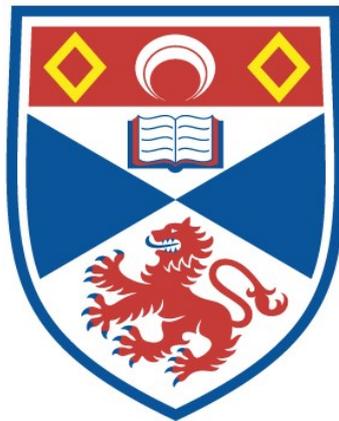


**"WESTERN NON-INTERPOLATIONS" AND RELATED  
PHENOMENA IN THE GOSPELS**

**Volume I**

**John Philipose**

**A Thesis Submitted for the Degree of PhD  
at the  
University of St Andrews**



**1975**

**Full metadata for this item is available in  
St Andrews Research Repository  
at:**

**<http://research-repository.st-andrews.ac.uk/>**

**Please use this identifier to cite or link to this item:**

**<http://hdl.handle.net/10023/11063>**

**This item is protected by original copyright**

'WESTERN NON-INTERPOLATIONS'

AND RELATED PHENOMENA

IN THE GOSPELS

by

JOHN PHILIPOSE

A thesis submitted for the degree of

Ph.D

in the Department of Divinity, St. Mary's College,

University of St. Andrews, Scotland, March 1975





## **IMAGING SERVICES NORTH**

Boston Spa, Wetherby  
West Yorkshire, LS23 7BQ  
[www.bl.uk](http://www.bl.uk)

**PAGE NUMBERING AS  
ORIGINAL**

'WESTERN NON-INTERPOLATIONS' AND RELATED PHENOMENA IN THE GOSPELS

by

John Philipose

A thesis submitted for the degree of Ph. D. in the Department of Divinity at the University of St. Andrews, Scotland, March 1975

ABSTRACT

This thesis is a critique of the theory of 'Western non-interpolations' propounded by Westcott and Hort, the famous British scholars in the Introduction to their critical edition of the Greek New Testament, 1881. The theory concerns eight crucis interpretum in the concluding section of Luke's Gospel and one in the last but one chapter of Matthew's Gospel. The eight Lukan passages, though missing from Codex Bezae (D) and its allies ('Western' family), are read by Codex Vaticanus (B) and Codex Sinaiticus (X) as well as by (P<sup>75</sup> and) the vast majority of the other ancient witnesses. (The pattern of the textual evidence for the Matthaean passage is rather different, although B and X are ranged against the omission shown by D). In these contexts Westcott-Hort decided to apply the principle of brevior lectio potior. This was indeed a radical departure from their low estimation of the 'Western' group in general vis-a-vis the B X (Alexandrian) group which they idealised as 'neutral' and as having a very high degree of purity. Having once rated the latter so high it was too awkward for our editors to admit that their favourite Text had gone corrupt at those crucial points. Hence they devised the face-saving term 'Western non-interpolations' to emphasize that while their characterisation of the 'Western' Text as containing numerous 'interpolations' still

held good, in the eight cases listed the 'Western' omissions were to be reckoned as real 'non-interpolations'.

This writer believes that although Westcott-Hort's intuition regarding the eight passages was correct (except in one instance), the premise from which they started needs to be revised. Omissions of NT texts including the above were caused by various factors. They are confined neither to one family, nor to a single Gospel, nor even to one particular area of a Gospel. Westcott-Hort's inclusion of one Matthaean passage in the above 'classic' (the designation is mine) category speaks for itself. Moreover, they also called attention to aighteen other 'Western' 'omissions' scattered across all the Gospels although they attached only a secondary degree of suspicion to the longer texts. I submit that it was our editors' creation of this subtle distinction between these two categories of readings, as also their exaggerated delineation of the 'purity' of the Alexandrian family as against the 'Western' that has given rise to various other theories regarding the editorial vicissitudes of the Third Gospel. In my first volume I examine the twenty-seven passages in question and conclude that out of the eighteen 'non-classic' ones, at least five in Mt represent genuine omissions. In volume II, seven other passages where Westcott-Hort detected the characteristic tendency of 'Western' witnesses, viz. that of 'expansion', are examined. Of these I recommend two for full reinstatement, two for partial reinstatement and a fifth one for full reinstatement, though not in its traditional position. If these findings have any credibility, then Westcott-Hort's depiction of the 'Western' Text was indeed a gross exaggeration as far as the Gospels are concerned.

The Methodology of this thesis consists in a judicious application of the 'eclectic' principle. Each passage is studied on its own merits under three main heads:

1. External Evidence (Inferences to be drawn from MS data).
2. Internal Evidence:
  - a) Transcriptional Probability (The possible attitude of a scribe in inserting or rejecting a passage as the case may be; also the probabilities of accident behind a reading).
  - b) Intrinsic Probability (Considerations such as vocabulary, style and theological propensity of an author; also his general handling of his sources. In fact the application of the principles of Source Criticism, Form Criticism and Redaction Criticism in deciding whether a reading suits its context or not).
3. Conclusion: It is emphasised that the External Evidence is to be carefully balanced against the Internal Evidence in establishing the authenticity or otherwise of a given text rather than reading one's pre-conceived notions into it. The contributions of outstanding commentators and textual critics over the span of about 90 years since Westcott-Hort's time have been critically examined in drawing the inferences of each section.

The concluding chapter which is a summation of the above inferences serves to highlight the fallacy of Westcott-Hort's pre-suppositions and of certain subsidiary theories and demonstrates why the theory of 'Western non-interpolations' should be discarded as a principle of Textual Criticism.

DECLARATION

I hereby declare that the following thesis is based on the results of research carried out by myself, that it is my own composition and that it has not previously been presented for a Higher Degree. The research was carried out at the University of St. Andrews under the direction of Principal Matthew Black

John Philipose

CERTIFICATE

I certify that Mr. John Philipose has fulfilled the conditions of the resolution of the University Court, No.1, and that he is qualified to submit this thesis in application for the degree of Doctor of Philosophy

Principal Matthew Black,  
St. Mary's College,  
University of St. Andrews.

C O N T E N T S

	<u>Page</u>
PREFACE	1
INTRODUCTION	4
Statement of the Problem	
Methodology	
CATEGORY I :	
THE CLASSIC 'WESTERN NON-INTERPOLATIONS' OF WESTCOTT-HORT	
A. The Last Supper (Lk 22:19b-20)	33
B. The Empty Tomb Story	
a. The Christological Title (Lk 24:3)	77
b. The Angelic Announcement (Lk 24:6a)	97
c. The Role of Peter (Lk 24:12)	122
C. The Resurrection Appearance to the Disciples	
a. The Dominical Greeting (Lk 24:36b)	152
b. The Visible Proof (Lk 24:40)	
D. The Ascension	
a. The <i>ἀνάληψις</i> (Lk 24:51)	186
b. The Worship of the Departing Lord (Lk 24:52)	
E. The Piercing of the Lord's Side (Mt 27:49b)	218
CATEGORY II :	
THE OTHER 'WESTERN NON-INTERPOLATIONS' OF WESTCOTT-HORT	
A. The Matthaean 'Non-interpolations'	
a. 'their trespasses' (6:15)	227
b. 'or what you shall drink...' (6:25)	233
c. '...by the prince of demons' (9:34)	239
d. 'Another parable...' (13:33)	254

	<u>Page</u>
e. 'And he who falls on this stone...' (21:44)	259
f. 'and of the plate' (23:26)	270
B. The Markan 'Non-interpolations'	
a. 'But new skins for new wine' (2:22c)	275
b. The Interrogators at Peraea (10:2)	287
c. 'saying the same words' (14:39)	299
C. The Lukan 'Non-interpolations'	
a. The Parable of the Connoisseurs of the Old Wine (5:39)	310
b. Jesus' Reply to Martha: The Most Excellent Choice (10:41f)	322
c. The Happy-go-lucky Way of Life (12:19)	336
d. The Rich Fool's Destiny (12:21)	345
e. 'he would have been awake...' (12:39)	354
f. The Penitence of Peter (22:62)	360
g. The Return from the Tomb (24:9)	369
D. The Johannine 'Non-interpolations'	
a. 'He who comes from above is above all...' (3:31f)	374
b. The Comment on the Relation between the Jews and the Samaritans (4:9b)	383
CATEGORY III :	
THE 'GREATER INTERPOLATIONS'	
a. The Face of the Sky (Mt 16:2b-3)	392
b. The Conclusion of Mark (Mk 16:9-20)	411
c. The Refusal of a Samaritan Village to Receive Jesus (Lk 9:54-56)	469

	<u>Page</u>
d. The Ministering Angel and the Bloody Sweat (Lk 22:43f)	491
e. Jesus' Prayer for Forgiveness from the Cross (Lk 23:34a)	516
f. The Angel and the Pool of Bethzatha (Jn 5:3b-4)	537
g. The Pericope of the Adulteress (Jn 7:53-8:11)	548
CONCLUSION	570
APPENDIX	
BIBLIOGRAPHY	591

ATR	Anglican Theological Review
BJRL	Bulletin of the John Ryland's Library
BZ	Biblische Zeitschrift
Bib	Biblica
CBQ	Catholic Biblical Quarterly
CSCO	Corpus Scriptorum Christianorum Orientalium
EncBib	Encyclopaedia Biblica
E.T.	English Translation
Exp	The Expositor
ExpT	Expository Times
GCS	Die griechischen christlichen Schriftsteller der ersten Jahrhunderte
GK	Geschichte des neutestamentlichen Kanons und der altkirchlichen Literatur (T. Zahn)
HDB	Hasting's Dictionary of the Bible
HTR	Harvard Theological Review
ICC	The International Critical Commentary
IDB	Interpreter's Dictionary of the Bible
JBL	Journal of Biblical Literature
JTS	Journal of Theological Studies
NEB	The New English Bible
NT	New Testament
NTS	New Testament Studies
NovT	Novum Testamentum

NovT Suppl	Supplement to Novum Testamentum
OT	Old Testament
PG	Patrologia Graece (ed. J.P. Migne)
PL	Patrologia Latina (ed. J.P. Migne)
PTR	Princeton Theological Review
ProcBrAc	Proceedings of the British Academy
RB	Revue Biblique
RHP	Revue de Histoire et de Philosophie
RSV	Revised Standard Version
RTP	Revue de Theologie et de Philosophie
RV	Revised Version
RecSR	Recherches de Science Religieuse
RevBén	Revue Bénédictine
RevExp	Review and Expositor
RevHR	Revue de l'Histoire des Religions
RevSR	Revue des Science Religieuses
SNTS	Society for New Testament Studies
ScJT	Scottish Journal of Theology
StEv	Studia Evangelica
TBT	The Bible Translator
TDNT	Theological Dictionary of the New Testament
TR (= S)	Textus Receptus
TSK	Theologische Studien und Kritiken
TU	Text und Untersuchungen zur Geschichte der altchristlichen Literatur
ThLZ	Theologische Literaturzeitung

UBS

United Bible Societies

VC

Vigilae Christianae

ZNW

Zeitschrift für die Neutestamentliche  
Wissenschaft und die Kunde der älteren  
Kirche

## PREFACE

Like the majority of theologically trained men in India, I had been 'conditioned' to take the Text of the New Testament for granted until I became involved in Bible translation under the auspices of the United Bible Societies, in association with the Bible Society of India. The traditional attitude of accepting whatever the 'experts' from the West have said as true, and my deep preoccupation with parish ministry, had made me accept the British and Foreign Bible Society's Nestle Text as the last word in Textual Criticism. In India, whether in the direct ministry of the Church, or even in the specialised job of Bible translation, one's interest in the actual 'Text' is not normally expected to go beyond comparing the various English translations with the aid of Commentaries. One is, however, really inviting trouble when one begins to question the traditionally accepted Scriptures. They are widely held to be so divinely inspired as to be infallible. They have been preserved in the Textus Receptus (for Protestants) or the Peshitta (for the Orthodox) or the Vulgate (for the Roman Catholics) and have been so faithfully translated into the regional languages by the good old missionaries or devout fathers. (The very fact

that these three Texts are different seems to bother very few). Anyone who attempts to alter the word of God is destined to bring upon himself the 'plagues' mentioned in Revelation 22:18! It is not therefore surprising that ministerial candidates who are willy nilly initiated at theological institutions into the Text of the New Testament have to confine their knowledge to themselves! I must confess that even after joining the Bible Society, I tended to assume against my better judgment that the 1966 edition of the United Bible Societies might, after all, have solved all textual problems once and for all. But the knowledge that the same book had to undergo a second revision in 1968 and a third revision subsequently, has made my outlook change radically. Metzger's Textual Commentary, the companion volume to the forthcoming third edition, has served as a great eye-opener to make me aware more than ever before of the inescapable facts confronted by those who have to edit the Text of the N.T. My association with no less an authority on Biblical Criticism than Principal Matthew Black of St. Mary's College, St. Andrews, has given considerable momentum to this volte-face. With the barest minimum qualification to undertake a doctoral dissertation involving a critical examination of the work of Westcott and Hort who did create history in

the field of Textual Criticism would not have been any lighter for me but for the sympathetic understanding, gentle forbearance and wise counsel of Prof. Black. If this dissertation would find recognition from the scholarly world, I would have no hesitancy in adapting the famous words of the Psalmist to acknowledge that 'his gentleness has made me great'.

## INTRODUCTION

Christianity is essentially a way of life centred on the historical revelation in the person of Jesus of Nazareth. Unlike in Judaism or Islam or some other religions, the followers of the Christian faith are not called upon to be a people of a Book. Most of the dark chapters in the history of Christianity would not have been written that way if only all its adherents through the last twenty centuries had recognised this unique feature of their faith. It is true that we have inherited the sacred writings of Judaism and have, in addition, the record of the crowning of that divine revelation in the person of Christ. However, the closing of the Church's Canon was not the result of any official deliberation, but a gradual acknowledgment of the traditional consensus within a wide spectrum of Christendom<sup>1</sup>. The differences of opinion that still exist between the Roman, Orthodox and the numerous categories of Protestant traditions as to the exact number of books that constitute the sacred Canon is sufficient proof of the fact that this consensus has by no means been universal. This diversity of tradition should at any rate remind us that the Christian Scripture, the Bible, is only an imperfect earthen vessel that has been chosen to hold the treasure of 'the faith which was once for all delivered to the saints'<sup>2</sup>. The

chequered story of how the Bible has reached various lands in diverse languages, while most fascinating and inspiring, is also a further warning to us of this inherent imperfection of our Holy Writ<sup>3</sup>.

The question of the proper interpretation of the Bible, and the N.T. in particular, has, alongside of the wonderful saga of the preaching and teaching and propagation of the Christian faith, remained a divisive factor within Christendom. Yet, down the centuries, the Church has produced men of the calibre of Origen, 'the father of Textual Criticism', on the one hand, and translators like Jerome, Luther, Wyclif, Tyndale on the other. These men, by their labours of love, have, against heavy odds, upheld the basic insight about the essential nature and function of the Christian Scripture: "The Bible is more than a historical document to be preserved...It is a record of God's dealing with men, of God's revelation of Himself and His will. It records the life and work of Him in whom the word of God became flesh and dwelt among men. The Bible carries its full message, not to those who regard it simply as a heritage of the past or praise its literary style, but to those who read it that they may discern and understand God's Word to men"<sup>4</sup>. If Systematic Theology has developed in

the course of the Church's attempt to codify and preserve the faith against heresies and to make it relevant to every new situation, Textual Criticism has evolved in the context of the transmission of the Bible, resulting in both accidental and deliberate deviations from the original autographs. The tasks of the Bible translator and of the textual critic are only two sides of the same coin. The roles of both are to be viewed in terms of the Church's kerymatic and didactic responsibility.

The problem of establishing the most authentic text of the N.T. has become more important than ever before since the first discovery of the Egyptian Papyri in 1898. The earlier discovery of the Sinaitic Syriac (in 1892) was in itself a great contribution towards the progress of Textual Criticism before the close of the last century. Every subsequent MS that has come to light in this century, such as W (in 1906),  $\text{\textcircled{A}}$  (ed. 1913), P<sup>45</sup> (in 1931) has marked a significant step forward. However, the acquisition of P<sup>32</sup>, P<sup>46</sup>, P<sup>52</sup>, P<sup>64</sup>, P<sup>66</sup>, P<sup>75</sup> and P<sup>77</sup> (all dated c.200)<sup>5</sup> has been particularly epoch-making in that they have brought us very close to the original autographs of at least parts of the N.T.<sup>6</sup>. In fact P<sup>77</sup> (dated second century) and P<sup>52</sup> (dated c.125) lead us even farther back in time than A.D.200.

Now the question arises as to how far we can attain to the work of the apostolic authors themselves. Westcott and Hort, the famous British editors of the Greek N.T., claimed through their work to present "the closest possible approximation to the apostolic text itself"<sup>7</sup>. The contemporaneity of the debate is lucidly highlighted by J.K. Elliott in an article in a recent issue of Theology<sup>8</sup> under the caption: 'Can We Recover the Original New Testament?' In the end he boldly asserts: 'We may with confidence answer the question...with a resounding "yes" '<sup>9</sup>. However, the problems that are going to be discussed in the following pages will warrant Aland's caution<sup>10</sup> in this regard, especially in view of the fact that no two modern editors or critics are agreed in all respects as to the most authentic text!

The so-called 'young churches', whose vernacular Scripture texts have been dictated by the Textus Receptus tradition, have a great stake in the matter. An investigation into the topic that forms the title of this dissertation is of no small significance in this connection.

## STATEMENT OF THE PROBLEM

The subject relates to a textual theory propounded by Westcott-Hort in the Introduction to their edition of the N.T., first published in 1881. In this volume, which proved to be an important landmark in the history of Bible translation<sup>11</sup> as well as of Textual Criticism, they contrasted the characteristics of the 'Western' Text with those of the B $\mathcal{S}$  (or 'neutral') type. While rating the B $\mathcal{S}$  type very high, they considered the 'Western' family with D at its head as very impure and mixed because of its pronounced tendency to enrich the text by means of paraphrase, assimilation or borrowings from oral tradition and even extra-canonical sources<sup>12</sup>. This tendency of the 'Western' Text was facilitated by the fact that "in the second century oral traditions of the apostolic age were still alive; that at least one written Gospel closely related to one or more of the four primary Gospels, together with various forms of legendary Christian literature concerning our Lord and the Apostles, was then current in some churches; and that neither definition of the Canon of the New Testament nor veneration for the latter as distinguished from the substance of its sacred records had advanced far enough to forbid what might well seem their temperate enrichment from such sources as these"<sup>13</sup>. In 9 special cases, however, Westcott

and Hort judged that the 'Western' Text exhibits precisely the opposite tendency, in that it omits readings which are found in the 'neutral' type. In their opinion, under some strange circumstances, while their favourite Text type has gone wrong in these places, it is the usually adulterated Text which preserves the original readings. These omissions of varying lengths which occur at Mt 27:49b; Lk 22:19b-20; 24:3,6a,12,36,40,51 and 52, were termed by our editors as 'Western non-interpolations' to emphasise their claim that the 'Western' group is generally characterised by its interpolating tendency and that despite this anomaly, the B $\mathcal{L}$  group is still worthy of its high dignity of being a non-interpolating Text<sup>14</sup>.

In order to demarcate these 'spurious' passages from the rest of the Scriptures, the editors applied double square-brackets to them, but retained them within the body of the text. In addition to these 9 passages, Westcott and Hort called attention to "an intermediate class of Western omission", 18 in number, at Mt (6:16,25); 9:34; (13:33); 21:44; (23:26); Mk 2:22 (10:2); 14:39; Lk 5:39; 10:41f; 12:19,21,39; 22:62; (24:9); Jn 3:31f; 4:9, their "attestations being precisely similar to those of the non-interpolations which we accept, and the internal

evidence, intrinsic and transcriptional probability being open to some doubt". While it was felt that "it would be rash to condemn them absolutely", yet a lesser degree of certainty was attached to them. Even within this sub-category they maintained a subtle distinction without proper explanation by enclosing some of the references within brackets<sup>15</sup>. This type of distinction was carried over into the actual text by applying just half square-brackets to Lk 10:41, 12:39 and Jn 3:31f while enclosing most of them within single square-brackets.

Another group of readings which these editors marked out for special treatment includes 6 of the famous passages known as "the Greater Interpolations"<sup>16</sup> found in the 'Western' family; none of these is read by the B<sub>4</sub> group. They are: Mt 16:2b-3; (Mk 16:9-20)\*; Lk 9:54f; 22:43f; 23:34a; Jn 5:3b-4 and 7:53-8:11. These passages were also provided with double square-brackets to indicate that the editors had the same degree of certainty about their non-genuineness as they had for the 'Western non-interpolations'.

An inquiry into the External and Internal Evidences<sup>17</sup> pertaining to these 34 passages (i.e. 9+18+7) with a view to determining the justifiability of the term 'Western non-interpolations' as well as

of the basic assumptions that underlie this nomenclature constitutes the scope of this thesis. For this purpose we will do well to follow Westcott-Hort's classification without committing ourselves to any of their claims as to the spuriousness or otherwise of any particular passage.

Anyone who attempts to tackle these problematic passages will be baffled by the conflicting judgments they have evoked. Critical comments are sometimes too brief, subjective, or non-committal to be convincing. However, the volume of literature that at least some of these passages still continue to inspire is evidence for the contemporaneity of the debate as to their Scriptural status triggered off by Westcott and Hort nearly a century ago.

Among the welter of conflicting opinions, one comes across some blanket decisions aimed to cover either the entire body of 'Western' variants, or the 'Western non-interpolations' in particular. Below are given four such solutions proposed by well-known authorities in the field:-

(i) A.C. Clark: "Nowhere is the falsity of the maxim brevior lectio potior more evident than in the New Testament. The process of textual cor-

ruption has been one of contraction, not of expansion. The primitive text is the longest, not the shortest. It is not to be found in B<sup>N</sup>, or in the majority of the Greek manuscripts, but in the 'Western' family, i.e., in the ancient versions and the Codex Bezae (D). If my analysis is sound, we are brought back to an archetype of the four Gospels in book form, which cannot be later than the middle of the second century"<sup>18</sup>.

Clark was so convinced of his theory of the authenticity of the 'Western' readings and of omission due to homoeoarcton/homoeoteleuton (or  $\delta\mu$  for short as he designated it) as a cause of textual corruption, that he held his stichometric calculations as the clue to the solution of many of the disputed texts in the N.T. including some of our passages<sup>19</sup>. We will eventually see how unjustified he was in this over-emphasis, even if his theory might hold good in a few cases here and there.

(ii) F. Blass: According to his famous theory of the double recension of Lk-Ac by the author himself, the destinations of the primary edition of the two books were different. In the case of Acts, Luke sent the earlier and longer text to Rome, which formed the archetype of the 'Western' Text; the later and abridged edition was sent to Theophilus,

which has eventually survived in the B  $\delta$  group. But as regards the Third Gospel, it was the original and longer text that reached Theophilus, while the revised and shorter edition that the Roman church received was to become the exemplar of the 'Western' Text<sup>20</sup>.

Scholars who are prepared to accept this theory in respect of Ac,<sup>21</sup> have not extended the same degree of favour to the Third Gospel chiefly for three reasons:<sup>22</sup>

(a) The 'Western' Text is not limited to the Third Gospel alone; for each Gospel Blass offers separate explanations which are not satisfactory.

(b) Blass does not explain why the 'Western' Text of Lk (ch. 3), unlike its counterpart in the traditional text, contains an exact copy of the genealogy of our Lord in Mt 1:1ff.

(c) The 'Western' Text of Lk exhibits numerous verbal alterations in relation to the B  $\delta$  Text, which are inexplicable on the assumption that the author himself was re-editing his work in an abridged form.

(iii) M. Black: "...these 'Western non-interpolations' so far from proving that the texts which they have succeeded in ousting are spurious, are, nothing more, originally, than Tatianic excisions or 'non-interpolations', undertaken in the interests of an abridged Gospel book". He surmises that the absence of these disputed passages from the 'Western' MSS "may have been simply due to the continuing influence of Tatian's bowdlerized Gospel"<sup>23</sup>.

The fact of the matter, however, is that we do not yet possess either the complete text or the original form of Tatian's Diatessaron<sup>24</sup>. Even the language in which he composed his Gospel Harmony is uncertain<sup>25</sup>. In the absence of any conclusive evidence, whatever theory be proposed pertaining to the influence of the Diatessaron on other Texts, it will remain as a mere conjecture. E.g., with regard to the possible influence of Tatian's Text on the Old Syriac, Burkitt observes: "At every turn we find ourselves confronted with reasonable suspicion that the special features of an Old Syriac reading may be due to the influence of the Diatessaron, while at the same time we are without direct information as to the reading of the Diatessaron in the passage in question"<sup>26</sup>. According to Metzger, "not a few instances of

harmonization of the text of the Gospels in certain witnesses (notably the Western witnesses) are to be ascribed to Tatian's influence<sup>27</sup>." Even if this is conceded, since all our passages do not involve harmonizations, Tatianic influence cannot be made applicable to all of them.

Of course, Black's statement is admittedly of a tentative nature<sup>28</sup>. An exhaustive analysis of the passages under investigation here was outside the scope of the monograph concerned. I quoted him in order to underscore a relevant observation of Streeter, namely that "it is wrong to assume that in regard to genuineness, the whole series, whether major or minor 'non-interpolations' must stand or fall together. This is a fallacy<sup>29</sup>." Streeter's judgment had already been anticipated by Burkitt: "A glance at the table of these passages along with their witnesses will tell us that Old Syriac is divided on this issue [of genuineness] and this alone is enough to show that the 27 readings were not homogeneous. They cannot all be the result of one critical process, whether we decide to retain or omit them"<sup>30</sup>.

(iv) F.C. Burkitt: Although I have just quoted him approvingly, I notice that he did change his

mind on this issue at least in respect of the 'non-interpolations' in the last chapter of Lk. For in a subsequent article he observes that these passages are editorial insertions necessitated by the separation of the Gospel from Ac to form a separate volume alongside of the other three Gospels<sup>31</sup>.

It will be seen in due course that Burkitt has made a sweeping generalisation in respect of all the passages in Lk ch. 24. Against his better judgment which he had expressed in his earlier work, he has brought them under the umbrella of "one critical process"<sup>32</sup>. My contention in this thesis is that various processes had been at work in the evolution of the traditional Gospel text and that each passage, whether it be a 'Western non-interpolation' or a 'Western interpolation', should be judged on its own merits. I hope that the results of this inquiry will lead to a general recognition that the assumptions of neither Westcott-Hort, nor the four scholars cited above, were absolutely right and that while some of the readings in our list, whatever be the label that have been assigned to them, have a claim to originality, others belonging to their own category have to be rejected.

## METHODOLOGY

That brings us to the problem of methodology. The principle that is adopted in the following discussions is to keep an open mind with regard to each passage as far as possible, and not to be carried away by either time-honoured hypotheses or new-fangled theories<sup>33</sup>. Although I find myself in sympathy with the 'eclectic method', I have been learning to be aware of its inherent dangers as clearly demonstrated by two of the recent translations of the Bible, the RSV and the NEB. According to Colwell, the chief errors of eclecticism are:

- (i) negligence of the history of MS tradition;
- (ii) over-emphasis on Internal Evidence<sup>34</sup>.

Klijn supports him when he says: 'We believe that the present alternative between "the eclectic method" and following of a particular "good" manuscript or text is a fallacy. The "eclectic method" would need to be followed only if we did not know anything of the history of the text. But we do know something about these things and we know what influences came to bear upon the manuscripts and the groups of manuscripts because of their historical background. This we have to take into account when we choose a

particular reading<sup>35</sup>.

I have no quarrel with these scholarly pieces of advice in regard to the vast majority of variant readings in the N.T. MSS. But I do not think that either of these scholars ~~is~~ particularly referring to the texts with which we are concerned. Colwell is right in upholding the two cardinal principles of Hort<sup>36</sup>, viz.

- (i) "ALL TRUSTWORTHY RESTORATION OF CORRUPTED TEXTS IS FOUNDED ON THE STUDY OF THEIR HISTORY"<sup>37</sup>;
- (ii) "KNOWLEDGE OF DOCUMENTS SHOULD PRECEDE FINAL JUDGEMENT UPON READINGS"<sup>38</sup>.

Nor can anyone find fault with him for raising the slogan of 'Hort Redivivus' in view of the new pieces of evidence that have come to light since the days of Westcott-Hort's generation. For he says that Hort's basically sound principles can be revived and his work re-evaluated in relation to the evidence now available from:

- (a) P<sup>45</sup>, P<sup>46</sup>, P<sup>47</sup>, P<sup>66</sup>, P<sup>75</sup>;

- (b) lectionary MSS;
- (c) continuing study of the Church Fathers;
- (d) Versions, especially the Old Latins<sup>39</sup>.

Incidentally, Colwell does not find the evidence of P<sup>75</sup> to be drastically revolutionary: "The discovery of additional manuscripts, including the papyri has not demanded a drastic revision of Hort's two basic principles"<sup>40</sup>.

All this is well said. But what relevance have these statements got for the unfolding of the enigma that surrounds the 'Western non-interpolations'? In my opinion, none at all, because all the means of fresh evidence mentioned by Colwell have to do with external attestation<sup>41</sup>. But he himself has underrated the data provided by P<sup>75</sup>, the earliest extant Greek MS<sup>42</sup>. Both he and Klijn criticise Aland with sufficient justification for laying undue stress on the relevance of P<sup>75</sup> for the study of these disputed passages:<sup>43</sup> "... Aland reverses Westcott and Hort on the Western non-interpolations because P<sup>75</sup> disagrees with them in agreeing with Codex Vaticanus. But there is nothing in that agreement that is novel to Hort's theory"<sup>44</sup>.

However, the basic thing that we have to remember in our approach to the 'Western non-interpolations' is that they are, according to Hort's own admission, "exceptional" readings. They are of the essence of abnormality and offer a challenge to the traditional principles of Textual Criticism. They are abnormal because, contrary to the notorious tendency of the 'Western' Text to expand, they are all omissions which defy easy explanation. "Omissions of genuine words and clauses in the Alexandrian and Syrian texts are very rare, and always easy to explain. In the Western text, they are bolder and more numerous, but still almost capable of being traced to a desire of giving a clearer and more vigorous presentation of the sense. But hardly any of the omissions in question can be so explained, none in a satisfactory manner"<sup>45</sup>. They are a challenge to the time-honoured canons of Textual Criticism because if it were merely a choice between the shorter and longer readings or between the more difficult and simpler readings, according to the ordinary principles, we are left with only one set of choices, viz. the shorter and the more difficult readings. But Westcott and Hort were no mediocre scholars. If External Evidence were to be the chief criterion in decision-making in the context presented by the 27 passages designated as 'Western non-interpolations',

they could invariably have ruled out the 'Western' readings as spurious even as they did in the case of the vast majority of other texts because of their esteem for the 'neutral' group. Similarly, with regard to the remaining 7 'Western interpolations' in our list, these editors could have straightway cast their votes in favour of the deletion of the words if their concern was chiefly for External Evidence. But the very fact that they were not prepared to do so suggests that they did value the Internal Evidence as well. This is particularly striking in regard to their treatment of Mk 16:9-20 and Jn 7:53-8:11, although Hort's 'Notes' are too brief on Mt 16:2b-3, Lk 9:54f, 22:43f, 23:34a and Jn 5:3b-4. However, Westcott-Hort's decision to dishonour their most favourite Text in the case of the 9 classic 'Western non-interpolations' and their expression of serious doubt about the authenticity of 18 others shows their emphasis on Internal Evidence in such contexts. They write: "... with the exception mentioned above (sc. the 'Western non-interpolations'), it is our belief (i) that readings of  $\aleph$  B should be accepted as the true readings until strong internal evidence is found to the contrary, and (ii) that no readings of  $\aleph$  B can safely be rejected absolutely..."<sup>46</sup>.

At first sight it would appear that Westcott-Hort are exempting the 'Western non-interpolations' from the discipline of Internal Evidence. But when we examine Hort's 'Notes' on these passages we realise that he devotes very little space for the discussion of MS evidence. It is true that these 'Notes' are lamentably short on these passages (except for Lk 22:19b-20) as far as Internal Evidence is concerned and that Hort has no 'Notes' at all for the 18 passages outside the 9 classic ones. But a glance at the 'Notes' on the 9 passages is enough to convince us that the chief yard-stick Hort employs there is Transcriptional Probability. Intrinsic Probability is considered only in the case of Lk 22:19b-20.

From what has been said above, it is clear that despite the idiosyncrasies of his methodology, Hort was convinced of the need for looking into the Internal Evidence relating to these exceptional passages. Why he did not consistently follow his policy is a mystery. But he has given us sufficient indication that neither his 'Non-interpolations' nor the related textual phenomena should be treated on a par with other problematic passages in the N.T. The readiness shown by Westcott-Hort in resorting to conjectural emendations in respect of about 60 N.T.

passages<sup>47</sup> (out of these 2 instances are in Mt, 1 each in Mk and Lk, 9 in Ac and 3 in Jn)<sup>48</sup> where they suspected a 'primitive error', only underlines their recognition that exceptional cases need exceptional treatment. Their statement regarding conjectural emendation is worth noting: "Like the process of detecting corruption, it can make no use of any evidence except Internal Evidence of Readings, but it depends on Intrinsic and Transcriptional Evidence alike"<sup>49</sup>. Although none of our passages comes under this category, I think that since they too have got to do with 'primitive error', this remark on the value of Internal Evidence is equally applicable to them. Hence, without being unfair to Westcott-Hort, I would advocate a via media between MS tradition and Internal Evidence in tackling the readings under discussion.

