted from most critical texts, and is thus likely to be as spurious as they are. Strange (1992, pp. 50, 69-77) however argues on internal grounds for their inclusion. Also Kilpatrick (1992, pp. 417-418), in proving that Acts 7:56 should contain the phrase "Son of God", treated verse 37 as genuine, saying that the writer was insensitive to the repetition of the phrase in the same immediate context. He supports this evaluation by quoting other examples in Acts, including the fourfold repetition of "eunuch" in verses 36-39, although he hardly needed the example in verse 37 to prove his point.

There are no clear signs of serious corruption in the vicinity--in the absence of any evidence from D. The intact state of the text (5 to 10 verses either way of the verse) points to its possible genuineness.

**Reasonableness:** Miller earlier included in this seventh note of Burgon several of the internal canons made familiar by Griesbach and others (1886, p. 121), for example, "Choose the reading which most easily explains the others". On this basis there follow arguments for and against omission:

First, arguments for omission: Scrivener sees it as a gloss added to make explicit what Luke's text assumed, namely, that confession of faith was needed before baptism. Thus the ΠΙΣΤΕΥΩ clause had been earlier placed in the margin, being extracted from some Church Ordinal (1894:2, p. 369). Metzger (1971, p. 359) says: "There is no reason why scribes should have omitted the material, if it had originally stood in the text." However, although Aland says: "The voice which speaks is from a later age" (1987, p. 299), there are reasons for inclusion. Assuming the words were included by Luke, there are two reasons why verse 37 may have been dropped out of the original Text:

(1) The text was omitted later "as unfriendly to the practice of delaying baptism, which had become common, if not prevalent, before the end of the third century" (Hills, 1956, p. 201). Accordingly, the doctrinal implications of verse 37 offended the views of Hermas and Tertullian, who approved of postponing baptism until Christian character was fully formed. This was in the belief that repentance for serious post-baptismal sin was possible only once. Such an omission would have been widely accepted (Brown, 1973).

(2) W. A. Strange (1992, p. 70) offers another reason for omission: "There was an esoteric dimension to second-century Christianity which should not be ignored." Accusations of dreadful crimes committed at the closed meetings of Christians led to early persecution, which thereafter

made them cautious. Strange says, in support of  $\delta$  of Acts: "There may have been a difference of opinion about the amount which might be revealed to outsiders concerning the nature of Christian worship" (p. 70). A cautious scribe might have felt that verse 37 was too frank in describing something so determinative as admission to fellowship, at a time when such rubrics tended increasingly to surround themselves with secrecy (pp. 70-71). Examining both the linguistic detail and literary context of the verse, he suggests how to explain the second variant, found in E: εἶπε δὲ αὐτῷ ὁ Φίλιππος, 'Εάν πιστεύεις ἐξ ὅλης καρδίας σου σωθήσῃ. This is not a further and later variation to a gloss, but is an attempt to show that the expressed condition for baptism was reflected elsewhere in the New Testament, for example, Rom 10:9: "The most that can be said with regard to the eunuch's confession is that in its content it is not anachronistic as a part of Acts" (1992, p. 75). Metzger (1971, p. 359) says the phrase τὸν Ἰησοῦν Χριστόν is not a Lukan expression. But the use of the phrase, unique in Luke-Acts, is in harmony with Luke's concern for using historical materials; thus he prefers rather to reproduce a received formula than to offer free composition (Strange, 1992, p. 75).

Arguments from reasonableness exist for both inclusion and exclusion, but on balance the possible causes of the variants favour verse 37 being genuine.

Number clearly suggests omission. Variety speaks both ways. Continuity favours inclusion. The note of weight ("respectability of witnesses") is indeterminative. However arguments from both favour its inclusion.

Continuity, context and reasonableness together are barely enough to outweigh the sheer force of numbers of manuscripts in favour of omission. However, Acts 8:37 is very possibly genuine.

The three notes of variety, number, and continuity are the most important, and interdependent. Where all three agree, practical certainty is assured in the making of textual choices. Nevertheless "concerning the seven Notes of Truth, the student can never afford entirely to lose sight of any of them" (p. 67).

*Summary of Findings*

In each case Aland has followed the Alexandrian reading as superior. This is consistent with his fivefold classification where he places the Alexandrian text in his most important Category 1. It is not consistent, however, with his statements elsewhere in discussion with Epp and other where he placed more reliance on simple papyrus readings per se, and disclaimed any reliance on a "star" manuscript. In the latter case he could have relied solely on the papyrus evidence for his conclusions on two of the six texts examined, namely, John 5:3b-4 and Acts 8:37.

By not focusing specifically on the papyri evidence for these verses, he seems, rather, to suggest that the papyri are merely supportive to the Alexandrian text in these places, that is, their witness has no independent status.

If Hort's view is accepted as to the superior nature of the readings of Cod B--they being often supported by R--it is natural to see Hort's characterisation of the Byzantine text as authoritative. The view is consistent with the inference that the Byzantine editor has arbitrarily harmonised the Gospel in the interests of a fuller text. In each of the Synoptic examples examined, the argument from harmonisation is used prominently and with unquestioned confidence, by Aland and the *UBS*



editors, to assert the rightful marginalisation of the texts. In only one case, Mark 9:46,48, is harmonisation to the immediate context a factor.

The battle between text-types is reflected in the discussion of Acts 8:37. W. A. Strange is able to accept the authenticity of this text on ostensibly internal grounds. It is difficult to avoid the impression, however, that he finds those internal grounds so appealing because he is already predisposed to the external evidence for the longer Western text, that is, he tends to judge its longer readings as non-interpolative. [Indeed, the same argument could be used for the conclusions drawn from the internal evidence examined in this Paper, namely, a predisposition towards the Byzantine text encourages the writer to discover cogent literary or historical reasons which serve to justify the textual choices he has already made mostly on the basis of external evidence.]

The disparity in text-critical method between reasoned and radical eclectic critics is highlighted in 3 of the 6 texts, namely, Mark 11:26, Mark 15:28, and Acts 8:37. Kilpatrick and Elliott have dissenting views to offer on these verses. In the first two, homoioteleuton is an obvious internal reason which helps to explain how the texts became omitted on the first occasion. The critics' freedom from devotion to NB as oracular, explains their apparent willingness to acknowledge that Byzantine readings reach back to the second century, and may be good readings. In the case of Acts 8:37, Kilpatrick gives no particular reason for including it as genuine, as the focus of his discussion is on the following chapter of Acts. Presumably his inclusion of the verse is based on the belief that not all longer Western readings can be dismissed as interpolative, any more than all shorter Alexandrian readings should be rejected merely for the sake of protesting against Hort's excessive devotion to Cod. B. However, their freedom to ignore any particular text-type, as neither better nor worse than any other,

carries with it the duty to explain why, for example, Acts 8:37 should be accepted or rejected, if not on documentary grounds. It is puzzling why Kilpatrick felt under no special obligation to provide a reason for his inclusion of Acts 8:37 in the Text, whether by way of intrinsic or transcriptional probability. In default of an explanation, it seems critical intuition is the natural one. Hort himself relied on it in preferring the shorter readings of Cod. B. Intuition is defined as "knowledge or belief obtained neither by reason nor by perception." Is this a stable philosophical basis to justify safe choices between textual variants?

The discussion on John 5:3b-4 is relevant to the fact that textual critics are increasingly recognising that scribes made deliberate alterations in the text for theological or dogmatic reasons. It is of course possible that verse 4 was omitted by scribes who dismissed the possibility that an angel could interfere with water. In a more scientific age it is natural for critics to be equally predisposed against the text, partly because of its supernaturalist perspective. After all, the evangelists have angels moving a heavy gravestone with the resurrection of Jesus, and freeing St. Peter from metal chains. A theological presupposition, or at least a philosophical one, may be at work in the critic's mind helping to predispose his choices as he evaluates the text.

The basic internal canon: "Choose the reading which explains the origin of all the others" is an important rule behind the analysis expressed in the *UBS Textual Commentary* on Matt 17:21, Mark 15:28, and John 5:4 and Acts 8:37. It is closely linked to the reasoning reflected in the editors' expression: "There is no good reason why the passage, if originally present . . . should have been omitted" (pp. 43, 119, 359). However in all four texts, this study has provided possible reasons, which help to explain their omission.

In arguing the case for the *Majority text*, four of the six texts were practically decided on the basis of "the threefold cord" alone, namely, number, variety, and continuity. The two remaining texts were short of evidence in either variety (Mark 9:46,48) or in number (Acts 8:37). However Burgon did not believe it was wise to leave the establishing of the text to these three alone. For thoroughness all seven notes must be used. This allows for the possible genuineness of Acts 8:37 in spite of the fact that it does not belong to the *Majority text*. Presumably this latter choice is unacceptable for *Majority text* advocates, like Pickering or Hodges, who would rather rely solely on the argument from statistical probability, to prove the superiority of only and all Majority readings. Burgon would possibly judge the latter approach to be both unnecessary and doubtful, inasmuch as the assumptions made about the history of transmission are difficult to prove. As to the case for Mark 15:28, Burgon may prove to be possibly ahead of his time by his use of evidence from the lectionaries to help explain how the text became marginalised.

## CHAPTER 5

## Evaluation of the Majority Text Method

*Burton and His Successors Compared*

The recent *Majority text* debate traces its source to John Burton; however the ethos and emphases of Hodges, Pickering, and Van Bruggen seem to be quite different from that of Burton and Miller. The latter set out seven tests by which to determine the true readings among competing variants. In contrast, the former, though recognising the worth of Burton's Seven Notes, give the impression that "majority rule" is enough to determine which is the preferred reading; thus the suspicion remains that the method is in effect one note masquerading as seven. Wallace (1992, p. 37) shares Fee's view that the method is no more than an invitation to count manuscripts as the one way to evaluate readings.

Chapter 4 examined six examples from the Gospels and Acts in the attempt to show that the description of the *Majority text* method as merely "counting noses" is misleading. The dominant emphasis on number, which characterises the latest expression of the *Majority text* method, can be found nowhere in Burton's writings. Burton and Miller made it clear that the first three tests among the seven are all equally important, and must be taken together: they are number variety and continuity. Where these act as "a threefold cord" no further corroboration is needed. Where one of the three tests is defective, the others must help to decide the issue. Antiquity is obviously an important factor, but should not be given the predominant and undue weight so characteristic of most text-critical method.

The weight or credibility of each manuscript witness is partly measured by another test, namely, the state of the text in the immediate context of the variant, as measured by the *Majority text*. Burgon gave very little room for his seventh test of reasonableness, and in spite of Miller's earlier views to the contrary, little if any weight has ever been given by *Majority text* advocates to internal evidence, as usually understood.