Coming back to eclecticism, Metzger's warning as to caution in the application of this method has special relevance to our topic. He advises us to be conscious of possible change in style of a particular author and of the harmonising tendencies of scribes<sup>50</sup>. I must add that one should also be aware of the possibility of an author's undue devotion to his source-material as a contributing factor for the phenomenon of exceptional readings. Therefore,

while steering clear of the distorted representation of some over-zealous advocates, let us admit that eclecticism, as a scientific discipline, has a definite role to play in the restoration of the text of the N.T. It is in fact an inter-disciplinary science, and hence a helpful tool for Textual Criticism. In the words of Vaganay, "Eclecticism is no shutting up of the branches of science into water-tight compartments; verbal criticism, external and internal criticism, all have their part to play"<sup>51</sup>. A judicious use of this method can, therefore, serve as a constructive rather than a destructive tool in our quest for that Text which could possibly be the nearest approximation to the original autographs.

Recognizing the confused state of difficult passages in the N.T. MSS, Klijn observes: 'We never can speak of a "bad text" or a "good text" in general, only a text with "many good readings" or "few good readings" '<sup>52</sup>. This warning has to be borne in mind when we set out to evaluate the External Evidence relating to our passages because they are a constant reminder that at least in these cases undue devotion to one particular set of evidence is to be avoided. Hence the relevance of Housman's advice that an emendator should be free of any "favourite method" and that "An emendator with one method is as foolish

a sight as a doctor with one drug"<sup>53</sup>.

All these might sound too commonplace. But as I am not convinced that the debate as to the proper method of approach to the 'Western non-interpolations' in particular is over, I thought it necessary to make myself clear. There is, however, one problem that has confronted me over and over again in my study of the 34 passages listed earlier, viz. the problem of subjectivity: one becomes enamoured of a particular hypothesis - maybe the result of a brain-wave - and upholds it as the solution to a certain textual phenomenon. When this develops into an obsession all other considerations are conveniently ignored or are made to subserve the defence of this hypothesis. One has only to read the article on the Lord's Supper by Vööbus<sup>54</sup> to be refreshingly aware of this. H.I. Bell rightly remarks: "Orthodox scholars, however scrupulous, naturally incline to favour suspected readings which are doctrinally or liturgically important; 'liberal' scholars are often tempted to overdo destructive criticism and even some of the orthodox, in a laudable desire to avoid prejudice, may be led into making concessions too readily"<sup>55</sup>. This is precisely the environment in which I have found myself while trying to enrich myself with the wisdom of

erudite minds in my attempt to find a solution to our passages. For here we are up against texts involving such vital theological and liturgical issues as the Lord's Supper, the Resurrection and the Ascension on the one hand, and the Lord's Agony and the Prayer for Forgiveness from the Cross on the other, to cite just a few of the most important ones. Is it any wonder that scholars find it extremely difficult to align themselves on the side of scientific objectivity if some of these texts pose a direct challenge to their orthodoxy, devotion and religious sentimentality?

The present writer cannot altogether extricate himself from these natural human frailties. All that he can say is that he has earnestly endeavoured to be as objective as possible. He has also tried to consult a wide spectrum of scholarly view-points ranging from conservative Roman Catholicism to liberal Protestantism on the one hand, and the Anglo-Saxon lands to the European Continent on the other.

Summarising the United Bible Societies' Committee decisions with regard to the 9 'Western non-interpolations' Metzger writes, "...the Bible

Societies' Committee did not consider it wise to make as it were, a mechanical or doctrinaire judgment concerning the group of 9 Western non-interpolations but sought to evaluate each one separately on its own merits and in the light of fuller attestation and newer methodologies"<sup>56</sup>.

As for the findings of this Committee, however, we are told that "the majority of the Committee, having evaluated the weight of evidence differently, regarded the longer readings as part of the original text"<sup>57</sup>.

These statements highlight the problem of subjectivity only too well. For as I have already pointed out, the methodology that is pursued in the present investigation is that advocated by Metzger above. Yet I am constrained to disagree with the U.B.S. Committee's majority decisions not only in respect of these 9 passages, but elsewhere too<sup>58</sup>. This is in spite of my constant use of Metzger's Textual Commentary and my indebtedness to that book in making at least some of my decisions. Therefore I am only too conscious of my being open to the charge of subjectivity in my judgments. But considering the enormous number of decisions that have to be made in dealing with every single passage, an element of subjectivity is probably unavoidable. It has been my earnest endeavour, however, to be fair not

only to the issues raised in the following pages,  
but also to the numerous scholars into whose labours  
it has been my privilege to enter.

<sup>1</sup>See C.R. Gregory, *Canon and Text*, pp.286ff.

<sup>2</sup>Jude, v.3.

<sup>3</sup>Pope Leo's Encyclical: *Providentissimus Deus* (1893) says: "All the books and the whole of each book which the Church receives as sacred and canonical were written at the dictation of the Holy Spirit; and so far as it is from being possible that any error can co-exist with divine inspiration that not only does the letter itself exclude all error, but excludes and rejects it with the same necessity as attaches to the impossibility that God Himself, who is the supreme truth, should be the author of any error whatsoever". Similar views have been held by other denominations as well. Such theories of the verbal inspiration of Scripture do not square with actual historical facts. (See T.W. Manson, *Companion to the Bible*<sup>2</sup> revised edn. by H.H. Rowley, pp.6-10).

<sup>4</sup>Revised Standard Version, Collins, London and Glasgow (1952), Preface, p.vii.

<sup>5</sup>According to K. Aland, *NTS* 21 (1974), p.357, n.6, the latest in the list of the Papyri is P<sup>86</sup> although only up to P<sup>82</sup> have been published.

<sup>6</sup>The contents of these Papyri are as follows:-  
P<sup>32</sup> - Parts of Titus; P<sup>46</sup> - Epistles (with lacunae); P<sup>52</sup> - Parts of Jn; P<sup>64</sup> - Parts of Mt; P<sup>66</sup> - Parts of Jn; P<sup>75</sup> - Parts of Lk and Jn (with lacunae); cf. Metzger, *Text of the N.T.*<sup>2</sup>, Appendix, pp.247ff.

<sup>7</sup>*The New Testament in the Original Greek*, 2 vols; Vol.I contains the edited text; Vol.II contains their Introduction, followed by 'Notes on Select Readings' by Hort (hereafter to be designated simply as 'Notes'). The joint opinion of the editors will be referred to after Westcott-Hort.

<sup>8</sup>*Theology* 77, no.649 (1974), pp.338-53.

<sup>9</sup>*Ibid.*, p.352. I do not understand Elliott's claim (p.349): "A text based on eclectic principles stands a good chance of representing the original text of the New Testament authors" (vide infra).

<sup>10</sup>Cf. *The Bible in Modern Scholarship*, ed. J.P. Hyatt, pp.341f.

<sup>11</sup>As is well-known, Westcott-Hort's scholarship lies behind the Revised Version, 1881. The Nestle text which has been very popular with the Bible Societies and Theological Colleges in many countries for many generations, incorporates the texts of Tischendorf, Westcott-Hort, Weiss and Weymouth. It is now reaching its 26th edition. The United Bible Societies' edition of 1966 (a third edition is now under way) is admittedly very much indebted to the Westcott-Hort text (cf. R.P. Markham, TBT 17 (1966), pp.106-13; M. Black, ScJT 19 (1966), pp.486-88).

<sup>12</sup>Cf. Westcott-Hort, *op.cit.*, II, pp.120-26; 130-32; 148-51; 170-72.

<sup>13</sup>*Ibid.*, p.174.

<sup>14</sup>*Ibid.*, pp.175f.

<sup>15</sup>*Ibid.*, p.176. I presume that the editors attached lesser certainty to the ones within brackets.

<sup>16</sup>The asterisked passage does not belong to the category of 'Greater Interpolations'. The complete list of this category consists of 12 passages, 6 of which are universally acknowledged to be spurious. They are the additions at Mt 3:15 (The Light at the Baptism); Mt 22:28 ('Seek from little to increase'); Mk 16:3 (The Light at the Resurrection); Lk 6:5 (The Man Working on the Sabbath); Lk 23:2,5 ('Loosening the Law...our sons and wives...') and Lk 23:53 (The Great Stone). Cf. Burkitt, *Evangelion*, II, p.216.

<sup>17</sup>By 'External Evidence' we mean the data provided by the MSS. 'Internal Evidence' is essentially the exegetical aspect of a passage. It is made up of 'Transcriptional Probability' and 'Intrinsic Probability'. In the former we concentrate on the scribe while in the latter we concentrate on the author. (Vide Westcott-Hort, *op.cit.*, vol.II, pp.19ff.).

<sup>18</sup>*Primitive Text*, p.53.

<sup>19</sup>E.g. Lk 10:41f, 12:19; 23:34; 24:6,51; Mt 9:34; 27:49b; cf. *ibid.*

<sup>20</sup>Cf. *Philology*, pp.139-42.

<sup>21</sup>C.S.C. Williams, *Commentary on Acts*, p.49, challenges the Lukan authorship of the expanded form of Acts. "Whatever may be the origin of the Western text of the Gospels, which is marked by omissions rather than by additions, that the text of Acts, which is characterised by additions, may have been the result of a 'publication' of the text in the first half of the second century; this may have incorporated harmonizing details and gossip' to attract the lay reader becoming familiar with the Apocryphal Gospels".

<sup>22</sup>Vide F. G. Kenyon, *Handbook to the Textual Criticism of the N.T.* pp.350-54; cf. C.S.C. Williams, *Alterations*, pp.54ff.

<sup>23</sup>*Syriac Versional Tradition*, p.131.

<sup>24</sup>Cf. J.R. Harris, *The Diatessaron of Tatian*, p.27.

<sup>25</sup>Cf. V. Taylor, *Text of the N.T.* (1961), p.13.

<sup>26</sup>*Evangelion*, II, p.235.

<sup>27</sup>*Text of the N.T.*<sup>2</sup> (1968), p.92. M. Black, *op.cit.*, pp.126ff, cites several examples to illustrate the influence of the Diatessaron on the Old Syriac. But the instances he chooses from our list are confined to Lk ch.24. It is not clear whether his theory is applicable to all the 27 passages under the category of 'Western non-interpolations'. Another difficulty in accepting this theory in toto is that it assumes that the Diatessaron was the work of an abbreviator. This is to overlook the numerous apocryphal additions in this Harmony. Why did this Harmony fail to influence the Old Syriac at these points? (Cf. Harris, *op.cit.*, p.35).

<sup>28</sup>*Op.cit.*, p.126.

<sup>29</sup>Four Gospels, p.142.

<sup>30</sup>Op.cit., p.228.

<sup>31</sup>JTS 28 (1926-27), pp.180-81.

<sup>32</sup>Similarly Harris, op.cit., p.54, offers a blanket solution for the problematic readings in Lk ch.24: "The solution of this perplexity seems to lie in the following direction; there must have been a previously existing harmony, probably a harmony of the Passion-Gospels, which Tatian used, and which has also been used as a revising factor in the ancestry of all texts of the Gospels except the non-interpolating Westerns".

<sup>33</sup>As we have already seen, A.C. Clark's emphasis on the genuineness of the 'Western' text led him to accept many of its readings in his edition of Acts. C.H. Turner in a series of articles in JTS between 1924-27 argued in favour of many readings in the 'Western' and Byzantine type of Texts. Kilpatrick, in Essays in Memory of G.H.C. MacGregor (1965), pp.189-206, showed how many of the readings in A, the Textus Receptus and the Byzantine witnesses were superior to the B $\alpha$  group. All these critics have sometimes convincingly offered a challenge to the traditional theories of Textual Criticism as well as to the claims of Westcott-Hort about B $\alpha$ .

<sup>34</sup>Studies in Methodology, p.155.

<sup>35</sup>Survey, II, p.70.

<sup>36</sup>Op.cit., p.156.

<sup>37</sup>Westcott-Hort, Introduction, p.40.

<sup>38</sup>Ibid., p.31.

<sup>39</sup>Op.cit., p.157.

<sup>40</sup>Ibid., p.156.

<sup>41</sup>Colwell's earlier article in JBL 66 (1947), pp.109-33 is a healthy counter-balance to his views cited here (see also my last chapter).

- <sup>42</sup>Cf. V. Martin and R. Kasser, Papyrus Bodmer XIV-XV (Bibliotheca Bodmerina, 1961).
- <sup>43</sup>Vide Aland, NTS 12 (1965-66), pp.193-210.
- <sup>44</sup>Colwell, loc.cit., cf. Klijn, Survey, II, p.49, n.2; contrast Haenchen, Acts of the Apostles (E.T.), p.59; Snodgrass, JBL 91 (1972), pp.369-379.
- <sup>45</sup>Westcott-Hort, Introduction, p.175.
- <sup>46</sup>Ibid., p.225.
- <sup>47</sup>Vide, Hort, Notes, pp.1-140.
- <sup>48</sup>For a consolidated list of all the passages, see Metzger, Text of the N.T.<sup>2</sup>, p.184 and n.2. Westcott-Hort mark out these passages in their edition of the Greek N.T. with obeli.
- <sup>49</sup>Introduction, p.71.
- <sup>50</sup>Op.cit., pp.178-79.
- <sup>51</sup>Introduction to the Textual Criticism of the N.T. (E.T.), p.91.
- <sup>52</sup>Survey, I, p.168.
- <sup>53</sup>A.E. Housman, ed., Manilii Astronomicum, liber primus, Cambridge, 1903, pp. liiif, quoted by Metzger, op.cit., p.211, n.4.
- <sup>54</sup>NTS 15 (1968-69), pp.457-63.
- <sup>55</sup>JTS (n.s.) 3 (1952), p.260.
- <sup>56</sup>Textual Commentary, p.193.
- <sup>57</sup>Ibid.
- <sup>58</sup>For a rather fair evaluation of Metzger's Textual Commentary, see J.K. Elliott, loc.cit.; cf. Elliott

Novum Testamentum 15, Fasc.4 (1973), pp.278-300.  
However, Elliott's higher praise for the NEB for  
its thoroughgoing eclecticism in comparison with  
the UBS text is not quite justified. See also  
his article in Theology, July 1972.

CATEGORY I

THE CLASSIC 'WESTERN NON-  
INTERPOLATIONS' OF WESTCOTT-HORT

The 8 Lukan passages that Westcott-Hort isolated for special mention by means of double square brackets and whose authenticity was categorically ruled out by them, may conveniently be studied under the following heads:

- A. The Last Supper (22:19b-20)
- B. The Empty Tomb Story:
  - a. The Christological Title (24:3)
  - b. The Angelic Announcement (24:6a)
  - c. The Role of Peter (24:12)
- C. The Resurrection Appearance to the Disciples:
  - a. The Dominical Greeting (24:36b)
  - b. The Visible Proof (24:40)
- D. The Ascension:
  - a. The *ἀνάληψις* (24:51)
  - b. The Worship of the Ascending Lord (24:52)

The concentration of so many remarkable variants in the closing chapters of Lk has intrigued scholars and has given rise to speculations. Burkitt opines that the shorter reading in each case is original and that the interpolations in the

archetype of the non-Western Texts are nothing but editorial embellishments adopted when the Gospel was separated from Acts. The material for such expansion was derived from Mk or Jn. Thus, according to Burkitt, 24:6a was derived from Mk 16:6; 24:12 from Jn 20:4-6; 24:36 from Jn 20:19; 24:40 from Jn 20:20: "We see then that these editorial changes are marked by a desire to complete the narrative by simple verbal additions, without any attempt at historical criticism, very much in the manner that the Diatessaron itself was compiled. It seems to me that the same hand added xxii 19-20...What more natural than that the editor who had touched up the last chapter of the Gospel to fit it for public reading in Christian assemblies along with 'Matthew' and 'John' should supplement Luke's meagre narrative with the apostolic words of St. Paul?"<sup>1</sup>

The reason why the Lukan autograph was so brief at these points has been explained by Salmon<sup>2</sup> in terms of the limitations of ancient book-making. Every author had somehow to adjust his material to the length of papyrus roll which was available to him as a finished product<sup>3</sup>. According to this theory, when Luke was about to conclude his book he became conscious of the fact that his papyrus roll was running short and so had to make up his mind about the

actual material he would incorporate in his current treatise. "...The manner in which the Gospel closes indicates that the author contemplated a continuation"<sup>4</sup>.

That this theory does not adequately explain the phenomena under discussion will become clear after we have discussed all the passages for the simple reason that the omissions are not all due to one critical process and that various factors including theological considerations, or perhaps even chance, had something to do with the absence of these readings from certain groups of Texts. Suffice <sup>it</sup> for the time being to remind ourselves that so long as we do not subscribe to Westcott-Hort's assumption that the 8 passages in question are of such extraordinary significance as to be designated as spurious, we are relieved of the burden of explaining the concentration of such passages in the last three chapters of Lk. On the contrary, it is the contention of this thesis that at least some of the 18 other passages which Westcott-Hort have considered as of secondary significance from the point of view of spuriousness are of equal weight. (Moreover, it will also be shown that at least a few of the 'Western interpolations' of Westcott-Hort really do not deserve to be labelled so). Since both

categories of such passages are spread across all the four Gospels, any theory based on a special label for the concluding chapters of Lk is out of the question.

<sup>1</sup>JTS 28 (1926-27), pp.180-81.

<sup>2</sup>Some Thoughts on Textual Criticism, pp.100-1.

<sup>3</sup>Cf. Rüdigg, TSK, 1896, pp.94-101, especially p.96:  
"Die antiken Papyrusfabriken lieferten nämlich den  
Autoren nicht lose Blätter oder Bogen, wie sie uns  
zur Verfügung stehen, sondern gleich fertige Rollen;  
und diese Rollen halte eine Minimal-und eine  
Maximalgrösse über die Schriftsteller nicht  
hinausgehen, der Verfasser eines mehrbändigen Werkes  
zu rechnen, und es war keinswegs jedem gegeben,  
dieselbe mit der wünschenswerten Leichtigkeit zu  
überwinden".

<sup>4</sup>Salmon, loc.cit., cf. the rather abrupt conclusion  
of Acts: "...If the author had lived he would have  
given us a continuation of the history". So also  
Rüdigg, op.cit., p.100.

NOTE

The External Evidence provided in this thesis has been derived from three sources:

1. For all the 'Western Non-interpolations' in Lk and Jn except Lk 24:9 and for Lk 22:43f: K. Aland, "Die Bedeutung des P<sup>75</sup> für den Text des Neuen Testaments", in Studien zur Überlieferung des Neuen Testaments und seines Textes, Band 2, Berlin, 1967, pp.157ff.
2. For all the remaining passages except Mt 13:33: The Greek New Testament<sup>2</sup>, United Bible Societies, 1968.
3. For Mt 13:33: Tischendorf, Novum Testamentum Graece<sup>8</sup>, I.

I.A. Lk 22:19b-20 : The Last Supper

Disputed words : [τὸ ὑπὲρ ὑμῶν διδόμενον· τοῦτο ποιεῖτε εἰς τὴν ἐμὴν ἀνάμνησιν· καὶ τὸ ποτήριον ὡσαύτως μετὰ τὸ δειπνῆσαι, λέγων, Τοῦτο τὸ ποτήριον ἡ καινὴ διαθήκη ἐν τῷ αἵματί μου, τὸ ὑπὲρ ὑμῶν ἐκχυννόμενον.]

External Evidence :

Nestle<sup>26</sup>

Greek NT (UBS)

καὶ λαβὼν ἄρτον εὐχαριστήσας ἔπλασεν καὶ ἔδωκεν αὐτοῖς λέγων· τοῦτό ἐστιν τὸ σῶμά μου τὸ ὑπὲρ ὑμῶν διδόμενον· τοῦτο ποιεῖτε εἰς τὴν ἐμὴν ἀνάμνησιν. <sup>20</sup> καὶ τὸ ποτήριον ὡσαύτως μετὰ τὸ δειπνῆσαι, λέγων· τοῦτο τὸ ποτήριον ἡ καινὴ διαθήκη ἐν τῷ αἵματί μου τὸ ὑπὲρ ὑμῶν ἐκχυννόμενον (vorher: τὸ ὑπὲρ — ἐκχυννόμενον in Doppelklammern)

καὶ λαβὼν ἄρτον εὐχαριστήσας ἔπλασεν καὶ ἔδωκεν αὐτοῖς λέγων, Τοῦτό ἐστιν τὸ σῶμά μου [[τὸ ὑπὲρ ὑμῶν διδόμενον· τοῦτο ποιεῖτε εἰς τὴν ἐμὴν ἀνάμνησιν. <sup>20</sup> καὶ τὸ ποτήριον ὡσαύτως μετὰ τὸ δειπνῆσαι, λέγων, Τοῦτο τὸ ποτήριον ἡ καινὴ διαθήκη ἐν τῷ αἵματί μου, τὸ ὑπὲρ ὑμῶν ἐκχυννόμενον]]

17. 18. 19. 20 P<sup>75</sup> N A B C K L T<sup>vid</sup> W X Δ Θ Π Ψ 063 f<sup>1</sup> f<sup>13</sup> 565 700 892 1009 1010 1071 1079 1195 1216 1230 1241 1242 1253 1344 1365 1546 1646 2148 2174 N<sup>l</sup> Lect aur c f q r<sup>1</sup> vg syr<sup>pal</sup> sa bo arm georg || 17. 18. 19 a (—19 b —20: τὸ ὑπὲρ ὑμῶν ... ἐκχυννόμενον) D a d ff<sup>2</sup> i l syr<sup>h</sup> || 19 a (καὶ λαβὼν ... σῶμά μου), 17. 18 b e || 19. 17. 18 syr<sup>c</sup> || 19. 20 l<sup>32</sup> syr<sup>p</sup> bo<sup>ms</sup> || 19. 20 a (μετὰ τὸ δειπνῆσαι), 17. 20 b (τοῦτό μου τὸ αἶμα ἡ καινὴ διαθήκη) 18 syr<sup>s</sup>

### Observations

(i) If we follow the long reading as attested by the vast majority of MSS, it would appear that unlike the Markan (14:22-26) and Matthaean (26:26-30) accounts, the Lukan version of our Lord's last meal with His disciples has the order cup-bread-cup - one cup being before supper (v.17) and the other after supper (v.19). This is, of course, contrary to the Church's tradition which is supposed to trace back its origin to the regular bread-cup order as

found in the earliest N.T. account, viz. 1 Cor. 11:24-25.

(ii) On the other hand, if the omission of the 'Western' witnesses is taken as conforming to the authentic Lukan record, we are left with yet another anomalous order of cup-bread besides the serious lacuna in the statement of the Lord regarding the significance of the bread.

(iii) The picture is made more confusing by the transposed order of the verses in question by two of the Old Latins b, e as well as by two of the Old Syriac witnesses, S, C.

(iv) Neither the divided testimony of the 'Western' type of Text, nor the overwhelming support for the long reading by the 'best' MSS, can be taken as a safe basis for settling the textual problem here<sup>1</sup> because all the classic 'Western non-interpolations' listed by Westcott-Hort more or less present the same phenomenon. Therefore Aland's statement in favour of the long reading is an over-simplification : "...abgesehen davon, dass syr<sup>S</sup> die Reihenfolge der Verse ändert, bleibt Vers 19b bei ihm erhalten, ein weiterer Beweis dafür, dass der Langtext der ursprüngliche ist. Syr<sup>S</sup> beweist darüber hinaus, dass die Vetus Syra den Langtext durchaus kannte.

Wenn er auch hier...anders gruppiert ist, so ist das ein Hinweis auf die inhaltlichen Schwierigkeiten des Langtextes, dessen Anstösse man entweder durch Umgruppierung oder durch auf verschiedene Weise vorgenommene Kürzungen zu beseitigen suchte. Dass P<sup>75</sup> mit dem Langtext geht, kann eigentlich nicht überraschen, durch ihn wird das Zeugnis dafür so überwältigend, dass eine Diskussion über den primären Charakter des Langtextes unter textkritischen Gesichtspunkten sich eigentlich erübrigt"<sup>2</sup>.

(v) Patristic evidence for such a crucial passage is so confused that the UBS edition of the Greek N.T. completely leaves this out. Benoit<sup>3</sup> enlists the support of Tatian, Marcion, and Justin for the long text. But R.D. Richardson<sup>4</sup>, after a careful analysis of the relevant data has reached the following conclusions:

- (a) "...Tatian cannot be quoted as one who knew the longer Lucan reading as a whole, but at most, only as one who knew the words of remembrance; none of the witnesses to Tatian's text vouches for more than these"<sup>5</sup>.
- (b) "...we cannot legitimately claim that [Marcion] knew the longer reading in the form in which we now have it..."<sup>6</sup>.

(c) "...Justin can be quoted as the earliest non-canonical witness to the words of remembrance, but not to the pericope of institution as a whole. The usual assumption that he must be quoting either from 1 Cor. XI or from the longer Lucan text is unwarranted"<sup>7</sup>.

It is thus evident that on the whole the External Evidence does not lead to any firm conclusion with regard to this unusually difficult textual problem<sup>8</sup>.

### Internal Evidence

#### Transcriptional Probability

##### (i) Possibilities of Insertion

At the outset it must be borne in mind that the dictum brevior lectio potior<sup>9</sup> cannot be pressed in favour of the originality of the short text here because we are dealing with one of the most complicated 'Western non-interpolations'. That the words bracketed by Westcott-Hort are not authentic may be inferred from two considerations;

(a) Since these words bear close resemblance to Paul's language in 1 Cor 11:24f, they give rise to the suspicion of having been borrowed from that source. Of course, the two passages are not exactly identical. Lk has the additional word *διδόμενον* in respect of the bread-word along with a few minor transpositions or alterations, which may be supposed to be within the bounds of freedom of adoption from parallel texts<sup>10</sup>, granting that assimilations to passages outside the Gospels are extremely rare. But the presence of *τὸ ὑπὲρ ὑμῶν ἐκχυστόμενον* in place of the injunction to perpetuate the Lord's memory seems to pose a more serious difficulty. Hort's confident explanation<sup>11</sup> may not be convincing to everyone: "St. Paul's corresponding sentence being implicitly contained in his *τοῦτο ποιεῖτε εἰς τὴν ἐμὴν ἀνάμνησιν* already appropriated, a neater ending was obtained by taking a phrase from Mc (cf. Mt) with the substitution of *ὑμῶν* for *πολλῶν* in accordance with St. Paul's *ὑπὲρ ὑμῶν* in the former verse". As a matter of fact the same methodology could be ascribed to the author. It could even be argued that Luke had access to the same stratum of tradition as is preserved in 1 Cor 11, that the similarities between the two passages are due to Luke's familia-

rity with liturgical practice and that the presence of certain non-Lukan expressions in the present passage (vide infra) is also explicable along this line<sup>12</sup>. But this is no doubt open to the counter-argument that the discrepancies in Lk could as well have arisen due to scribal borrowing from liturgy.

(b) The readings of b, e and Syr<sup>S,C</sup> testify to the fact that vv.19b-20 were absent from the MSS of some branches of early Christendom. The order of the verses relating to the Last Supper in the above MSS are as follows:<sup>13</sup> (vide Appendix p.viia).

b, e : 19a,17,18 (with slight variations in v.18)

Syr<sup>S</sup> : 19a,17,18 (+5 additions including 19b)<sup>14</sup>

Syr<sup>C</sup> : 19a (+1 Cor 11:24b), 17,18  
(enlarged with borrowing from 1 Cor 11:24)

The fact that scribes had to transpose vv.17 and 18 in all these cases to the position immediately after the mention of the bread-breaking suggests that there was a void in the place of the second cup in their exemplars<sup>15</sup>. Thus we have an unmistakable evidence for the existence of the short reading as early as the middle of the second century<sup>16</sup>.

What was the most likely reason behind the expansion of the text if the original did not have vv.19b-20?

1. A possible explanation is that the scribe in question did identify the cup of v.17 as the cup of the Jewish Passover rite (vide infra) so that when he came to v.19a, he was puzzled by the absence of a proper Eucharistic rite in his copy of the Third Gospel in comparison to the first two Gospels. But this is not satisfactory because the absence of such a rite is true of the Fourth Gospel as well.

2. The usual explanation for the origin of the long text is more convincing. The scribe interpreted v.17 to be the Eucharist wine, and being puzzled by v.19a because of the reversed order of the 'elements', tried to 'normalise' it. This attempt proceeded independently along 2 different lines in various Christian circles. The first type of expansion is manifested by the Old Syriac and b e - filling in the lacuna by means of shifting the verses containing the reference to wine to a position immediately after the bread-word<sup>17</sup>. The second type is attested by the majority of Greek MSS and their allies - borrowing from the liturgical formula found in 1 Cor 11.

3. Another explanation for the expanded form of the text is that offered by Blass<sup>18</sup> and Graefe<sup>19</sup>, viz. that it goes back to the revised edition of the Gospel by Luke himself. The first edition, meant for Theophilus, had no need for an account of the Institution as he must have been already familiar with it. But in the second edition, the author must have felt the obligation to instruct the congregation in Rome on this vital sacrament of the Church, and so modified his text accordingly.

Against this, it must be noted that it has found little appeal among scholars. After all, it is incredible that even at a secondary stage, the author would have penned such an insertion involving a clumsy style<sup>20</sup>. And as for the 'primary edition', it involves not only an apparently irregular order of the Eucharistic elements, but also a truncated bread-word. This is precisely the reason why Blass, the better known proponent of the double-recension theory of Lk, had to do away with v.19a, although without proper supporting argument<sup>21</sup> (vide infra).

(ii) Possibilities of Omission

The origin of the short text from a supposedly long one has been variously accounted for:

(a) Kenyon and Legg<sup>23</sup> hold that all forms of the short text can be explained in terms of an attempt to get rid of one of the two cups presented by the fuller text. "The whole difficulty arose...from a misunderstanding of the longer version. The first cup given to the disciples to divide among themselves should be taken in connection with the previous verse (ver. 16) as referring to the eating of the Passover with them at the reunion in Heaven. This is followed by the institution of the Sacrament, to be repeated continually in memory of Him. This gives an intelligible meaning to the whole, while at the same time it is easy to see that it would occasion difficulties of interpretation, which would give rise to the attempts at revision that appear in various forms of the shorter version".

This hypothesis assumes that Luke had in fact a narrative of the Institution, which is open to debate (vide infra). It also fails to explain why anyone would dare to tamper with a passage involving the most sacred rite of the Church if it were already part of the Third Gospel.

In the same vein Metzger writes:<sup>24</sup>

"It is easier to suppose that the Bezan editor,

puzzled by the sequence of cup-bread-cup, eliminated the second mention of the cup without being concerned about the inverted order of institution thus produced, than that the editor of the longer version, to rectify the inverted order, brought in from Paul the second mention of the cup, while letting the first mention stand".

This argument too is not convincing. Once we concede the motive of wilful suppression as a means to obviate a glaring anomaly, we must also admit that the scribe could have brought this about more naturally by eliminating the first cup (vv.17f)<sup>24</sup>. (The residual text would then read quite smoothly, the ἀρτον of v.19 being well-suited to go with the φάγω of v.16). Added to this is the difficulty of explaining how v.19 came to be shortened in b, e<sup>25</sup>. Further, as Hort remarks<sup>26</sup>, "this explanation involves the extreme improbability that the most familiar form of the Words of Institution, agreeing with St. Paul's record should be selected for omission; while the vaguer, less sacred, and less familiar words, in great part peculiar to Luc, were retained". W. Manson has rightly held Hort's argument here as unassailable<sup>27</sup>.

(b) Despite the fact that this is a passage involving 153 letters, A.C. Clark<sup>28</sup> has tried to explain its absence from the 'Western' Text due to accidental omission in copying. This is clearly an attempt to pin down all types of possible omissions to his stichometric system. But omissions of passages of this magnitude due to a slip of the eye is quite inconceivable (vide on Mk 16:9-20).

But the strongest argument against a mechanical omission is the fact that the passage gives every indication of its having undergone either a deliberate rearrangement of verses (as we noted in the case of b, e and the Old Syriac) or an editorial manipulation in some other way. This is all the more convincing because even scholars who hold opposing views on the genuineness of the passage have arrived at the same conclusion. Benoit who believes the long text to be genuine, explains the omission thus: "Il ne s'agit pas ici d'une corruption accidentelle due à quelque bévue de copiste, mais bien d'un remaniement intentionnel et réfléchi"<sup>29</sup>. Burkitt<sup>30</sup>, on the other hand, believes that the long text is the result of an editorial revision. He maintains that this passage, like the Lord's Prayer, would have been committed to memory by Christian

scribes, so that they would not be entirely dependent upon a 'faulty' exemplar while copying such texts.

(c) According to the famous theory of disciplina arcani, the early Church, anxious to safeguard the mystery of the Eucharist from profanation by the heathen, took official steps to delete the words relating to the Institution. It was, in the words of Kilpatrick, "a cut which the faithful would know how to supplement, but the uninitiated little...The abruptness of the ending is deliberate to preserve the arcanum of the rite"<sup>31</sup>.

The fallacy of this theory is at once clear:

(i) Why should Luke's text be singled out for this special precaution while the more glaringly liturgical text of Paul as well as the texts of the first two Gospels were spared?

(ii) The truncated text would still retain a specific identification of the bread with the body, which would be detrimental to the very purpose of the supposed arcanum! Was there a more suitable grist to the heathen's mill, seeing that one of their early charges against Christians was cannibalism?