### *The Numerical Argument*

The credibility of the argument from number depends entirely on whether the lack of evidence for the Byzantine text between the years 100 AD-350 AD is prima facie evidence that the text-type is a recension from the fourth and subsequent centuries. As Van Bruggen (1976) pointed out: "We must agree with the modern textual criticism that the majority *in itself* is not decisive. Not the majority of manuscripts but the weight decides" (p. 14). Pickering's method, as described by Fee and Wallace, is dismissed; Pickering gives a simplistic impression because no detailed examples are offered to allay suspicions, and to show how Burgon's method actually works. Wallace (1991, pp. 158-159, n. 31) explains that his evaluation is based on "Appendix C" of Pickering's book, which puts forward statistical arguments for the *Majority text*. The argument purports to prove that this text is, prima facie, closer to the Original than any other text-type.

However, this overlooks two facts: first, the Appendix is an edited abstract of Z. C. Hodges and D. Hodges, and was in effect not the author's work; thus any impressions gained from it must be put in the wider context of the book. Secondly, Pickering spends ten pages explaining and developing Burgon's seven-fold method, which as chapter 4 has shown, necessitates a thorough consideration of all the external evidence--especially the weight of the manuscript tradition.

The fact that Pickering has expounded Burgon's method faithfully suggests that he intends to take it seriously. Accordingly, *Majority text* advocates who see themselves in the tradition of Burgon should take steps to avoid giving the impression that his note of number determines virtually every textual decision.

The supposed irrelevance of counting manuscripts in determining the true text is criticised by Wallace (1992, pp. 21-50) for its lack of certainty, inasmuch as not infrequently a decision has to be made between two variants within the majority tradition. Wallace also faults *The Greek New Testament According to the Majority Text* in that the "assumption as to what really constitutes a majority is based on faulty and partial evidence (e.g. von Soden's apparatus) not on an actual examination of the majority of manuscripts." (p. 37)

Wallace (1995, pp. 311-312) also argues from the silence during the earliest centuries of extant witnesses to the *Majority text*, that it is tantamount to concrete historical evidence that the *Majority text* did not exist as such at that time. Burgon explained the absence of such evidence as due to the scribal habit of tearing up the exemplar after the copy was made, due to its damaged or over-worn state. Wallace (1995, pp. 311-312) believes it is right to assume from the lack of historical evidence that the Byzantine text did not become the *Majority text* until the ninth century. How then, he says, did the *Majority text* ever gain dominance if Burgon's description of scribal habits is correct? *Majority text* advocates, in his view (1992) have no right to "tacitly assume that since most Greek manuscripts extant today belong to the Byzantine text, most Greek manuscripts throughout Church history have belonged to the Byzantine text" (p. 30). These assumptions and counter-assumptions suggest two conclusions: (1) It is a *reductio ad absurdum* to make the numerical principle the main, and

in effect, only genuine test of an authentic reading, and (2) it is an unsound method to use inferential data from debatable textual arguments in order to establish historical facts, inasmuch as such an approach mistakes theoretical conjecture for hard evidence. One person's inevitable conclusion becomes another's gratuitous assumption. Wallace (1995, p. 315) however, in harmony with most textual scholars, believes it is artificial not to allow historical conclusions to be drawn from textual studies in such a way. If it is valid to view history and textual criticism as interdependent disciplines, the *Majority text* advocate believes the nature of the text invites theological considerations (for example, the belief in providential preservation of the best text through the centuries) to be included, in helping to determine in fine detail the true text. As long as eclectic critics evaluate external evidence differently inter se, a *Majority text* advocate will promote the study of this text in the belief that the Byzantine peculiarities are misnamed—they occur frequently in other text-types. The argument from genealogy is irrelevant to this question, as research on the Byzantine text to date indicates that the many manuscripts should be treated as independent witnesses, rather than as the result of a deliberate editorialising process which took the Church further away from the Original, instead of bringing it closer to it.

Wallace (1989a, pp. 286-287) questions further the validity of the numerical principle by asking why on this basis should only Greek manuscripts be counted? There is a twofold answer to this question: (1) Burgon advocated counting all manuscripts, versions, lectionaries and patristic references, as a disciplined way of focusing on all the evidence, not just some of it, and (2) the contemporary emphasis on Greek manuscripts is in tacit harmony with the prevailing view among textual scholars that the Greek manuscripts have unique significance, in that they represent a more

direct link with the Original than any other textual form. Wallace raises two examples which make the numerical principle unsafe to apply. First, he asks as an example, whether the eight thousand Latin Vulgate manuscripts should be counted--for this may give many Alexandrian readings a numerical preponderance. However, no *Majority text* advocate would admit to using the numerical principle in a mechanical way. Versional evidence, though relevant, is clearly secondary to Greek evidence; besides, the recensional status of the Vulgate is an historical fact--the Vulgate was the result of one man's attempt to bring order out of chaos within the very complex and diverse manuscripts of the Old Latin tradition. Unlike the evidence for a Byzantine recension, the certainty of the Vulgate revision is independent of inferences drawn by critics from the subtle and sometimes incorrect use of internal criteria. The second example Wallace (1989a) raises relates to the result of a possible future discovery of large quantities of manuscripts, whose textual character is non-Byzantine. He asks: "Could the majority text view survive the blow of a 'Greek Ebla'? . . . Such a cache is not out of the realm of possibility" (p. 287). He cites the discovery in 1975 of over 3000 manuscripts in St. Catherine's monastery, of which a substantial number are Biblical manuscripts. This would put in question the dominating position of the *Majority text*, from a numerical perspective. However, there is hardly a probability that this will happen.

### *The Evidence of Variety*

The argument for variety has a double-focus on geography and kinds of text, that is, it looks for evidence of wide geographical distribution to support the validity of a reading. Also, as an integral part of that wide distribution, it expects that reading to show up not only in several or most of the versions, but also in patristic references to the particular text, again,



in as wide a spread as possible. Even if the reading is not represented in most of the Greek manuscripts, this may not be decisive if it easily passes the tests of variety and continuity--Acts 8:37 is a case in point. It is only by applying this test strictly to a variant can it be known whether a manuscript witnessing to it is free of the effects of arbitrary editorial emendation.

Scrivener (1894) pitted variety against number by saying:

In weighing conflicting evidence we must assign the highest value not to those readings which are attested by the greatest number of witnesses, but to those which come to us from several remote and independent sources, and which bear the least likeness to each other in respect to genius and general character. (p. 301)

In saying this Scrivener parted company with both Burgon and Hort: with Burgon, because he believed that real agreement between the earlier uncial tradition was a more significant index to the true reading on the basis of age; he also parted company with Hort, and most contemporary critics, who place trust in Alexandrian readings more than in any other manuscript tradition. This distrust highlights Burgon's definition of the nature and importance of this test. Pickering sets out its implications:

If the witnesses which share a common reading come from only one area, say Egypt, then their independence must be doubted. It seems quite unreasonable to suppose that an original reading should survive in only one limited locale. . . . It follows that witnesses supporting such readings are disqualified. (p. 133)

Much of the force of this test of variety is lost in contemporary debate where the emphasis is on the text-types rather than on individual variants. The discussion also places the main focus on the value of the Greek manuscript witness, and tends to down-play the value of other kinds of evidence. Thus Wallace (1991, pp. 156-157) in effect faults Pickering for

rejecting the Alexandrian witness in preference for the *Majority text*. Wallace says he is overlooking the fact that the *Majority text* was unknown in the mainstream of Egyptian textual study and usage, as shown by Ehrman's study of Didymus the Blind in the Gospels. For Burgon, this would have been an argument from silence, rather than circumstantial evidence for the non-existence of the Byzantine text before Nicaea. The main problem with the "best text" approach, is that there is a lack of consensus among textual critics as to the superiority of any one textual tradition. However the impression is given that the Byzantine text is not worth considering. It treats issues opened up by 20th-century papyri studies as if they were still closed.

#### *The Argument from Continuity*

The third strand of the threefold cord is the argument from continuity. This is often dismissed as, in essence, a theological principle, born of Burgon's commitment to High-Anglican dogma, and therefore irrelevant to textual criticism. However, the *Majority text* advocate believes it is not possible to isolate textual criticism of the New Testament from theological presupposition and belief, in as much as the goal of the New Testament writers was to produce more than great literature; it was designed to make a theological statement. It should therefore be evaluated textually with the same presuppositions that the writers themselves brought to their writing, namely, their belief that they were revealing and recording infallible truths which had permanent and detailed significance to all Christians who read them. If this presupposition is correct, it provides a necessary basis to Burgon's third note. The argument from Continuity seems to be that in every age the majority of Christians will both recognise and preserve the text nearest to the Original. Thus the text most preferred

by the widest and most numerous audience will more likely reflect the text most carefully preserved and most extensively used. For Burgon the argument from continuity takes seriously the corporate dimension of belief. It thereby interacts with the collective awareness of most Christians through the centuries by affirming a numerically overwhelming choice of preferred text. This evaluation is similar to Hort's embracing of "morally preferred readings." The acceptance of most Christians of the *Majority text* gave these readings, for Burgon, the "ring of genuineness," in the same way that Hort trusted his own intuition when preferring the readings of  $\alpha B$  to Western or Byzantine ones.

Wallace (1992, pp. 29-31) makes Burgon's note of Continuity irrelevant by assuming the focus should be on the witness of text-types through the centuries, rather than on variant readings per se. Burgon distrusted Hort's analysis of text-types. He was concerned merely to discover whether a reading manifested itself throughout the history of the manuscript tradition, whether through patristic citation, versional usage, or in uncials and cursives. If it did, it carried weight. But it only carried weight in connection with variety and number; that which had been accepted *quod semper, quod ubique, quod ab omnibus* was not lightly to be tossed aside. He treated the presence or absence of a reading in an ostensible text-type as irrelevant--in the same way as does a "rational" critic, though the latter may have more respect, at least in a notional sense, for established text-types.

### *The Age of the Text*

The age of the text is obviously an important factor, but should not be given the predominant and undue weight so characteristic of most text-critical method. Burgon measured antiquity by the first six centuries. He

agreed that antiquity is the most important single principle, but not by itself, that is, not that of a very few manuscripts, as if their age made them oracular. Within a very short time between the Original and the first extant manuscript, scribes and students of that period "evinced themselves least careful or accurate . . . in their way of quoting it" (1896a, p. 29). Thus the whole body of manuscripts, including the cursives, must be consulted. "The more ancient testimony is probably the better testimony" (p. 40), but "not by any means always so"--hence the need for other tests. The study of Byzantine readings by Sturz (1984, pp. 67-71) gives Pickering reason for saying: "Not only may age be demonstrated by a single early witness, but also by the agreement of a number of later independent witnesses--their common source would have to be a good deal older" (p. 129). Sturz finds distinctively Byzantine readings in the early papyri; by "distinctive readings" he means those readings which are not found in the principal manuscripts of the Alexandrian and Western texts. Sturz implies that the presence of these readings in the second century is consistent with the belief that everything truly characteristic of the Byzantine text-type will be found in the second or third-century papyri--but Fee does not believe Sturz has proved his thesis. However, if this is the inevitable inference from Sturz' study, then it follows that the Byzantine text may be no more recensional than any other textual stream. On the strength of this inference, Pickering is able to say in relation to the test of antiquity: "Any reading that has wide late attestation almost always has explicit early attestation as well" (p. 129).