(iii) Chadwick<sup>32</sup> so aptly remarks that if the early Church had such scruples about secrecy concerning one of its cardinal tenets, it would hardly have evangelised the world!

(iv) Finally, the evidence of the Old Syriac is another blow to the above theory: "for these MSS were written at a time when the Syriac Church quite certainly regarded the eucharist as a dread mystery, so that the disciplina arcani, had it existed, must surely have worked in favour of retaining the abbreviated reading"<sup>33</sup>.

(d) Schürmann<sup>34</sup> has propounded that the deletion of vv.19b-20 synchronised with the separation of the Eucharist from the Agape consequent to the abuse of the rite by some wayward Christians, based on a misinterpretation of the phrase *μετὰ τὸ δεῖπνῆσαι* to mean that wine could be drunk before the Eucharistic meal (cf. v.17).

This is no doubt a speculative suggestion. First, as Richardson points out<sup>35</sup>, "it is very doubtful whether a division between eucharist and agape existed until late in the second century", evidence being not forthcoming in Justin or the

Didache or Irenaeus. Secondly, the theory does not explain why 1 Cor 11:25 or the accounts in Mt and Mk did not come in for a similar emendation<sup>36</sup>. Finally, the deletion of 19b is not in any case accounted for by Schürmann's explanation<sup>37</sup>.

(e) Tatian's influence has been proposed by von Soden as a possible factor in the shaping of the short text<sup>38</sup>.

We have already seen that the Diatessaron does not provide a conclusive proof for or against the long text. Easton<sup>39</sup> confirms this by noting that the relevant passage in that Harmony, 45:12-16, is made up of Markan and Matthaean material "with a few words at the end that may be from Lk but may equally well be from 1 Cor...This appears to have no bearing on the omission of Lk vv.19b-20".

The picture that has emerged clearly shows that there is hardly any justification for the visualisation of a reading in Lk that could be identified as an original account of the Institution in the normal sense. On the contrary, we have at least a few indications which would possibly suggest that scribal activity was in just the opposite direction,

namely, trying to fill in what was felt to be a gap in the text.

#### Intrinsic Probability

(i) Two difficulties associated with the short reading have already been mentioned incidentally:

(a) If it were original, the extremely awkward abruptness at v.19a cannot be easily ascribed to the original autograph. This is further complicated by the fact that the position of 19a is not the same in all the witnesses, which is a clear indication of scribal interference.

(b) The short text would entail the irregular order cup-bread, so that the long text with its normal order for the Eucharistic elements would suggest itself to be genuine. Those who argue along this line take for granted that the first cup was one of the Paschal cups, as outlined in the Mishnah<sup>40</sup> - or in other words, that Jesus was observing the Passover according to the traditional Jewish pattern<sup>41</sup>. Plummer's contention that we do not have adequate information as to whether the Jewish ritual involving several cups was in vogue at the time of Christ<sup>42</sup> may find little favour among scholars today.

However, as J.A. Robinson cautions us<sup>43</sup>, "our conception of the original institution of the Eucharist must not be dominated by the consideration of the elaborate ceremonial of the Passover celebration". Richardson says<sup>44</sup>: "The truth appears to be that Christian rites only began to be modelled close to Jewish forms in the third century..." On the other hand, Jeremias<sup>45</sup> claims that although the Eucharistic celebration of the early Church was at variance with the Passover ritual, the very fact that the synoptics describe the Last Supper as a Passover meal is proof for its authenticity, being a recollection of firmly established tradition.

It must be pointed out in this connection that there exists a certain degree of confusion among modern scholars about the whole purport of the Synoptic tradition regarding the Last Supper. There is no doubt that Matthew and Luke, following the Markan tradition, have attached at least some Paschal associations with this Supper. It is really immaterial for our purpose as to which chronology of the Supper, Synoptic or Johannine, vis-à-vis the date of the Crucifixion, is accurate<sup>46</sup>. Even if the Last Supper according to the Synoptic reckoning did not coincide with the Jewish calendar, it

is quite clear from the Lukan account that the author did identify it as a Passover meal. Even granting that the Synoptic tradition dates the Last Supper some twenty-four hours before the Passover proper<sup>47</sup>, Jesus Himself is represented as observing it with the full awareness of the implications of that event in Salvation History<sup>48</sup>. Therefore to suggest that the last meal that Jesus had with His disciples was either a qiddush celebration<sup>49</sup> or a habburah meal<sup>50</sup> is to dissociate Him from that history and also from the community in which that history was enacted. Thus W. Manson's comment<sup>51</sup> in this connexion seems to be utterly out of place: "No serious issue is raised by the controversy, but if reasons should exist for holding that the Synoptic reckoning is the result of some confusion of ideas, it is a confusion which is easily explained. Jesus had suffered at the Passover season. The Passover rite was in his thoughts at the time of the institution. He was known to his followers as "our Passover" (1 Cor v.9). What was more natural than that the ordinance which set forth his Passion should come to be regarded as the Christian Passover, and the Supper at which it was inaugurated identified popularly with the Jewish festival?" Surely the confusion is in our modern minds and not in the

minds of the early Christians! This modern confusion is intensified by the problem of having to find a suitable explanation for the two cups presented by the long text in Lk. The solution seems to lie in visualizing Jesus' celebrating a simplified form of the ancient Passover rite with the full import of its symbolic and proleptic nature in relation to Heilsgeschichte<sup>52</sup>. This approach alone would satisfactorily explain the words ascribed to the Lord by Luke in vv.15-18 (vide infra). This is not to detract from the possibility that the Synoptic and Pauline accounts may have been to some extent coloured by the liturgical practice of the early Church. But we are here concerned only with what was in Luke's original autograph.

(ii) There are various linguistic and stylistic factors which must be taken into consideration if we are to do full justice to our author's purpose underlying the present narrative:

(a) The final nominative of τὸ ὑπὲρ ὑμῶν ἔκχυστόμενον (v.20) is clumsy if intended to stand in agreement with αἵματι.<sup>53</sup> "The words appear formally to belong to πρὸ τήριον, but following the analogy of the explication of the words

about the bread, must be joined to  $\alpha\acute{\iota}\mu\alpha$  ", says Dibelius<sup>54</sup>. Chadwick<sup>55</sup>, however, does not consider this to be a serious objection against the genuineness of the words because "one can pour out either a cup or its contents". But Zahn comments as follows:<sup>56</sup> "That  $\alpha$  (sc. long reading) grew out of  $\beta$  (sc. short reading) in such an artificial way is betrayed...by the fact that  $\tau\acute{o}\ \acute{\upsilon}\pi\epsilon\rho\ \acute{\epsilon}\kappa\chi\upsilon\nu\nu\acute{o}\mu\epsilon\nu\omicron\nu$  , v.20, does not in its context suit  $\tau\acute{o}\ \pi\omicron\tau\acute{\eta}\rho\iota\omicron\nu$  nor its wording  $\tau\acute{\omega}\ \alpha\acute{\iota}\mu\alpha\tau\iota$  as apposition. Neither a solecism so bad, and in no way necessary, is to be credited to a Luke, nor the impossible thought that the cup which Jesus hands to the disciples was poured out or shed in their behalf". We may note that from the spiritual point of view there is something to be said in favour of Chadwick's position. But on stylistic considerations, the criticism of Zahn and others has to be taken seriously. Benoit<sup>57</sup> tries to circumvent the difficulty by suggesting Luke's indebtedness to liturgical language and by pointing to some clumsiness of style on his part at Lk 23:34a, Ac 10:37; 26:3. As regards borrowing from liturgy, a scribe is equally entitled to the benefit of the doubt. Tasker raises another objection:<sup>58</sup> "the argument that we cannot expect

to find Lucan idiom where a liturgical formula is being followed may be countered by the observation that tradition was not exact in handing down liturgical matter, either in the N.T. or in MSS of the liturgies". As for the references Benoit cites, the first two involve textual variants, and as such, cannot be confidently held as part of Luke's original texts; the last passage could certainly be claimed to conform to idiomatic style<sup>59</sup> - the present case cannot indeed be treated on a par with the same passage especially in view of its obvious links with 1 Cor and Mk which bespeak a scribe's hand rather than the author's composition. Strangely enough, Kenyon and Legg<sup>60</sup> who have correctly identified the sources of this bit of the long reading in Lk, completely overlook its stylistic difficulty and utilizes its origin as an argument to defend v.20 as original: "If this had come to us in an obviously independent narrative, it would have had weight; but in a narrative compounded from two earlier records...its weight is little or none as against the earlier witnesses. Two independent and earlier witnesses, must, on any critical principles, be given the preference over one later one, who, so far as language is concerned, is dependent wholly upon them". Jeremias<sup>61</sup>, however, ascribes the stylistic

abnormality in v.20b to the Vorlage which Luke is only reproducing faithfully. But V. Taylor<sup>62</sup> characterizes this suggestion as "no more than a scientific guess". In the face of such conflicting views on this point we would do well to await the results of our investigation into other aspects of the case.

(b) Long ago, Blass<sup>63</sup> drew attention to an important stylistic feature of the Third Gospel which seems to have registered little with commentators as a clue to solving the mystery of the present section. He showed how v.18 with the λέγω γὰρ ὑμῖν . . . . . links up naturally with the πλὴν ἰδοὺ . . . . . of v.21 on the analogy of the contrast between 18:8a and 8b as well as 19:26 and 27 (cf. 13:32 and 33).

This in itself may not be a weighty argument against the deletion of vv.19b-20 because there are several instances in Lk where other verses intervene between a solemn statement of Jesus beginning with λέγω . . . ὑμῖν or its equivalent and a subsequent statement by Him beginning with the adversative πλὴν : e.g. 10:18 and 20 (cf. vv.13 and 14); 11:39 and 41 (cf. 12:29 and 31). But the recurrence of the pattern of the contrast in the

first set of instances mentioned by Blass cannot be lightly set aside. It is all the more significant in view of the remarkable parallelism between the *λέγω γὰρ ὑμῖν ὅτι οὐ μὴ φάγω* of v.16 and the *λέγω γὰρ ὑμῖν ὅτι οὐ μὴ πίω* of v.18. Cf. also the phrases *ἕως ὅτου* (v.16) and *ἕως οὐ* (v.18) as well as the repetition of the *βασιλεία τοῦ θεοῦ* . 64

(c) To the best of my knowledge, the conventional order in any language is to mention 'eating' first followed by 'drinking', and not the other way round. Luke himself provides plenty of illustrations of this usage which we listed in connexion with the discussion of 12:19 (cf. 1 Cor 10:3f; 11:21, 22, 26, 27-29, 33f; Didache 10:3). Now v.14 here informs us that the eating is already in progression (cf. *ἀνέπεσεν* ) v.15 serves to supplement this information and to suggest that Jesus Himself partook of the meal<sup>65</sup>. His participation in the meal is unmistakably impressed upon our mind if we read v.15 in the proper perspective of the narrative<sup>66</sup> beginning at v.7. The sole emphasis of the present narrative is on the proleptic fulfilment of the fellowship meal (*φαγεῖν μεθ' ὑμῶν* ) - a fellowship which

is about to be broken temporarily due to the impending betrayal (v.21), but will be consummated in the Kingdom of God (vv.16,18,30)<sup>67</sup>. Thus it is clear that there is a well-knit sequence of thought in the Last Supper narrative in Lk which makes any reference to an institution of the Eucharist out of place. Hence the great significance of Blass's stress on the natural link between vv.18 and 21.

(d) Linguistic objections to the suggestion that Jesus' participation in the actual eating of the meal is conveyed by v.15 are sometimes too hair-splitting. It is not therefore surprising that R.H. Lightfoot<sup>68</sup> characterizes this verse as "the despair of commentators". Bultmann<sup>69</sup>, for instance, without textual evidence, goes to the extent of substituting 'bread' for 'Passover' on the ground that 'eating the Passover' is not idiomatic. Chadwick<sup>70</sup> calls the expression 'odd'. But if 'eating the Passover' in the rather loose sense of eating the Passover meal is not permissible, then 'drinking the cup' (Mk 10:38f par. Mt 20:22f; 1 Cor 11:26; Jn 18:11; cf. Mt 26:39; Lk 22:42; Mt 14:36) is also not permissible<sup>71</sup>. Wellhausen<sup>72</sup> sees no Passover significance here because of the absence of any reference to the Paschal Lamb. But

C.K. Barrett<sup>73</sup> identifies  $\pi\lambda\sigma\chi\alpha$  with the Paschal lamb. J.M. Allegro<sup>74</sup> claims that the Passover Supper had, by Jesus' time, become just one type of the Messianic meal, so that it was not obligatory for a Passover lamb to be eaten on that occasion. Richardson<sup>75</sup> notes that "the words 'to eat this Passover' and 'until it be fulfilled in the Kingdom of God' can hardly refer to less than the whole meal as a type of the Messianic Banquet", while "the words about the cup lack any allusion whatsoever to the Passover". However, standing by the majority view among modern scholars about the Synoptic dating of the Last Supper<sup>76</sup>, it is not difficult to see that Jesus and His disciples could not have in any case availed themselves of a Paschal lamb, especially because it had to be slaughtered in the premises of the Temple according to the religious regulations<sup>77</sup>. Moreover, as Westcott-Hort have cautioned us<sup>78</sup>, "it is needful to remember that authors are not always grammatical, or clear, or consistent or felicitous; so that not seldom an ordinary reader finds it easy to replace a feeble or half-appropriate word or phrase by an effective substitute; and thus the best words to express an author's meaning need not in all cases be those which he actually employed".

(e) The presence of non-Lukan expressions in the disputed passage has been adduced as an argument in favour of its deletion. For example, the use of ὑπέρ with reference to the atoning work of Christ<sup>79</sup>; the rare word ἀνάμνησις (cf. Ac 10:4: μνημόσυνον )<sup>80</sup>; the attributive use of the possessive pronoun ἐμός in contrast to Luke's predicative and pronominal use elsewhere<sup>81</sup>; the order καὶ . . . ὡσαύτως (contrast 5:10,33; 10:32; 22:36; even the isolated instance of the Markan expression at 12:21 has been reversed by Luke at 20:31 in conformity with his style)<sup>82</sup>; the absence of the copula ἐστίν in v.20b (contrast 1 Cor 11:25)<sup>83</sup>; and finally, the presence of the definite article before ποτήριον in v.20 which does not square with Luke's anarthrous use of ποτήριον and ἄρτος in 17a and 19a.<sup>84</sup>

Any of these stylistic points, taken in isolation, may not carry sufficient weight. But collectively, their adverse evidence in respect of such a relatively short passage cannot be easily ignored. What now remains for us is to confirm this evidence by means of other data.

(iii) In what follows, I shall attempt to examine the approaches of P. Winter<sup>85</sup>, Chadwick<sup>86</sup> and Blakiston<sup>87</sup> on the one hand, and H.N. Bate<sup>88</sup> on the other and to evaluate the relative merits of these two types of solutions. Since probably more literature has been produced on the textual problem of this passage than that of any other N.T. text, one cannot pretend to be original on all aspects of the topic. I shall, however, try to bring my own judgments to bear on each individual issue as it arises in the debate.

The basic premise from which I approach this problem is that of Blass<sup>89</sup>: "We must find an original text which was responsible for inviting such a whole mass of variations. Luke is not to be supposed to have given a mutilated account, but either no account at all or a complete one of the Lord's Supper".

Like Blass, P. Winter rightly detects the crux interpretum to be an explanation for the possible origin of the enigmatic v.19a. Winter's solution may be summarised as follows: Lk 22:14-19a is composed of three strands: (a) a pre-Markan source (vv.14-17); this is of Semitic origin as is evidenced by the "Semitic colouring" of vv.15f

(cf. ἐπιθυμία ἐπεθύμησα ); (b) v.18,  
derived from Mk 14:25; (c) v.19a, derived from  
Mk 14:22.

Luke's special source also contained a  
section parallel to vv.14-19a ending at ἔδωκεν  
αὐτοῖς , but lacking the rest of v.19<sup>90</sup>.  
This source may be supposed to have included a  
mention of the blessing and passing around of the  
bread on the pattern of the description of the cup  
at v.17. Winter argues that the Evangelist may  
have exchanged the phraseology of his L source for  
the Markan one, and unwarily retained the words  
beginning at λέγων and ending at σῶμα μου  
little realizing that there was no counterpart to  
this in the preceding cup section.

The weakness of this argument involving  
a naïve manipulation of sources by Luke is that  
while it explains the abruptness of the reference  
to the 'body', it takes for granted that Luke's  
original text, derived from L, did include a thanks-  
giving for and distribution of the bread. Winter's  
explanation for the apparently reverse order of  
the Institution is that he had preserved a very  
early tradition which conformed to the formula of

the Jewish grace at meals where the thanksgiving for 'the fruit of the vine' is said before that of 'the bread from the earth'. However, in the end, his solution does not remove the stigma of a 'mutilated account' of the Lord's Supper.

Lampe<sup>91</sup>, while admitting the possibility of the short reading being original, meets this problem in a spirit of resignation and says: "Luke seems indifferent to the order of the bread and cup, perhaps because in his view the normal eucharist is merely the 'breaking of bread', or because liturgical practice varied". Three points emerge from the above statement, namely; that Luke had an account of the Institution; that 'breaking of bread' was synonymous with the Eucharist in the view of the author; and that liturgical practice so varied from place to place as to justify the cup-bread order. All these are moot points. A full debate of them would be beyond the scope of this thesis. Suffice it to mention here that once we concede that Luke did not originally have an institution of the Eucharist, the other two points would become irrelevant; and this is precisely the line that is being pursued in the argument that will follow presently. Even at this stage it may be worth mentioning that Blass<sup>92</sup> has shown how the Institution was not within Luke's

plan of the Gospel by pointing to the latter's omission of Mark's report (14:20) about Jesus' disclosure of the identity of the betrayer ('one who is dipping bread in the same dish with me').

As regards the connotation of 'breaking of bread', E.J. Kilmartin<sup>93</sup> would argue that 'though it seems reasonable to say that "breaking of bread" was the characteristic name given to the primitive Eucharistic celebration, this does not imply that the cup was placed in the background of the early liturgical service". Just the opposite seems to have been true!" A. Wright<sup>94</sup> has more convincingly argued that 'breaking of bread' is in any case not an appropriate description of the type of meal that Jesus had in the company of His disciples on the eve of His Crucifixion. The least acceptable explanation is that proposed by Easton<sup>95</sup>. He thinks that the short Lukan account which goes back to the source originated from liturgical practice. The extreme frequency of 'the breaking of bread', coupled with the high cost of wine, finally gave rise to a custom of wineless Eucharists, "a custom that would be certain to lead to the belief that the original eucharist was wineless. So v.18 may even be meant as a protest against what was thought a luxurious custom; such an attitude would be quite in keeping

with the character of L, with which Lk no doubt sympathized". It is incredible that Easton should make this kind of conjecture with regard to the possible link between wineless Eucharists and v.18, seeing that there is an express mention of the use of wine not only in v.17, but also in the first two Gospels and in 1 Cor in connection with the Last Supper!

A. Wright<sup>96</sup> explains the 'reversal' of the order of the 'elements' in the short text in Lk as "the reflex action of elastic liturgies upon the oral Gospel". He thinks that in the provenance of the Third Gospel the cup was administered before the bread and cites 1 Cor 10:16ff and Didache 9:2f in support. But M.A. Smith<sup>97</sup> has demonstrated that the assumption of the influence of liturgy on the Biblical text is baseless: "In conclusion I can state confidently that in the East the liturgical text is secondary to the biblical text, and gradually diverges from it as time proceeds, first by harmonization, then by addition. There is some secondary influence of the biblical text in late MSS by reminiscence. In the West the two processes are more closely interwoven, but the same is generally true. The liturgical form may affect an individual

MS, but there is no evidence that the habits of the worshipping community have materially altered the original recoverable biblical text". Further, the reference to 1 Cor 10:16ff is too weak a basis to establish that the reverse order of administration of the elements was in practice at least in Pauline circles in view of Paul's unambiguous statement to the contrary in 11:23ff<sup>98</sup>. As for the bearing of Didache 9:2f on the earliest stage of Eucharist practice, Lietzmann,<sup>99</sup> from a comparative study of the references to the 'breaking of bread' in Ac 2:42,46; 20:7,11; 24:30,35, the Clementine writings and the Acts of Peter, has shown that "there existed a form of the rite which contained only the consecration of the bread, and knew nothing of the wine. The Lord's Supper...began with the breaking of bread and a communion of the fragments; upon this followed a common meal for the assembly, now attuned to gladness...

"Doubtless through the influence of the Pauline type, or of the customary Jewish practice, the cup came to be used in conjunction with the bread and was commonly filled with wine. Thus there developed the form which we have in Didache".

While accepting the historical reliability of Lietzmann's extensive research on this point, it must be mentioned that we are still left in the dark as to why Luke deviated from both Paul and Mark (= Matthew) in representing the first Eucharist in this fashion<sup>100</sup>. Even Richardson's critique<sup>101</sup> of Lietzmann's survey does not come up with a better alternative: "For Luke, the significance of the Supper-rite was centred in the breaking of bread; but on occasions like the first day of the week, when Christians were gathered into an "upper room", the drinking of a cup - not necessarily, but normally - came first. That Luke sets these two actions at the very beginning of his account of the Last Supper, whereas in Mark the sacral moments are within the meal itself, reveals in Luke an unmistakable linking of Christian procedure to a Jewish model - that of the Sabbath-supper".

All the above explanations are, in a way, desperate solutions to defend the presence of the appendix-like v.19a which has excellent MS support. The above scholars should be given due credit for their sincerity. (The External Evidence does not seem to bother Blass!) One can very well sympathize with Jeremias' indignation<sup>102</sup> when he decries the deletion of 19a by modern editors as an "outrageous

and inadmissible act". According to Chadwick<sup>103</sup>, it is Luke's misunderstanding of the expression 'to eat the Pascha' in v.15 (found originally in L) that accounts for his retention of 19a. Luke had not realised that v.15 actually did include the bread by implication and so inserted the reference to the 'bread' from His Markan source. He also wanted to preserve the parallelism of vv.15-16 and 17-18 without disrupting the Markan material. This, Chadwick assures us, was in conformity with Luke's characteristic respect for his source-material. "On this interpretation of the evidence it was the third evangelist himself who initiated a long development of correction and expansion. Luke not merely opened the door for the supplement which added the second cup, he made its eventual addition inevitable".

In arriving at this solution, Chadwick is conscious of the fact that one has "to presuppose an excessively stupid evangelist" (!) However, he defends his hypothesis thus<sup>104</sup>: "But it may be freely admitted that it raises sharply the all too familiar issue whether the third Gospel is to be regarded as the delicate work of a fine theological artist to whom creative inspiration may be readily

attributed, or whether it is rather a literary Pisa Cathedral in which many pre-existing bits and pieces have been put together for admitted theological purposes to form a coherent structure, which, nevertheless, is not so coherent as to make it quite impossible to discern some of the original blocks that have gone to its making". In other words, Chadwick reduces Luke to the level of one who has simply performed a scissor-and-paste operation. But even if this unseemly delineation be conceded, one must remember that the scissor-and-paste method itself cannot be done in a mere mechanical way without some pre-planned design. A Pisa-Cathedral necessarily bespeaks a genuine artist behind it. It is true that here and there Luke's twin works betray their sources. But anyone who studies Lk-Ac objectively cannot fail to be impressed by the author's personality and intellectual abilities as historian and author. It is pertinent to quote Chadwick himself<sup>105</sup>: "Modern writers tend, if anything, to suffer from exaggerated notions of the lack of intelligence discernible in early Christian literature; but even when the most generous allowances are made I cannot escape the feeling that this is over-subtle. Moreover, it seems to presuppose that the early Christians were self-conscious about the 'words of

consecration' in a way that later Christians have come to be. It is an easy assumption for minds accustomed to the use of the Roman Canon or the Book of Common Prayer, but it is a false perspective if it is read back into the second century". This is a well-thought-out point. Chadwick should have changed the phrase 'second century' to 'first century', and his statement would then have been readily applicable to St. Luke.

Let us proceed along this guide-line steering clear at the same time of the very violation of his precept which Chadwick has unwittingly demonstrated in the above article. But before we do that, let us examine another contribution to the present textual problem, namely that of Blakiston<sup>106</sup>. He begins by recognizing the difficulties of both the short and the long Lukan readings and denies that either of them has a claim to originality. Particularly, the presence of the additional details found at 22:15,16 and 21 in comparison to the other two Synoptists calls for a complete explanation of the origin of the Lukan account of the Last Supper. His solution<sup>107</sup> consists in regarding the entire section 22:14-23 as belonging not to the Synoptic tradition, but to "a deliberate, though intentionally

incomplete conflation of two distinct, independent, and perhaps equally original narratives of the Institution". These are designated by him as L and S and he goes on to reconstruct these two narratives which ultimately gave rise to the various accounts of the Institution, Blakiston's presuppositions<sup>108</sup> are given below:

L = Luke's special narrative =  
recollections of a disciple  
of the Lord, possibly John.  
= Lk 22:14-18, 21 + Mk 14:25 +  
Mt 26:29.

S = Narratives common to Paul and  
the Synoptics = Peter's recol-  
lections of our Lord's words.  
= S<sup>P</sup> (= Pauline narrative) + S<sup>m</sup>  
(= Markan/Matthean account of  
the Institution).  
= (Lk 22:19, 20 par. 1 Cor 11:24f) +  
(Mk 14:22-24/Mt 26:26-28).

1. "Paul's version of the Institution is the oldest in its present form and also the simplest. It appears to be a slightly condensed form of S, as quoted from memory; S may have been in St. Paul's time not a document at all, but an oral narrative incorporated in an inchoate liturgy.

2. "Mark gives S from a document, after it has absorbed a fragment of L probably from oral tradition and after it has been affected by the liturgical formulae of the Eucharist.

3. "Matthew uses Mark or his documentary authority with considerable freedom.

4. "Luke must have been acquainted with both L and S. It is probable that he used both 1 Cor. 11 and Mark. He must also have come across L which he conflated with these narratives with a minimum of alteration (e.g. omitting words actually common to both and the retention in its proper place only of the sentence about the fruit of the vine which had strayed from L to S). He preserved both the phrases about the Tradition of the Cup because they were different; he omitted the phrase about the Breaking of the Bread because it was the same, though it is difficult to see why he made the omission in the first rather than in the second part of his compound narrative".

From the foregoing it is clear that Blakiston's approach is highly conjectural from start to finish. Its only merit, as he points out, is that it "would fall in with the view, which I believe to be the only one consistent with the statements of the Gospels

taken as a whole, that the Last Supper was an anticipated Passover, and that the Lord's Supper is a dematerialized perpetuation of the Paschal sacrament"<sup>109</sup>. But he admits that this theory has been proposed with a view to soliciting corroboration from patristic or other data in order to redeem Textual Criticism from the possible charge of being incapable of solving serious textual difficulties as the present one. But the fact that his assumptions have been received with very little enthusiasm is proof of their weak foundation<sup>110</sup>.

Enough has been said to highlight the extreme difficulty of explaining even the short text in Lk relating to the Last Supper. There is only one real alternative to the desperate or highly conjectural solutions we have examined. This was proposed by H.N. Bate<sup>111</sup> as early as 1927. In my judgment, this is the best approach to an extremely complicated textual problem. While I welcome some of the by-products of the research on the Last Supper passage in Lk, I have no hesitancy in saying that much of the literature produced by way of solving the riddle of the text is really off the mark. But it is surprising that Bate's proposal has not yet received the attention it deserves.

The essence of Bate's approach is that it lays the charge of misunderstanding of the reference to 'eating the Pascha' on an early scribe and not on an intellectual like Luke. Even if we may not give assent to all his views<sup>112</sup>, any fair solution to our problem should take his methodology into account.

One of the basic assumptions of Bate is this: "The shorter text ought not to be thought of as narrating a complete eucharistic Institution in an abnormal order. Its difficulty is not one of order but of redundance"<sup>113</sup>. This is, of course, in direct antithesis to Hort's position<sup>114</sup> which has been followed by the defenders of both the short and long readings. Hort has justified the phenomenon of the reversal of order of the Synoptic material by Luke on the strength of the change of sequence in the Temptation story<sup>115</sup> (4:5-8, 9-12 cf. Mt 4:8-10, 5-7). But the analogy cannot be really pressed until we are decided as to whether Luke did have a narrative of the Institution.

Another basic premise from which Bate proceeds is that "there is a direct road from the shortest of all the possible 'short forms' to any of the longer ones"<sup>116</sup>. This shortest form of text

he finds in e which is the most primitive text we possess. This view is held by Chadwick himself<sup>117</sup>; "...it is highly probable that the earliest form of the shorter text consisted of 15-19a and that the text of it <sup>b e</sup> represents the first stage of a process of correction and rationalization which is completed in the Old Syriac texts...In any case the attestation of the shorter text goes back to a very early date... and is found to be current in Western Europe, Africa and Syria. On grounds of both time and space its witnesses command respect". Bate strikes the key-note of his solution when he states<sup>118</sup>: "In order to explain the other and later forms we are forced to go back behind all extant manuscript authority, and to discern behind e a text in which there was no explicit eucharistic reference at all. Such a text must have seemed from the first to cry out for amplification".

Bate's reconstruction of the original text is based on a comparative study of the readings of D and e at 19a. The basic difference between the two is that whereas D has the words *καὶ λαβὼν ἄρτον* . . . . . τὸ σῶμά μου after the clause *ἕως ὅτου ἡ βασιλεία τοῦ θεοῦ ἔλθῃ,* e has the same words appended to the clause: *ἕως ὅτου πληρωθῇ ἐν τῇ βασιλείᾳ τοῦ θεοῦ.*

This difference may have arisen due to an early gloss<sup>119</sup>. Bate quotes C.H. Turner<sup>120</sup> in support of this surmise: "...one might suppose that the first change was that a very early annotator put the *καὶ λαβῶν . . . . σῶμά μου* clause between two columns of his papyrus roll, and that e's ancestor copied it into his copy in a left hand column of the text, D's ancestor into the column to the right, that is at a later point". Bate thinks that the wording of the gloss at 19a must have originated from Mk<sup>19</sup> rather than from Mt or 1 Cor 11 because of the following reasons: a) the words *ἔδωκεν αὐτοῖς* appear in Mt as *δοῦς τοῖς μαθηταῖς* and *ἄτε* absent from 1 Cor 11; b) with its omission of *λάβετε*, 19a is closer to the Markan text of k which Turner believes to be the authentic form.

Thus after having identified 19a as a gloss and giving a plausible explanation for its entry into the Lukan text, Bate reconstructs the original reading of 22:15ff as under<sup>121</sup>:

15: *καὶ εἶπεν πρὸς αὐτούς· ἐπιθυμίᾳ ἐπεθύμησα  
 τοῦτο τὸ πᾶσα φαγεῖν μεθ' ὑμῶν πρὸ τοῦ με παθεῖν.  
 16 λέγω γὰρ ὑμῖν ὅτι οὐ μὴ φάγω αὐτὸ ἕως ὅτου  
 πληρώθῃ ἐν τῇ βασιλείᾳ τοῦ θεοῦ.*

- 17 καὶ δεξάμενος ποτήριον εὐχαριστήσας εἶπεν·  
 λάβετε, διαμερίσατε εἰς ἑαυτοὺς.
- 18 λέγω γὰρ ὑμῖν ὅτι οὐ μὴ πίω ἀπὸ τοῦ νῦν  
 ἀπὸ τοῦ γενήματος [ τοῦτου ] τῆς ἀμπέλου  
 ἕως οὗ ἢ βασιλείᾳ τοῦ θεοῦ ἔλθῃ.
- 21 πλὴν ἰδοὺ χεὶρ τοῦ παραδίδόντός με . . . . .<sup>122</sup>

This is indeed a neat solution and meets the criticism of Blass about the alien nature of 19a in a satisfactory manner. The elimination of 19a had been anticipated by Wellhausen<sup>123</sup> and E. Meyer<sup>124</sup> also. Although this solution also has to do with some conjecture it is minimal and not so wild as the ones we saw earlier; it also respects the MS tradition, but at the same time avoids the temptation to be carried away by the numerical strength of the MSS. There is in fact no undue stretch of imagination demanded of us. When we remember that the passage goes back to a period of wild growth of texts, there is nothing inexcusable about resorting to a reasonable degree of conjecture.

M. Kiddle<sup>125</sup> has convincingly argued that in the Passion narrative Luke never states or even implies that Jesus died on behalf of anyone. This, Kiddle avers, is quite in keeping with Luke's consistent endeavour to avoid any reference to a sense

of tragedy pertaining to the death of Jesus and to insist that the death was significant only insofar as it was the necessary prelude to the Resurrection (24:26). "In editing this part of his gospel, as elsewhere, he was dominated by the conviction that the Church had already won the essential fruits of victory, and that its destiny was completely under the control of God, and that the death of Jesus had been vindicated by the glory of the Resurrection and the gift of the Holy Spirit. For Luke 'the strife is o'er, the battle done'. He sees no tragedy in the good news about which he writes"<sup>126</sup>. Applying this theological insight about the Third Gospel to the present passage, Kiddle vigorously defends the short text on the ground that Luke was determined to stick to his principle and to avoid an institution of the Eucharist<sup>127</sup>. Although Kiddle has no suggestion regarding the removal of 19a he does make his point convincing as far as the elimination of 19b-20 is concerned<sup>128</sup>.