Two facts make the test of antiquity highly problematical for textual criticism: (1) There is general agreement that most variants probably arose before the end of the second century. Thus the age of the manuscript is no index per se as to whether it has suffered greater or lesser corruption, and (2) "Of even greater importance than the age of the document itself is the

date of the type of text which it embodies" (Metzger, 1992, p. 209). It is assumed that the passing of time increases the opportunities for corruption. Thus the test of antiquity remains important, as Burgon (1896a) says: "It remains true, notwithstanding, that until evidence has been produced to the contrary in any particular instance, the more ancient of two witnesses may reasonably be presumed to be the better informed witness" (p. 40).

However, an early date for a reading may do little to commend its genuineness to a critic who finds its text-typical setting uncongenial.

Neither Burgon's distrust of the then five famous early uncials  $\alpha$  ABCD, nor his belief in the trustworthiness of the united witness of the great body of uncials and cursives to the true text, is shared by most contemporary critics of the New Testament text. However the *Majority text* advocate will probably do better to examine further the supposed characteristics of the Byzantine text as a way forward, rather than trying to justify the emphasis on the numerical principle by the statistical method--which unfortunately creates a simplistic impression as to what the Majority method is about.

### *The Best Manuscripts*

Burgon's test of weight, or respectability of manuscripts, depends on an assumption whose validity seems to be denied by most critics, namely, that the quality of scribal faithfulness in copying a manuscript should not be ignored as a basis for evaluating the worth of the textual readings contained in it. This seems to be a guiding principle for Burgon by which he compared the value of the five ancient uncials known with the later manuscript tradition. Pickering unwarrantedly simplifies the matter by referring to the careless scribe as, not merely "morally impaired", but telling repeated lies (p. 126). However, for a critic who is at a loss to know which

text-type or "star" manuscript to follow, the relevance of a moral factor should not be ignored. By "a moral factor" is meant the responsibility of the scribe to take all necessary care to copy exactly and conscientiously from his exemplar, in awareness of the magnitude of his responsibility as an essential transmitter of ostensibly revealed truths--insights which are indispensable to human well-being and morally transforming. Pickering reminds us that the scribes of  $\alpha$ B hardly understood their task in such terms:

As to B and Aleph, we have already noted Hoskier's statement that these two MSS disagree over 3,000 times in the space of the four Gospels. Simple arithmetic imposes the conclusion that one or other of them must be wrong 3,000 times--that is, they have made 3,000 mistakes between them. (If you were to write out the four Gospels by hand, do you suppose you could manage to make 3,000 mistakes, or 1,500?) Aleph and B disagree, on the average, in almost every verse in the Gospels. Such a showing seriously undermines their credibility. (p. 120)

This example may be particularly relevant, as according to Burgon (1896a, p. 164), there may be palaeographical evidence that the scribe of Cod. B copied at least some of  $\alpha$ , and both manuscripts may have been produced in the same location. The significance of the moral factor, as described, is more obvious for those who believe the Bible is not merely a record of religious experience, but is also a necessary and sufficient provision of a metaphysical basis for objective truth.

Once the question of weight is no longer seen as dependent mainly on the age of a manuscript or of a text-tradition, then other factors are more readily brought to bear. This was emphasised in chapter 4 in relation to Mark 11:26. If age does not indicate weight, what does? Burgon argued that if a variant is attested by  $\alpha$ B, together with most Old Latin

manuscripts, plus one or two Fathers, "then there ought to be found at least a fair proportion of the later Uncials and the Cursive copies to reproduce it" (1896a, p. 60). In other words, a reading should be rejected if later uncials are silent, or the main body of cursives are silent, or many Fathers know nothing of the matter. Burgon believed that we are more justified in disregarding uncial evidence than cursive (1896a, p. 202, 206). The text of the later uncials is the text of the majority of all uncials. This same text is similar to that of the majority of the cursives, and the majority of versions, and the majority of Fathers (p. 206).

Holmes (1983, pp. 17-18) agrees with Fee in faulting the *Majority text* method as only seven different ways of counting manuscripts. This, he says, is shown by the circular argument involved, namely, of weighing a manuscript not by trying to evaluate its influence and importance in the history of the textual tradition, but against an arbitrary assumption that the *Majority text* is the original Text. Holmes implies that the only way to avoid circular reasoning is to weigh manuscripts on the basis of internal criteria--by such rules text-types are discerned and readings are evaluated as superior or otherwise. This explains why internal rules are rejected by, for example, Pickering and Van Bruggen. Holmes sees Van Bruggen's advocacy of the Byzantine text as a plea to eliminate textual criticism altogether, and as a rejection of internal reasoning. However, it is better to interpret Van Bruggen (1976) as making a plea to stop working on the basis of theoretical and uncertain reconstructions of the history of the text, "because [the supposed recensional process] has left surprisingly few trails behind in the historiography" (p. 18). Burgon's (1896a) writing presents a different approach again, for he sets out a detailed reconstruction of the history of the text to justify his commitment to the "Traditional Text", whilst explicitly rejecting the kind of internal canons that Hort relied on, by

which the latter came to prefer Alexandrian readings. Holmes further implies that Pickering should not have set out Burgon's sevenfold method as characteristic of the *Majority text* method, as it cannot be employed without the use of internal criteria, the use of which Pickering has rejected. However it is more accurate to say that the *Majority text* method rejects not internal reasoning as such, but the well established canons, in preference for its own internal guidelines. This is shown by Hodges' attempt to decide between readings within the Majority tradition, when applying stemmatics to some portions of the New Testament. All this suggests that *Majority text* advocates would do well to return to a closer detailed study of Burgon with the aim to understand better the nature of external evidence, and to avoid the mistake of oversimplifying the *Majority text* method.

The weight or credibility of each manuscript witness is partly measured by another test, namely, the textual characteristics in the immediate vicinity of the context of the variant, using the *Received text* as a basis of comparison. Burgon, for example, evaluated Hort's "Western non-interpolations" (found in Cod. D) very differently from Westcott and Hort. Thus Hort found eight singular readings in Luke 22 and 24--a singular reading is "a variation of text that is supported by one Greek manuscript but has no other (known) support in the Greek tradition" (Epp, 1993, p. 52). In these eight places Hort believed the scribe of D was alone right to omit material from the text.



Pickering summarises Burgon's point well:

According to [*Westcott and Hort's*] own judgment, codex D has omitted 329 words from the genuine text of the last three chapters of Luke plus adding 173, substituting 146 and transposing 243. By their own admission the text of D here is in a fantastically chaotic state. . . . How can *any* value be given to the testimony of D in these chapters, much less prefer it above the united voice of every other witness?!?! (p. 136)

### *The Note of Reasonableness*

Burgon gave very little room for his seventh test of reasonableness, and in spite of Miller's earlier views to the contrary, little if any weight has ever been given by *Majority text* advocates to internal evidence, as usually understood. Chapter 4 of this thesis has applied one or more of the seven canons earlier approved by Miller under this heading, even though it seems the latter was persuaded by Burgon to abandon the internal rules of evidence he had learned from Scrivener.

### *Strengths and Weaknesses of the Majority Text Approach*

The major criticisms of the *Majority text* approach relate to matters of both external and internal evidence. However, the dominant critical spotlight focuses on external evidence for two reasons. Firstly, there still seems to be at least passive agreement among textual critics that the Byzantine text is the least valuable source of readings, being the result of late editing. If this is true, then the *Majority text* approach is ill-founded and lacks credibility *ab initio*. Secondly, the *Majority text* method is understood as a simple matter of "counting noses"; moreover, Burgon's one

note of number is seen as having been dressed-up in a sevenfold masquerade.

Nine areas have been discussed in relation to external evidence. These are [1] the importance of the age of a manuscript, [2] the relationship between the evidence from the earliest papyri and later text-types, [3] the validity of the numerical argument, [4] the normality or otherwise of textual transmission, [5] the use or misuse of the genealogical principle, [6] the meaning and significance of text-types, [7] the special characteristics of the Byzantine text, [8] the evidence from versions and lectionaries, and [9] the patristic use of the Byzantine text. Two other issues relate to the use of internal evidence, namely, the need for confidence in the reliability of internal canons, and the desire for certainty, versus the acceptance and use of conjectural emendation. The strengths and weaknesses of the *Majority text* approach are determined by a response to each of these eleven issues.

#### *The Importance of the Age of a Manuscript*

A reasoned eclectic approach aims to discover the earliest representation of the original Text, especially in the second century, as of supreme importance. The *Majority text* advocate agrees that the earliest extant evidence is important and desirable, but by no means crucial to the discovery of the Original. He finds an ally in a radical critic like Elliott (1972), who makes more than a theoretical statement about textual method in saying:

The age of a manuscript should be no guide to the originality of its text. One should not assume that a fourth-century manuscript will be less corrupt than say a twelfth century one. . . . Some peculiar readings in papyri, for example, are paralleled only in late Byzantine manuscripts. (p. 343)

However, most scholars active in textual criticism probably agree that, in general, earlier manuscripts are more likely to be free from those errors that arise from repeated copying. Again, this is more than a theoretical statement, for there seems to prevail a tacit assumption that the earlier the age of a manuscript, the more likely it is that the date and character of the text-type within it will also be earlier.

### *The Relationship between Papyri and Text-types*

The relationship between papyri and text-types is highly controverted. Aland (1979, p. 11) suggested the irrelevance of text-types for making textual decisions, in favour of simple papyrus readings which were closer to the Original than any text-type. Fortified by disagreements between scholars, such as those that were later expressed in an exchange of views between Epp and Aland, Pickering (1977, pp. 55-56) feels confident to assert that the heavy presence of "mixture" in the earliest papyri calls in question the very existence of text-types. This leaves the *Majority text* advocate able to pursue Burgon's method, ignoring the supposed strength of a variant which is represented in all the recognised text-types.

Alternatively, Sturz attempted to show from the papyri that the Byzantine text-type is present in the third century, along with the other text-types. Fee more adequately represents the reasoned eclectic view; unconvinced by Sturz' reasoning, he believes he has successfully proved from papyrus study that the Alexandrian text-type existed in the second century in a relatively pure form. This, for Fee, vindicates the status quo. He believes that it is fatal to Hodges' view that there are no examples of the *Majority text* in the early papyri. He claims to have shown from the relationship of Cod. B with p<sup>75</sup> that they share an Alexandrian text-type which is non-

recensional, and which reaches back to an archetype in the early or mid-second century. For Wallace, the discovery of almost 100 New Testament papyri over the last century, none of whose earliest examples reflects the homogeneous text characteristic of later Byzantine manuscripts, is sufficient evidence to prove the Byzantine text is late and recensional.

Holmes (1995) is more sceptical of the latter point:

Ever since Zuntz's study of P<sup>45</sup> demonstrated the antiquity of more than a few Byzantine readings . . . it appears that we have quietly ignored Westcott and Hort's untenable view of the origins of the Byzantine textual tradition. (p. 351)

Elliott (1986) also believes Sturz has rehabilitated the Byzantine text by providing evidence for its possible existence in the second century as an unedited text. Sturz, he says, has offered evidence which gives textual criticism a healthier future.

#### *The Validity of the Numerical Argument*

The guiding principle for Hodges, who began the debate, was Hort's admission of a theoretical presumption. This was that the text which has been copied more continuously and consistently than any other has a better claim to represent the original. Thus if a reading is attested by a majority of independent witnesses, it is likely to be genuine. Unanimous attestation provides the desired certainty. The *Majority text* advocate believes there are good grounds for believing the Byzantine manuscripts have been shown to be individual witnesses in their own generation. Until empirical proof is provided, it is right to assume with Burgon that the Byzantine manuscripts were in a majority at *every* historical stage of their transmission. It is assumed that this text reaches back to the autographs in so far as no

evidence has yet been produced, for example by study of the earliest papyri, which clearly invalidates the assumption. Thus, as part of the weighing process, manuscripts must also be counted. Burgon (1896a) however, was far too aware of the several kinds of evidence to suggest that readings should be decided by counting as the supreme test: "Number then constitutes Weight . . . not absolutely, but *caeteris paribus*, and in its own place and proportion" (p. 44).

Fee believes Hodges' use of Hort's theoretical presumption to be "totally illogical", in as much as the farther removed a manuscript is from the autograph, the more it will reflect the errors made during the history of its transmission. Fee accepts the long-held belief that the Byzantine text resulted from deliberate editing, and he believes the text gradually evolved as it reached each new editorial stage. T. J. Ralston pointed out the difficulty in working with an explanation of the text which contains an inner contradiction. Thus, on the one hand, "normal transmission" carries with it the implication that a text-tradition will become more diverse as scribal errors and emendations are added with each new generation; yet on the other hand the way the Byzantine text developed shows that it grew more homogeneous over time. Both propositions are generally acknowledged to be true; it follows that the inner contradiction must be somehow explained. It is also generally acknowledged that editorial revision must be assumed to have some negative as well as positive effect on the text, that is, it probably both restores and corrupts the Original. If these are facts, they seem to make Hodges' statistical presupposition simplistic and irrelevant. The *Majority text* advocate however denies that his preferred text is the result of systematic editorialising, although he would explain the Alexandrian and Western texts by such a process.

Elliott (1983) is more sympathetic with the numerical principle, though warns that it must not be accepted uncritically, as if majority readings were always superior to their rivals. He also questions "the wisdom of basing a printed edition [of Hodges and Farstad] exclusively on a head count of manuscripts" (pp. 590-591). However, the editors of *The Greek New Testament according to the Majority text* make no claim to have arrived at the original Text:

[We] do not imagine that the text . . . represents in all particulars the exact form of the originals. . . . [The Majority Text] represents a first step in the direction of recognising the value and authority of the great mass of surviving Greek documents. . . . All decisions about MP<sup>t</sup> [that is, readings where the Majority tradition is divided] are provisional and tentative. . . . The text may very well be improved with different choices in many cases. (pp. x, xxii)

The numerical principle may constitute at one and the same time the method's greatest strength and, paradoxically, its greatest weakness. Its great strength lies in the obligation it places on the critic to consider absolutely all the external evidence, not just a part, nor only a few favoured manuscripts. It highlights for the critic both the supreme importance of understanding the history of the text and at the same time reminds him of the underlying technical reason which explains the lack of unity behind text-critical methodology. However, its great weakness is the feeling it engenders in those who follow it, namely, that textual decisions can be reached by that means alone, without considering in detail all the factors outlined by Burgon. This weakness is exposed at the point where the *Majority text* advocate tries to decide which of two readings within the Majority tradition may be the original. The numerical principle is thus used as if it is equivalent to a mathematical proof, as if textual criticism were "objective" science rather than subjective art.

*The Normality of Textual Transmission*

The question of the normality of textual transmission is connected with the previous discussion. The *Majority text* advocate believes that under normal circumstances the older a text is than its rivals, the greater are its chances to survive in a plurality or a majority of the texts extant at any subsequent period. But the oldest text of all is the autograph. Thus it ought to be taken for granted that, barring some radical dislocation in the history of transmission, a majority of texts will be far more likely to represent correctly the character of the original than a small minority of texts. This is especially true when the ratio is an overwhelming 8:2. Under any reasonably normal transmission conditions, it would be for all practical purposes quite impossible for a later text-form to secure so one-sided a preponderance of extant witnesses. Fee's objections to this have already been given. Metzger also cites several upheavals which show the transmission was not "normal," for example, the Imperial persecutions and the destruction of libraries on a large scale. He also explains that explicit references to readings which were familiar in many manuscripts, for example, in the time of Origen or Jerome, are now not available in extant copies. They were once widely known but now are in few witnesses, or in none. "Such a situation rules out any attempt to settle questions of text by statistical means" (Metzger, 1992, p. 292). Pickering's attempts to explain the Majority phenomenon as the inevitable result of successive generations choosing the best text, have so far carried conviction with very few textual critics.

*The Use of the Genealogical Principle*

There seems to be a current consensus that this principle neither affirms nor denies any particular textual tradition. Epp explained that the older simplistic genealogical approach (stemmata and archetypes) has been abandoned almost entirely by New Testament textual critics, except in connection with small "families" of manuscripts, because it is both inapplicable to the massive and disparate New Testament data and ineffectual in tracing sure developmental lines through manuscripts with such complex mixture as those of the New Testament textual tradition. Fee agrees the genealogical argument is defective. Kilpatrick (1978) is also sceptical about genealogy. He says: "Rigorous arguments based on . . . the imprecise grouping of manuscripts in local texts or text-types . . . cannot be employed in this way" (p. 142). Metzger however shows the debilitating effect which Hort's genealogical argument had on confidence in the *Received text*, in that it proved that numerical preponderance is not evidence per se of superiority of text. The *Majority text* approach to this question is ambivalent. Pickering on the one hand asserts that the many intentional and religiously motivated scribal errors make the genealogical approach too difficult to apply to the New Testament. Hodges on the other hand, attempts to apply stemmatics to it as the only logical method. Wallace believes the attempt to apply the genealogical principle has an unfortunate effect on the *Majority text* approach in that the editors, in the process of choosing between more than one variant within the Majority tradition, not infrequently choose a reading supported by only a minority of manuscripts within it. In sum, the genealogical argument should be seen as irrelevant to the status of the Byzantine text as it is ineffectual in tracing sure developmental lines through manuscripts. However if the Byzantine



text were allowed to vie with the other text-types in a claim to originality, then the numerical preponderance of the *Majority text* would be seen to be more significant.

*The Meaning and Significance of Text-types*

The significance of text-types is the crucial area of debate as between *Majority text* advocates and a reasoned eclectic approach. Pickering (1977, pp. 62,87-88) extrapolates from Kilpatrick's and Elliott's free use of Byzantine readings to conclude that their method is only possible on the assumption that the Byzantine text deserves some other description of its peculiarities than the one Hort gave to it. Thus Wisselink (1989, p. 245) sees in the Alexandrian text as many instances of assimilation as in the Byzantine. Kilpatrick felt able to explain some of Hort's "conflations" as original readings, and the smoothing-out process, supposedly characteristic of fourth century editing, is explained as the understandable influence of Semitic style on the New Testament writers. Wallace (1995) criticizes the *Majority text* approach on the same basis as Fee. Thus he says: "Although isolated Byzantine readings have been located [in the early papyri], the Byzantine text has not" (p. 313). He thus accepts that there is no good reason to question the accuracy of Hort's analysis and description of the peculiar characteristics of the Byzantine text. For Pickering (1977, pp. 56-57), contemporary studies of papyri arbitrarily assign portions of the text to various text-types other than the Byzantine, when the data could as easily be interpreted in its favour. He extrapolates from Aland's rejection of text-types for the earliest centuries, by doubting its validity for the later centuries also.

These familiar groupings remain axiomatic for reasoned eclectics. The use of Byzantine readings by Kilpatrick and Elliott has tended to restore the dignity of the Byzantine text. A strength of the *Majority text* approach is its ability to identify with "radical" critics in recognising the worth of the Byzantine tradition. For example, Kilpatrick (1978, p. 144) doubted the relevance of text-types in making textual decisions. Similarly, Elliott (1978, p. 108) acknowledges that there is a history to trace but no reconstruction so far attempted is significant for textual decisions, because "the ability to trace such a history is doubted by so many critics nowadays." A parallel weakness of the Majority approach, at least psychologically, is its recommendation that the very notion of "text-type" is gratuitous and should be retired (Pickering, 1977, p. 110). Metzger (1992) distinguishes a text-type as "a more broadly-based form of text that evolved as it was copied and quoted in a particular geographical area of the early Christian world" (p. 287). This definition shows that for many critics the concept still holds, although the geographically-oriented terminology has been modified to some extent, if only to greatly reduce the work of sifting a multiplicity of manuscripts. Pickering's recommendation is much more radical than that of other critics. Leon Vaganay and C-B. Amphoux (1986/1991, p. 70), for example, believe that the validity of text-types should be established afresh. Meanwhile, Fee (1993, pp. 247-251, 272) and perhaps many, if not most active critics, tacitly imply that further study and the passage of time will only confirm the essential correctness of the traditional classification of text-types, and the superiority among them of the Alexandrian tradition. It is not surprising therefore that some critics find the *Majority text* approach to the nature of text-types a cause of some chagrin. This is shown by their reviews of *The Greek New Testament according to the Majority text* by Zane Hodges and Arthur Farstad (1982). A reasoned eclectic approach

disapproves of an edition of the Greek New Testament which offers a simple choice between the Byzantine and Alexandrian traditions. Thus Fee (1983) says that the *Majority Text* "reflects highly selective and sometimes misleading use of historical data" (p. 113). Birdsall (1992) is no more happy either:

The Codex Bezae is excluded from the apparatus on the grounds of its highly idiosyncratic text. . . . The evidence on which WESTCOTT and HORT based their analysis and reconstruction is therefore very gravely obscured, and their understanding of the 'Majority Text' misrepresented. (pp. 165-166)