Creed<sup>129</sup> is very much appreciative of Bate's suggestion regarding the deletion of 19a. "The omission of these words would effect a great simplification in the narrative. We should then have two parallel sayings referring: (1) to the eating of the Paschal victim; (2) to the drinking

of the wine, in each case with an anticipation of the fulfilment of the Passover in the coming Kingdom". But at the same time he demurs on the ground that "the case for their rejection seems not to have been made out". Apart from the lack of MS evidence, Creed's main objection to Bate's solution is based on Luke's preference for 'the breaking of bread'. He thinks that if the phrase had stood in the Markan source, Luke is not likely to have rejected it. But this objection cannot stand when we rule out on theological grounds that Luke had any intention of giving an account of the Institution at all. Any suggestion that vv.17,18 contain "an anticipatory reference to the Institution of the Cup"<sup>130</sup> is not tenable in the light of the well-pronounced parallelism between vv.16 and 18 (vide supra).

On the whole, I should think that a solution which completely eliminates vv.19-20 deserves a better hearing than those aimed at defending either the most complicated long text<sup>131</sup> or even the strange appendix, 19a. J. Moffatt<sup>132</sup> has done well in grouping Lk 22:19-20 in the company of such widely acknowledged spurious texts as 1 Jn 5:7-8; Jn 7:53-8:11 and Mk 16:9-20.

## THE TEXTUAL DATA

(Cf. Kenyon & Legg, The Ministry and the Sacraments, pp. 284f)

NB &c.	Dad fil	be	Syr. cu.	Syr. sin.	Syr. resh.
<p>17. καὶ δεξάμενος ποτήριον εὐχαριστήσας εἶπεν, λάβετε τούτο καὶ διαμερίσατε εἰς ἑαυτοὺς.</p> <p>18. λέγω γὰρ ὑμῖν, οὐ μὴ πῖω ἀπὸ τοῦ νῦν ἀπὸ τοῦ γενήματος τῆς ἀμπέλου ἕως οὗ ἡ βασιλεία τοῦ θεοῦ ἔλθῃ.</p> <p>19. καὶ λαβὼν ἄρτον εὐχαριστήσας ἔκλασεν καὶ ἔδωκεν αὐτοῖς, λέγων, τούτο ἐστὶν τὸ σῶμά μου τὸ ὑπὲρ ὑμῶν διδόμενον· τοῦτο ποιεῖτε εἰς τὴν ἐμὴν ἀνάμνησιν.</p> <p>20. καὶ τὸ ποτήριον ὡσαύτως μετὰ τὸ δειπνῆσαι, λέγων, τούτο τὸ ποτήριον ἡ καινὴ διαθήκη ἐν τῷ αἵματί μου, τὸ ὑπὲρ ὑμῶν ἐκχυνόμενον.</p>	<p>17. καὶ δεξάμενος τὸ ποτήριον εὐχαριστήσας εἶπεν, λάβετε τούτο, διαμερίσατε ἑαυτοῖς.</p> <p>18. λέγω γὰρ ὑμῖν ἀπὸ τοῦ νῦν οὐ μὴ πῖω ἀπὸ τοῦ γενήματος τῆς ἀμπέλου ἕως οὗ ἔλθῃ ἡ βασιλεία τοῦ θεοῦ.</p> <p>19. καὶ λαβὼν ἄρτον εὐχαριστήσας ἔκλασεν καὶ ἔδωκεν αὐτοῖς, λέγων, τούτο ἐστὶν τὸ σῶμά μου.</p>	<p>19. καὶ λαβὼν ἄρτον εὐχαριστήσας ἔκλασεν καὶ ἔδωκεν αὐτοῖς, λέγων, τούτο ἐστὶν τὸ σῶμά μου.</p> <p>17. καὶ δεξάμενος τὸ ποτήριον εὐχαριστήσας εἶπεν, λάβετε (τούτο, om. ε) διαμερίσατε εἰς ἑαυτοὺς.</p> <p>18. λέγω γὰρ ὑμῖν (ὅτι, om. ε) ἀπὸ τοῦ νῦν οὐ μὴ πῖω ἀπὸ τοῦ γενήματος (+ τοῦτου δ) τῆς ἀμπέλου (+ ταύτης β) ἕως οὗ ἔλθῃ ἡ βασιλεία τοῦ θεοῦ.</p>	<p>19. καὶ λαβὼν ἄρτον εὐχαριστήσας ἔκλασεν καὶ ἔδωκεν αὐτοῖς καὶ ἔλεγεν, τούτο ἐστὶν τὸ σῶμά μου τὸ ὑπὲρ ὑμῶν· τοῦτο ποιεῖτε εἰς τὴν ἐμὴν ἀνάμνησιν.</p> <p>17. καὶ δεξάμενος τὸ ποτήριον εὐχαριστήσας εἶπεν, λάβετε τούτο, διαμερίσατε εἰς ἑαυτοὺς.</p> <p>18. λέγω ὑμῖν ὅτι ἀπὸ τοῦ νῦν οὐ μὴ πῖω ἀπὸ τοῦ γενήματος τούτου τῆς ἀμπέλου ἕως οὗ ἔλθῃ ἡ βασιλεία τοῦ θεοῦ.</p>	<p>19. καὶ λαβὼν ἄρτον εὐχαριστήσας ἔκλασεν καὶ ἔδωκεν αὐτοῖς λέγων, τούτο ἐστὶν τὸ σῶμά μου τὸ ὑπὲρ ὑμῶν διδόμενον· τοῦτο ποιεῖτε εἰς τὴν ἐμὴν ἀνάμνησιν.</p> <p>20<sup>a</sup>. καὶ μετὰ τὸ δειπνῆσαι,</p> <p>17. δεξάμενος τὸ ποτήριον εὐχαριστήσας εἶπεν, λάβετε τούτο, διαμερίσατε εἰς ἑαυτοὺς.</p> <p>20<sup>b</sup>. τοῦτο ἐστὶν τὸ αἷμά μου ἡ διαθήκη ἡ καινὴ.</p> <p>18. λέγω γὰρ ὑμῖν ὅτι ἀπὸ τοῦ νῦν οὐ μὴ πῖω ἀπὸ τοῦ γενήματος τούτου ἕως οὗ ἔλθῃ ἡ βασιλεία τοῦ θεοῦ.</p>	<p>19. καὶ λαβὼν ἄρτον εὐχαριστήσας ἔκλασεν καὶ ἔδωκεν αὐτοῖς καὶ ἔλεγεν, τούτο ἐστὶν τὸ σῶμά μου τὸ ὑπὲρ ὑμῶν διδόμενον· τοῦτο ποιεῖτε εἰς τὴν ἐμὴν ἀνάμνησιν.</p> <p>20. καὶ ὡσαύτως καὶ τὸ ποτήριον μετὰ τὸ δειπνῆσαι, λέγων, τούτο τὸ ποτήριον ἡ καινὴ διαθήκη ἐν τῷ αἵματί μου τὸ ὑπὲρ ὑμῶν ἐκχυνόμενον.</p>

<sup>1</sup>Against Metzger, *Textual Commentary*, pp.173f, 176; F.W Beare, *The Earliest Records of Jesus*, p.224; Fitzmeyer, *CBQ* 24 (1962), pp.170-79; J. Jeremias, *ExpT* 83 (1971-72), p.200.

<sup>2</sup>*NTS* 12 (1965-66), pp.202f. Aland's view with regard to the Old Syriac contrasts with that of Burkitt who thinks that *Syr<sup>C</sup>* approximates better to the original than *Syr<sup>S</sup>* while neither of these MSS is based on the long text (*JTS* 28 (1926-27), p.179): "It must be noticed that no genuine Old Latin MS, and neither C nor S has a text of Luke xxii that speaks of the two cups... In such cases where these anti-Nicene witnesses support D over against the mass of Greek MSS, the external authority is weak"; Cf. C.S.C. Williams, *Alterations*, p.50.

<sup>3</sup>*RB* 48 (1939), p.359; cf. C.S.C. Williams, *op.cit.*, p.49.

<sup>4</sup>Supplementary Essay to H. Lietzmann's *Mass and Lord's Supper* (E.T.), Fascicle 4, pp.216ff.

<sup>5</sup>*Ibid.*, p.236.

<sup>6</sup>*Ibid.*, p.252; contrast Lagrange, *Critique Textuelle*, II, p.69.

<sup>7</sup>*Ibid.*, p.239.

<sup>8</sup>Even Benoit (*op.cit.*, p.360) who is impressed by the MS evidence for the long text, is prepared to admit that both the long and short readings have equal claim to antiquity in that Internal Evidence is a more reliable clue in our investigation.

<sup>9</sup>See Metzger, *op.cit.*, p.176; Tasker, *The Greek NT*, *ad.loc.*

<sup>10</sup>*Notes*, p.64.

<sup>11</sup>*Ibid.*

<sup>12</sup>So Metzger, *op.cit.*, pp.176f.

<sup>13</sup>Cf. J. Jeremias, *Eucharistic Words* (E.T.), pp.143f. Jeremias is convinced that Syr<sup>S</sup> is derived from Syr<sup>C</sup> because of the identity of their readings except in the expanded parts of the former. He has convincingly argued that both Old Syriac versions are to be regarded as expanded short texts rather than as having been derived from the long text.

<sup>14</sup>The five additions in Syr<sup>S</sup> are as follows (see *ibid.*, p.143; p.144, n.1): (a) The particle 7 before the bread-word, being the introduction of direct discourse; (b) 'I give' in reference to the body. Burkitt (*Evangelion*, II, p.301) regards Jn 6:51 as the source for this. But Jeremias rightly thinks that it is a stylistic addition; (c) 'after they had eaten supper' (from 1 Cor 11:25) introducing the cup; (d) 'this is my blood, the new covenant' (from Mt 26:28a). See Aphraates 221; Burkitt, *loc.cit.*, Jeremias, *op.cit.*, p.141, n.2; (e) 'For' at the beginning of v.18 (from Mt 26:29). These data are enough to prove the contention of the previous note.

<sup>15</sup>Hort, *op.cit.*, p.63; cf. R.D. Richardson, *Supplementary Essay to Lietzmann's Mass and Lord's Supper*, fascicle 4, p.232; T.W. Manson, *JTS* (n.s.) 1, (1958), p.202; Chadwick, *HTR* 50 (1957), p.249. I am surprised that Jeremias (*loc.cit.*) makes use of the same evidence to assert that "the attestation speaks decisively for the originality of the Long Text". He is, of course, in good company. See Kenyon and Legg, *Ministry and Sacraments*, pp.280ff; M. Goguel, *Life of Jesus* (E.T.), pp.458ff; Aland, *loc.cit.*; cf. Tasker, *The Greek NT*, *ad.loc.*; In giving the arguments in favour of the long text he writes: "The numerous and widely divergent transpositions in MSS and Versions may all be readily understood as 'more or less clumsy surgery' designed to force Luke's account into the proper liturgical pattern; this presupposes the 'longer' text". On the other hand, Jeremias, *ExpT* 83 (1971-72), pp.196ff, maintains that whereas Lk 22:19-20 reflects a narrative style, the corresponding Corinthian passage bespeaks a liturgical style, especially at the beginning ('The Lord Jesus'). His inference is that both Paul and Luke are drawing upon a pre-Pauline formula whose text was not completely fixed; Cf. *Eucharistic Words* (E.T.), p.104.

<sup>16</sup>Cf. Jeremias, *Eucharistic Words* (E.T.), p.144.

<sup>17</sup>Richardson, *loc.cit.*, defending the originality of the short text on the basis of the readings of Old Syriac and b e, says: "... there is little difficulty in thinking that the two texts effected the same transposition independently".

<sup>18</sup>TSK, 1896, p.734.

<sup>19</sup>TSK, 1898. pp.135f.

<sup>20</sup>Against Torrey, *Our Translated Gospels*, pp.126-28, who holds that the long text, as it stands in the Greek had already achieved this form as a conflation in the Aramaic source. The awkwardness of the reading is explained by him as being due to Luke's mechanical reproduction of his source-material.

<sup>21</sup>TSK, 1896, p.734; *Philology*, p.180.

<sup>22</sup>*Op.cit.*, pp.285f.

<sup>23</sup>*Op.cit.*, p.174; cf. Kenyon and Legg, *loc.cit.*

<sup>24</sup>So Loisy, *Luc*, p.511; cf. Easton, *Luke*, p.321.

<sup>25</sup>Cf. Easton, *loc.cit.* This is, however, easily understandable if we admit of a lacuna at this point in the original.

<sup>26</sup>*Op.cit.*, p.63; contrast Lagrange, *Critique Textuelle*, II, p.69, who says that since 19b is absent from Mk and Mt, it was naturally suspect as being a gloss from 1 Cor 11:24; this might be, in his opinion, the cause of omission of 19b. But he is actually silent on 20.

<sup>27</sup>*Jesus the Messiah*, p.137.

<sup>28</sup>*Primitive Text*, pp.77f; cf. H.I. Bell, *JTS* 3 (1952), p.262.

<sup>29</sup>*Op.cit.*, pp.359f.

<sup>30</sup>JTS 28 (1926-27), pp.178ff. His theory about the 'Western non-interpolations' in the closing chapters of Lk has been discussed in another context (vide on 24:51f).

<sup>31</sup>JTS 47 (1946), p.53; cf. Jeremias, *Eucharist Words* (E.T.), p.158.

<sup>32</sup>HTR 50 (1957), p.254.

<sup>33</sup>Richardson, *op.cit.*, Fasc.5, pp.261-4; contrast Jeremias, *op.cit.*, pp.106-37. V. Taylor, *Passion Narrative*, p.56 and n.3 vigorously supports Jeremias and says that "Jeremias has greatly extended the evidence". But this defence is not tenable in light of Richardson's arguments against the theory as a whole.

<sup>34</sup>Bib 32 (1951), pp.539ff.

<sup>35</sup>*Op.cit.*, p.271.

<sup>36</sup>Cf. Chadwick, *op.cit.*, p.253.

<sup>37</sup>Cf. Lampe (*Peake's Commentary*), p.840.

<sup>38</sup>*Schriften des N.Ts.*, I, p.1571.

<sup>39</sup>St. Luke, pp.321f.

<sup>40</sup>Cf. Strack-Billerbeck, II, pp.844ff.

<sup>41</sup>Cf. M.-J. Langrange, *L'Evangile de Jésus-Christ*, pp.495-7; W. Farrar, *St. Luke*, p.326. Easton, *St. Luke*, p.326; Jeremias, *Eucharistic Words* (E.T.), pp.15-88, especially, pp.84-8. But Hort, *op.cit.*, p.64, points out the difficulty of this assumption: "The identification involves, however, a startling displacement both of the only command to drink or receive recorded by Lc in connexion with a cup, and of the declaration λέγω ὑμῖν, οὐ μὴ πίω κ.τ.λ. attached to the Institution of the Cup by Mt and Mc: divorcing them from the Institution itself and transferring them to the time of the rites preparatory to the Supper".

<sup>42</sup>HDB iii, p.148; contrast Easton, loc.cit.

<sup>43</sup>Enc Bib, col. 1419, 1, §4.

<sup>44</sup>Op.cit., fasc.5, pp.267f.

<sup>45</sup>Op.cit., p.62.

<sup>46</sup>Dalman, Jesus-Jeshua (E.T.), pp.86-184 and Strack-Billerbeck, II, pp.812-53 defend the Synoptic dating while most scholars favour the Johannine chronology.

<sup>47</sup>Cf. W. Manson, Jesus the Messiah, p.139.

<sup>48</sup>Cf. H.E.D. Blakiston, JTS 4 (1902-3), pp.548-55; cf. Easton, op.cit., p.320; Jeremias, op.cit., p.88; contrast Goguel, op.cit., p.430.

<sup>49</sup>So W.O.E. Oesterley The Jewish Background of the Christian Liturgy, p.175: This was a sanctification meal in preparation for the sabbath or a feast day.

<sup>50</sup>So R. Otto, The Kingdom of God and the Son of Man, pp.278-80: This was a fellowship meal, commonly observed among friends. For a refutation of the views of Oesterley and Otto, see Jeremias, op.cit., p.29, n.7 and p.30, n.10. There is a growing tendency to associate the Last Supper with either of these Jewish celebrations without stating a preference. Cf. W. Manson, loc.cit., W. Bowman (Peake's Commentary), p.745. J.A. Robinson (loc.cit) associates it with ordinary Jewish meals.

<sup>51</sup> loc.cit.

<sup>52</sup>Against Lietzmann, Mass and Lord's Supper (E.T.), fascicle 3, pp.172ff, who completely rules out any paschal significance for the Synoptic tradition of the Supper. Also, Richardson, op.cit., p.308, n, comments on this Lukan section thus: "For his introductory section, verses 7-13(14), Luke was without question dependent upon M<sub>C</sub> xiv 12-16(17), and was therefore committed to presenting the Last Supper as a Passover meal. But his tradition of the Supper itself, judged by his words concerning the sharing of the cup and the bread, lacked

Passover connotations. Apparently, therefore, when he represented Jesus as not actually partaking of the Passover but as, instead, making forward references to the Messianic Supper and the Christian rite, he was forging a link between the two traditions" (italics mine). This position is also not acceptable because the 'forward references' have their raison d'être only in the historical background of the concept of the Messianic community. For a balanced estimate of the problems involved, see J. Reumann, *Jesus in the Church's Gospels*, pp.319ff; Cf. also Behm, TDNT, ed. Kittel, III, pp.730ff; Margoliouth, *Exp.T.* 35 (1923-24), pp.412ff.

<sup>53</sup> So Chadwick, *op.cit.*, p.252.

<sup>54</sup> *From Tradition to Gospel* (E.T.), p.210.

<sup>55</sup> *Loc.cit.* For a similar position, see H.G. Goetz, *TSK* 108 (1937), pp.90f.

<sup>56</sup> *Introduction*, III (E.T.), p.41; cf. J.A. Robinson, *Enc Bib II*, col. 1419, §2. He refers to the present wording as "an awkward juxtaposition of words from 1 Cor. with words from Mk...": "it is difficult to ascribe this to so careful a writer as Lk". Cf. V. Taylor, *Passion Narrative*, p.57; Nestle, *Introduction* (E.T.), pp.276f; Richardson, *op.cit.*, pp.258f.

<sup>57</sup> *Op.cit.*, p.362 and n.4; cf. Jeremias, *op.cit.*, p.155: "In a liturgical text the incongruence of the participial construction...is not intolerable".

<sup>58</sup> *Loc.cit.*

<sup>59</sup> Cf. M. Zerwick, *Biblical Greek* (E.T.), p.6, §15, n.13; Winer, *Grammar of the Idiom of the N.T.*,<sup>7</sup> p.231.

<sup>60</sup> *Op.cit.*, pp.282f.

<sup>61</sup> *Loc.cit.*

<sup>62</sup> *Loc.cit.*

<sup>63</sup> *Philology*, p.181; *TSK*, 1894, pp.733-7; cf. Graefe, *TSK*, 1898, pp.134f.

<sup>64</sup>Cf. Creed, St. Luke, p.265.

<sup>65</sup>Ibid.; cf. Easton, op.cit., p.320; Holtzmann, Hand-Commentar<sup>3</sup>, ad.loc.; Contrast Wellhausen, Das Evangelium Lucae, ad.loc.

<sup>66</sup>Cf. R.H. Lightfoot, History and Interpretation, pp.167f.

<sup>67</sup>Cf. W. Manson, op.cit., p.242. M. Black, Aramaic Approach<sup>3</sup>, pp.230-33, however, thinks that there is no justification for regarding the fulfilment idea in v.30 as referring back to v.16 because of the possibility that Luke's source at v.30 may be different from that of the present narrative. But A.M. Perry, Sources of Luke's Passion Narrative, pp.39, 41, assigns both sections to the "Jerusalem source", J.

<sup>68</sup>Op.cit., p.168.

<sup>69</sup>History of the Synoptic Tradition (E.T.), p.265.

<sup>70</sup>Op.cit., p.255.

<sup>71</sup>This issue cannot be compared to that of v.20b which we discussed earlier. There it is a question of bad grammar; here it is one of idiomatic usage which Luke shares with several NT writers.

<sup>72</sup>Loc.cit.

<sup>73</sup>JTS 9 (1958), pp.305-307; cf. Jeremias, op.cit., p.162; contrast Chadwick, loc.cit.

<sup>74</sup>The Dead Sea Scrolls, p.116; cf. Easton, op.cit., p.320.

<sup>75</sup>Op.cit., fasc. 6, p.306, n.2.

<sup>76</sup>Cf. Jeremias, op.cit., p.19.

<sup>77</sup>Cf. T. Keim, Jesus of Nazara (E.T.), vol.v, p.302.

<sup>78</sup>Introduction, p.21; cf. Richardson, op.cit.,

p.307, n.2 : "Over-emphasis on the Aramaic and Hebrew character of special expressions and constructions, and appeal to detailed Jewish customs recorded only later, can be a snare".

<sup>79</sup>Jeremias, op.cit., p.154.

<sup>80</sup>Ibid.

<sup>81</sup>Kilpatrick, JTS 47 (1946), p.51.

<sup>82</sup>Schürmann, Bib, 32 (1951), pp.368,383; Einsetzungsbericht, pp.35f.

<sup>83</sup>Cf. Cadbury, Style and Literary Method, p.149. He cites several instances where, in copying Mk, Luke supplies the deficiency of the copula: Lk 3:22; 8:25,30; 9:8,19; 17:35; 18:27; 20:24; 22:42. On the other hand, Benoit, op.cit., p.362, points to a few exceptions to discredit Cadbury's generalization with regard to Luke's style: Mk 1:27 vs. Lk 4:36 (cf. 24:17,44,48); Mk 10:43 vs. Lk 22:26. But Benoit himself admits that in this last instance Luke is deliberately trying to evade an inherent difficulty in Mk. So it may well be that the few exceptions only go to prove the rule. Benoit's explanation of the omission of *ἐστιν* in Lk as a skilful adaptation of Paul's circumlocution could be conceded if this were the only stylistic point in favour of the short text in our present context.

<sup>84</sup>Jeremias, loc.cit.

<sup>85</sup>St.Th. 7-8 (1954-55), pp.155ff.

<sup>86</sup>Op.cit., pp.249-58.

<sup>87</sup>JTS 4 (1903), pp.548ff.

<sup>88</sup>JTS 28 (1926-27), pp.362ff.

<sup>89</sup>Philology, p.181; cf. TSK, 1896, pp.733ff; Graefe, TSK, 1898, pp.134ff.

<sup>90</sup>Cf. A.M. Perry, Sources of Luke's Passion Narrative, p.39. He ascribes this section to the 'Jerusalem document', J.

<sup>91</sup>Peake's Commentary, p.840.

<sup>92</sup>Philology, p.181; TSK, 1896, p.734; P. Winter, op.cit., p.155, says that the title 'Institution of the Eucharist' might be a misleading caption for the section Lk 22:14ff since all that it narrates is a common meal which Jesus shared with His disciples. Although we have already rejected the common meal idea, the first part of the comment is worth noting.

<sup>93</sup>CBQ 24 (1962), p.33; cf. Loisy, Luc, p.512, who suggests that the Didache only reflects the normal Jewish order of the benedictions at meals, but in the actual distribution the orders would be reversed. cf. *Evangelies Synoptiques*, II, p.528; Loisy goes on to say: "De ce point de vue théologien, la fraction du pain, symbole de la mort, s'offrait la première à l'esprit, et permettrait de comprendre sans difficulté le symbolisme du vin."

<sup>94</sup>N.T. Problems, p.140.

<sup>95</sup>Op.cit., p.323.

<sup>96</sup>Op.cit., pp.136f.

<sup>97</sup>TU 103 (1968), p.218.

<sup>98</sup>Cf. J.A. Robinson, op.cit., col. 1419, §2. But his insistence on the short text as being the Lukan version of the Eucharist is not tenable.

<sup>99</sup>Op.cit., fasc.3, p.203. Lietzmann shows that the precise regulations regarding the earliest form of the Eucharist have been preserved for us in the writings of Hippolytus (Church Order, ed. Hauler, pp.111ff.); cf. *ibid.*, pp.161ff; 203, n.1. If this is historically true, Zahn's view [Introduction (E.T.) III, p.40] that the prayers over the wine and bread in Did. 9. 2f do not belong to the Eucharist in the narrower sense, but to the introductory Agape has to be modified.

<sup>100</sup>In any case, as R.D. Middleton, JTS 36 (1935), pp.263f, has pointed out, "to build up any argument from evidence supplied by the Didache is a risky procedure", again, "I do not think that he (sc. the

author of the Didache) has made any useful contribution to our knowledge of the Early Church, or that his work is worthy of anything like the serious attention that was at one time given to it" (p.267).

<sup>101</sup>Op.cit., fasc.6, p.318.

<sup>102</sup>Op.cit., p.157.

<sup>103</sup>Loc.cit.

<sup>104</sup>Op.cit., p.258.

<sup>105</sup>Ibid.

<sup>106</sup>JTS 4 (1903), pp.548-555.

<sup>107</sup>Ibid., pp.549-551.

<sup>108</sup>Ibid., pp.551f.

<sup>109</sup>Ibid., p.554-55.

<sup>110</sup>Attention has already been drawn to another highly conjectural suggestion by A. Vööbus (cf. Introduction of this thesis, p.22). He terms his approach "motif-history and cult-tradition". His explanation of the relation of this section in Lk to its original source in terms of a lemma and commentary is not at all convincing. For a criticism of this article, see Snodgrass, JBL 91 (1971-72), p.374, n.15.

<sup>111</sup>JTS 28 (1926-27), pp.362-68.

<sup>112</sup>E.g. He attributes the motive of disciplina arcani to the duplication: οὐ μὴ φάγω... οὐ μὴ πίω:  
I do not agree with this (cf. *ibid.*, pp.367-68).

<sup>113</sup>Ibid., p.364.

<sup>114</sup>Op.cit., p.70.

<sup>115</sup>Cf. A. Wright, loc.cit.

116<sup>Op.cit.</sup>, p.367.

117<sup>Op.cit.</sup>, p.249.

118<sup>Loc.cit.</sup>

119<sup>Op.cit.</sup>, p.366.

120<sup>Inaugural Lecture<sup>2</sup></sup>, Oxford (1920), p.71.

121<sup>Loc.cit.</sup>; Cf. Blass, TSK 1896, pp.734f, for an identical reconstruction, though without sufficient supporting argument.

122<sup>In his reconstruction Bate actually changes</sup>  
ἕως οὗ in v.18 to ἕως ἔργου and transposes  
the ἐλθῆναι to stand before ἡ βασιλεία so as  
to make the parallelism more pronounced. But there  
is no need for this kind of artificiality. Even  
otherwise the parallelism is evident, so that we  
should regard the present wording as going back to  
Luke.

123<sup>Das Evangelium Lucae</sup>, pp.121f.

124<sup>Ursprung und Anfänge</sup>, 1, p.178, n.1.

125<sup>JTS 36 (1935)</sup>, pp.267-80.

126<sup>Ibid.</sup>, p.269.

127<sup>Ibid.</sup>, pp.277-78. Jeremias' attempt to discredit this view by quoting Ac 20:28 and also 2:23; 3:13,14,18; 5:30; 7:52; 10:39; 13:27,29; 17:11; 22:14 in addition to Lk 22:37, has not shaken Kiddle's basic contention (op.cit., p.157, and n.5,6).

128<sup>While I do not subscribe to Kiddle's suggestion that the Supper was only a fellowship meal, and that Jesus did not partake of the food, I agree with his emphasis on the natural link between the short text and the saying about the traitor, vv.21ff.</sup>

129<sup>Op.cit.</sup>, p.264.

<sup>130</sup>Hort, op.cit., p.64.

<sup>131</sup>In defence of the long text some scholars have gone to the extent of discerning two Passovers, the Jewish Passover (15-18), which Jesus celebrated and the Christian Passover (19-20), which He wanted His disciples to perpetuate in His memory! (See Loisy, Luc, p.511, who rightly refutes it, arguing that there is no warrant for this interpretation in the text).

<sup>132</sup>Historical N.T., p.612.

I.B. The Empty Tomb Story

a. Lk 24:3: The Christological Title

Disputed words: [τοῦ κυρίου Ἰησοῦ]

External Evidence:

Nestle<sup>26</sup>

Greek NT<sup>1</sup> (UBS)

τοῦ κυρίου Ἰησοῦ (unverändert) τοῦ [κυρίου] Ἰησοῦ  
txt P<sup>75</sup> א ABC K L W X Δ Θ Π Ψ 0124 f<sup>1</sup> f<sup>13</sup> 28 33 565 700 892  
1009 1010 1079 1195 1216 1230 1242 1253 1344 1365 1546 1646  
2148 2174 30 Lect aur c f q vg syr<sup>h</sup>.p<sup>21</sup> sa bo arm aeth georg Eus<sup>1/2</sup> ||  
τοῦ Ἰησοῦ 1071 1241 syr<sup>c</sup>.s.p sa<sup>ms</sup> bo<sup>ms</sup> || — D a b d e f f<sup>2</sup> | r<sup>1</sup> Eus<sup>1/2</sup>

Observations

(i) The chief characteristic of the shorter readings in the last chapter of Lk is that they are all attested almost exclusively by the 'Western' family. The reason why Westcott-Hort singled them out is obvious. If we were to settle the question of authenticity based on MS evidence alone, then in all the 8 cases the majority of the authorities including the 'best' invariably support the long text<sup>1</sup>. That they need special attention cannot be denied. But our decision in each case will have to depend upon a careful analysis of the Internal Evidence.

(ii) One striking fact brought to light by the MS data is the relation that this passage bears to Mk 16:19. There is a remarkably unusual correspondence between certain groups of MSS in each context:

Lk 24:3: τοῦ Ἰησοῦ : 1071 1241 syr<sup>c,s,p</sup>  
cop<sup>sa</sup> ms bo<sup>ms</sup>

Mk 16:19: Κύριος Ἰησοῦς : 1071 1241 syr<sup>c,p,h,pal</sup>  
cop<sup>sa</sup> bo

Lk 24:3: τοῦ κυρίου Ἰησοῦ : AXΘΨ 28 700 892  
1009 1010 1195 1216 1242 1253 1344 1365  
1646 2148 2174 Byz. Lect\*

Mk 16:19: Κύριος : AXΘΨ 28 700 892 1009 1010  
1195 1216 1242 1253 1344 1365 1646 2148  
2174 Byz. Lect\*

This suggests that the scribe who supplied the deficiency of the truncated Markan text must have originally used the simplest form of the Christological title, *Κύριος*, at 16:19. It is widely recognized that the longer ending of Mk, vv. 9-20, is constituted mostly of elements condensed from the other Gospels, particularly from Lk and Jn: Mk 16:19 is no more than an abridged version of Lk 24:50-51, the Ascension passage. There is, however, no mention of any Christological title in this Lukan context. Therefore the reading *Κύριος Ἰησοῦς* at Mk 16:19 is not likely to have originated from Lk. In other words, at the earliest stage of textual transmission the title had not reached the enlarged form. Since the scribe behind the composition of Mk 16:19 was

obviously well-versed in Lk and Jn, the form *κῦριος* must have been suggested to him by its frequent usage in these Gospels; the fuller form *κῦριος ἰησοῦς* both at Lk 24:3 and Mk 16:19 would thus represent a secondary stage of textual emendation. Thus the External Evidence in fact turns out to be a proof of the originality of the omission of the disputed words at Lk 24:3.

(iii) Burkitt<sup>2</sup> and Lagrange<sup>3</sup> have pointed to the difference in wording between Syr<sup>S</sup> and *Ⲛ* B etc. at this point even though these witnesses are united against D etc. in presenting a longer reading. This, in their opinion, is an indication of the spuriousness of both the longer readings. Creed confirms this observation by noting that the MS evidence bears evidence to an early interpolation in the text<sup>4</sup>. The divided witness of the Old Latins, however, does not produce a clear picture.

(iv) Patristic evidence is wanting here except for that of Eusebius; but he attests both the long and short readings.

### Internal Evidence

#### Transcriptional Probability

##### (i) Possibilities of Insertion

(a) Scrivener regards all the longer readings in

Lk 24 as part of the revised and enlarged edition of the Gospel by the hand of the author himself<sup>5</sup>.

This is, of course, in line with Blass's theory of the double recension of Lk-Acts which, as we have seen, finds little favour among scholars as far as the Third Gospel is concerned. In fact Blass does omit the additional words here in his edition of the N.T.<sup>6</sup>.

(b) According to Klostermann<sup>7</sup> the bracketed words may perhaps be a later interpolation from Mk 16:19.

The difficulty in accepting this suggestion is that the Markan passage under reference has to do with the Ascension, whereas the present text is associated with the story of the Empty Tomb.

(c) Aland suggests that the reading *τὸ σῶμα τοῦ κυρίου Ἰησοῦ* is a conflation of two shorter readings. The original form was simply *τὸ σῶμα* to which *τοῦ Ἰησοῦ* was first added in the process of transmission. The next stage was marked by the insertion of *κυρίου* as well<sup>8</sup>.

This would seem to be an attractive solution except that it ignores the striking MS

evidence which throws light on some possible link for the present reading with Mk 16:19 (vide supra).

In any case, from an observation of the 3 variant readings at Lk 24:3 several scholars are agreed that it is an instance of interpolation rather than deletion. But no one is in favour of the Old Syriac reading being original. As for the other longer text, Loisy thinks that it could possibly be an explanatory addition<sup>9</sup>. Lagrange says: "L'addition vient du respect pour le corps de Jésus"<sup>10</sup>.

(ii) Possibilities of Omission

(a) Although Graefe is rather neutral in his verdict on the textual problem here, he is right in stating that it is difficult to account for the omission if the disputed words originally stood in the text. He rather vaguely suggests that confusion with v.23 may have contributed to the deletion<sup>11</sup>. Metzger is more specific when he explains the shortest reading as an attempt at assimilation to v.23<sup>12</sup>.

This is not, however, a convincing argument. If proximity to a corresponding part of the same Gospel is a reliable clue, we have at 23:52 the phrase τὸ σῶμα τοῦ Ἰησοῦ, followed at v.55 by τὸ σῶμα αὐτοῦ. Since the wording at 24:23 is the same as at 23:55, it is difficult to imagine

that the former context which is farther removed from 24:3 had a greater influence in giving shape to its phraseology than the latter. On the other hand, stylistic considerations would suggest that τὸ σῶμα is most likely to be what Luke had originally written as a sequel to 23:52 and 55.

(b) Assimilation to Mt 27:58 or Mk 15:43 has been suggested, again by Metzger,<sup>13</sup> as an explanation for the absence of *κύριου* in certain MSS.

This might be possible, but is not adequate to explain the other omission in the majority of Texts. Since we have already rejected Metzger's explanation for this majority reading, we have to reject the present one too as he had treated them as being mutually complementary. The chances of two successive omissions seem to be less likely than those of two stages of interpolation in view of the MS data we have examined earlier.

(c) A more serious point for consideration in this connexion has been mooted by Ignace de la Potterie<sup>14</sup>. He claims that of the 19 occurrences (including the present case) of the title *κύριος* used by Luke in indirect speech as referring to Jesus, in almost every case we find a variant reading and that in

the vast majority of these contexts the variant involves the replacement of *κύριος* by [ὁ] Ἰησοῦς. His inference from this is that "les copistes ont senti, dirait-on, que l'emploi du titre dans ces textes avait quelques choses d'anachronique".

As a matter of fact, however, this is a grossly exaggerated observation. Of the 18 such occurrences (not 19) of this Christological title in Lk<sup>15</sup>, according to Kilpatrick<sup>16</sup>, only 12 are genuine. In the remaining 6 cases he thinks that the original did read Ἰησοῦς. In some cases, alteration had most probably nothing to do with theological considerations, but only with smoothness and elegance, i.e. to avoid repetition (e.g. 12:41, cf. 42,43,45,46; 10:39, cf. 40,41; 19:8; 22:61). At 10:1; 11:39, if the text had originally no express mention of the subject, scribes would choose to insert either ὁ κύριος or ὁ Ἰησοῦς; and hence the variants. Of the remaining 3 instances, only in one case (4:1) has the variant to do with the replacement of ὁ κύριος by Ἰησοῦς; in another instance (6:23) the term κύριος is left out by the 'Western' witnesses, but the context clearly shows that the omission was not due to considerations based on that term. In the third instance (20:25), the variant leaves the title intact<sup>17</sup>.

Therefore we notice that our author's sweeping generalization about the scribal motive behind the alteration of the title *κύριος* was made without actual verification.

Moreover, a study of the term *κύριος* in Lk should be more broad-based than Ignace de la Potterie's<sup>18</sup>. It should look into the question of the liberal interspersing by Lk (and by Jn) of the other titles and forms of reference to Jesus used in both direct and indirect speech. As mentioned in a foot-note at the beginning of this study, Aland has rightly emphasized that a close scrutiny of the Lukan use of the term *κύριος* is the clue to the solution of the textual problem of this verse. But if it is to be convincing it should be studied in the context of the other Christological titles in Lk and in comparison to John's usage of such titles. This will have to wait until we take up the Intrinsic Probability.

It is now clear that we have come across no satisfactory explanation for the disappearance of the additional words at 24:3 from so many MSS belonging to the different branches of tradition. As we will have occasion to note in many instances,

this situation in itself is a strong point in favour of the shortest reading. We have already seen that there is a strong case for conflation in the long process of transmission through several generations extending over various lands. Taking all our findings so far, I propose the following Transcriptional Probability:

It is my contention that despite ~~the~~ several verbal resemblances between Lk 24:1-3 and Mk 16:1-5, Luke was primarily dependent upon another source for his story of the Empty Tomb<sup>19</sup>. There is nothing surprising if Luke had read Mk 16:1-8 in view of his profuse use of Mk in the earlier parts of his Gospel. He might even have borrowed some expressions from Mk just as any modern author who is well-versed in another author's work is bound to be indebted to the latter even if it be in an unconscious way. Easton aptly remarks<sup>20</sup>: "Lk has incorporated reminiscences of Mk into a narrative based primarily on L". But Luke had every reason to turn to other sources to furnish his account of the Resurrection because of the truncated condition in which the Second Gospel had reached him (vide. on Mk 16:9ff).

The story of the Empty Tomb as well as

the Emmaus episode must initially have circulated as separate pericopes (vide on 6a, infra). At this stage the present verse would essentially have carried the words τοῦ Ἰησοῦ, or rather, τοῦ κυρίου since the subject has to be specified at the beginning of a narrative. Later, when the pericope of the Empty Tomb was incorporated into the Gospel, Luke could, for stylistic reasons, dispense with the possessive in view of the occurrence of the τὸ σῶμα τοῦ Ἰησοῦ and the τὸ σῶμα αὐτοῦ of 23:52 and 23:55 respectively. The next stage in the textual transmission marked the interaction of τοῦ Ἰησοῦ and τοῦ κυρίου giving rise to the present conflated reading. If the passage had originally contained τοῦ Ἰησοῦ, the τοῦ κυρίου could be accounted for by the frequent use of the title in the earlier parts of the Gospel - a scribe would have naturally borrowed this characteristic Lukan phrase. On the other hand, if it had read τοῦ κυρίου initially, the τοῦ Ἰησοῦ could be regarded as having been borrowed from the corresponding passage in Mk or Mt; (it could as well have been deliberately added to produce more dignity to the dominical title, as we commonly find in the day-to-day utterances of many a believer). Considering all the aspects of the case, it seems more likely that the expansion took

place along the latter line as it conforms to the MS data we have considered elsewhere.

The advantage of this theory is that it is in keeping with the well-known scribal tendency to expand rather than to omit (except accidentally). This finding will be corroborated by the Intrinsic Probability. Suffice to note here that this solution is better than that offered by either Aland or any of the other supporters of the theory of interpolation.

#### Intrinsic Probability

(i) An objection that is usually raised against the combination *ὁ κύριος Ἰησοῦς* being original is that it is not found in the Gospels except in the secondary reading at Mk 16:19<sup>21</sup>.

This argument, however, is not conclusive in itself because it could be pointed out that the compound title is at any rate Lukan as we find it in frequent use in Acts (1:21; 4:33; 8:16; 11:20; 15:11; 16:31; 19:5,13; 20:21; 21:13; 24:35)<sup>22</sup>. But again, one might argue back that in Acts the title is put in the mouth of the Apostles who have already been convinced of the reality of the Resurrection and who are bold witnesses of that conviction. In Lk 24:3, on the contrary, it is not part of a statement of the

Apostles: they are in fact at this stage in a state of bewilderment and unbelief.

(ii) That brings us to a theory proposed by Ignace de la Potterie in regard to the development of the title *τοῦ κυρίου Ἰησοῦ*. He thinks that the author must have designed it as befitting the morning of the Resurrection event in order to mark the transition from the public life of Jesus to His resurrected life as Lord of His Church: "On comprend dès lors pourquoi l'expression double *ὁ κύριος Ἰησοῦς* ne se trouve qu'ici dans l'évangile: c'est que ce verset sert précisément de charnière entre le temps de la vie publique et le temps de la Résurrection; on passe en ce moment du temps de Jésus au temps du Seigneur présent dans son Eglise"<sup>23</sup>.

This suggestion is not so plausible as it looks on the surface. In the first place, the fuller title actually makes rather awkward reading in the present context, thus betraying its origin from a pious scribe than from an accomplished author. Further, a transitional title would necessarily mean two things: (a) first, that Luke uses the term *ὁ κύριος* in a venerable sense in the rest of the Resurrection narrative. However, the only occurrence of that title in the rest of the chapter is

in v.34, where it is put in the mouth of the disciples who are still not committed believers in the fact of the Resurrection (cf. vv.38,41). Therefore much weight cannot be attached to the reference in v.34 (cf. Ac 1:6, *Κύριε* ); (b) second, that Luke does not attach any divine association to the term in the earlier part of the Gospel. This is belied by such confessional references as *Μήτηρ τοῦ Κυρίου* (1:43) and *Χριστὸς Κύριος* (2:11,26) at the very beginning of the Gospel. Again, if such a solemn title had been intended by the author, a position after the angelic announcement of the Resurrection, but before the actual recognition scene, such as v.15, would have been a more appropriate place to introduce it. Ignace de la Potterie tries to link up certain references at the close of ch.23 with ch.24 to draw up an original design in the mind of the author in introducing the double title at v.3: 'On voit donc ce que désigne l'expression "le corps du Seigneur Jésus" en xxiv 3: c'est le corps de Jésus (xxiii 52), de celui qui avait été livré aux mains des pécheurs (xxiv 7a), qui avait dû souffrir (v.26) et être crucifié (v.7b), mais qui désormais est ressuscité (vv.6a, 7c, 34a), vivant (vv. 5c, 23) entré dans la gloire (v.26), et qui se manifeste aux siens comme Seigneur (v.34)<sup>24</sup>. One has no

objection to this exercise in Biblical theology which is sound indeed; but what is not convincing to me is the alleged choice of the point of introducing the double title. What is awkward about its present position is that Luke has not yet prepared his audience for the disclosure of the fact of the Resurrection; thus it would be too premature to read any confessional motive into the author's mind at this specific point in his narrative.

There is a still more formidable objection to accepting the above exegesis in support of the authenticity of the longest text at 24:3. It has to do with an issue which we had deferred for further discussion, viz. how much Christological content we can associate with Luke's use of the term *κύριος* in the Gospel as a whole. V. Taylor has pointed out that the title in its various combinations represents an advanced Christology which is the hall-mark of Acts and the Pauline Epistles<sup>25</sup>. Luke's own Christology, however, is a Son-Servant Christology<sup>26</sup>. So wherever he is found to employ the term 'Lord', he owes it to his sources (i.e. Proto-Luke): he did not try to impose his Christology on his sources<sup>27</sup>. "Indeed, the use of *κύριος* in Proto-Luke is demonstrably earlier than that of

the Acts, where the Lord is the object of faith and the only hope of salvation...In proto-Luke *κύριος* is always used in narrative, and does not rise above the level of a term of respect or high regard...The evidence as a whole suggests that in the use of the word there is no more than the rudimentary beginnings of a Christology - a bridge...by which to pass over to the fuller development of the term in the Pauline theology and in the Acts"<sup>27</sup>.

It must be borne in mind that *κύριος* is only one of the titles used by Luke as referring to Jesus. The bulk of these are found in Proto-Luke, being most often in common with Mk and Mt (i.e. Q). Thus besides *ὁ κύριος*, we come across *ὁ Χριστός*, 'Son of Man', 'Son of God' and 'the Son'; Jesus is also referred to as 'a prophet' and as 'the King of the Jews'<sup>29</sup>. But while these other titles reflect the confessional usage of the early Church, we cannot be so confident about the term *ὁ κύριος* because of the diversity of its application in all the four Gospels:

	Lk	Jn	Mt	Mk
1. As a title of respect or of a master (i.e. Sir, or 'milord')	17	5	30	9
2. As a title for God	38	4	19	9
3. As applied to Jesus:-				
a. Used by persons other than the disciples	23	20	10	1
b. Used by the disciples	12	18	8	-
c. Used by Jesus Himself	6	2	9	4
	<hr/>	<hr/>	<hr/>	<hr/>
	96	49	76	23

Another possible grouping of the use of the term is as follows:

1. As a general title of respect for Jesus and others	21	30	34	10
2. As a title for 'master'	19	3	23	4
3. As referring to God	38	4	19	9
4. As applied to Jesus in a confessional sense	18	12	-	-
	<hr/>	<hr/>	<hr/>	<hr/>
	96	49	76	23

The foregoing tabulation<sup>30</sup> makes it abundantly clear that *kūrios* does not necessarily connote a confessional sense either in Lk or in Jn, the relatively later Gospels, which could, as such, be expected to reflect the confessional usage of the early Church. This inference is further corroborated by the fact that in Proto-Luke alone the vocative *kūrie* (the polite form of address) as well as the oblique cases of *kūrios* occur 15 times each<sup>31</sup>. Besides,

the frequent interspersing of the terms *κύριος* (5:8; 10:17; 10:40; 11:1 etc.), *ἐπισητά* (5:5; 8:24,45; 9:33,49; 17:13) and *διδάσκαλε* (9:37; 12:13; 21:7 cf. 7:40; 20:21) along with *Ἰησοῦς* makes it difficult to believe that Luke was attaching any note of veneration in his use of *κύριος*.

However, since the author was writing his Gospel after a considerable lapse of time from the Resurrection, we might allow for a certain element of confessional significance to the term in passages like 1:43; 2:11,26 etc. On the other hand, this effect is counterbalanced by the realization that in places where much solemnity is inherent, the title *κύριος* does not figure at all: e.g. the Transfiguration, the Last Supper, the Ascension (cf. Ac. 1:11, *ὁ Ἰησοῦς* ). It is also noteworthy that in none of the miracles which Lk has in common with Mt and Mk is there a reference to Jesus as *κύριος* in indirect narrative. The two miracle stories where the title does appear are the Raising of the Son of the Widow of Nain (cf. 7:13) and the Healing of a Crippled Woman (13:15), both being peculiar to Lk. This would suggest that the references had originally belonged to Luke's special source, which he is reproducing. All these evidences jointly validate the thesis cited above that as far as the

Third Gospel is concerned, Luke's Christology was still in its rudimentary stage in relation to *κύριος* 32.

In Jn too we find various forms of reference to Jesus admixed with *κύριος* and *κύριε* which makes it difficult to decide whether the latter terms reflect an advanced Christology:

- Ῥαββί* (= *Διδάσκαλε* ) : 1:38,49; 3:2,26; 4:31 (cf. Mk 14:45 par. Mt 26:49), used by disciples, would-be disciples or sympathisers) cf. *Ῥαββουσί* , 20:16.
- Μεσσίας* = *Χριστός* : 1:41 cf. 4:26 .
- κύριε* : 4:11,15 etc., 5:7; 9:36, used by strangers.
- κύριε* : 6:34, used by the crowd
- κύριε* : 11:3,12,21 etc., 13:6,9,25,36; 14:4,8,22; 21:15 etc., used by the disciples.
- Ὁ διδάσκαλος μου καὶ ὁ θεός μου* : 20:28  
(declaration of faith in the Risen Lord by Thomas).
- ὁ κύριος* : (6:23); 11:2; 20:2,13,18,20,25,28; 21:7,12 (in indirect narrative; all related to the Passion Week except 6:23).

In view of 20:28, and of the widely acknowledged theological orientation of the Fourth

Gospel, one might say that at least the references to Jesus in the indirect narratives have a greater theological overtone than in Lk<sup>33</sup>. But even this concession is no argument in support of an advanced Christology in Jn built around *κύριος* because the emphasis in this Gospel is on the Father-Son relationship rather than on the Lordship of Christ. If this is true of the latest of the Gospels, it is all the more true of the Third Gospel<sup>34</sup>. In any case, our findings give the lie to Ignace de la Potterie's contention that *κύριος* has a special significance in combination with *Ἰησοῦς* at 24:3 as a transitional title.

If these observations have any validity, the note on Mk 16:19 in Metzger's Textual Commentary<sup>35</sup> is questionable: "Among the several titles applied to Jesus by the Church, the use of *κύριος* standing alone appears to be a later development more solemn than *κύριος Ἰησοῦς*".

### Conclusion

It is clear that despite its apparent simplicity, the present problem is a rather complicated one. The fact that commentators like J. Weiss<sup>36</sup> ignore this problem may be taken as a

sign of their conviction as to the spuriousness of the additional words. Among those who have commented upon it, the vast majority are in favour of the shortest reading. Alford's objection against this is that (a) τὸ ἄνωμα is not used absolutely in Luke's narrative and that (b) ὁ κύριος Ἰησοῦς is a common expression in Acts<sup>37</sup>. Against the first objection it may be remarked that the occasion is unique here in Lk and that a rare usage cannot be held against its authenticity without other valid reasons. As for the second objection, we have already answered it by noting that the theology of Acts reflects a more developed Christology than that of the Third Gospel. W. Manson<sup>38</sup> accepts the longest reading without comment while Graefe<sup>39</sup>, Lampe<sup>40</sup>, and Aland<sup>41</sup> are uncommitted in their evaluation. But on the whole we can be confident that we are on sure ground in our approach to the problem here.

<sup>1</sup>Cf. Aland, NTS 12 (1965-66), pp.203-4: "Die Feststellung, dass der handschriftliche Befund eindeutig dagegen (sc. Kurzform) spricht...mag allein manchen nicht überzeugen. Da mehr zu sagen nur bei einer Diskussion des lukanischen Gebrauchs des Kyrios-Titels möglich wäre, müssen wir auf eine vollständige Erörterung des Textes verzichten" (vide infra).

<sup>2</sup>Evangelion, p.230; cf. Souter, Text and Canon, p.128; J.R. Harris, Diatessaron of Tatian, pp.52-53.

<sup>3</sup>Critique Textuelle, II, p.71.

<sup>4</sup>St. Luke, p.293.

<sup>5</sup>Scrivener, Introduction, II<sup>4</sup>, p.298.

<sup>6</sup>Cf. Euangelium secundum Lucam sive Lucae ad Theophilum liber prior, Leipzig, 1897, ad.loc.

<sup>7</sup>Lukasevangelium, p.599.

<sup>8</sup>NTS 12 (1965-66), p.203.

<sup>9</sup>Luc, p.567.

<sup>10</sup>Loc.cit.

<sup>11</sup>TSK, 1896, p.267.

<sup>12</sup>Textual Commentary, p.183.

<sup>13</sup>Ibid.

<sup>14</sup>Mélanges...Béda Rigaux, p.118; cf. Nevius, ATR 48 (1966), p.76. The instances under reference are 1:43; 2:11; 7:13,19; 10:1,39,41; 11:39; 12:42; 13:15; 17:5,6; 18:6; 19:8; 22:61 (twice); 24:3; 24:34.

<sup>15</sup>Cf. J. Reid, Exp.T. (1900-1901), pp.425-30, especially p.428.

<sup>16</sup>Mélanges...F.-J. Leenhardt, 1968, pp.66ff.  
The genuine occurrences of *κύριος* in the original  
are at 1:43; 2:11; 10:39,41; 12:42; 17:5,6;  
18:6; 19:8; 22:61 (twice); 24:34. Kilpatrick  
concludes that *κύριε* should always be translated  
as 'Sir' in Luke.

<sup>17</sup>Cf. Synopsis of the Four Gospels edited by K. Aland,  
ad.loc. In any case, as Nevius, loc.cit., admits,  
there are at least 6 instances where scribal activity  
has been in the reverse direction, viz. of replacing  
*ἡσούς* by *κύριος* (5:8; 18:42; 23:34,42,52; 24:3).  
This observation, coupled with the findings of  
Kilpatrick's study of the title, cited above, is  
enough to expose the weakness of the views of both  
de la Potterie and Nevius.

<sup>18</sup>This author investigates only the use of *κύριος*  
in indirect narrative in Lk. Reid, loc.cit., looks  
into the use of *κύριος* in Lk in all its oblique  
cases, but not that of the other Christological  
titles. He does not find any evidence to reject  
the longest reading at 24:3. Streeter's Study  
(Four Gospels, pp.213-4) is too inadequate and his  
statistics too misleading in that they are limited  
only to the use of the vocative *κύριε* in Lk  
(see further, infra).

<sup>19</sup>Contrast P. Winter, St.Th. vii-viii (1954-55),  
p.167. He believes that the whole of 24:1-11 is derived  
from Mk.

<sup>20</sup>St. Luke p.357, n. ; Cf. V. Taylor, Passion  
Narrative, p.105: "It must be conceded...that the  
case for regarding Lk xxiv.1-11 as an independent  
narrative derived from a non-Markan source, 10a  
being a Markan insertion, is strong". It is true  
that Lk has some words at 24:1-3 in common with Mk;  
Cf. B. Weiss, Markus u.Lukas (Meyer's Kommentar) 7p.677.  
Taylor, op.cit., p.106: "Taken by themselves the  
'common' words can be explained without affirming  
their Markan origin" (i.e. in terms of tradition).  
Yet on p.107 he concludes: "Considered as a whole  
they suggest that in some way yet to be determined  
a knowledge of Mk xvi 1-5 is presupposed" (italics  
mine). Streeter, Four Gospels, pp.216, 222 n.1,  
says that Lk 24:1-3, 9f may have been derived either  
from Mk, or it may represent Proto-Luke, partially  
assimilated to the Markan parallel.

<sup>21</sup>So Hort, Notes, p.71; cf. Plummer, St. Luke, I.C.C., p.547; Creed, St. Luke, p.293.

<sup>22</sup>So Ignace de la Potterie, op.cit., p.122.

<sup>23</sup>Ibid., p.123.

<sup>24</sup>Ibid.

<sup>25</sup>Passion Narrative, pp.137-38; cf. Taylor, Person of Christ in N.T. Teaching, p.144: The title 'the Lord', or its various combinations such as 'the Lord Jesus', 'the Lord Jesus Christ', and 'our Lord Jesus Christ', occur 42 times in Acts while 'the Son of God' and 'my Son' occur only once each.

<sup>26</sup>Passion Narrative, p.137, n.1: The title 'Son of God' (including 'the Son' and 'my Son') appears 11 times in Lk, but is absent from passages belonging to Luke's special source (exception, 22:70).

<sup>27</sup>This point, however, has to be modified by the fact that Taylor overlooks the occurrence of *κύριος* in 1:43, 2:11 and 2:26 in a confessional sense. These passages, of course, do not form part of "Proto-Luke"; but still, they may belong to oral tradition. These exceptions, however, do not detract from the central point of Taylor's thesis regarding Lukan Christology.

<sup>28</sup>V. Taylor, Behind the Third Gospel, p.266.

<sup>29</sup>Ibid., p.265.

<sup>30</sup>I owe this tabulation to Reid, op.cit., pp.429-30. However, instead of drawing this conclusion, Reid asserts that the use of *κύριος* in Lk reflects the influence of the Apostolic Church. His basis for this is the absence of the confessional sense of the term in Mt and Mk as against the other two later Gospels (cf. pp.428-9).

<sup>31</sup>Cf. V. Taylor, loc.cit. According to the editors of the 'Beginnings of Christianity' (Pt.I, vol.i, p.416), when the Aramaic  $\int\eta\eta$  was translated by the

Greek equivalent *κύριος* , it carried with it the Greek religious connotation of 'the Lord of a circle of initiates who worshipped him'. Even if we agree with these editors in respect of the rest of the N.T., it would be difficult to discern such an advanced view in any of the Gospels ; cf. Taylor, op.cit., pp.265f.

<sup>32</sup>Nevius, loc.cit., writes: "The Gospels may reflect a more primitive attitude, but some of the restraint may be due to a conscious effort to present the story of Jesus in a pre-Resurrection light. It is not, after all, surprising that *κύριος* is sometimes used to mean *Ἰησους* in the Gospels; what is remarkable is that there is not more evidence of this usage". But from the instances we have cited already, and the ones which immediately follow, the last part of the statement is not tenable.

<sup>33</sup> Kilpatrick, op.cit., p.69, notes that the use of the nominative *κύριος* rather than the vocative *κύριε* is significant.

<sup>34</sup> Contrast Nevius, op.cit., p.77. Against V. Taylor, *The Names of Jesus*, p.42, he argues: "...to say that *κύριος* in the Gospels is less primitive than *κύριος* in Paul is a dubious assumption unless "primitive" means strictly and only 'pre-Resurrection'." But despite this note of dissent, elsewhere he is very much inclined towards accepting Westcott-Hort's position with regard to Lk 24:3 : "If Westcott and Hort are correct in thinking that the Western text here represents the original, and that all other readings are interpolations, we can see in this multiple variant situation...an expansion from a simple phrase to the addition of *Χριστος* to the more complex *Ἰησους Χριστος ὁ κύριος ἡμῶν*". (Studies and Documents XXX, ed. J. Geerlings, p.67). This is indeed in conformity with our line of argument. For further confirmation, see Kilpatrick, op.cit., p.68. He is in full agreement with our findings. Later he says: "The early church effected a revolution in theological terminology... This revolution took place early in the post-resurrection church but infiltrated into the traditions about Jesus much later and to a very limited degree" (p.70).

<sup>35</sup> p.127.

<sup>36</sup>Schriften des N.T., ad.loc. So also Ellis,  
St. Luke, ad.loc., Tasker, Greek N.T. ad. loc.

<sup>37</sup>The Greek N.T., I<sup>5</sup>, ad loc.

<sup>38</sup>Luke, ad loc.

<sup>39</sup>TSK, 1896, p.267.

<sup>40</sup>Peake's Commentary, ad loc.

<sup>41</sup>Loc.cit.; cf. also A.B. Bruce, Expositor's Greek  
Testament, p.644, n.2.. Contrast, Leaney,  
St. Luke, p.291: "an interpolation". See also  
Wellhausen, Evangelium Lucae, ad loc.

I.B. The Empty Tomb Story

b. Lk 24:6a: The Angelic Announcement

Disputed words: [οὐκ ἔστιν ὧδε, ἀλλὰ ἠγέρθη]

External Evidence:

Nestle<sup>26</sup>

Greek NT (UBS)

οὐκ ἔστιν ὧδε, ἀλλὰ ἠγέρθη

[οὐκ ἔστιν ὧδε, ἀλλὰ ἠγέρθη]

(vorher in Doppelklammern)

txt 7<sup>175</sup> א B C<sup>3</sup> KL (W ανέστη) X Δ Θ Π Ψ 063 0124 f<sup>1</sup> f<sup>13</sup> 28 33 565

700 892 1009 1010 1071 1079 1195 1216 1230 1241 1242 1253

1344 1365 1546 1646 2148 2174 217 Lect aur f q vg sur<sup>c. s. h. p<sup>1</sup></sup> sa bo

arm (aeth) georg<sup>1. A</sup> || οὐκ ἔστιν ὧδε ἠγέρθη. C<sup>2</sup> syr<sup>P</sup> bo<sup>ms</sup> Epiph ||

ἠγέρθη Mcion || ἠγέρθη ἐκ νεκρῶν c || — D a b d e ff<sup>2</sup> l r<sup>1</sup> georg<sup>B</sup>

Observations

(i) Those who would defend the bracketed words on the strength of MS evidence have, of course, p<sup>75</sup> as their latest ally<sup>1</sup>. But as we have remarked in connexion with the previous passage, internal factors have to be weighed carefully against this.

(ii) It must be noted here that except for D and its few allies, the different variants have retained at least a part of the disputed words. It will be seen later that in such contexts the additional words provide room for suspicion as to their originality (vide on Mt 23:26; Lk 10:41f).

(iii) Aland denies the possibility of either Marcion's text or the c reading being additions to the shortest text<sup>2</sup>. But we have to view this in the light of the textual phenomena manifested by the different MSS. The deletion of  $\acute{\alpha}\lambda\lambda\acute{\alpha}$  in the Versions such as Syr<sup>p</sup>, g<sup>2</sup> and cop<sup>bo</sup><sup>ms</sup> may be explicable in terms of the freedom of discretion of translators because the particle is not absolutely essential for retaining the sense of the text. But the case of G\* is not merely a question of dispensing with  $\acute{\alpha}\lambda\lambda\acute{\alpha}$ ; it has to do with a text which is identical with that of Mk 16:6. Since the above Versions also have the same text, their common reading can be interpreted only as the result of one critical process.

(iv) Again, there is the question of Syr<sup>s</sup>. Burkitt<sup>3</sup> and Souter<sup>4</sup> are convinced that this is one of the few cases where it has suffered an assimilation to a parallel text in the company of the Alexandrian Texts. If this is true, it would confirm our suspicion of all the longer readings.

## Internal Evidence

### Transcriptional Probability

#### (i) Possibilities of Insertion

(a) Commentators are usually concerned only with the longest and the shortest readings. But as we have already mentioned, an examination of the shorter readings might offer some clue to the solution of the textual problem of the verse as a whole. C\* and related witnesses were most probably influenced by the Matthaean reading (28:6) which is only a transposed form of the Markan (16:6). It is very unlikely that the ἀλλὰ of an original text would be dropped out of preference for the reading in Mt. Therefore it is possible that the exemplar of those authorities had only the shortest text to begin with.

(b) The next point to consider is whether the c reading is likely to be the original. The ἐκ νεκρῶν at this point is unique to this MS; and in view of the fact that the same MS reads an identical phrase at Mt 28:7 in the company of the vast majority of ancient witnesses, it is likely that the words may have been borrowed from there. A more likely source of origin of the phrase is Mk 9:9 par. Mt 17:9. But one cannot be so precise about an expression

which was on the lips of every Christian believer: the scribe might as well have unconsciously inserted it! Since it is an isolated reading, there is every reason to agree with Plummer that it "is perhaps an independent insertion"<sup>5</sup>. Now the question remains whether the exemplar of c had initially read the shortest text or a text such as that of Marcion.

(c) Aland's view with regard to c and Marcion's text has already been cited. We notice that while C\* syr<sup>P</sup> g<sub>2</sub> omit ἀλλὰ as in Mt 28:6, eth transposes the words, omitting ἀλλὰ as in Mk 16:6. Is it natural that the smoother wording of the longest text would be replaced by a less elegant reading in the Ethiopic Version? Is it not more reasonable to assume that this assimilation to Mk 16:6 would be possible only if the exemplar of eth carried no additional words at all?

If the longer readings of C\* et al and of eth are unlikely to be authentic, it encourages us to believe that Marcion's text and the c reading are also likely to be spurious. But we will take this up again in the next section.

(d) The main issue with regard to this passage, however, is the authenticity or otherwise of the majority reading. Of course, there is only one possibility for the origin of this reading if it did not belong to the Lukan text: while it is exactly identical with Mt 28:6 except for the ἄλλ᾽, it is also identical with Mk 16:6 except for the deficiency of that particle and the transposed order of the words. The vast majority of commentators are agreed that the longest reading at Lk 24:6a is an assimilation to either Mt or Mk<sup>6</sup>. Our decision on this will depend upon our attitude towards Luke's alleged indebtedness to Mk and/or Mt. Loisy thinks that our author must have so judiciously adapted the Markan material that the present words can really be deemed as a superfluous assimilation to Mk 16:6.

'Notre auteur avait eu probablement l'intention de les remplacer par la jolie formule qu'on vient de voir, "pourquoi cherchez-vous" correspondant à Marc: "Vous cherchez Jésus le Nazarène le crucifié", et "le vivant" qui n'est point "parmi les morts" équivalent à: "il est ressuscité, il n'est point ici" <sup>7</sup>. Whether we agree with this view or not, our evidence so far has led us to suspect the authenticity of 6a. The relationship of Lk to the other two Synoptics will be taken up later.

(ii) Possibilities of Omission

(a) The absence of the disputed words from Codex Bezae and its satellites has been attributed to their superfluity: a scribe may have argued that since the women had already realized that Jesus was not there, the angels had no need to tell them so<sup>8</sup>.

This is precisely why I feel that these words cannot have originated with a good stylist like Luke. We know that when it comes to matters of style, Luke polishes Mark's redundant Greek: e.g. Mk 1:32 cf. Lk 4:40; Mk 1:35 cf. Lk 4:42. If we regard the ἡγέρθη as an elaboration of τὸν λῶντα we might say that the charge of redundancy applies only to the first part of the statement of 6a. So Luke would have retained at least ἡγέρθη, had it been found in his source; and in view of Marcion's attestation we could have considered this word to be part of the Lukan text. In that case the reading of the majority of witnesses could be explained in terms of the extreme brevity of the angelic message which was crying out for amplification. But in point of fact, v.5 already serves the purpose of the angelic announcement of the Resurrection, so that the redundancy becomes only too glaring. Therefore neither Marcion's text, nor its closest ally, viz. the c reading, has a claim to originality<sup>9</sup>, which is a con-

firmation of our earlier findings.

(b) A.C. Clark cites this passage as an example of accidental omission at the end of a *στίχος* <sup>10</sup>.

But since the MS data point in the direction of successive attempts at expansion of an originally shorter reading, this theory loses its validity. Moreover, even if this explanation for the omission in one group of MSS be admitted, it would not satisfactorily explain the omission in all the groups. Thus we have reason to endorse Plummer's view<sup>11</sup> that "A reason for the omission is hard to find". Easton supports this conclusion by stating that "the addition would have been much easier than the omission"<sup>12</sup>.

#### Intrinsic Probability

(i) Reference has already been made to the main point of deviation between the longest reading at 6a and its synoptic parallels, viz. the presence or absence of the *ἀλλά*. According to Metzger, the majority of the UBS Editorial Committee felt this adversative particle to be an indication of the Lukan rewriting of the source material<sup>13</sup>.

The same type of resourcefulness, however, could be ascribed to a scribe as well. Moreover, the second part of the statement of 6a has more of

an explanatory role than an adversative one in that it accounts for the absence of Jesus' body from the tomb. On the other hand, if the author had used the ἄλλὰ in its emphatic use [He is in fact (or, indeed) risen]<sup>14</sup>, then the first part would be quite superfluous; in that case the simple ἡγέρθη (cf. Marcion's text) would serve as an amplification of the message already conveyed by v.5. In any case, the ἄλλὰ cannot be proved to be of Lukan origin on syntactical grounds.