On the assumption that critics will continue to describe the Text in terms of text-types, recensions, and family "clusters of manuscripts", it is to be expected that *Majority text* advocates will hold to their commitment to the *Majority text* in the belief that it is only a matter of time before a papyrus from the second century appears which is shown to contain everything truly characteristic of the Byzantine text. Meanwhile *Majority text* advocates are aware that the the "rational" critical method of making textual decisions puts in serious question the relevance of text-types. They may feel this puts them in a strong position to offer a method of choosing between variants which does not depend on the superiority of the text-type from which they are supposedly derived. {

#### *The Special Characteristics of the Byzantine Text*

The critic's answer to the question: "Is the *Majority text* approach valid?" depends to a great extent on whether he believes there is cogency in these criticisms of the way Hort described Byzantine peculiarities. A *Majority text* advocate believes that Hort's analysis is faulty. The criticisms, express or implied, of Hort's description of Byzantine

peculiarities by Van Bruggen, Hodges, and Pickering were set out in chapter 2 of this study. They offer evidence that "conflate" readings are not exclusive to the text-type, and many of the preferred shorter readings can equally well be seen in another light as reduction-readings. It was noted that Kilpatrick questioned harmonisation as a Byzantine peculiarity because he saw this tendency at work in other text-types. Van Bruggen believed other explanations are equally available which do not reflect badly on the Byzantine text. Hort (1881, p. 135) himself admitted that the evidence of harmonisation and assimilating interpolations in the Byzantine text are "fortunately capricious and incomplete." As to the *Majority text* being a fuller text, Van Bruggen emphasises that the Byzantine text shares this reputation with the Western text, compared to which it is at many points shorter. As to roughness and difficult expressions which are supposedly smoothed out of the text, Kilpatrick shows this resulted not from fourth century revision, but has its source in a second century practice of eliminating semitisms, improving poor Greek, and "Atticising" here and there. He also believed the Semitic expressions of the Byzantine text with its smoother Greek style were in many cases part of the original text. Van Bruggen thus believes the Byzantine text is better understood as linguistic restoration rather than as a result of editorial freedom. Kilpatrick's studies show that one cannot speak of a typical secondary character of the Byzantine text as far as the language is concerned. Arguing from these criticisms, *Majority text* advocates assert that the characteristics of the *Majority text* are part of the Original. The *Majority text* approach supports a critic's choice of Byzantine readings as having a non-secondary character.

Closely linked with the foregoing is the question as to how the *Majority text* emerged. Holmes (1995, p. 351) says: "It appears that we have quietly ignored Westcott and Hort's untenable view of the origins of the Byzantine textual tradition." Reasoned eclectics agree that the Byzantine text-type is secondary, inferior, and recensional. Fee believes from his papyri studies that Hort's use of internal evidence to exalt the Alexandrian tradition was entirely justified. It is thus logical for him to accept Hort's analysis of the Byzantine text. Metzger and Fee still affirm that the text-type resulted from a deliberate revision by Lucian or his associates. Kenyon suggested that it was the result of a gradual and deliberate process over time. Fee agrees that the Byzantine text gradually evolved, and suggests deliberate editing in three stages. This editorial event or process is still accepted generally by textual critics, including the radical eclectic. However, a strength in Pickering's approach is his attempt to get to grips with the difficulties involved in substituting the process view for one involving official editorial recension. He (1977) says:

There is a more basic problem with the process view. Hort saw clearly, and correctly, that the Majority Text must have a common archetype. . . . Hort's genealogical method was based on community of *error*. On the hypothesis that the Majority Text is a late and inferior text form, the large mass of common readings which distinguish it from the so-called "Western" or "Alexandrian text-types" must be *errors* (which was precisely Hort's contention) and such an agreement in error would have to have a common source. The process view fails completely to account for such an agreement in error. (p. 113)

If Hort did isolate Byzantine characteristics on the basis of "community of error," and if he was right that the *Majority text* must have a common archetype, it seems easier to believe, in the absence of any independent historical evidence for its occurrence, that the common source was the

autographs, rather than a complex editing process. However, Hort's assumptions themselves might be wrong. It is well-known that he failed to actually use the genealogical method--his description of the Byzantine text was an inference drawn instead from the use of internal canons, and based on intuition. Hort never used genealogy to prove "community of error" because the task was too gigantic to be feasible. Therefore it is fruitless to assume, in the absence of proof, that the text-type descended from a single archetype. The lack of empirical evidence to back up claims as to how the *Majority text* came about is a weakness which is shared by all promoters of any one text-type over another.

*The Evidence from Versions and Lectionaries*

The reasoned eclectic, Wallace (1995, pp. 312-313) for example, emphasises that no versions before the fourth century use the Byzantine text. He tends to see this as very damaging to the *Majority text* position. This adds to the weight of the ostensible silence of patristic writings and papyri, during the first four centuries, as a witness to this text. Thus the Syriac Peshitta (Vulgate) is seen as having been conformed to the Byzantine text during the fifth century, whereas the Curetonian and Sinaiticus are placed in the third century, and contain many Alexandrian readings. In the Coptic tradition both the Bohairic and Sahidic versions are understood to be almost equally supportive of the Alexandrian type. The majority of manuscripts in the Old Latin tradition present mixed texts, with much variety. These manuscripts differ widely among themselves, so that Old Latin evidence is often on both sides of a doubtful reading (Kenyon, 1949, p. 141). The evidence for the presence of the Byzantine text in the Old Latin tradition is thus unclear. The value of the Armenian version is its strong affinity to the Old Latin in some places. The first presence of the

Byzantine text in the Versions is seen in the Gothic version of the fourth century.

Probably little if any of the above analysis is questioned by the radical eclectic critic. Yet by preferring free choice of readings--by ignoring the making of a choice between variants according to which text-tradition is preferred--he in effect denies the relevance of these analyses for textual criticism. The *Majority text* advocate does not ignore the relevance of the Versions for textual criticism. He includes their witness in assessing all the evidence, under Burgon's heading of number and variety. He looks for the widest representation geographically, and linguistically, as the necessary evidence for lack of collusion between manuscripts for any particular reading. The *Majority text* advocate is reluctant to accept the assertion that no versions before the fourth century use the Byzantine text. He believes, for example, that the date of the Peshitta depends on whether the Byzantine text existed pre-Nicaea. If the results of future research show that it reaches back to the original Text, then the earlier date of the second century may after all be accurate; correspondingly, the Curetonian and Sinaiticus versions would then be seen as later corrupt revisions of the Syriac Vulgate.

The lectionaries reflect the history of the text in the Middle Ages when the Byzantine text predominated. The prevailing view sees them as having received later assimilation from the ecclesiastical text. The earliest lectionary evidence is very uncertain but their origins reach back to the second century (Osburn, 1995, p.63). The *Majority text* advocate asks that assimilation to the Byzantine text be demonstrated rather than assumed, as in the case of patristic evidence.

*The Patristic Use of the Byzantine Text*

Reasoned eclectics believe that the patristic use of the Byzantine text is a key argument for the inferiority of the *Majority text*, inasmuch as it appears to be absent from writings of the Church Fathers before the fourth century (Hort, 1881, pp. 114-115). Fee (1993, p. 202) is convinced from his own patristic studies, and that of others, that the combined characteristics of the Byzantine text do not occur in the early Fathers.

*Majority text* advocates believe the text *is* clearly present in pre-Nicene Fathers. Pickering (1977, pp. 72-75) sees this as a fact which eclectics have cloaked by an unsound assumption, namely, that the presence of traditional readings is best explained as assimilation to the text by later scribes in the interests of uniformity with the developing ecclesiastical text.



In advocating the Majority view, Pickering (1997, pp. 64-77) appeals to 19th century patristic studies by Miller and Burgon to test Hort's view that that no ante-Nicene father used the Byzantine text. Miller's studies found that the ante-Nicene fathers quoted the Traditional Text (*Majority text*) against other variations with a ratio of at least 3:2 in its favour. If editorial assimilation to the later text should first be demonstrated, and not merely assumed (Pickering, 1977, pp. 72-75), then the patristic writings in the earliest centuries of manuscript transmission may be evidence for the presence of the Byzantine text before the fourth century. The first appearance of the *Majority text* is said to be in the homilies of Chrysostom. However the evidence available is quite scarce. The relevant Greek fathers mentioned by Hort are Irenaeus and Hippolytus in the West, and Clement and Origen in the East. This limitation means we are quite unaware of what the text of Antioch looked like in this period. Where are the contemporaries of Origen or Tertullian in Antioch to show us the textual colour of their New Testament? (Van Bruggen, 1976, pp. 22-23).

We have, at present, no way of knowing whether Western fathers and Alexandrian writers lived at a time and in a region where the textual tradition was at its best, or alternatively whether it was a tradition disturbed by all kinds of influences during the second century. The issues are not easily decided where difficult data seem, not infrequently, to be interpreted in various ways. If we assume on the basis of a few Fathers in three or four regions that the Byzantine text was unknown before AD 325, we encounter an even greater difficulty than the ambiguity of the evidence, and that is its sudden appearance in the scene. *Majority text* advocates believe that because fourth century writers used the Byzantine text as a normal text, they did not regard it as "new," but handed on from a previous age. Historical silence about Antioch's textual history before the fourth century should make us willing to restrict ourselves to the data we possess, rather than reaching speculative conclusions.

As a criticism of the Majority approach, Fee (1993, p. 202, 206, n. 38) emphasises that its advocates are insensitive to the need for improving the methods used in collating New Testament references in the writings of the Fathers. Studies are now more exacting, with the use of the quantitative method. Fee objects to Miller's quotations of patristic New Testament references as they falsely assume the general trustworthiness of their transmission, and they lack the required critical acumen. Hodges and Pickering have not based their appeal to rehabilitate the *Majority text* on first-hand study of patristic evidence, but they depend on studies which are seriously flawed by inattention to detail.

However, the strength of the *Majority text* approach is that it invites critics in a time of great uncertainty to acknowledge the validity of Burgon's "Traditional Text" instead of clinging to the dogmatism of an earlier period. K. W. Clark denied to modern critical texts any authoritative status:

The textual history that the Westcott-Hort text represents is no longer tenable in the light of newer discoveries and fuller textual analyses. In the effort to construct a congruent history, our failure suggests that we have lost the way, that we have reached a dead end, and that only a new and different insight will enable us to break through. (p. 124)

Pickering (1977, p. 110), however, appears to suggest that critics abandon completely the idea of the Alexandrian and "Western" texts as recensions, in favour of the authenticity of the *Majority text*. This "new and different insight" was very far from what Clark had in mind, for it seems to run counter to the consensus which the latter expresses: "We are all aware that several distinctive recensions circulated in the early church . . . but we are yet unable to trace a course of transmission among them" (p. 124).

Pickering does not make clear here whether he is denying the notion of recensions completely, or simply unwilling to acknowledge the existence of text-types. Burgon fully acknowledged the first idea, but not the second. For example, after studying and comparing readings between the early Alexandrian Fathers and the Egyptian versions, Burgon (1896a, pp. 148-152) asserted: "No manuscripts can be adduced as Alexandrian" (p. 150). The peculiarities of  $\alpha B$  he attributed to Origen (pp. 159-165). Concerning the characteristics of Cod. D he said:

We are presented with an alarming concurrence of a fabricated archetype and either a blundering scribe, or a course of blundering scribes. . . . D exhibits the highly corrupt text found in some of the Old Latin manuscripts, and may be taken as a survival from the second century. (pp. 188-189)

The fact that no attempt is made to distinguish between the idea of a recension, and that of a family of manuscripts sharing common characteristics, suggests a weakness in the contemporary Majority approach. It assumes that it is not necessary to interact with all the evidence, and in detail; the task of textual criticism is thus over-simplified. Although lip-service is paid to the need to reconstruct the history of the text (Pickering 1977, pp. 91-92), the practice of textual criticism from the Majority viewpoint is so simplified as, in effect, to obviate the need to study and compare textual phenomena in manuscripts, versions, early writers, and lectionaries. Burgon's writings clearly show that he believed that a thorough study of all the textual phenomena, from all sources, was necessary to establish the correct text of the New Testament.

### *Issues of Internal Evidence*

Two other issues relate to the use of internal evidence, namely, the need for confidence in the reliability of internal canons. The disunity among *Majority text* advocates as to the nature and importance of internal evidence is a serious weakness. Wallace (1989a, pp. 277-279) explains that the "shorter reading" rule is appealed to incorrectly on both sides of the debate:

The impression one gets, though never explicitly stated, is that the critical text will rarely if ever have a longer reading than the majority text, and the majority text will rarely if ever have a shorter one. . . . In this writer's count there are 657 places where the *Majority Text* is shorter than the critical. (p. 278)

Thus the reasoned eclectic may severely down-play the acknowledged fact that the *Majority text* is sometimes shorter than the critical text to uphold the "shorter reading" rule, whilst the *Majority text* advocate will avoid suggesting that the shorter reading is at times to be preferred or that the Byzantine text-type contains shorter readings--in his unwillingness to allow any validity to *brevior lectio*. Holmes (1995) frankly sums up the implications of several recent studies on this question:

The cumulative effect of many of these studies has been to weaken or require extensive modification of several of the traditional criteria. In the light of Royse's study the venerable canon of *lectio brevior potior* is now seen as relatively useless, at least for the early papyri. . . . The primary effect of recent discussions . . . has been to increase our scepticism. We are less sure than ever that their use, no matter how sophisticated, will produce any certainty with regard to the results obtained. (p. 343)

In the process of evaluation, it is impossible not to employ some form of reasoning from within the text itself; some guidelines are therefore necessary. The eclectic critic uses the well-known maxims which determine the issues of intrinsic and transcriptional probability. However, by these rules Hort exalted the Alexandrian text; *Majority text* advocates are unconvinced that Hort's critical intuition was sound. However, the *Majority text* advocate uses his own internal guidelines to decide which reading within the Majority tradition is the correct one. It seems to be equally difficult for critics, whichever method they use, to prove that their method of reasoning is objectively verifiable.

A second issue, linked with the ambivalent attitude of the *Majority text* advocate to internal evidence, is the desire for certainty, in contrast to the acceptance and use of conjectural emendation. Pickering (1997) quotes R. M. Grant to explain his predicament: "It is generally recognized that the

original text of the Bible cannot be recovered" (p.19). He shows that this admission is not easily coped with by textual critics, and so tends to be down-played:

Bruce Metzger says, "It is understandable that in some cases different scholars will come to different evaluations of the significance of the evidence." A cursory view of the writings of textual scholars suggests that Metzger's "in some cases" is decidedly an understatement. Further, it is evident that the maxims cannot be applied with certainty. No one living today knows or can know what actually happened. It follows that so long as the textual materials are handled in this way *we will never be sure* about the precise wording of the Greek text. (p. 18)

The strength of this approach is the conviction that lies behind it that, because of the unique nature and purpose of the Bible, the original text can indeed be recovered. This gives new hope and energy to the enterprise. The weakness of the approach is the conviction that because it ought to be possible to arrive at the correct reading in every case, therefore it is possible. This leads to an over-dependence on external evidence, and a false belief that because the usual internal canons have been dispensed with, all "subjective," that is, internal reasoning is at an end. The struggles experienced by *Majority text* advocates in deciding between variants within the Majority tradition shows that this is not so.

Wallace (1995) sums up the view of most textual critics, who believe it is necessary both to accept uncertainty, regardless of what methodology is used, and to reject the relevance of any theological principle in reaching certainty as to the original text:

One looks for a *probable* reconstruction on the basis of available evidence -- both external and internal. There is always a degree of doubt, an element of subjectivity. But this factor does not give one the right to replace the probable with the merely possible. Any approach that does so is operating within the constraints of an *a priori*. (p. 315)

In summary, current research indicates that the traditional description of text-types may be faulty, considering the absence of clear alignment with one text-type rather than another in any one manuscript among the earliest papyri. Likewise the evidence from patristic writings before the fourth century, as presented by Miller, needs reevaluating against more recent critical texts of the Fathers, and by a more exacting standard. Meanwhile the advocates of the *Majority text* feel justified in believing that an unreasonable bias has been at work against the possibility that the Byzantine text is a primary witness within the Greek manuscript tradition. They believe there is no known evidence to suggest that it gradually evolved as a deliberately edited official text. Moreover what evidence there is suggests that its distinctive characteristics may reach back to the early second century, indeed to the original Text itself. Perhaps it is necessary for the textual critic to take the *Majority text* more seriously than it was earlier thought. As far as the influence of internal reasoning is concerned, it should be understood that all critics, whatever their method, examine variant readings to some extent in the light of the intrinsic and transcriptional probabilities inherent to the context in which they appear. Whether they choose to use Hort's nomenclature is not directly relevant to the issue.

### *The Results of the Majority Text Debate*

The discussions over the *Majority text* have raised serious questions which relate to long-established views for example, concerning the history of the text, the value of geographical groupings and the nature of external evidence.

This study has defined and elaborated the historical causes and text-critical reasons for the debate between Hodges and Fee, among others. The exchange of views over one quarter of a century has been useful in two ways: (1) It has highlighted some of the real questions which constitute the underlying causes behind the conflict in methodology among those practising an eclectic approach to the Text, and (2) it has served as a catalyst to bring about publication of *The Greek New Testament according to the Majority Text* by Zane Hodges & Arthur Farstad (1982).

Apropos to the first point, Holmes says:

It must be stated that the Majority text advocates have highlighted some of the real questions and issues facing contemporary New Testament criticism. Their criticisms serve as a salutary reminder of the provisional character of current critical texts. . . . To treat what is printed in these editions as if it were the original is to commit the ironic mistake of substituting a 'new TR' for an old one. ( pp. 18-19)

D. A. Carson (1979) believes Pickering has helpfully drawn attention to some of the most significant studies which highlight the theoretical reasons behind conflicting methodology. His cautious, qualified approval suggests the seminal nature of the work:

The tragedy of Pickering's work, I believe, is that his important and pertinent questions will tend to be overlooked and dismissed by scholars of textual criticism, who will find many reasons to reject his reconstruction and therefore his questions, while many conservative Christians will accept his entire reconstruction without detecting the many underlying questions that will still go unanswered. (p. 108)

D. A. Carson predicted correctly that scholarly response to Pickering's work would probably be a negative one. What Carson had in mind is well illustrated by J. N. Birdsall's evaluation of Pickering's *modus operandi*: "A large part of the 'argument' consists in the quotation of excerpted words of



scholars from HORT to the present writer . . . . Not infrequently the excerptation is such that the original sense is obscured or changed" (p. 165). Pickering's sceptical approach exposed the inherent contradictions and conflicts at work in current text-critical method. Whether he intended to do so or not, such an approach is hardly congenial to scholars preferring to sail in calmer waters. Pickering was aware that he ran the risk of being misunderstood, thus inviting an *ad hominem* reaction; so he offered this disclaimer:

I have not knowingly misrepresented [these scholars]. . . . I take it that Colwell does reject Hort's notion of genealogy, that Aland does reject Hort's notion of recensions, that Zuntz does reject Hort's notion of "Syrian" conflation, and so on. However, I do not mean to imply . . . that any of these scholars would entirely agree with my statement of the situation at any point, and they certainly do not agree (as far as I know) with my total position. (p. 169)

Another outcome of the exchange of views between Hodges, Pickering, and Fee is that it has acted as a catalyst to bring about publication of *The Greek New Testament according to the Majority text* by Zane Hodges and Arthur Farstad (1982). This edition provides a convenient means by which critics can test Hort's view of the Byzantine text--does it have the peculiar characteristics he gives to it? Wallace (1989a) says:

Previous judgments about the character of the Byzantine text-type can now easily be examined. The *Majority text* has facilitated testing of the hypothesis that this text-type is a fuller, smoother, and more conflated text than the Alexandrian text-type or the text of the modern critical editions. (p. 275)

After setting out its main virtues and defects, Wallace (1983) concludes: "I strongly recommend the Majority Text for every student of the Greek NT,

regardless of his text-critical views. "The negative elements of the work all seem capable of correction" (p. 124).

The publication of the *Majority text* is evaluated in many book reviews for its usefulness to textual critics in the practice of textual criticism. Wallace (1989b) points out that because of its polemical thrust, most reviewers failed to notice that "this volume is *primarily* an edition of the NT - not an argument for the Byzantine text-type per se" (p. 609). Fee (1983, p. 107) and Wallace (1989b, pp. 613-618) suggest that the critical apparatus of the *Majority Text* could usefully serve as a more definitive collating base than the *Received text*, although its textual apparatus is quite minimal, and all of the manuscript data is drawn from other editions of the Greek New Testament. In suggesting this, Wallace reinforces B. H. Streeter's recommendation that textual work should be done with a critical apparatus based on the Byzantine text itself, rather than with some eclectic version of it. Wallace offers five significant reasons why the *Majority text* should be used: (1) Even where the editors have failed to put the Byzantine archetype in the text, this paradoxically is to the advantage of a collating process, for the most inferior text is the best baseline. Though the text was based on Von Soden's analyses, with all its inaccuracies, this is still a marked improvement on Erasmus' dependence on a few late cursives; (2) the *Majority text* makes for a better benchmark than the *Received text* by which to discover textual affinities. This is because, according to Wallace, some of the readings of the *Received text* are Caesarean rather than Byzantine; (3) if, as has been generally held, the Byzantine text-type was the latest to develop, it follows there is a greater antecedent probability that it is furthest from the original. Such a text provides a better base-line for defining diverging forms of the text; (4) it is the most economical procedure, where collations are listed from the baseline. As the Byzantine

manuscripts amount to 80-90% of all Greek manuscripts, "[such] a text which best represents the majority of extant witnesses will naturally reduce the size of the apparatus without sacrificing *any* of the data" (p. 612); and (5) the Byzantine text-type is now accessible in a easily quantified printed edition, compared with Von Soden's unwieldy and over-subtle classification of manuscripts. In order to overcome the problem of employing previously collated manuscripts against the *Received text*, using the *Majority text* should include the *Received text* as part of the collation, to preserve continuity with previous studies.