(ii) It was mentioned in the context of our discussion of 24:3 that Luke was not dependent upon Mk for his version of the Empty Tomb story. Except for the protagonists of the Proto-Luke theory<sup>15</sup>, scholars generally assume Luke's absolute reliance on Mk for his Passion narrative despite their acknowledgment that the Emmaus narrative, and possibly the account of the Appearance to the Eleven, were derived from an independent source, written or oral. The divergences from Mk in Lk 24:1-11 are explained by the latter group of scholars as due to Luke's thorough reworking of his Markan material. But even among the defenders of Proto-Luke, there is no unanimity as to Lk's relationship to Mk with regard to the discovery of the Empty Tomb. Streeter argues that "Luke is in the main reproducing

an account of the Passion parallel to, but independent of Mark, and enriching it with occasional insertions from Mk"<sup>16</sup>. As regards the narrative of the Empty Tomb, he maintains that this non-Markan source contained such a brief account of the Entombment as well as of the Crucifixion that "from 23:33 to 24:10a, Luke reverses his procedure and makes Mark his main source"<sup>17</sup>. In his Behind the Third Gospel, V. Taylor asserted that "only an indiscriminate use of the undoubted fact that in considerable portions of the Third Gospel St. Luke has used Mk. as a primary source, can blind us to the probability that in this narrative, as in nearly all those connected with the Passion and Resurrection Lk and Mk are independent works"<sup>18</sup>. He maintained this independence with particular reference to 23:56b-24:11<sup>19</sup>. But in his posthumous publication entitled The Passion Narrative of St. Luke he has modified this position to accommodate some degree of Markan influence at 24:1-3 (vide infra). Perry observes; "Certainly, the theory of a special source for the Passion narrative is to be preferred, since it explains more of the facts than does any other, even though the source must have been given a somewhat extensive revision when Mk came to hand"<sup>20</sup>.

In the light of such varying shades of scholarly opinion, it is imperative to examine afresh the nature and extent of the deviation of the Lukan account concerning the Empty Tomb from that of Mark. First, there are numerous stylistic differences between Lk and Mk: ἔτοιμάζειν, ἦλθον ἐπὶ, ὄρθρος, μνηῆμα (v.1); ἐγένετο with ἐν and the infinitive, ἀπορεῖν, ἰδοὺ ἄνδρες δύο, ἀσπάζειν, ἐσθῆς, ἐφιστάσθαι (v.4); ἔμφοβος (v.5); μνηστῆσαι (vv.6,8); ὡς (v.6); δέσμιαι, ἁμαρτωλός (v.7); ῥῆμα (v.8); ὑποστρέφειν, ἀπαχέλλειν (v.9); σὺν (v.10); ἐνώπιον, ὡσεὶ, λῆρος (v.11), are all expressions peculiar to Luke<sup>21</sup>. Μαρία ἡ Ἰακώβου may be from a source other than Mk: it cannot be the same source which furnished the list of the women of 8:2, but another tradition which carried the story of the Empty Tomb as a separate pericope. So it need not necessarily be from Mk<sup>22</sup>. Similarly, the ἡ μία τῶν σαββάτων is likely to have been established in Christian usage very early<sup>23</sup>: cf. Ac 20:7; Jn 20:1,19; 1 Cor 16:2. ἐπὶ τὸ μνημα (v.1) is not exactly identical with Mk's ἐπὶ τὸ μνημεῖον. ἀπὸ τοῦ μνημείου (v.9) is common to both Gospels, and it could well be a recollection here of the Markan material. The

εἰσελθούσας of v.3 may be claimed to be derived from Mk v.5, but since the verse as a whole conflicts with Mk in that it explicitly states that the women did not find Jesus' body, it is more reasonable to ascribe it to a non-Markan source<sup>24</sup>. In fact Easton asserts that "the style of vv.3-5 is much too Semitic to be due to Lk"<sup>25</sup>.

There are bits and pieces here and there which have been interpreted as Luke's editorial modification of Mk: e.g. Mk v.2, ἀνατείναντος τοῦ ἡλίου becomes ὄρθρου βαθείως in Lk (v.1); but it might be from a parallel tradition as well<sup>26</sup>. Cf. the ἄνδρες δύο of v.4 (see Ac 1:10) with Jn 20:12 as against the νεανίσκος of Mk 16:5 and the ἄγγελος of Mt 28:2. Why should such drastic changes be brought about by our author unless he is following some source other than Mk?<sup>27</sup> B. Weiss<sup>28</sup> regards v.2 as a condensation of Mk vv.3-4; but V. Taylor says that it could have originated in oral tradition<sup>29</sup>. Thus in the end the stylistic and verbal resemblances between the two parallel Synoptic passages turn out to be too superficial. Some of the expressions common to both may have been necessitated by the context, and so the theory of the reworking of his source-material by Luke cannot be established by the mere counting of words.

V. Taylor is right in pointing out that there is a basic difference between Lk 24:1-3 and 24:4-11 vis-à-vis their relationship to Mk 16:1-8. "In the former section the problem is mainly numerical; in the latter it is predominantly a question of subject-matter"<sup>30</sup>. However, in view of the facts mentioned in the two fore-going paragraphs, it does not seem relevant to point to the relatively high percentage of Lk-Mk contacts in the former section (15 words out of a total of 37 words, i.e. 40%)<sup>31</sup>. Taylor's later position is in fact a departure from his stance in Behind the Third Gospel<sup>32</sup>. There he had maintained against K. Lake<sup>33</sup> that Lk 23:56b-24:11 was independent of Mk. He had claimed that linguistic considerations were decisively in support of this: out of the 163 words in this section, only 37 are common to Lk-Mk (22.7%) and if we omit the suspected words at 24:3b and 9 (?) the number of words in common drops to 30 (19.7%). "The phraseology of the passage is marked by several distinctive Lukan words, and this is not sufficiently explained by the view that the Lukan story is a free rendering of Mk xvi.1-8"<sup>34</sup>. Although Taylor acknowledges his deference to the critics in modifying his views in his latest work<sup>35</sup>, I do not feel convinced that this change has been in the right direction. This will become clearer

as we pass on to consider the subject-matter of  
 24:1-11. Suffice for the present to be reminded  
 of Perry's observation that between any two Passion  
 narratives there is bound to be considerably more  
 similarity than what obtains between parallel accounts  
 in the earlier sections of the Gospels and that a  
 general similarity is no guarantee of documentary  
 relationship<sup>36</sup>.

The overwhelming non-Markan features of  
 the Lukan account of the Empty Tomb cannot be easily  
 explained. The main point of contact here is the  
 mention of Galilee (v.6). But the context of that  
 reference is radically different in Lk. In Mt the  
 angel's message to the disciples conveyed through  
 the women consisted of the imminent appearance of  
 the risen Jesus in Galilee in fulfilment of His  
 prophecy (cf. 14:28). But in Lk the women are  
 reminded of Jesus' prophecy concerning His Passion  
 and Resurrection while He was still in Galilee  
 (cf. 9:22). This angelic announcement is introduced  
 by a mild rebuke: *τί ἠγείτε τὸν λῶντα μετὰ  
 τῶν νεκρῶν* ;<sup>37</sup> Scholars generally explain  
 this deviation in Lk as the author's deliberate  
 alteration of the Markan material in order to  
 conform to his plan of restricting the Resurrection  
 appearances to Jerusalem and its vicinity<sup>38</sup>.

I find it difficult to accept this theory for the simple reason that I cannot ascribe wilful falsification of a given document to such a careful and trustworthy author like Luke. He could, of course, be charged with historical inexactitudes (cf. Ac 5:36ff) where his information was probably scanty or confusing. But in the present case, why should he suppress the Galilean tradition if it were known to him? One explanation is that a reference to the Galilean appearance would be in conflict with Lk 24:49 par. Ac 1:4<sup>39</sup>. But if we grant Luke's suppression of facts he could as well be expected to adapt his wording in such a way as not to mention Jesus' charge that the disciples should not leave Jerusalem before receiving the gift of the Holy Spirit. Galilee had great significance in the public ministry of Jesus as attested by all the Evangelists, and the reliability of the Galilean appearance which has the support of Matthew, John and possibly Paul (1 Cor 15:6) cannot be doubted<sup>40</sup>. If Luke were using the Markan source for his account of the Empty Tomb, he could be expected to have given us at least a passing reference to the Galilean appearance even if he had no means of elaborating on it. (Cf. His allusion to the appearance to Peter, v.34 which he does not narrate). So the absence of this reference in Lk can be better

explained in terms of the author's recourse to an alternative source which coincided with 6b-7. This would be in keeping with Luke's reputation for his adherence to his sources. Gardner-Smith's observation<sup>41</sup> is very pertinent here: "To what extent Luke is here relying upon tradition independent of Mark, or to what extent he is himself re-writing Mark in order to make way for his later narrative, it is difficult to say. The question is not important<sup>42</sup>. Either he or others who accepted the 'Jerusalem tradition' deliberately altered the account which is embodied in Mark and Matthew<sup>43</sup>, but the fact of the doubling of the angel seems to suggest that the change had been made in Jewish circles before Luke received his material. This much is plain: Luke did not consider Mark's written Gospel which lay before him to possess such authority that he might not properly contradict its statements if it suited his plan to do so. And yet he relied upon Mark for a great part of the material for his Gospel"<sup>44</sup>.

It must be noted that Gardner-Smith adduces only 'the doubling of the angel' as a valid reason for Luke's dependence upon a non-Markan source. The validity of his stance will become much stronger when we set out the three relevant Synoptic accounts in the following table:<sup>45</sup>

1. Luke     The narrative does not begin with the names of the women. In v.10 three women are mentioned by name although other women are also reported to be with them. Only two of the names are identical with Mark's list.
 

Matt     The narrative begins with the names of two women. The two names coincide with Mark's list, but one is introduced differently from Mk.

Mark     The narrative begins with the names of three women.
  
2. Luke     The women prepare spices in between the burial and the onset of the Sabbath.
 

Matt     No reference to spices.

Mark     The women buy spices on Saturday evening as soon as Sabbath was over (i.e. after 6 p.m.).
  
3. Luke     They set out at early dawn to the tomb (cf. Jn, while it was still dark).
 

Matt     They set out toward the dawn.

Mark     They set out very early...when the sun had risen.
  
4. Luke     The intention of ~~the~~ women to embalm the body is not mentioned but certainly implied.
 

Matt     No mention of their intention.

Mark     The intention of the women clearly specified.
  
5. Luke     No allusion to the women's fears concerning the stone.
 

Matt     No allusion to the women's fears concerning the stone.

Mark     The women express fear concerning the stone.

6. Luke No mention of the earthquake or the rolling away of the stone by an angel. Mention of 'two men' (= angels). No allusion to the size of the stone. The two men were in dazzling apparel.
- Matt Mention of earthquake; descent of an angel of the Lord (cf. Gen. 16:7) who rolled back the stone and sat upon it; no allusion to the size of the stone; the fright of the guards described. The appearance of the angel was like lightning, and his raiment white as snow.
- Mark All details missing as in Lk except for the reference to the largeness of the stone. Mention of a young man (= angel). The young man was dressed in a white robe.
7. Luke The women were afraid.
- Matt The reaction of the women was not mentioned.
- Mark The women were amazed.
8. Luke In place of a reference to Jesus' imminent appearance in Galilee, the angels remind the women of Jesus' prophecy while He was still in Galilee; these words are introduced by a mild rebuke. No invitation to the women to see the place where Jesus lay.
- Matt The angelic message contains a reference to Jesus' imminent appearance in Galilee in fulfilment of His prophecy; no rebuke, but words of encouragement, 'Do not be afraid', followed by: 'for I know that you seek Jesus who was crucified'; the angel recalls Jesus' prophecy; an invitation to see where He lay. 'Lo I have told you' is peculiar to Mt.
- Mark The angelic message is the same as in Mt except for a slight difference in the words of encouragement, 'Do not be amazed', followed by: 'You seek Jesus of Nazareth who was crucified'; the angel recalls Jesus' prophecy; an invitation to see the place where they laid Him. 'Lo I have told you' is absent.

9. Luke It is not clear whether the angels addressed the women inside the tomb or outside ('they went in'<sup>46</sup>, v.3, cf. 'two men stood by them'<sup>47</sup>, v.4; but '[they] bowed their faces to the ground', v.5).
- Matt The angel was seated on the stone outside the sepulchre.
- Mark The 'young man' was seated on the right side (where the body was). The women were also inside the sepulchre.
10. Luke The women are reported not to have found the body.
- Matt No parallel.
- Mark No parallel.
11. Luke The women remembered Jesus' words.
- Matt No such statement.
- Mark No such statement.
12. Luke The women report the news to the eleven and all the rest.
- Matt They depart quickly with fear and great joy and run to tell the disciples.  
Mt. does not actually state that the message was delivered.
- Mark They flee from the tomb in trembling astonishment; they hush the message because of fear.
13. Luke The Apostles dismiss the women's report as 'idle tale'.
- Matt The narrative does not refer to the reaction to the report of the women. This again, makes it doubtful whether the women reported the news at all.
- Mark The narrative ends abruptly on the note of fear of the women.

It is true that Luke's account of the Empty Tomb has one or two points of contact with the accounts of Mark and Matthew as in the case of the early hour of the women's visit<sup>48</sup> or the angelic allusion to Jesus' prophecy. Lk shares No.12 (the action of the women subsequent to the discovery of the Empty Tomb) in common with Mt. But these instances are too inadequate to establish any real indebtedness to either of the other two Synoptics. Easton regards the Lk-Mt contact just mentioned as inevitable because the women's alleged silence, according to Mk, had isolated the account of the Empty Tomb from the rest of the Resurrection evidence, so that the natural tendency would be to remove this isolation and let the women play an important role in conveying the good news<sup>49</sup>. But Easton overlooks the fact that the Lk-Mt contact here is too minimal: whereas Lk specifies the reaction of the Apostles to the women's report, Mt is content merely to state that the women ran to convey the news to the disciples. Moreover, Lk does not actually relate the discovery of the Empty Tomb to the fact of the Resurrection as specifically as Mt does; except for the reference to the recollection of the women concerning Jesus' prophecy of the Resurrection, there is no explicit statement

as to whether they immediately became believers in the fact of the Event. Mt, however, gives the impression of a sense of mission on the part of the women in that they are reported as departing quickly 'with fear and great joy' and rushing (a clear indication that they did believe that Jesus was risen indeed) to convey the glad tidings to the disciples. Is it not then more reasonable to believe that Luke and Matthew are relying upon their own sources at this point rather than rewriting Mk?

The few verbal resemblances between Lk and Mk which we have considered earlier, coupled with the above-mentioned points of contact between these Gospels, have been deemed by J. Schmitt<sup>50</sup> to be adequate proof of Luke's dependence on Mk 16:1-5 for the first part of his narrative (vv.1-4). Similarly, on the strength of the meagre data mentioned above he concludes that Luke drew from Mt 28:5-8 towards the concluding part of his account of the discovery of the Empty Tomb (vv.5-10).

On the other hand, Schmitt attributes some of the peculiarities of the Lukan narrative, vv.1-11, to the Jerusalem tradition: "Que valent ces traits? Selon toute vraisemblance, ils n'ont pas été "créés"

par l'auteur; ils remontent plutôt à la tradition hiérosolymitaine de la vie du Christ"<sup>51</sup>. Regarding Luke's mode of handling his sources, Schmitt writes: "...Bien qu'il suive, dans ensemble, les relations de Marc et de Matthieu, Luc les précise et les corrige, cependant, par des traits empruntés, selon toute vraisemblance, à la tradition hiérosolymitaine...Il témoigne de la liberté et de la maîtrise avec lesquelles l'évangéliste utilise ses sources. Par ailleurs, il annonce la place de premier plan tenue par la tradition judéenne dans la suite de notre récit"<sup>52</sup>. Schmitt argues that since Luke had followed the Synoptic tradition in the earlier part of the Passion narrative he gave preference to the same tradition in respect of the Empty Tomb story; moreover, in Luke's thought, the discovery of the Empty Tomb was in some way the epilogue of the burial account. Here and there he has retouched the Synoptic tradition by means of elements borrowed from the Jerusalem tradition because it was more primitive than the former; e.g. the minor details like the number of women, the number of angels etc.<sup>53</sup>

With regard to Lk's divergences from Mt, Schmitt maintains that they do not betray Lk's independence of Mt, but only the theological predilection of Luke: "Le troisième évangile - on

l'a vu - dépend du premier sur tous les autres points où il s'accorde avec lui... Les quelques discordances qu'accusent nos récits ne témoignent guère contraire. De fait elles offrent un intérêt plutôt secondaire et s'expliquent, à vrai dire, par la diversité des buts propres aux deux auteurs... Plus théologien, Luc souligne que l'histoire évangélique rend témoignage de la Résurrection"<sup>54</sup>.

However, a close look at the discrepancies between Mt and Lk on the one hand, and between Mk and Lk on the other, makes it extremely difficult to believe that Luke had made use of the first two Gospels as his primary sources for the account of the discovery of the Empty Tomb. It is not only the number of these divergences, but also their radical nature that confirm our suspicion of the kind of relationship maintained by Schmitt. We have already seen that there is reason to suppose that both Luke and Matthew were drawing upon their separate sources at this point. If this is extended to its logical conclusion, is it too hard to imagine that Luke owes all the peculiar traits of his narrative here to his special source, whether we prefer to call it L or the Jerusalem tradition? To highlight just one of the dissimilarities listed in the tabular columns, which is neither of a theological

nature (to be explained in terms of Luke's rewriting of Mk or Mt), nor a matter of minor significance (to be explained away in terms of the fading memory of the Christian congregations in regard to details such as the number of the women or the number of angels<sup>55</sup>); the three leading women in the group of witnesses to the Resurrection are mentioned by name for the first time at v.10. The list is, of course, different from that of Mk 15:47 and 16:1 cf. Mt 27:61; 28:1. In neither of the above two contexts where Luke has followed Mk, nor at 24:1, did he think it fit to introduce these names. The awkwardness of this verse here is evident from the various attempts at improving upon its syntax, as attested by the ancient MSS. Since the verse as a whole is found to be genuine, although not befitting Luke's reputation as an author, the only explanation that is plausible for its origin is that it belonged to an independent source from which Luke was borrowing<sup>56</sup>. It must especially be mentioned that Schmitt maintains his theory of Luke's indebtedness to Mt by defending the originality of v.6a which is the crux of the whole matter<sup>57</sup>. If we leave out 6a, the resemblances between Mt-Mk and Lk become very remote indeed. In fact 6a only interrupts the tone of the angel's rebuke recorded by Luke. Schmitt somehow wants to maintain Luke's reliance

on the other two Synoptics, but the evidence he provides is too meagre to be convincing. He admits of the existence of a special source behind Luke's narrative, viz. the Jerusalem tradition; but he subordinates its role to that of Mt and Mk. How much better would the recognition of a special source at the expense of the first two Gospels fit all the facts of the case!

### Conclusion

If the independence of Lk from Mk and Mt is accepted in respect of the episode concerning the discovery of the Empty Tomb by the women, the question of the authenticity of 24:6a resolves itself. Hence the justification for the lengthy discussion above. We may suppose that by the time the Third Gospel came to be written, the author had only a mutilated copy of Mk which he made ample use of up to the account of the Entombment. But the imperfect and abrupt nature of the Markan narrative concerning the Resurrection would have so disappointed our author that he was forced to rely upon another source<sup>58</sup>. His familiarity with Mk 16:1-8 may have indirectly affected the broad outline of his narrative in its earlier part, but the resemblances are so peripheral that they may be considered as accidental rather than purposive. Hence we conclude that the

redundant statement of 6a formed no part of the original.

The suspicion of harmonization arises from the fact that in the rest of the whole narrative the similarities with Mk or Mt are nowhere so pronounced as here. We have shown that the Lukan narrative is indebted to neither Mk nor Mt and that the adversative  $\alpha\lambda\lambda\alpha$  has no claim to be original. Under such circumstances, the principle that identity of reading suggests identity of origin may be applied with sufficient justification. Therefore it follows that 6a is the result of deliberate harmonization to Mt 28:6, or less probably to Mk 16:6<sup>59</sup>.

This conclusion is only a corroboration of the view held by the vast majority of scholars who have been cited in the present discussion<sup>60</sup>.

<sup>1</sup>Cf. Aland, NTS 12 (1965-66), p.205.

<sup>2</sup>Ibid.

<sup>3</sup>Evangelion, II, p.232.

<sup>4</sup>Text and Canon, p.128.

<sup>5</sup>St. Luke, ICC, p.548.

<sup>6</sup>Those who favour the suggestion that the interpolation is from Mt: J. Schmid, Lukas, p.253; Kilpatrick, Essays in Memory of G.H.C. Macgregor, p.193. Although Lagrange lists this under the category of uncertain passages, he expresses himself in favour of the theory of assimilation to Mt. Assimilation to Mk is favoured by Creed (St. Luke, p.293), Tasker (Greek N.T., p.424) Loisy (Luc, p.568), Hawkins (Oxford Studies, p.77). Those who reject the additional words as spurious, without committing themselves to either source: Hort, Notes, p.71; B. Weiss, Markus u. Lukas (Meyer's Kommentar), ad.loc.; Plummer, loc.cit.; Klostermann, Lukasevangelium, p.599; Leaney, St. Luke, p.292; Schubert, Bultmann Festschrift, p.168; J.A. Bailey, Traditions (Supplement to NovTest vii), p.81; J. Weiss, Schriften des NT, p.482; V. Taylor, Behind the Third Gospel, p.64. Among the uncommitted critics may be mentioned Lampe, Peake's Commentary, ad.loc. and Alford, Greek N.T. I<sup>5</sup>, ad.loc. K. Lake, Historical Evidence for the Resurrection, p.70, and W. Manson, Luke, ad.loc., accept the longer reading without comment.

<sup>7</sup>Loc.cit.

<sup>8</sup>So B. Weiss (loc.cit.), Plummer (loc.cit.), Klostermann (loc.cit.).

<sup>9</sup>Against Aland (loc.cit.) who holds that Marcion's reading is the result of deliberate amputation of a longer text. It may be that in Marcion's view the message of the Resurrection had to be announced in a more direct manner (He is risen), than would be permissible with the shorter Lukan reading.

<sup>10</sup>Primitive Text, p.53.

<sup>11</sup>Loc.cit.

<sup>12</sup>St. Luke, p.356.

<sup>13</sup>Textual Commentary, p.184; cf. J. Jeremias, Eucharistic Words (E.T.), p.149.

<sup>14</sup>Cf. Dana and Mantey, Manual Grammar, p.240.

<sup>15</sup>This theory is only an outgrowth of the theory of Luke's use of a third source first propounded by P. Feine, *Eine vorkanonische Überlieferung des Lukas* (Gotha, 1891); cf. J. Weiss, *Schriften des N.T.*, Göttingen (1906), B. Weiss, *Die Quellen der synoptischen Überlieferung*, Leipzig (1908). While Feine and B. Weiss included practically the whole of Luke 24 in this third source, J. Weiss left out the story of the Empty Tomb. On the other hand, V.H. Stanton, *The Gospels as Historical Documents*, II, Cambridge (1909), maintained that in addition to Mk, Luke used two other principal sources, one for the Infancy narrative and the genealogy, the other being an expanded form of Q (p.229). Stanton also ascribed nine sections including ch.24 to the Evangelist (310).

<sup>16</sup>Four Gospels, p.202, Cf. Preface pp.xi-xiii.

<sup>17</sup>Ibid., p.217.

<sup>18</sup>P.65.; cf. *Formation of the Gospel Tradition*, pp.191-201.

<sup>19</sup>Behind the Third Gospel, p.62.

<sup>20</sup>Exp.T. 46 (1934-5), p.259, Cf. *Sources of Luke's Passion Narrative*, p.99.

<sup>21</sup>Vide Easton, *St. Luke*, pp.357-8; V. Taylor, *Behind the Third Gospel*, p.64, n.2.

<sup>22</sup>B. Weiss thinks that the list of names here was derived from L plus Mk. (Lukas<sup>9</sup>, Göttingen, 1901, ad.loc.); cf. Easton, op.cit., p.358.

<sup>23</sup>So V. Taylor, *Passion Narrative*, p.106.

<sup>24</sup>J. Schmitt, *RevScR* 25 (1951), p.123 notes that *καὶ εἰσελθούσαι* of Mk (Aramaism) is replaced in Lk by *εἰσελθούσαι δὲ* (idiomatic Greek). But the phrase *εἰσελθούσαι δὲ* is textually suspect, and Wellhausen (*Evangelium Lucae*, ad.loc.) considers it as a gloss.

<sup>25</sup>Loc.cit.

<sup>26</sup>So Easton, op.cit., p.357.

<sup>27</sup>So Gardner-Smith, *Narratives of the Resurrection*, p.148, n.2. Holtzmann (*Hand Commentar*, ad.loc.) suggests that Luke added together the angels of Mk v.4 and Mt v.26; Morgenthaler, *Die Lukanische Geschichtschreibung als Zeugnis* (Zürich, 1948), I, pp.98,181; II, pp.7-10, ascribes this to Luke's predilection for "double witnesses"; cf. P. Schubert, *N.T.che Studien für R. Bultmann*, p.167. But such explanations, even if acceptable, do not account for the large number of major deviations in Lk.

<sup>28</sup>Markus u. Lukas (Meyer's Kommentar)<sup>7</sup>, p.677.

<sup>29</sup>Passion Narrative, loc.cit.

<sup>30</sup>Ibid.

<sup>31</sup>Ibid.

<sup>32</sup>Pp.62-64.

<sup>33</sup>Historical Evidence for the Resurrection, p.59.

<sup>34</sup>Op.cit., p.64.

<sup>35</sup>Passion Narrative, Preface, p.ix.

<sup>36</sup>ExpT 46 (1934-35), p.257.

<sup>37</sup>Cf. Creed, St. Luke, p.293; Schubert, op.cit., p.168.

<sup>38</sup>Cf. Creed, loc.cit., Gardner-Smith, op.cit., p.49.

<sup>39</sup>J. Weiss, Schriften des N.T., p.482.

<sup>40</sup>There is no need to doubt the authenticity of the Judean appearance as well, especially in the light of the Emmaus story which only few scholars have doubted. See Gardner-Smith, op.cit., pp.169-70. His theory that "Only in Jerusalem itself, where some of the chief actors in the drama lived, would the Galilee tradition be safeguarded..." is not tenable in view of the testimony of John. It is not a question of one tradition suppressing the other. The question is, which author had access to which tradition.

<sup>41</sup>Ibid., p.41.

<sup>42</sup>I think the question is important for our discussion of Luke's sources.

<sup>43</sup>I do not agree with this view.

<sup>44</sup>The last part of the quotation highlights Luke's faithfulness to his sources at any given section of his work.

<sup>45</sup> See Gardner-Smith, *op.cit.*, pp.34ff; J.Schmitt, *RevScR* 25 (1951), pp.121ff., Taylor, *Behind the Third Gospel*, pp.60-65.

<sup>46</sup> The variant ἐλθούσα in D Γ 0124 it<sup>c</sup> cop<sup>sa</sup> might suggest that in an earlier form of the story the conversation with the angels may have been reported as taking place outside the sepulchre as in Mt. (So Gardner-Smith, *Narratives of the Resurrection*, p.48,n.1).

<sup>47</sup> Taylor, *op.cit.* p.64, suggests that the verse implies a sudden appearance of the angels.

<sup>48</sup> With regard to the difficulties involved in the time-reference of the three Evangelists, vide Gardner-Smith, *op.cit.*, pp.36-39.

<sup>49</sup> *St. Luke*, p.358.

<sup>50</sup> *Op.cit.*, p.127.

<sup>51</sup> *Ibid.*, p.121.

<sup>52</sup> *Ibid.*, p.132.

<sup>53</sup> *Ibid.*, p.134.

<sup>54</sup> *Ibid.*, p.126.

<sup>55</sup> Cf. *ibid.*, p.134. According to K. Lake, *Historical Evidence of the Resurrection*, pp.67, 185, it was popularly held in Jewish circles that 'two angels' constituted the proper delegation on divine errands. Gardner-Smith, *Narratives of the Resurrection*, p.48, n.2, notes this deviation in Lk as "a clear evidence of a non-Marcian tradition" behind the narrative. Reumann, *Jesus Christ in the Church's Gospels*, p.356, n.13, suggests the influence of Dt 19:15 (two witnesses to confirm the truth).

<sup>56</sup> See Gardner-Smith, *op.cit.*, pp.47-48.

57 "En fait, le verset 6a offre toutes les garanties d'authenticité. Il est attesté par la presque totalité des manuscrits grecs and latins. Surtout, il représente un élément nécessaire du récit de Luc" (op.cit., p.125). Among those who maintain Luke's dependence upon Mk in the present pericope may be mentioned F. Hauck, Lukas, p.289; Klostermann, Lukasevangelium, p.232. Those who reject this view include A. Loisy, Les Evangiles Synoptiques (Paris, 1907), ad loc; Lagrange, Luc, (Paris, 1921), ad loc.

58 Gardner-Smith, op.cit., p.16: "There is every reason to believe that the mutilation of the MS of Mk happened at a very early date, before either St. Matthew or St. Luke had read it, perhaps before it had been copied, so that the first and third gospels give no indication of the way in which St. Mark concluded his narrative" (see also on Mk 16:9-20).

59 Cf. Lagrange, Critique Textuelle, II, p.70.

60 Graefe, TSK, 1896, p.268, strikes a dissenting note: 'So sehr ich an and für sich die Möglichkeit zugeben muss, dass die Engelbotschaft aus Matthäus oder Markus ergänzt wurde, so ist doch der Satz: "Er ist nicht hier, sondern auferweckt" kaum entbehrlich, zumal der Zusatz folgt: "Gedenkt daran, wie er es euch sagte, als er noch in Galiläa war". Die Art, wie Lukas die Landschaft Galiläa im Zusammenhang mit der Osterbotschaft erwähnt, scheint mir ohnehin sachlich den Vorzug zu verdienen vor der Art, wie Matthäus und Markus Galiläa als nächstes Reiseziel des Auferstandenen erwähnen'. But this view can only be judged as too subjective in the light of our analysis of the facts of the case.

I.B. The Empty Tomb Story

c. Lk 24:12: The Role of Peter

Disputed words: [Ὁ δὲ Πέτρος ἀναστὰς ἔδραμεν ἐπὶ τὸ μνημεῖον, καὶ παρακύψας βλέπει τὰ ὀθόνια μόνα καὶ ἀπῆλθεν θρυμάζων τὸ γεγονός.]

External Evidence:

Nestle<sup>26</sup>

Greek NT (UBS)

Ὁ δὲ Πέτρος ἀναστὰς ἔδραμεν ἐπὶ τὸ μνημεῖον, καὶ παρακύψας βλέπει τὰ ὀθόνια μόνα καὶ ἀπῆλθεν πρὸς ἑαυτὸν θρυμάζων τὸ γεγονός. (vorher nicht im Text)

[Ὁ δὲ Πέτρος ἀναστὰς ἔδραμεν ἐπὶ τὸ μνημεῖον, καὶ παρακύψας βλέπει τὰ ὀθόνια μόνα καὶ ἀπῆλθεν πρὸς ἑαυτὸν θρυμάζων τὸ γεγονός.]

txt P<sup>75</sup> א ABKLWXΔΘΠΨ 063 079 0124 f<sup>1</sup> f<sup>13</sup> 28 33 565 700 892 1009 1010 1071 1079 1195 1216 1230 1241 1242 1253 1344 1365 1546 1646 2148 2174 Ξ<sup>185m</sup> aur c f ff<sup>2</sup> vg syr<sup>c</sup> s. p. h. pal<sup>mss</sup> sa bo arm aeth georg Eus<sup>1,2</sup> Cyr II — D a b d e l r<sup>1</sup> syr<sup>pal<sup>mss</sup></sup> Mcion Eus<sup>1,2</sup>

Observations

(i) The repeated attempts at emendation of this text gives the impression that an uneasiness was associated with it for several centuries amongst ancient copyists. As Curtis observes, "The absence of *κεῖμένα* from *ABW* and its varying place in relation to *μόνα* <sup>1</sup>, make the verse suspect.

(ii) Despite the conflicting position of the Old Latins (4 of them supported by the Vulgate have the long text while 6 of them read the short), Aland<sup>2</sup> is impressed by the testimony of the Old Syriac in favour of the long reading alongside of the vast majority of the Greek MSS led by P<sup>75</sup>. Haenchen is

also impressed by the witness of P<sup>75</sup>, but admits that "The question with regard to this verse is not... easy..."<sup>3</sup>.

(iii) Both Burkitt<sup>4</sup> and Souter<sup>5</sup> have adjudged this verse to be one of the rare cases where the Syr<sup>5</sup> text has suffered a harmonization.

(iv) According to B. Weiss<sup>6</sup>, the witness of D and allies is too inadequate to render the verse spurious.

(v) Patristic evidence is meagre here. The omission in the Diatessaron has to be ignored because the occurrence of a fuller narrative in Jn corresponding to this passage would in any case have obliterated this report in Lk<sup>7</sup>. Eusebius' authority was cited by Tischendorf to justify the deletion of the verse<sup>8</sup>, but as a matter of fact Eusebius attests both forms of the text. Thus it is evident that MS data are not a reliable guide in the present context to solve the textual problem.