#### *A Way Forward*

Dialogue about the *Majority text* has raised serious questions which relate to long-established views for example, concerning the history of the text, the value of geographical groupings and the nature of external evidence.

The way forward inevitably focuses on the need for a clearer understanding of the true history of the text. Holmes (1995) says:

[This is] unfortunately, one of the major lacunae of NT textual research during the period [that is, since 1946] under review. While scholars have largely abandoned key components of the once-dominant views of Westcott and Hort, they have given relatively little attention to developing an alternative history of the text as a replacement. (p. 351)

It is hoped that this will provide an objective basis for making sound textual decisions. Holmes lists important studies done by Epp, Parker, K. Aland, B. Aland, and Fee that "illuminate specific areas or problems and that contribute important data that will eventually enable the larger picture of the whole to be studied" (p. 352).

Three important questions, discussed in chapter 2, should be considered further: (1) How did the Byzantine text originate, and what are its true characteristics? (2) Should the early papyri be described in terms of clearly defined text-types, or should they be treated as individuals, in the practice of textual criticism? (3) What place, if any, do the ante-Nicene fathers give to the *Majority text*?

As to the third question above, if patristic studies are pursued with sufficiently exact methods, this will provide the needed data which will enable the critic to place the Text in the context of fixed times and places. Fee (1992) agrees:

The judicious use of patristic evidence, based on presentations and analyses that are sensitive to the degrees of certainty, will aid in the task of using this evidence more confidently in our attempt to write the history of the NT text. (p. 204)

It has been shown in earlier chapters of this thesis that Burgon's reconstruction of the history of the text is very different from that of Hort. Burgon's unsuccessful attempt to champion the Byzantine text has resulted in subsequent critics largely ignoring his writings. If a consensus emerged which accepted that Westcott and Hort's views of the origins of the Byzantine text are no longer tenable, this would hopefully encourage scholars to study Burgon's work closely, and evaluate his attempt to reconstruct the history of the text. This in its turn could lead to a serious re-evaluation of the value of the Alexandrian tradition, in which Origen is the central figure, whether for better or for worse.

It is recognised that the process of quantitative analysis also provides a potentially effective vehicle by which to trace the relationships between manuscripts and so discover the nature and history of their relationships. Without a clearer understanding of the true history of the

text, all reasoning from internal evidence is vitiated by the conjectural element in attempting to explain how competing variants arose.

## CHAPTER 6

## Review

The would-be critic of the New Testament text faces a serious question at the outset of his quest: What trust should he place in the theoretical foundations which undergird the discipline of New Testament textual criticism? This question arises out of the fact that scholars place varying estimates on attempts to reconstruct the true history of the text. This has created a climate of uncertainty, where varying methods are being used to arrive at differing textual conclusions.

The purpose of this study has been to define, describe and test what has become known as "the Majority text" method. By analysing its theoretical basis, and its method of arriving at textual decisions, the aim has been to evaluate its usefulness, if any, to textual critics.

*Background to the Study*

Chapter 1 traced the successive stages of New Testament textual criticism in order to show how modern methods of analysing and deciding between significant variants developed. In this process four distinct historical stages were discerned, as New Testament textual criticism grew and developed: At first, literary concerns were subordinated to theological—up to and including Erasmus' first Greek New Testament. Then, a vast amount of materials was assembled for critical study and the classification of manuscripts into families proceeded along geographical lines. A third stage introduced a revised Greek text independent of the *Received text*, for example, Tischendorf's 1872 edition with complete apparatus. Finally, on

the assumption that the development of a standard Greek text was both desirable and achievable, a revised English Bible was produced. The changes introduced by this fourth stage were based, more or less, on Westcott and Hort's revised Greek New Testament.

The fourth stage began with the emphasis on the historical-documentary method characteristic of Tischendorf, Hort and Von Soden. It was reinforced later by the discovery of the Caesarean text-type. From the English-speaking viewpoint, the work of Westcott and Hort was a watershed, in as much as the results of their work were made generally available with the changes incorporated into the English Revised Version. Subsequent scholarship challenged Hort's championing of  $\alpha B$ ; this led the way to an alternative textual method, when A. C. Clark argued for the superiority of the Western text on the basis that the internal canon *brevior lectio potior* was faulty. The battle over text-types led to a sceptical attitude by some scholars towards external evidence as a basis for textual decisions. Nevertheless a parallel force has developed through study of the early papyri, which has strengthened confidence in the traditional threefold classification of text-types. These studies claim to locate the characteristic readings of each text-type firmly in the second and third century, though all mixed together. Three distinct textual methods have surfaced over the last 150 years, known as the historical-documentary method (Hort), the reasoned eclectic method (B. Metzger, G. Fee, Eldon E. Epp), and the radical eclectic method (Kilpatrick, J. K. Elliott), although the first method seems to be merely a theoretical position--in practice it is merged into the second method. In consequence, no real agreement seems to exist that (1) Hort's preference for  $\alpha B$  is after all correct, or that (2) the Western and Caesarean texts are correctly described.

The process of translating the revision of the Greek text into English was not without controversy. On the one hand Scrivener dissented from Hort's textual conclusions, namely, his preference for "neutral" B readings, yet he was repeatedly overruled as a result of Hort's persuasiveness. On the other hand, the publication of the English Revised Version of 1881 sparked a controversy between certain scholars, involving particularly J. W. Burgon.

Further opposition to Hort's textual reconstruction came from Vaganay and Colwell on the basis that the genealogical principle could not be used to prove the antecedents of any text-type before 200 AD. However, Zuntz claimed to have done just that--to have discovered a prototype of Hort's Alexandrian text in the reservoir of the second century. Zuntz' work, and further studies of recently discovered papyri by Aland, Klijn, and Birdsall, all confirmed that Hort's views needed serious revision. For many, Hort's "neutral" text is no longer convincing, the Caesarean text has all but disintegrated, and the "Western" lacks homogeneity.

Ongoing scepticism as to the comparative worth of text-types has been accompanied by what Epp calls "the crisis of the criteria." Doubt exists as to which internal canons are to be relied on. This doubt surfaces when the use of internal canons conflicts with textual choices made on an external basis, for example, the "oldest and best manuscripts." The reasoned eclectic position arose out of general dissatisfaction with any one text-type, in preference for a "pick-and-choose" method between them all. Emerging from this battle were the few radical eclectics who, in showing distrust towards any one text-type over above another, feel free to choose variants regardless of their text-typical association. In consequence, scholars like Kilpatrick and Elliott rely solely on internal rules. Others, more in the majority, put their faith in one or other text-type, or all of them



together where they agree on a variant. They look to documentary evidence in one of two ways. Either it will confirm a decision already made on the basis of internal canons, or it will guide them as to which choice to make, where it is difficult to apply internal rules consistently. For example, choosing an Alexandrian reading (which is usually shorter) may force the critic to abandon the "harder reading" rule, if the harder reading also happens to be the longer variant.

The current uncertainty among New Testament textual critics over methodology was anticipated by Burgon, who believed Hort's evaluation of internal textual phenomena was unsound, just as his theory of the text lacked the required independent historical witness in its support. This assessment of Hort's theory and practice inevitably resulted from a conviction that any method which dismissed 19/20ths of the evidence is patently suspect, where it is based on intuitive preferences for readings which had "the ring of genuineness." He believed that closer study of all the evidence would eventually prove Hort's judgements on the *Majority text* were incorrect, and that the day would come when the Byzantine tradition will no longer be dismissed as late, edited and recensional, and the *Majority text* would be reinstated.

Hort's view on text-types became sufficiently well-established for critics to consider the status of the Byzantine text to be a closed matter by the 20th century inter-war period. However after a silence of 30-40 years, the earlier debate was effectively revived by Zane Hodges and Gordon D. Fee. W. Pickering's work provided more substance to the Majority view, though it was severely criticised by Fee. The relevance of this discussion is shown by the increasing disparity between the views of critics. This disparity stems from the perplexing nature of the early papyri, and the

difficulties found in trying to evaluate the results of papyri studies in relation to hitherto well-established views on text-types.

The focus of the *Majority text* debate is on the status of the Byzantine text-type, that is, if it can be shown to vie with the other text-types as having origins which reach back to the first or second century, then the way is opened to consider the nature and value of Burgon's text-critical method as a viable and alternative method to reasoned or radical eclectic criticism.

### *The Theoretical Issues*

Chapter 2 set out the arguments on both sides of the debate between Hodges/Pickering and Fee/Wallace for and against the *Majority text*. These arguments have two foci: (1) the nature and value of the Byzantine text-type when choosing between variants, and (2) the cogency and relevance of internal canons in trying to decide (a) whether one text-type is superior to another, and also (b) which variant is to be preferred above another, when choosing between several options.

The argument focuses, firstly, on Hort's analysis of the Byzantine text. As to the status of text-types in general, the familiar groupings remain axiomatic for many, if only to greatly reduce the work of sifting a multiplicity of manuscripts. However the significance of such groupings remains contested. The use of Byzantine readings by radical eclectics has made space for further study and discussion as to the value of the *Majority text*. The characteristics of the Byzantine text were outlined. The criticisms of Van Bruggen and others were noted; if these criticisms are valid, they indicate that the characteristics of the Byzantine text are not peculiar to it after all, in comparison with other text-types.

Hort's appeal to the genealogical principle is seen as inapplicable to the peculiar nature of the New Testament data and therefore ineffectual. Reasons were given to show that the genealogical argument should be seen as irrelevant to the status of the Byzantine text.

What is the earliest evidence for the *Majority text*? Studies of the second and third century papyri have described the relationship between them and later manuscripts in a way which is controverted, as between, say, Colwell and Aland on the one hand, and Fee and Epp on the other. The first-mentioned believed the "mixture" to be found in the papyri indicates the absence of text-types in the second century. The latter believe that the major text-types are there in prototype, excepting the Byzantine. In response, Pickering proposes that scholars abstain from allotting papyri to text-types until relationships between the later manuscripts have been empirically plotted. Studies in ante-Nicene patristic sources were also noted. Hort's view that the *Majority text* does not occur in the early Fathers is supported by Fee, based on studies of Origen and others. Miller's studies tried to prove that the "Traditional Text" goes back to the second century, but these have not yet been fully tested in the light of the more exact critical methods. Majority text advocates believe that further studies may yet show that the *Majority text* is present in the ante-Nicene fathers. They believe that what evidence there is suggests that its distinctive characteristics may reach back to the early second century.