### Internal Evidence

#### Transcriptional Probability

##### (i) Possibilities of Insertion

(a) V.12 has several verbal resemblances to Jn 20:5-10<sup>9</sup>:

<u>Lk</u>		<u>Jn</u>
ἔδραμεν	corresponds to	ἔτρεχον (4a) and προέδραμεν (4b).
ἀπῆλθεν πρὸς ἑαυτὸν	corresponds to	ἀπῆλθον . . . πρὸς αὐτοὺς (10).

(note the accusative which is common to both).  
 Besides, παρακύψας βλέπει, τὰ ὀσθία, μηνμείων  
 and the historic present of βλέπειν are common  
 to both Gospels.

This has led many scholars to the hasty  
 conclusion that the Lukan reading was due to an  
 assimilation to Jn 20:3-10<sup>10</sup>. The reason for this  
 insertion has been variously conjectured:

J.A. Bailey writes: "a scribe who thought  
 that the reference to the disciples' having gone to  
 the tomb in xxiv.24 required a mention of the event  
 when it occurred, used for this purpose a synopsis  
 from memory of an incident from John thereby  
 incidentally softening the harsh picture given in  
 xxiv.11"<sup>11</sup>.

In Klostermann's view<sup>12</sup>, the interpolation was  
 necessitated by the disharmony between the disbelief  
 of the disciples (v.11) and the statements in vv.24  
 and 34. Lagrange<sup>13</sup> follows the same line of

argument and ascribes the interpolation to a scribe who was obsessed with the desire to present Peter in a favourable light in view of v.34. Since in the same chapter of Lk there are other signs of borrowing from Jn he thinks that there is a strong case for the assimilation theory, although he lists this passage under the category of the most difficult and uncertain texts.

According to P. Rohrbach's hypothesis<sup>14</sup>, in the early part of the second century, a group of Presbyters of Asia Minor proceeded to set up a four-Gospel canon. They had felt the need for harmonizing the various Gospel accounts concerning the vital topic of the Resurrection so as to facilitate their preaching. For this purpose they chose the Fourth Gospel as their standard, and made certain minimal insertions in the Synoptic Gospels such as Mt 28:9,10, the Long Conclusion of Mk, and Lk 24:12. Moffatt combines all these three hypotheses when he writes: "The insertion of the passage (condensed from Jn xx.3-10) is probably to be attributed to some harmonistic editor, or to the Asiatic presbyters, as a reply to the natural objection - why did not some of the apostles go to the tomb and see for themselves? or to a sense that v.24 required some such episode"<sup>15</sup>.

Since all these theories have originated from the verbal similarities between Lk and Jn, it is very important to examine this phenomenon in greater detail. The following facts deserve to be mentioned in this connection:<sup>16</sup>

While *παράκύπτειν* occurs nowhere else in Lk, it is found twice in Jn (20:5,11). *Μυημεῖον* is more characteristic of Jn than of Lk: Jn has the word 16 times, but no other synonym; Luke uses the synonym *μυήμα* 5 times in Lk-Ac alongside of *Μυημεῖον*.

The expressions *ὁθόνιον* and *ἀπέρχεσθαι πρὸς ἑαυτὸν* also are peculiar to v.12 in Lk whereas Jn exhibits the former four times and the latter once. In fact *ὁθόνιον* occurs only in the present context in Lk and Jn (cf. Jn 19:40; 20:5,6) and nowhere else in the N.T.

Even in the context of the Burial story Luke, following Mk, uses the word *σινδών*, not *ὁθόνιον* (23:53 cf. Mk 15:46). Could it suggest assimilation to Jn here?

As for syntactical comparisons, *ἀπέρχεσθαι πρὸς* with the accusative is unique to v.12 in Lk-Ac, but is found 4 times in Jn as against its 3 occurrences in the rest of the N.T.

In Jn the historic present appears 162 times (cf. Mk: 151 times). As a rule, Luke eliminates this even in portions copied from Mk

(exception: 8:49); he uses it only 24 times in Lk-Ac.

It might, however, be argued that verbal similarities are by no means restricted to this passage. Schniewind calls attention to several parallels in the same chapter of Lk outside the present context<sup>17</sup>. But on closer examination they are found to bear no comparison to the relationship that exists among the Synoptic parallels where dependence on an identical source is clearly discernible:

<u>Lk</u>	<u>Jn</u>	<u>Subject Matter</u>
24:9-11	20:2	The message of the women to the Apostles and to <u>all</u> the rest
<u>Remarks:</u> The 'resemblance' is too far-fetched. John mentions <u>one</u> woman who reports to <u>two</u> of the Apostles.		
24:18	19:25	Cleopas
<u>Remarks:</u> Jn has Clopas; the contexts are quite different even if the names may be identical and are not found elsewhere in the N.T.		
24:25	20:27	Rebuke of unbelief.
<u>Remarks:</u> The context, as also the content and tone of the rebuke, are different.		

<u>Lk</u>	<u>Jn</u>	<u>Subject Matter</u>
24:39	20:27	Invitation to feel the nail-marks on the hands and the side.
<u>Remarks:</u> The context and the actual wording are by no means the same.		
24:41	21:5	The risen Lord asking for food.
<u>Remarks:</u> The resemblance is too remote. Cf. 'Haven't you caught anything?' (TEV).		
24:42	21:10	Serving of the fish.
<u>Remarks:</u> No real resemblance. Jesus acts as the host here!		
24:49	20:21	The commission.
<u>Remarks:</u> No verbal resemblances.		
24:49	20:22	Promise of the Spirit.
<u>Remarks:</u> Luke does not mention an immediate giving of the Spirit as in Jn.		

It is now clear that Schniewind's argument can hardly be used as evidence for or against the authenticity of Lk 24:12 where the verbal resemblances are of a closer degree<sup>18</sup>. J. Schmitt mentions other instances of Lk-Jn contacts which deserve better attention<sup>19</sup>:

<u>Lk</u>	<u>Jn</u>	<u>Subject Matter</u>
7:38b	12:3b	The anointing of Jesus' feet.
<u>Remarks:</u>	The women characters and the setting are different. The few verbal similarities are only incidental when viewed in the context of the dissimilarities. <u>Parallel tradition</u> better explains the phenomenon than dependence of either Gospel on the other.	
22:34	13:38b	Jesus foretells Peter's denial.
<u>Remarks:</u>	Verbal similarity is confined to this part only; but since these parallel verses do not show any textual variant, <u>parallel tradition</u> may be the clue to their similarity.	
21:37b	18:26	Jesus' routine on the week preceding the Passion.
<u>Remarks:</u>	Resemblance too remote.	
23:53c	19:41c	Description of the Tomb.
<u>Remarks:</u>	Verbal similarity is confined to this part of the Burial story. <u>Parallel tradition</u> might lie behind this.	
24:1a	20:1a	The time of the women's visit to the Tomb.
<u>Remarks:</u>	Since similarities are common to all the Gospels, <u>an oral tradition</u> may have given rise to this phenomenon which is limited to this part of the Empty Tomb Story.	

It may be that the solution to the problem under discussion lies in the identical inference that we have drawn in this tabulation except in the third instance where the 'resemblance' is too far-fetched. In all other instances, although the passages on either side are not identical, there is a considerably high degree of correspondence. The case of Lk 24:12 is on a par with the phenomena presented by the above parallels. Therefore it is reasonable to assume that a parallel tradition was at the root of the Lk-Jn contact at this point.

On the other hand, there are several Lukan characteristics in v.12 which might suggest the possibility of the verse going back to the author himself. This consideration has to be deferred until we take up the Intrinsic Probability. What has so far emerged from our discussion is that a convincing case cannot be made in favour of the theory of assimilation to Jn based on the few linguistic similarities alone. Such similarities might as well have arisen out of the use of parallel traditions by the two authors. For example, M. Black points out that the expression  $\pi\rho\acute{o}s \acute{\epsilon}\delta\upsilon\tau\acute{o}\nu$  (cf.  $\pi\rho\acute{o}s \acute{\alpha}\upsilon\tau\acute{o}\nu\varsigma$ , Jn 20:10) is an Aramaism which might suggest such a possibility<sup>20</sup>. The

similarities could also have been dictated by the context. As Jeremias says, "The use of these words is required by the nature of the material and there can be no question of a specifically Johannine idiom"<sup>21</sup>.

The dissimilarities in the wording of v.12 also may point away from indebtedness to Jn: e.g., there is no mention here of Jn's σουδᾶριον; Lk has ἀνοστᾶς and θαυμάλιων τὸ γεγονός as additions. Above all, while Jn mentions the 'other disciple', the disputed text in Lk mentions only Peter. This one factor alone suffices to undermine the notion of scribal assimilation to Jn. Besides, according to Jn 20:4-5, ὁ ἄλλος μαθητὴς is the subject of παρακύψας βλέπει, not Peter, as Lk 24:12 asserts. Why should assimilation proceed along such a half-hearted and devious line<sup>22</sup>? If it was in the interest of magnifying Peter's role at the expense of the other disciple, this imaginary interpolator contravenes his very purpose. For, although the two disciples are presented as being exposed to the same experience, the Fourth Gospel associates 'belief' with the other disciple and not with Peter (v.8). Even the 'belief' of this disciple was not conviction in the reality of the Resurrection

in view of John's statement in v.9 as to their lack of understanding of the Scriptures. An interpolator who was relying on the Johannine account would not have failed to take note of this. He would no doubt have transferred at least some degree of 'belief' to his 'favourite' Apostle if he were definitely intent on ignoring the other disciple. So Hort's comment regarding the spuriousness of Lk 24:12 sounds arbitrary in the extreme; "Text comes from Jn xx 3-10 (except ἀναστᾶς and θαυμάζων τὸ γεγονός ) condensed and simplified, with the omission of all that relates to the other disciple"<sup>23</sup> (!).

E.A. Abbott's statement that "it is incredible that anyone but a heretic or a rejector of the Fourth Gospel could interpolate such a truncated and falsified version of John's consistent narrative without even taking the trouble to reconcile it with Luke's later statement "some of those with us"<sup>24</sup> (sc. at v.24) seems to be a fitting criticism of the theory of interpolation based on Jn.

(b) B. Weiss's suspicion of a gloss here is an improvement upon the above theory. He says that neither the absence of ἄλλος μαθητῆς, nor the disharmony with v.24 speaks against v.12 being a gloss<sup>25</sup>.

Of course, it could be conceded that the scribe had overlooked the *Ἰησοῦς* in v.24. But was it that easy for him to overlook the mention of the other disciple even if he were only making a summary of the Johannine story in the margin of his copy of Lk? F.C. Grant's proposal may be supposed to be an attempt to answer this basic question. According to him, the passage "is obviously a gloss summarising the paragraph in John for completeness' sake, and inserted by some scribe who was unaware of John's purpose in supplementing and correcting the Markan account (sic) by having the Risen Lord appear, not to Peter and John, but to Mary Magdalene"<sup>26</sup>. But again, if the aim of this scribe was to provide a fuller account, why the omission of all reference to the other disciple?

(c) The Sinaitic and Curetonian Versions of the Old Syriac exhibit a striking variation of the name Peter at v.12, namely *Šem'ûn* which is peculiar to this passage in Lk. In Mk and Lk these MSS usually have the name *Kēpha*. This has led Burkitt to conclude that the verse must be a later insertion by a scribe who had a special preference for *Šem'ûn*<sup>27</sup>. Burkitt further corroborates this inference by pointing to the fact that whereas in Syr<sup>S</sup> the verse ends a paragraph, Syr<sup>C</sup> begins a paragraph with this verse<sup>28</sup>.

No argument has so far been forthcoming against this evidence. Even Jeremias who defends the originality of v.12 has noted this phenomenon, though in a foot-note, without contradicting it<sup>29</sup>. Could this suggest scribal insertion from a source parallel to the Johannine tradition? Our final decision will depend upon how the other aspects of the case turn out.

(ii) Possibilities of Omission

(a) Haenchen accounts for the absence of the verse from 'Western' witnesses due to its conflict with v.24 which refers to *τινες τῶν σὺν ἡμῖν* as against Peter<sup>30</sup>. On the other hand, Gardner-Smith expresses difficulty in believing that our author would have referred to such a prominent personality of the early Church as Peter in so casual a manner<sup>31</sup>.

It seems incredible to me that a scribe would contemplate the omission of a whole verse even if he had recognized the conflict involved. A simpler alternative for him would be to alter the *τινες* to *εἰς* along with a corresponding change of the *ἀπ᾿ἠλθον* to *ἀπ᾿ἠθεν*. In the opinion of Alford, such discrepancy as in v.24 is bound to arise while speaking in a loose way to an

utter stranger. He adds; "But what wonder if the reports of such a day of anxiety and confusion were themselves disjointed and confused"<sup>32</sup>.

The first part of this argument has indeed some validity. But in the second part Alford is pre-supposing that Luke is giving us a verbatim report of the conversation in question. This is far from the actual Lukan practice which we meet with here and in the speeches of Ac. Although the author gives ample expression here to the anxiety and sorrow of the Emmaus disciples, the entire conversation is cast in Luke's own skilful style, so that the question of confusion does not arise!

(b) Another possibility for deletion may be thought to lie in the disharmony between v.34 and v.12, as proposed by Graefe: "Da möchte ich vermuten, dass v.34 zu der Streichung des Anlass bot. Dort wird vorausgesetzt, dass der Auferstandene persönlich dem Simon am Morgen erschienen sei. So konnte der Urheber des westlichen Textes vielleicht an der Darstellung von v.12, welche dem Petrus nur das leere Grab zeigt, Anstoss nehmen. Die Christenheit würde viel lieber hier die Beschreibung einer Begegnung zwischen Christus und Petrus lesen als diese dunkel Notiz, die erst aus Johannes ihr volles Licht empfängt"<sup>33</sup>.

This argument could be countered by saying that v.12 need not necessarily constitute an offence if it is taken as a primary stage in the author's development of how Peter finally came to belief. Peter did not initially believe because he could see nothing but the empty grave. When he was naturally puzzled over the significance of the Tomb being empty, the Lord Himself appeared to him. V.34 thus points to the fact that Peter was eventually restored to faith through this personal encounter. If this is so, it might be asked why the author does not make this explicit in v.34. The answer would be that Luke was simply reproducing what was originally an integral part of an independent source<sup>34</sup>. By the same token it could also be argued that Luke had not intentionally meant v.34 to refer back to v.12<sup>35</sup>. At any rate, an omission on account of the presentation of Peter's role in respect of the Empty Tomb appears to be very unrealistic.

(c) A third suggestion with regard to a possible omission in D and its associates has been made by E.A. Abbott: "...almost all of these MSS place John before Luke in their pages, and after writing John's elaborate account, the scribes of these MSS might naturally shrink from inserting Luke's account

using the same rare words but in a narrative so curt and (as it would seem to them) so one-sided"<sup>36</sup>.

In order to substantiate this theory, one has to show that this group of scribes tended to alter every passage in a succeeding Gospel which was imperfect in comparison to the preceding Gospel. This is obviously not the case. Cf. Mark's brief reference to the Temptation with Matthew's descriptive account; Luke's curt account of the cleansing of the Temple as against the detailed account of it in all the other Gospels.

(d) What has been said above is a partial answer to any suggestion regarding omission based on the conflict between the Lukan and Johannine accounts. Further, from our study of scribal interference in the interest of harmonization it is difficult to believe that a scribe would prefer to delete a passage rather than assimilate it to a fuller account.

(e) Marcion's omission of our verse has been explained by Aland as being due to the former's distaste for Peter<sup>37</sup>. This in turn, according to this theory, may have affected the 'Western' Texts as well.

That this theory is least defensible is

readily seen from Marcion's retention of Peter's declaration of faith, Lk 9:20<sup>38</sup>.

Thus in the end we are left with no satisfactory solution to the mystery surrounding the omission of v.12, had it stood in the Lukan text. Burkitt avers that "accidental omission of the verse is highly improbable and that it is impossible to conceive why it should have been intentionally cut out"<sup>39</sup>. Lagrange's view with regard to the possibility of insertion has already been cited. Our inquiry seems to confirm this. A scribe's interest in honouring Peter by means of such an interpolation is quite understandable, whereas the omission of such a reference to Peter would only mean elimination of the chance to present the leader of the Apostolic group in a favourable light. On the other hand, any one who was determined to tarnish Peter's image would not have spared v.34 while deleting v.12.

On more than one occasion we have seen that a situation like this where a cogent argument for the absence of a passage from the original is not forthcoming, there is every justification for suspecting the authenticity of that passage. All

that now remains is to verify this finding in respect of the Intrinsic Probability.

#### Intrinsic Probability

(i) In defending the present reading Jeremias lists three Lukan expressions<sup>40</sup> which call for some comment: e.g. the pleonastic use of ἀναστῆς is strange to the Fourth Gospel; Mt has it once, Mk five times, but Lk-Ac 28 times. However, as Curtis has pointed out, Luke's practice is invariably to place the noun after the participle: 1:39, Ac 1:15 (after a proper noun); 23:1; Ac 5:6,17,34; 11:28; 23:9 (after a common noun)<sup>41</sup>. This would considerably weaken the claim of ἀναστῆς to be original to our context. θαυμάζειν is listed by Jeremias as another of Luke's favourite words. It is true that Luke uses the word several times: e.g. Lk 8:25 cf. Mk 4:41; but the corresponding passage in Mt (8:27) also has θαυμάζειν. Lk 9:43 has the word, but the Synoptic parallels omit the reaction of the crowd altogether. The only place where Lk exhibits a distinctly Lukan preference for the word is at 11:14. Cf. Mk (no parallel); Mt 12:23. These instances are thus too inadequate to establish Jeremias' claim. Besides, as Jeremias himself admits, θαυμάζειν with the accusative as in the present context is found in

Jn 5:28; Jude 16 in addition to Lk 7:9; Ac 7:31, so that it cannot be treated as being peculiar to Lk. *Τὸ γεγρονός* is found in Mt 5:14, but occurs more frequently in Lk (5 times) and Ac (3 times). Harnack characterises Lk 24:12 as 'a sentence in good Lukan style'<sup>42</sup>. To an impartial observer, however, these three expressions in themselves constitute only meagre evidence for the claim that no writer other than Luke would have used them, given a context like that of v.12. So Jeremias' statement that "The frequency of Lukanisms compels us to conclude that Lk 24:12 is the original text of Luke"<sup>43</sup> is too hasty a conclusion especially in the light of the proportionately larger Johannine expressions we have encountered here. Since we have already eliminated the possibility of an assimilation to Jn, the only inference that we can now draw from linguistic considerations is that the verse could be either of Lukan authorship or a scribal insertion from a parallel tradition.

(ii) Lampe<sup>44</sup> asserts that the verse is a parenthetical comment designed by the author himself to exempt Peter from the disbelief attributed to the Apostles in v.11. Apparently on the same consideration, scholars like Graefe<sup>45</sup> and Gore<sup>46</sup> are

prepared to link this verse with a re-edition of the Gospel by the author himself although they do not want to press the suggestion.

But by the same token, the longer reading can be condemned as spurious: a similar concern for Peter's reputation could be attributed to a scribe. So unless other evidence is forthcoming in support of this suggestion, it cannot be taken seriously. The very fact that v.12 does not state anything about Peter coming to faith makes one wonder if the author would have thought in terms of defending Peter.

(iii) Metzger's comment, "The passage is a natural antecedent to v.24",<sup>47</sup> seems to be unwarranted. If it were a natural antecedent, the controversy around it would not have arisen at all. There is something unnatural about the position and content of the verse as the MS evidence clearly shows. Even the originality of vv.22-24 has been called into question<sup>48</sup>, although there is no textual evidence to support this suspicion. But it highlights, at any rate, the sense of disharmony that may have been probably felt by ancient scribes. No doubt v.25 provides a better link with v.21 than the three verses immediately preceding it. So

Holtzmann's statement, "Wie 23 auf v.34, so sieht 24 auf 12 zurück, aber doch so, als ob Petrus nicht allein am Grabe gewesen wäre"<sup>49</sup>, also seems out of place.

(iv) E.A. Abbott writes: "The inconsistency in Luke, who in the bracketed passage mentions Peter alone, but later on, 'some of those with us' as going to the tomb is an additional reason for supposing that the former passage is genuine, and that Luke copied it verbatim from early tradition, not altering the words, although he knew that 'Peter' in such traditions often meant more than one disciple, and although he himself implies more than one later on"<sup>50</sup>. Ellis, in the same vein, asserts: "...the omission of the 'beloved disciple' mentioned in John is better explained by Luke's representative role for Peter than by a later scribe's oversight (cf. 12:41). In this section Luke is dependent upon a source also used by the Fourth Gospel. This fact and the repetition of the incident in the following Emmaus story (xxiv.24) also support the genuineness of this text"<sup>51</sup>.

A significant difference in the two statements cited above is worth noting: both scholars find a representative role for Peter in his alleged

march to the Tomb; but while Abbott attributes this reference to early tradition (the author's part being that of a passive transcriber), Ellis treats it as being due to Luke's deliberate design. I admit that the verse had something to do with early tradition. Whether Luke simply copied it from his source or not has yet to be decided. But I find it difficult to accept the proposition that whether in such tradition or in Luke's own delineation, "Peter" would stand for more than one disciple. That Peter represented the consensus of the disciples when he declared that Jesus was the Messiah (9:20 par.) cannot be denied; this does not automatically assign a representative role to him whenever he spoke up, as at 12:41, or wherever his actions are mentioned. Very often his statements or actions only bespeak his characteristic rashness and enthusiasm. This is all the more true in the context under consideration. Whoever be the author of v.12, it represents an attempt to qualify the statement in v.11: although the apostles in general did not believe the women's report, yet Peter got up and ran to the grave...etc.<sup>52</sup>. If this is granted, the theory of Peter's representative role in hurrying to the Tomb is not at all defensible because he is singled out as an exception. If the

author had wanted to allude to more than one disciple, why should he be shy of being a little more explicit? Cf. the Emmaus story which begins with 'two of them' (v.13), although the author mentions only one of them by name in v.18. The fact of the matter is that the particular tradition that lies behind v.12 knew only of a single individual, Peter, going to the Tomb on the first Easter morning.

This brings the question of the person responsible for its insertion here into sharp relief. An observation made by P. Schubert may be of relevance in solving this problem. According to him, the Empty Tomb story in Lk has only nominal significance as an evidence for the Resurrection. "This judgment", he says, "strengthens the case for the omission of the well-attested but doubtful reading of v.12"<sup>53</sup>. It must be borne in mind that nowhere in his narrative of the Empty Tomb does Luke specifically mention that the discovery of the same was conducive to faith in the reality of the Resurrection; v.8 cf. Mt 28:8 (vide on Lk 24:6a). If this point is conceded, we realize that no additional purpose is served by the presence of v.12 in the Lukan narrative as it does not refer to the discovery of the Empty Tomb by Peter as being instrumental in producing faith. This would hardly be the case if the author himself were inserting these words as a qualifying

rider to his generalization in v.11.

(v) V.12 actually disrupts the connexion between vv.11 and 13<sup>54</sup>. In v.9, the body of disciples include the Eleven and 'all the rest'. But in v.10 the whole group is loosely identified as 'Apostles' (cf. Ac 14:4) who form the subject of the ἡ πλειστούν of v.11. But v.13 opens with δύο αὐτῶν, referring back to the 'they' of v.11. So v.12, where the subject is ὁ Πέτρος cannot be regarded as the logical link between v.11 and 13.

(vi) There is one more point to be considered before we draw the final conclusion. What significance, if any, was Luke likely to have attached to the role of Peter in connexion with the Resurrection event as a whole? We have already considered it partially in the context of the Transcriptional Probability. How do we account for the extreme brevity of the report relating to the Resurrection appearance to Peter? The general scholarly opinion that v.34 was already embedded in this form in the narrative which came into Luke's hands has been mentioned. Then why did he not try to expand this passing reference into a fuller narrative? The

answer may be that Luke's information was scanty, as Gardner-Smith suggests<sup>55</sup>. As a matter of fact the Emmaus episode raises a number of issues on which Luke leaves us in the dark<sup>56</sup>. For example, who was the unnamed disciple in the story? If we follow the D reading, λέγοντες, with Origen, the subject of the participle becomes the two Emmaus disciples; that would mean that one of the two was Simon (Peter)<sup>57</sup>. It would be very strange for them to refer to their own experience in the words: 'The Lord is risen indeed and has appeared to Simon'. K. Lake suggests that these words might embody "a misunderstood and half-forgotten reminiscence" of the report of certain disciples on their return to Jerusalem from Galilee where they had heard of the appearance to Simon<sup>58</sup>. Gardner-Smith prefers to retain the reading of the Alexandrian Text, λέγοντας, and suggests that v.34 is a recollection of the testimony of the Eleven given to the two at a later time<sup>59</sup>. In any case, the appearance to Simon, if it belongs to reliable tradition (cf. 1 Cor. 15:5), is least likely to have taken place on the same day as the Emmaus appearance. For the Emmaus disciples, when they set out in the late afternoon on their journey of 60 στάδια, were not aware of an appearance to Peter. It was already dark when the Lord disappeared from them at Emmaus. Is it likely that He did

appear to Peter during the day in between the commencement of their journey and the time of His joining them in disguise? Or did the appearance occur later at night when the two men were still on their way back to Jerusalem? Since according to vv.36ff, the group who witnessed the next appearance included the Emmaus disciples, it is not likely that the three events would have happened in such quick succession on the same day<sup>60</sup>. All this suggests that in the Emmaus story we are dealing with highly condensed material which may ultimately be traced back to a written source. Luke had probably no means of checking his information or supplementing it in the way we would have liked to<sup>61</sup>. In that case neither v.24 nor v.34 should be linked with the section relating to the story of the Empty Tomb which is widely acknowledged to be from another independent source. In other words, the genuineness or otherwise of v.12 is not dependent upon the subsequent section of ch.24. One might charge the author with negligence of proper editorial modification of his source-material which has led to the present discrepancy between v.12 and v.24 and to the lack of cohesion between the story of the Empty Tomb and the Emmaus story. However, we will do well to remember that the anomalies of this chapter are not out of tune with Luke's occasional editorial handling

of his material (vide on Mt 9:34).

It is also striking that none of the Gospels describes an appearance to Peter exclusive of the other disciples. In Jn there is a special audience to Peter which occurs, however, not in the corresponding chapter 20, but in ch.21, suggestive of an entirely different and later setting. Scholarly opinion is divided as to whether it was Luke who shifted a post-Resurrection appearance backwards to synchronise with the call of Peter (5:1-11 cf. Mk 1:16-20), or it was John (or a later editor) who transferred the tradition concerning Peter's call into a post-Resurrection milieu (ch.21)<sup>62</sup>. Although in recent years the historical reliability of the Fourth Gospel is becoming increasingly established, when we compare passages like that of the cleansing of the Temple (2:13-22) with its Synoptic parallels, it is clear that if a charge of anachronistic manipulation of a historical incident could be levelled at all against an Evangelist, John is more liable to that accusation than Luke. As was mentioned elsewhere (vide on Lk 22:19-20), at the most Luke might alter the order of a sequence found in his source (4:1-12 cf. Mt 4:1-11), or even retain an obviously confused historical detail found in a document he is copying for want of better information (cf. Ac 5:36ff); but scholars are

generally agreed about Luke's trustworthiness as a historian. So the extreme brevity of the reference concerning the appearance to Peter in v.34 may be due to lack of detailed information (vide supra). It is equally possible that by the time the Gospels came to be written, the appearance to Peter had lost its primary significance<sup>63</sup>. If this is a valid observation, then the significance of Peter's role in regard to the Empty Tomb narrative also becomes minimal in Lk.

Luke's hesitancy in narrating the appearance to Peter seems well-founded. The first two Evangelists do not show any interest in this. Even the Johannine narrative of ch.21 loses much of its significance as an exclusive appearance to Peter because of its setting in Galilee rather than in Jerusalem (motif of fulfilment of prophecy, Mk 16:7; Mt 28:16ff). The three-fold questioning of Peter's loyalty is, of course, in keeping with the motif of reassurance and reinstatement of the penitent Apostle. But the author's interest in the other disciples is unmistakable from the very beginning of the narrative, especially at vv.20-24. There is no doubt that the Fourth Evangelist gives more prominence to the appearance to Mary Magdalene (first witness) and to Thomas than that granted to Peter, even if his authorship of ch.21 is conceded.

Thus the Fourth Gospel's basic attitude to Peter in relation to the Resurrection is not different from that of the Synoptists.

### Conclusion

To sum up: the balance of probabilities was seen to be opposed to the recognition of Lukan authorship to v.12. The reading here was also proved to be not an assimilation to its counterpart in Jn. The possibility of both passages going back to a common tradition was considered. Our acceptance of this is permissible only on condition that Lukan authorship is still not to be presupposed. The solution to the textual problem of v.12 then consists in acknowledging scribal insertion from an early source with which the Johannine Gospel also shared a few verbal similarities<sup>64</sup>. In adopting this passage the scribe was concerned with the problem of squaring the statement of v.24, and possibly the allusion to Peter in v.34, with the Empty Tomb narrative. But in his obsession with levelling up the most glaring discrepancies he overlooked the fact that he was not improving upon the author's statement in v.11 as far as Peter's faith was concerned. Nor did he realize that the discovery of the Empty Tomb had not constituted an essential proof of the Resurrection in Luke's view-point.

In my view this line of approach alone will satisfactorily explain all the facts of the case we have explored. All other theories pertaining to this verse stand exposed as hasty conclusions based on some outstanding aspects, to the neglect of the others<sup>65</sup>.

<sup>1</sup>JTS 22 (1971), p.513.

<sup>2</sup>NTS 12 (1965-6), p.206; cf. Snodgrass, JBL 91 (1972), p.373 ; Graefe, TSK, 1896, p.268; Scrivener, Introduction, II<sup>4</sup>, p.299: He also points to the authority of Codex Fuldensis and the Eusebian Canon in favour of the long text.

<sup>3</sup>Acts of the Apostles (E.T.), pp.58-9.

<sup>4</sup>Evangelion, II, pp.96,142,408; also, Cambridge Texts and Studies, IV (1896), p.53; cf. p.232.

<sup>5</sup>Text and Canon, p.128.

<sup>6</sup>Markus u. Lukas (Meyer's Kommentar)<sup>7</sup>, p.634,n.

<sup>7</sup>Against von Soden's insistence on Tatianic influence for the omission here, see Lagrange, RB (n.s.)<sup>10</sup> (1913), p.507.

<sup>8</sup>Novum Test. Graece, I, pp.723f; cf. Blass, Philology, pp.188f.

<sup>9</sup>Vide Leaney, NTS 2 (1955-6), p.112; Curtis, op.cit., p.513.

<sup>10</sup>Hort, Notes, p.71; cf. Gardner-Smith, Narratives of the Resurrection, p.48; Klostermann, Handbuch, p.601; H.D.Betz, Interpretation, Jan. 1969, p.35,n.9; P.Winter, StTh vii-viii (1954-55), p.167; Kilpatrick, Essays in Honour of G.H.C.MacGregor, p.193; MacKinnon and Lampe, The Resurrection, p.48; M.H.Shepherd, JBL 64 (1945), p.230,n.12; J.Weiss, Schriften des N.T., p.482. (He compares it to the interpolation at Mt 27:49 from Jn); Tasker, Greek N.T., p.424. Lagrange, Luc, p.601f, defends the Lukan passage and argues that Jn is dependent on Lk. Torrey, Our Translated Gospels, pp.80-1, also defends v.12, but wonders if John knew Lk.

<sup>11</sup>Traditions Common to Luke & John, p.85,n.3.

<sup>12</sup>Handbuch zum N.T., p.601.

<sup>13</sup>Critique Textuelle, p.70. This is a deviation from his defence of the verse (Luc, p.601) based on style.

<sup>14</sup>Der Schluss des Markus-Evangeliums der Vier Evangelien-Kanon und die Kleinasiatischen Presbyter, quoted by A.B. Bruce, The Expositor's Greek Testament, I<sup>6</sup>, p.337.

<sup>15</sup>Introduction to the Literature of the N.T.<sup>3</sup>, pp.275-6.

<sup>16</sup>Vide Jeremias, Eucharistic Words<sup>3</sup>, (E.T.,1966), p.150; Curtis, JTS 22 (1971), p.514; cf. ExpT 83 (1971-72), p.344; also, Bailey, loc.cit.

<sup>17</sup>Die Paralleelperikopen bei Lukas und Johannes, pp.88-91. I have deliberately left out the two disputed passages at Lk 24:36 and 40 which Schneiwind includes in this list.

<sup>18</sup>F.L.Cribb's warning against "parallelomania" seems to be quite applicable here! Cf. JBL 90 (1971), p.424.

<sup>19</sup>RevScr 25 (1951), Part 1, p.223.

<sup>20</sup>Aramaic Approach<sup>3</sup>, pp.102-4.

<sup>21</sup>Eucharistic Words<sup>3</sup>, (E.T.,1966), p.150, Cf. Curtis, op.cit., pp.512-5; ExpT 83 (1971-72), pp.344-5; Snodgrass, JBL 91 (1972), p.379.

<sup>22</sup>Cf. Loisy, Luc, p.572; Meyer (Meyer Kommentar), 1, Göttingen (1853), p.499.

<sup>23</sup>Notes, p.71.

<sup>24</sup>Johannine Vocabulary, pp.299ff § 1803.

<sup>25</sup>Markus und Lukas (Meyer's Kommentar)<sup>7</sup>, p.634.

<sup>26</sup>JBL 56 (1937), pp.300-1. There is a glaring howler in this statement (*italics mine*): The incomplete Markan account does not refer to an appearance to Peter and John! Therefore the question of John correcting Mark does not arise. No doubt, John might have wanted to supplement Mark's account if he had received only the truncated form of this Gospel.