The second focal point in the debate, for and against the *Majority text*, relates to the nature of internal evidence. Are the familiar internal canons still cogent and relevant? The use of internal rules has a double importance, for the way they are used tends to decide whether one text-type is seen as superior to another, or which variant is to be preferred above another, when choosing between several options. Hort's preference

for the Alexandrian text was based *inter alia* on the "shorter reading" rule. The discussion in chapter 2 showed that the longer reading may be validly promoted as a rule to guide the critic, for example, where the scribe was doctrinally motivated to omit, or was repeatedly careless in his work. If it were to be generally admitted that the shorter text is not usually to be preferred, this would invite a reexamination of the origin of the Byzantine text. The question is whether the Byzantine text has been filled out in the interests of clarity and completeness, or alternatively whether the Alexandrian text has been condensed in the interests of literary style.

In describing the relevance of internal canons, modern attitudes to conjectural emendation were examined. It was suggested that the increasing tendency to accept conjectural solutions is bolstered by a sceptical epistemology, which seems to deny the possibility and necessity for certainty in textual conclusions. The *Majority text* approach may itself not produce the required certainty any more than any other text-method; however it is possibly more appealing in its approach to the task, in that it attempts to be consistent with the belief that practical certainty is required of an assertion, fact or idea before it may be considered to be objectively true.

Chapter 2 defended the view that the characteristics of the Byzantine text may reach back to the second century, and therefore possibly to the original Text itself. Future work done on both papyri and patristic material may yet seriously call in question whether the Byzantine text is late, inferior, and recensional. As to internal criteria, the increasing scepticism shown by some critics towards these internal canons is noted as a possible sign that they may have been used somewhat arbitrarily, to justify a prejudice against the Byzantine text. Even though conjectural emendation may be becoming more accepted, it does not sit easily with the

need and desire for certainty in drawing conclusions from the text. These facts make space for a consideration of Burgon's text-critical method, as a viable alternative to other better-known approaches.

### *Understanding the Majority Text Method*

The third chapter examined the claim of *Majority text* advocates, that if the history of textual transmission were correctly interpreted, the statistical predominance of one text-type would be seen as evidence for its superior quality over other text-types. Burgon's text-critical method was described.

"Is there firm historical evidence to explain how one text-type could emerge as numerically dominant?" Two possible answers are: The *Majority text* may have predominated from the first, and remained in the majority thereafter, or the *Majority text* gained dominance by official church sanction as an ecclesiastical revision. If it achieved its position from the first, this suggests that it was copied more continuously and consistently than any other text-type from the beginning .

Many critics believe that recent evidence only strengthens the denial that the Byzantine text existed prior to the fourth century. The *Majority text* advocate, however, avers that the results of studies in more recent papyri and patristic sources can be interpreted in more than one way. Thus the data need to be evaluated by more exacting critical methods before coming to firm conclusions one way or the other. The *Majority text* advocate believes that it is highly unlikely that the *Majority text* won its position by official church sanction.

He believes that some independent trace of historical evidence would remain to substantiate a revision which had such a far-reaching influence on the shape of the Text. The modified view that it came about by a gradual and guided process over several centuries also needs independent historical evidence to substantiate it.

The second aim of chapter 3 was to describe Burgon's text-critical method. The nature and significance of Burgon's Seven Notes were examined. They are (1) age, (2) number, (3) variety, (4) continuity, (5) weight, (6) evidence of the entire passage, or context, and (7) internal considerations, or reasonableness. Thus the age of a manuscript is only one important factor to consider, and not the main basis of evaluation. The variant should be shown to have occurred early, and there should be continuity, that is, every period of church history should show its use; evidence for the use of the variant should be present throughout Christendom, whether in manuscripts, Fathers, versions or lectionaries; the manuscript witness should be "respectable", that is, generally reliable; the witness should be "credible," that is, its textual context not suspiciously confused with conflicting variants; the numerical preponderance of manuscripts witnessing to the variant should be as "full" as possible; finally, the reading claimed as original should be "logically possible," that is, not grammatically or scientifically unsound.

An explanation of the principles aimed to show that their strength is in their cooperation, that is, that no *one* note, for example, the numerical factor, is sufficient to establish the text. There must be other evidence for the variant; it must show its presence continuously from antiquity, and some evidence of its presence must come from several remote and independent sources.

### *Testing the Methodology*

K. Aland's response to Hodges' and Farstad's *The Greek New Testament according to the Majority text* was to invite those drawn to the Majority approach to take the opportunity of "forming an independent judgment of them as well as of the newly proclaimed return to the Textus Receptus" (1987, p. 292). Aland discussed 15 New Testament texts which are relegated to the critical apparatus of *UBS*<sup>4</sup>. In this study several of these texts were examined, and conclusions reached from the *Majority text* standpoint. Textual decisions were compared with those of Aland and Metzger, on the one hand, as well with those of Kilpatrick and Elliott, on the other. The six texts studied were Matt 17:21, Mark 9:44 and 46, Mark 11:26, Mark 15:28, John 5:3b-4, and Acts 8:37. These (with one exception) were selected because the five *UBS* editors unanimously agreed to marginalise the verses.

In arguing the case for the *Majority text*, 4 of the 6 texts were practically decided on the basis of number, variety, and continuity, the "threefold cord". The two remaining texts were short of evidence in either variety (Mark 9:46,48), or in number (Acts 8:37). However Burgon did not believe it was wise to leave the establishing of the text to these three tests alone. For thoroughness all seven notes must be used.

The results of a *Majority text* approach were compared with conclusions drawn from other methods. In each case the result came out markedly differently; this was not surprising given that the external evidence, by which to decide between competing readings, was evaluated differently. The inclusion of the numerical factor leads inevitably to a tendency to prefer Byzantine readings, over above Western ones, or those readings of the Alexandrian text preferred by the *UBS* editors. It is noteworthy that the textual conclusions drawn from 3 of the 6 texts discussed are shared by radical eclectics who do not reject Byzantine readings on principle, and work solely on the basis of internal evidence.

In each case it was found that Aland followed the Alexandrian reading, consistent with his fivefold classification of manuscripts. It is not consistent, however, with his statements elsewhere which show that he placed more reliance on simple papyri reading per se, and disclaimed any reliance on a "star" manuscript. In each of the Synoptic examples examined, the argument from harmonisation is used prominently by Aland and the *UBS* editors. This usage assumes that Hort's view as to the relative importance of the text-types is correct, and that the Byzantine editor has acted in the interests of producing a fuller text.

The battle between text-types is reflected in the discussion of Acts 8:37. W. A. Strange accepts the authenticity of the text, presumably because he is already predisposed to accept the authenticity of the longer "Western" text.

The disparity in text-critical method between reasoned and radical eclectic critics is highlighted in 3 of the 6 texts, namely, Mark 11:26, Mark 15:28, and Acts 8:37. Kilpatrick and Elliott have dissenting views to offer on these verses. These critics' freedom from devotion to  $\alpha\beta$  as oracular, explains their apparent willingness to acknowledge that Byzantine readings



reach back to the second century, and may be good readings. However, their freedom to ignore any particular text-type, as neither better nor worse than any other, carries with it the duty to explain why, for example, Acts 8:37 should be accepted or rejected, if not on documentary grounds. The discussion on John 5:3b-4 is possibly relevant to the fact that textual critics are increasingly aware that scribes made deliberate alterations in the text for theological or dogmatic reasons.

### *Evaluation and Way Forward*

In chapter 5 Burgon's textual method was evaluated in the light of the previous chapter. This led to the following conclusions: The numerical argument is overstated by *Majority text* advocates; current discussion does not take all the external evidence into account. The note of variety has force only in cooperation with number and continuity; it also ensures no conjectural emendation has occurred. For example, although Acts 8:37 is not numerically supported, the evidence from a wide geographic area over many centuries attests to its genuineness. The argument from continuity introduces a theological a priori, in support of the belief of the Biblical writers themselves that they were producing more than great literature. This approach is apparently unacceptable to most critics, yet philosophically justified. As to manuscript age, the belief that most variants arose by 200 AD changes the focus of the discussion from readings per se, to their ostensibly associated text-types. The internal canons have an uncertain value; therefore, manuscripts should be weighed in the light of the *Majority text*. The weight of a reading should depend on the condition of the text, that is, in its immediate context.

Because it is impossible not to employ some form of reasoning from within the text itself, some guidelines are necessary. However, whichever guidelines are chosen, it is difficult to prove that the method of reasoning is entirely objective.

The Majority approach believes that recent papyrus studies cast doubt on Hort's description of the Byzantine text. If this were acknowledged, it would then be seen that the presence of Byzantine readings in the second century also indicates the presence of the Byzantine text there. The numerical principle makes it necessary to consider absolutely all the external evidence; however, as currently practised, it tries to reach textual decisions by that means alone. Pickering's attempt to explain the Majority phenomenon has not convinced most textual critics, but it is understandable in the light of the fact that the genealogical argument is ineffectual in tracing sure developmental lines through manuscripts. If the Byzantine text were allowed a claim to originality, the numerical preponderance of the *Majority text* may be seen to be more significant. The relevance of text-types is doubtful because the ability to trace a history of the text is questioned by many critics. The current Majority approach proffers a method independent of any text-type, although such an extreme view seems ambiguous and simplistic. No acknowledgement is apparently made of the existence of recensions, as distinct from text-types; the task of textual criticism is thus over-simplified. Nevertheless, further studies may yet show that a second-century papyrus contains everything ostensibly characteristic of the Byzantine text. Kilpatrick showed that the language of this text is not typically secondary in character, and *Majority text* advocates believe that it reaches back to the Original. The use of versional evidence depends on agreement as to dating, for example, of the Syriac Peshitta. A *Majority text* advocate believes, for

example, that the results of future research may show that it reaches back to the second century. He believes that patristic evidence will eventually show that the *Majority text* is present in the ante-Nicene Fathers; accordingly, editorial assimilation to a later text should be demonstrated, rather than assumed. However, the appeal to rehabilitate the *Majority text* from patristic evidence is currently flawed by inattention to detail. Disunity among *Majority text* advocates as to how to weigh internal evidence is a serious weakness, but their rejection of conjectural emendation strengthens the conviction that the original Text is recoverable. Nevertheless, this belief is vitiated by an over-dependence on external evidence. Its efficacy is also weakened by the delusion that, because the usual internal canons have been dispensed with, all "subjective" reasoning is at an end.

The way forward focuses on the need for a clearer understanding of the history of the Text. The judicious use of patristic evidence will aid in this task. If Hort's description of the nature and origins of the Byzantine text becomes no longer tenable, this may encourage scholars to study Burgon's work more closely, and reach a consensus as to the real value of the Alexandrian tradition.

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