<sup>27</sup>Evangelion, II, pp.96,231. The only other instance in syr<sup>s,c</sup> where 'Petros' is rendered by 'Simon' is at Mk 14:31. In our present context the 'interpolator' is following a later trend demonstrated so well in the Peshitta. This text has 'Kepha' in Mt and Mark, but invariably 'Simon' in Lk and Jn.

<sup>28</sup>Supplementary Notes to Hort's 'Notes on Select Readings', p.147.

<sup>29</sup>Op.cit., p.149,n.5.

<sup>30</sup>Acts of the Apostles (E.T.,1971), pp.58-9.

<sup>31</sup>Narratives of the Resurrection, p.68,n.1.

<sup>32</sup>The Greek N.T., I<sup>5</sup>, ad.loc.

<sup>33</sup>TSK, 1896, p.268.

<sup>34</sup>B.Weiss, op.cit., p.644:"Die Beziehung auf die dem Petrus gewordene Erscheinung, welche Luk. nicht berichtet hat (s. aber 1 Kor. xv.5), ist ein deutliches Zeichen einer anderen Quelle"; cf. Bailey, op.cit., p.88.

<sup>35</sup>Cf. Loisy, Luc, pp.571-2: "...mais c'est (sc. v.34) encore moins une référence à notre passage, où il n'est question que du tombeau vide".

<sup>36</sup>Loc.cit.

<sup>37</sup>Loc.cit.

<sup>38</sup>Cf. J.Knox, Marcion and the N.T., p.86; Harnack, Marcion, p.182.

<sup>39</sup>Op.cit., p.231. Contrast Snodgrass, loc.cit.: "Omission due to scribal accident or an attempt to do away with the minor divergence (italics mine!) from Jn".

<sup>40</sup>Op.cit., p.150.

<sup>41</sup>JTS 22 (1971), p.515; cf. K. Lake, Historical Evidence for the Resurrection, p.95.

<sup>42</sup>Marcion<sup>2</sup>, p.247; cf. p.238. This reference in the second edition is cited by Jeremias, op.cit., p.150.

<sup>43</sup>Loc.cit.

<sup>44</sup>Peake's Commentary, St. Luke, ad.loc.

<sup>45</sup>Loc.cit.

<sup>46</sup>Gore's Commentary, ad.loc.

<sup>47</sup>Textual Commentary, p.184; Hoskyns, Hoskyns and Davey, The Fourth Gospel<sup>2</sup>, ad.loc., maintains the originality of v.12 and claims that v.24 is based on it; however, he overlooks the conflict between these verses.

<sup>48</sup>Cf. F.C. Grant, JBL 56 (1937), pp.300-1. Blass, Philology, pp.188-9, questions the authenticity of both v.12 and v.24, despite the absence of MS evidence for the omission of the latter.

<sup>49</sup>Hand-Commentar, p.307.

<sup>50</sup>Op.cit., § 1803.

<sup>51</sup>Luke, pp.272-3; Cf. J. Schniewind, loc.cit.

<sup>52</sup>Cf. Plummer, loc.cit.

<sup>53</sup>N.T.che Studien für R. Bultmann, pp.167,171,172 n.18. Contrast Ellis, op.cit., p.273, who claims that the earliest Christians could not conceive any distinction between Resurrection and 'grave-emptying'. This does not seem to square with the evidence of ch.24 where we find lingering scepticism on the part of the disciples in spite of their awareness of the Tomb being empty.

<sup>54</sup>Cf. Moffatt, loc.cit.;" [v.12] breaks awkwardly into the flow of the story"; cf. Lampe, Peake's Commentary, ad.loc. Plummer, ICC, St. Luke, p.550: "The verse has probably no connexion with what precedes." Gardner-Smith, Narratives of the Resurrection, p.65,n.1: "If verse 12 be no part of the true text, ἀὐτῶν in verse 13 probably refers back to ἀποστόλους in verse 12; Cleopas was not strictly an apostle, so that there is some reason to suppose that his companion was one of the eleven. But Luke's traditions are fragmentary."

<sup>55</sup>Ibid., p.68: "The inclusion of v.24 ...may be compared with [Luke's] mention of the appearance to Simon in verse 34. Both are indications of the fragmentary and confused state of tradition at the time the Gospel was composed."

<sup>56</sup>Ibid., pp.66ff.

<sup>57</sup>K,Lake, Historical Evidence for the Resurrection, p.101, supports Origen; R. Annand, ScJT 11 (1958), pp.180ff. Contrast B. Weiss, Markus u. Lukas (Meyer's Kommentar), p.644.

<sup>58</sup>Op.cit.,p.102.

<sup>59</sup>Op.cit., p.70; cf. pp.66-67,160. Contrast Lake, op.cit., p.109.

<sup>60</sup>Gardner-Smith, op.cit., pp.69-70,160-61.

<sup>61</sup>Ibid., p.67: "If it be objected that so careful and skilful a writer as St. Luke would not have left a reference to Peter hanging in the air without any explanation, we can only reply that Luke may have been conscious of the fact that his information was incomplete, and having these words in his source he did not feel justified in altering them."; Cf. Lake, op.cit., pp.101-102: "If Luke XXIV:12 were genuine it would be possible to think that the meaning [of v.34] is that in the light of the experience of Cleopas the disciples saw that St. Peter's visit to the grave was the equivalent of an appearance. But this is extremely harsh, and textual probability is against the genuineness of the passage." Lake even suspects the originality of v.34.

<sup>62</sup>Cf. Cullmann, Peter: Disciple-Apostle-Martyr (E.T.), pp.59ff; V. Taylor, Behind the Third Gospel, p.229: "The facts more than warrant the suspicion that the story of Jn XXI:1-17 is a literary fusion of disparate elements of which the Lukan narrative is but one. An alternative view posits a confusion in early Christian tradition by which the story of Lk V:1-11 has been transformed into a post-Resurrection narrative. In either case, the priority of the Lukan story is implied, but in view of the late date and doubtful value of the Johannine Appendix little more can be said"; cf. K. Lake, op.cit., pp.137ff: "...there is something to be said for the suggestion that both in Lk V and Jn XXI we have nothing more than elaborated (and in the latter case misplaced) accounts of the call of St. Peter, of which we have a simpler and early narrative in Mark. But it is indeed probable that St. Luke regarded his narrative as a substitute for the Marcan one, and it is probable that this preference ought to be followed; it is also probable that the Johannine account refers to the same event as the Lukan one". Loisy, loc.cit., suggests that Luke, after having made a decision in favour of an encounter between Peter and Jesus as occurring in Galilee (ch.5), would have found difficulty in recounting a similar post-Resurrection event in the vicinity of Jerusalem; hence Luke's deletion of the tradition adopted in Jn 21. Leaney, St. Luke, p.29, traces Lk 24:13-35 to a special source, v.34 being perhaps an insertion. Leaney accounts for the brevity of the reference to Peter in v.34 in terms of the author's earlier adaptation of the story in 5:1-11.

<sup>63</sup>So N. Clark, Interpreting the Resurrection, pp.87f. Cullmann, op.cit., pp.62-3, shows from the Pseudo-Clementine Literature (Homilies 17,19) how the apostolic authority of Paul was being questioned in Jewish Christian circles on the ground that it was based on a mere vision. Cullmann surmises that the same tendency might have been at work in curtailing the significance of the role of Peter in connexion with the Resurrection and that it may find its reflection in the last ch. of Lk.

<sup>64</sup>See Lagrange, Critique Textuelle, II, p.70; Cf. Lake, op.cit., p.95: "This is possibly an interpolation from the tradition which lies behind John xx.6. Even without accepting the theory implied by the phrase 'Western non-interpolation', it is difficult to see how the addition can be defended"; Torrey, loc.cit., traces the origin

of the words in the parallel passages in Lk and Jn under review to a Judaeian Aramaic source; cf. M. Black, loc.cit. But my contention is that since the evidence rules out the possibility of Lukan authorship, the words in Lk should be ascribed to a scribe rather than to the Evangelist. Contrast E.A. Abbott, op.cit., §1804, who defends v.12 as "an isolated tradition inserted by [Luke] ...without attempt to explain its exact meaning or to reconcile it with other traditions". He goes on to claim that "John writes with allusion not only to Luke, but also to other traditions...".

<sup>65</sup>A further clue as to the spuriousness of 24:12 may be derived from the External Evidence for 22:62 which has a bearing on the role of Peter at the Tomb. The Greek and Latin sides of Codex Bezae do not omit 22:62, as also the majority of the Greek MSS. But the Old Latins a b e ff<sup>2</sup> ilr<sup>1</sup> omit both the above verses. Scholarly opinion, as we shall see, is overwhelmingly in support of retaining 22:62. There is the possibility that this verse was left out due to homoeoarcton. However, if this is ruled out, the next alternative is to consider the psychological factor that motivated the omission: a scribe may have been offended by the mention of the chief among the Apostles lamenting over his guilt. If consideration for Peter's honour operated behind the deletion of this verse, the same reason might be adduced in support of the retention of 24:12. But since the former text is confirmed to be genuine, it follows that 24:12 cannot be original. Otherwise the same group of authorities would not have left out both.

I.C. The Resurrection Appearance to the Disciples

a. Lk 24:36b: The Dominical Greeting

Disputed words: [καὶ λέγει αὐτοῖς, Εἰρήνη ὑμῖν]

External Evidence:

Nestle<sup>26</sup>

Greek NT<sup>1</sup> (UBS)

καὶ λέγει αὐτοῖς· εἰρήνη ὑμῖν. [καὶ λέγει αὐτοῖς, Εἰρήνη ὑμῖν]  
(vorher nicht im Text)  
txt P<sup>75</sup> א B K L X Δ Θ Π Ψ f<sup>1</sup> f<sup>13</sup> (28 εἶπεν für λέγει) 33 565 700  
892 1009 1010 1071 1079 1195 1216 1242 1344 1365 1546 1646  
2148 2174 Ɔ Ɔ<sup>185m</sup> syr<sup>s</sup> sa bo Eus Chr Aug Cyr II καὶ λέγει αὐτοῖς,  
εἰρήνη ὑμῖν· ἐγὼ εἰμι, μὴ φοβεῖσθε P 1230 1241 1253 aur c f vg  
syr<sup>p</sup>· h· pal<sup>1</sup> bo<sup>mss</sup> arm aeth georg Amb Aug II καὶ λέγει αὐτοῖς, ἐγὼ  
εἰμι, μὴ φοβεῖσθε· εἰρήνη ὑμῖν W II — D a b d e ff<sup>2</sup> l r<sup>1</sup>

b. Lk 24:40: The Visible Proof

Disputed words: [καὶ τοῦτο εἰπὼν ἔδειξεν αὐτοῖς τὰς χεῖρας  
καὶ τοὺς πόδας]

External Evidence:

καὶ τοῦτο εἰπὼν ἔδειξεν αὐτοῖς [καὶ τοῦτο εἰπὼν ἔδειξεν αὐτοῖς  
τὰς χεῖρας καὶ πόδας. (vorher τὰς χεῖρας καὶ τοὺς πόδας.)  
nicht im Text)  
txt (z. T. mit ἐπέδειξεν) P<sup>75</sup> א B K L W X Δ Θ Π Ψ f<sup>1</sup> f<sup>13</sup> 28  
33 565 700 892 1009 1010 1071 1079 1195 1216 1230 1241 1242  
1253 1344 1365 1546 1646 2148 2174 Ɔ Ɔ<sup>185m</sup> aur c f q vg syr<sup>p</sup>· h· pal<sup>1</sup>  
sa bo arm georg Eus Ath Chr Cyr JohDamasc II — D a b d e ff<sup>2</sup> l r<sup>1</sup>  
syr<sup>c</sup>· s Mcion

Observations

(i) It was pointed out in the Introductory chapter that the 'Western non-interpolations' in ch.24 are exceptional readings and that External

Evidence should not be the chief criterion in evaluating the trustworthiness of these passages. Despite the well-known fact that all the 7 'classic' 'non-interpolations' in the last chapter of this Gospel are represented more or less by the same group of witnesses, Plummer seems to stress that the negative attestation for vv.36b and 40 by the 'Western' authorities has equal validity against both verses:

"The evidence against this verse (sc. v.40) is exactly the same as that against the doubtful words in v.36 with the addition of Syr<sup>c</sup>"<sup>1</sup>. The authenticity of the two passages will no doubt stand or fall together because of the fact that both are embedded in the same pericope which Lk has in common with Jn<sup>2</sup>. In other words, the resolution of the textual problem of these verses is inextricably connected with the inter-relation of the pericopes in question. However, the very fact that scholarly verdict on the External Evidence is sharply divided shows that it cannot be a reliable basis for our purpose. Aland observes: "Die altlateinischen Handschriften sind in beiden Fällen geteilt... Bei Luk.xxiv.40 ist die griechische Überlieferung, das Zeugnis der Versionen...und der Kirchenväter einhellig für den Langtext - eine Position, die eigentlich nicht zu erschüttern ist. P<sup>75</sup> belegt, dass der Langtext bereits um 200 im

Lukasevangelium gelesen wurde, er findet sich in allen Textformen und Provenienzen; wenn er sekundär sein sollte, müsste er ganz früh eingefügt worden sein"<sup>3</sup>. But it must be pointed out in the light of what has been said above that the same kind of comment can be made about any of the 8 disputed passages in this chapter. This applies equally to Grant's claim in support of the opposite position, viz. that "though many of the 'best' MSS have it (sc. v.40), D, the Itala, the Curetonian and Sinaitic Syriac rightly omit it"<sup>4</sup>. Similarly Metzger's remark about the "preponderance of external attestation"<sup>5</sup> for v.36b is also beside the point. Finally, while Graefe<sup>6</sup> finds the witness of the Alexandrian and Old Syriac sufficient evidence for the originality of v.36b, Gore<sup>7</sup> thinks that the MS evidence for the same is doubtful.

(ii) Burkitt<sup>8</sup> and Souter<sup>9</sup> are confident that as in the case of 24:6,12, so in v.36, Syr<sup>S</sup> has suffered an interpolation.

(iii) Ellis concludes that "the textual evidence for the omission of v.40 is somewhat stronger than in the case of v.12"<sup>10</sup>. Similarly Aland notes that v.40 is less strongly attested than v.36<sup>11</sup>.

(iv) Harnack claims that the witnesses to the omissions at both 5:39 and 24:40 are the same and that therefore if the former passage is recognized as original, the latter should also be so honoured. "Dass 24,40 lukanisch ist, folgt besonders aus der Vergleichung mit 5,39; hält man (mit Recht) diese Stelle für ursprünglich, so darf man 24,40 dem Lukas nicht absprechen; denn sie hat dieselbe Überlieferung wie 5,39. Es ist auch an sich ganz unwahrscheinlich, dass ein Zusatz zu Lukas in alle Mss. gekommen ist mit Ausnahme Dab e ff<sup>2</sup> l syr<sup>cu</sup>, während die umgekehrte Annahme, dass er in dem Archetypus dieser Zeugen gestrichen war, keine Überlieferungsgeschichtliche Schwierigkeit macht"<sup>12</sup>.

This claim ties in with Aland's theory cited above, and what was said against it holds good here.

(v) The section in which the present readings appear already exhibits several signs of scribal meddling: e.g. *μη φοβείσθε, ἐγὼ εἰμι*, read by various groups of witnesses, could be an assimilation to either Mk 6:50 (par. Mt 14:27)<sup>13</sup> or to Jn 6:20<sup>14</sup>. The shifting position of *Εἰρήνη ὑμῶν* in W may be another indication in the same direction. But the most notable alteration

by copyists is the turning of v.43 into a meal reminiscent of the Last Supper (cf.  $\Theta$  f<sup>13</sup> Vg Syr<sup>c</sup>)<sup>15</sup>. Some of the rather early and notable authorities, along with others, (E\*  $\Theta$  f<sup>13</sup> Vg it<sup>mss</sup>) also add *καὶ ἀπὸ μελισσίου κηρίου* (ὅρ κήριον ) in v.42. Under these circumstances suspicion of scribal interference at vv.36 and 40 cannot be precluded.

### Internal Evidence

#### Transcriptional Probability

##### (i) Possibilities of Insertion

With regard to v.36b, it is obvious that it is identical with Jn 20:19,26. This has naturally led an overwhelming majority of scholars to conclude that it is a case of assimilation to Jn. "If anything in the Gospel of Luke is an interpolation, this passage contains an example of it", is the verdict of F.C. Grant<sup>16</sup>.

A modified form of this view is the suggestion that the reading could have originated first as a gloss.<sup>16\*</sup>

Similarly v.40 is identical with Jn 20:20

with the significant difference that τὴν πλευράν has been substituted by τοὺς πόδας . The question arises as to who is more likely to have made this substitution, assuming that the words have their source in Jn. Here too, the majority of commentators who would delete v.36 have subscribed to the assimilation theory, although some have expressed doubt because of the slight difference mentioned above<sup>17</sup>.

Gardner-Smith<sup>18</sup> and F.C. Grant<sup>19</sup> consider v.40 to be a gloss adapted from Jn which eventually found its way into the text.

This view can, no doubt, be held of any early interpolation and can never be proved absolutely. What is important for our purpose is to assess the balance of probabilities.

The objection against the interpolation theory regarding v.40 has been variously stated by a minority of scholars. E.A. Abbott holds that v.40 "could not have been interpolated from John without violent alteration of πλευράν "<sup>20</sup>. Alford<sup>21</sup> finds difficulty in the fact that the disputed verse does not show any sign of scribal

attempt at altering the *πόδας* to *πλευράν*. Lagrange's objection is based on the presumable influence of the Diatessaron on subsequent Texts. He argues that a harmonist like Tatian should be expected to include both 'feet' and 'side' in his Text besides 'hands'; through the influence of this Text the *τὴν πλευράν* would have become substituted for *τοὺς πόδας* in many exemplars. But von Soden<sup>22</sup>, cites only one such MS, viz. Codex Fuldensis. From this Lagrange draws the hasty inference that v.40 cannot be an assimilation to Jn.<sup>23</sup>. Metzger claims that "had the passage been interpolated from the Johannine account, copyists would probably have left some trace of its origin by retaining *τὴν πλευράν* in place of *τοὺς πόδας* (either here only, or in v.39 also)"<sup>24</sup>.

In answer to these objections it must be pointed out that Luke has no account of the piercing of the Lord's side by a soldier. A scribe must have been an extremely absent-minded person to fail to notice the *χεῖρας* and *πόδας* of v.39 and to insert *πλευράν* in v.40<sup>25</sup>. This could rather be a case of deliberate insertion because of the very fact that the scribe in question exercised enough discretion to make the necessary adjustment

to suit the Lukan narrative<sup>26</sup>. As for Lagrange's point about Tatianic influence, von Soden's evidence which he cites could at best only mean the absence of the influence of the Diatessaron in this context on subsequent MSS, even if we concede such influence elsewhere. Probably the view that Lagrange holds in his *Critique Textuelle*<sup>27</sup> may be taken as his better judgment on the matter. Here, under the caption: "L'omission est plus probablement la leçon authentique", he observes; "le context de Lc. exclut plutôt cette clause. On avait même ajouté encore d'après Jo xx.20".

(ii) Possibilities of Omission

With regard to the absence of v.36b from the 'Western' Text, Loisy<sup>28</sup> opines that it could simply be accidental, although he does not elaborate on the possible manner of the accident. It may just be that such a short line of 24 letters had constituted the end of a *στίχος* in an exemplar and thus got overlooked by a copyist. But the same group of witnesses as here testifies to a lacuna at v.40 (47 letters) where this simple solution seems to be less applicable<sup>29</sup>. Therefore it seems sound to attribute the omission of the two passages to the same critical process, whatever that process be.

In E.A. Abbott's view the deletion of v.36b may have been due to the mention of 'fear' in the disciples' mind despite the risen Lord's greeting of peace<sup>30</sup>.

As a matter of fact this very argument can be directed against the genuineness of these words: could Luke be expected to represent the Lord's greeting as having no salutary effect on the disciples? As J. Schmitt remarks, "Semblable salutation du Christ aux 'disciples' n'est guère compatible avec la suite du récit, où l'auteur souligne qu'à la vue du Ressuscité, les apôtres furent saisis de 'crainte' et de 'frayeur' (vv.37-39)"<sup>31</sup>.

So the above arguments have very little merit in themselves unless there be compelling Intrinsic Probability to support the originality of v.36b.

What are the possibilities that would account for the disappearance of v.40 in the 'Western' text if the verse originally belonged to the Lukan autograph?

(a) It has been suggested that the superfluity of the words may have given rise to their deletion by a fastidious scribe<sup>32</sup>.

The answer to this is that since superfluous repetitions are not true to Luke's taste, the question of superfluity itself may constitute an argument in favour of the authenticity of the shorter text<sup>33</sup>.

By the same token, Loisy's comment<sup>34</sup> that in the absence of this verse the continuing disbelief of the disciples would be less appropriate, is not convincing. The author's purpose is already served by the successive words ἴδετε . . . . ψηλα φῆσατέ με καὶ ἴδετε . . . . θεωρεῖτε in v.39, so that the link between vv.39 and 41 is perfectly smooth as far as the diction is concerned. The very gesture of Jesus is perfectly conveyed by v.39 itself.

(b) Was it the discrepancy between v.40 and Jn 20:20 that led to the omission of the former verse?

It seems incredible that a whole verse would be deleted by a scribe for the sake of just a single word. Judging from scribal habits in regard to the 'Western' group in general, the more natural tendency would have been to expand the text by borrowing the reference to the piercing of the Lord's side from Jn 19:34 and introducing it at the appropriate

place and then to exchange the *τοὺς πόδας* of v.40 for *τῆν πλευράν*. However, the fact that the 'Western' Text, in contrast to the Alexandrian, does not exhibit such an additional reading at Mt 27:49 suggests that the scribe concerned was not interested in a similar adventure at Lk 24:40 either. It is true that the present Alexandrian reading is not the same as the one at Mt 27:49. But the tendency of the Alexandrian scribe to assimilate the Johannine narrative at Mt 27:49 tilts the balance of probabilities towards suspecting a similar tendency in the present context too. The cogency of this argument is all the more striking when the textual problems of Lk 24:36 and 24:40 are held together.

(c) Jeremias comments that the present lacuna is "probably rather a harmonizing omission in the Western text due to the supposed contradiction with John 20:17, unless it is simply a scribal oversight"<sup>35</sup>.

We have already ruled out the possibility of scribal oversight in this context. But with regard to the main point of Jeremias' inference, it is no doubt hasty and unfortunate. It is obvious that he is referring to Jesus' invitation in the present pericope to feel Him, in contrast to His prohibition to Mary Magdalene in Jn. But Jeremias

overlooks the fact that it is v.39 which actually contains this invitation! Again, in order to silence such arguments once and for all, it is legitimate to ask why the Johannine passage in question was not chosen for deletion especially in view of its incomprehensibility. Moreover, Jn 20:17 had every chance of being expunged by a wilful scribe because of its contradiction with Jesus' invitation to Thomas in v.27!

(d) Harnack<sup>36</sup> has proposed Marcion's influence as a possible cause of the omission of v.40.

But Marcion's objection was against the reference to 'flesh and bones' in v.39 which he tried to alter. In fact Tertullian uses v.40 to answer Marcion's misuse of v.39<sup>37</sup>, although the former's knowledge of the verse alone is not an adequate guarantee for the originality of v.40. It is not therefore clear why v.40 should get deleted in the controversy with Marcion.

Thus in the end we are left with no convincing argument for a possible omission of v.40. Graefe, who expresses difficulty with regard to the discrepancy between the Johannine and Lukan parallels, however remarks: "Ein Grund, der zur Streichung des Verses führen konnte, wenn er echt war, ist mir nicht

bekannt"<sup>38</sup>. This is precisely the situation we find ourselves in with regard to the previous verse as well. As we have seen on various occasions, such a situation, as a rule, is an accurate index of the spuriousness of a given passage. What now remains is to confirm this finding in terms of Intrinsic Probability.

#### Intrinsic Probability

John's indebtedness to Luke, and even vice versa, have been proposed by Bacon<sup>39</sup> and Loisy<sup>40</sup> respectively, to explain the textual phenomenon of the present pericope. Blass's theory of the double recension of Lk has been pressed by Graefe<sup>41</sup>, Scrivener<sup>42</sup> and E.A. Abbott<sup>43</sup> in this context as well. Suffice here to state that neither the Transcriptional Probability nor considerations of Lukan style would favour these theories.

A third alternative is the theory of the use of a common tradition by Luke and John<sup>44</sup>. Of course, this theory can be brought in to defend any set of parallel readings. The validity of this has to be assessed by probing the degree of similarity between the two pericopes in question. It was pointed out in connexion with 24:6a that verbal correspondence

between narratives relating to the Passion need not necessarily imply indebtedness to a common source because of the very nature of the material. Such popular stories as the accounts of the Crucifixion and Resurrection could naturally be expected to receive certain fixed elements in the various branches of Christian tradition<sup>45</sup>. However, it will be shown shortly that although there is ample evidence for the use of a common tradition behind the Third and Fourth Gospels, this fact does not in any way constitute a decisive proof for or against the disputed verses.

(a) Regarding v.36, the crux of the matter is this: Does the form of Semitic greeting in this passage conform to the Lukan diction, seeing that its Johannine stamp is so unmistakable? (cf. Jn 20:19,21,36). Even if the words of salutation at 20:19,26 be regarded as part of the customary etiquette, the greeting at 20:21 is of a piece with our Lord's parting gift of peace<sup>46</sup> (Jn 14:27; 16:33 cf. Phil 4:7). As in the case of many an allusion in John's Gospel, great weight has to be attached to his deliberate use of this formula in three consecutive stages in his account of the drama of the post-Resurrection appearances. If we accept Gardner-Smith's most attractive

emendation at Jn 20:17, viz. *μη̄ πτόου* for  
*μη̄ (μου) ἄπτου*,<sup>47</sup> we cannot fail to notice  
 a consistent motif behind all the appearances narrated  
 by John: the Risen Lord is portrayed as reassuring  
 and comforting His disciples according to their  
 individual and collective needs. He fills them  
 with joy and peace, empowers them with His Spirit and  
 commissions them to continue His work of reconciling  
 men to the Father. It is instructive to note in this  
 connexion that in the Fourth Gospel we have no reference  
 to Jesus' greeting any one except in the post-Resurrection  
 milieu (cf. 20:15, 'Mary!'; 21:5 *Παῖδιά*)<sup>48</sup>.  
 On such occasions the greeting is accompanied by a note  
 of intimacy and tenderness, so that the three salutations  
 in the accounts of the appearance to the disciples are  
 to be viewed in that light.

This is indeed something which distinguishes  
 Lk from Jn. Although there are several occasions of  
 formal salutation in Lk, Jesus does not figure in any  
 of them. In none of these contexts does Luke use  
 the Semitic formula of peace either at the beginning  
 of a personal encounter or in connexion with 'fear'.  
 The word used by Luke in the famous greeting contexts  
 at 1:40,41,44 is *ἀσπάζομαι*. In 1:11-13  
 where the appearance of the angel Gabriel is associated  
 with 'fear' on the part of Zechariah, there is no

mention of 'peace', but only the formula of encouragement:

μη φοβοῦ Ζαχαρία : cf. 2:9,10. Similarly  
at 1:28 Gabriel accosts Mary with the expression χαιρε;  
and later at v.29 his words of encouragement to her  
are introduced by μη φοβοῦ Μαρίας.

The atmosphere of the Lukan narrative of the appearance to the apostolic community is fraught with fear and doubt. So if we decide to retain the disputed words at v.36, the result would be a continuing sense of fear and doubt in spite of Jesus' words (cf. v.37). This inconsistency would be all the more glaring in the light of v.34. If the Lord had already appeared to Peter, the disciples should evince a sense of anticipation of His reappearance and not be terrified<sup>49</sup>. It is also strange that Luke does not put his characteristic μη φοβεσθε (Lk 8:50 par. Mk 5:36; 12:7 par. Mt 10:28,31; Lk 12:32; (21:9); Ac 18:9) in the mouth of Jesus in response to the 'fear' of the disciples. Note that whereas in Jn the greeting of peace is repeated despite the absence of any mention of 'fear', in Lk the same salutation is followed by a lingering note of fear. This sort of anomalous situation in a narrative cannot be attributed to an accomplished writer like Luke whose intellectual ability is remarkably reflected in his twin writings in the N.T.,

despite one or two oddities we have discussed elsewhere (vide on v.12). On the contrary, the present reading is highly suggestive of the intrusion of an unimaginative scribe. In his enthusiasm to assimilate the Johannine account, he might have copied these words in the wrong place, little realizing the consequences. However, if the same scribe be held responsible for an 'assimilation' at v.40, he must be credited with some degree of discretionary skill as is evident from his handling of Jn 20:20. The MS evidence would suggest that probably it may be this dilemma which has necessitated the suggestion that both vv.36b and 40 were glosses to begin with. This would relieve the scribe who introduced these verses into Lk for the first time of the charge of being unimaginative. In that case their positioning in the actual text could be attributed to an innocent copyist at a subsequent stage. Speculations apart, what appears to be most certain is the un-Lukan origin of the words at v.36b. J. Schmitt rightly characterizes this consideration as a "Fait décisif" and adds: 'l'idée de "paix" don du Seigneur et fruit de l'Esprit, est inconnue de Luc, alors qu'elle est caractéristique du quatrième évangile"<sup>50</sup>. So Metzger's note that "the regular form of the Semitic greeting might well be expected on this occasion"<sup>51</sup> cannot meet with approval.

(b) The next point to consider is the degree of resemblance between Lk 24:36ff and Jn 20:19ff. As we have seen in connexion with 24:6a, when the extent of similarity is small, the occurrence of identical readings in parallel passages becomes highly suspect. Actually the verbal contacts between Lk and Jn are fewer in number than what obtains between Mk 16:1-8 and Lk 24:1-12. The following correspondence may be noted<sup>52</sup>:

	<u>Lk 24</u>		<u>Jn 20</u>	
v. 36	αὐτὸς ἔσται ἐν μέσῳ αὐτῶν	:	ὁ Ἰησοῦς ἔσται εἰς τὸ μέσον	v. 19
v. 38	καὶ λέγει αὐτοῖς	:	καὶ λέγει αὐτοῖς	v. 20
v. 41	ἀπὸ τῆς καρτῆς	:	ἐχάρησαν	v. 20

With regard to subject matter, the two Gospels agree only in the barest outlines<sup>53</sup>: on the day of the Resurrection, the Lord suddenly appears to His disciples and shows them the nail-marks on His body. The disciples are overjoyed on recognizing Him. He gives them a commission and assures them of divine power to fulfil it.

But the disagreements between them are so many that we will do well to tabulate them<sup>54</sup>.

Lk

Jn

- |  |  |
|--|--|
| 1. Time of appearance:<br>late in the night.   | Time of appearance:<br>evening.  |
| 2. 'Disciples' include<br>others besides the<br>Eleven; no indi-<br>cation of Thomas<br>being absent.  | 'Disciples' = Apostles;<br>Thomas absent.  |
| 3. No reference to doors<br>being shut.  | The scene is behind<br>closed doors: fear of<br>the Jews.  |
| 4. Appearance of the Lord<br>produces fear.  | No mention of fear of<br>the disciples   |
| 5. Jesus alone eats food.  | No reference to food <sup>55</sup> .   |
| 6. Persistent effort on<br>the Lord's part to<br>convince disciples of<br>the corporeality of<br>His person. He even<br>mildly rebukes them.   | No such effort on Jesus'<br>part except the showing<br>of His hands and side.  |
| 7. The eventual joy of<br>disciples not unmixed<br>with wonder and<br>unbelief.  | Disciples glad on being<br>shown the stigmata:<br>instant belief.  |
| 8. The words in vv.46ff<br>have more to do with<br>motif of 'proof-<br>from-prophecy' than a<br>definite commission-<br>ing.   | Definite emphasis on the<br>commissioning of the<br>Apostles (vv.21ff).  |
| 9. Giving of the Spirit<br>a subsequent event<br>(disciples to await),<br>although Luke too<br>believes that the work<br>of the Church is to<br>begin with the coming<br>of the Holy Spirit. | The departing Lord<br>bequeaths His Spirit to<br>the Apostles. In the<br>power of the Spirit they<br>will continue His work. |
| 10. Farewell appearance:<br>climax - Ascension(?)  | Not farewell appearance <sup>56</sup> .  |

Now how are we to evaluate these deviations in Lk from the Johannine account over against the points of contact between the two Gospels? Different critics have approached this differently. Loisy<sup>57</sup>, affirming Luke's indebtedness to Jn at v.36b, states that the close resemblance between v.40 and Jn 20:20 is not decisively against the authenticity of that verse, as the context might demand such correspondence. Although Streeter maintains John's dependence upon Lk at this point, he is not prepared to commit himself on the authenticity of either of the disputed readings<sup>58</sup>. C.H. Dodd holds that there is "certainly no strong evidence of borrowing" of either author from the other, and adds: "If we were to postulate borrowing on the one side or the other, Luke must be the borrower. But there is in fact so little coincidence in verbal expression that there is no sufficient ground for the hypothesis of literary dependence". He also notes that the Lukan account of the showing of the hands and feet by the Lord "is far more elaborate and undisguisedly apologetic", despite his verdict that both vv.36 and 40 "are in all probability due to assimilation to John"<sup>59</sup>. On the other hand, Aland, while defending both verses, maintains that the Gospel parallels in question belong to a common tradition which each Evangelist restructured in his

own way<sup>60</sup>. Gardner-Smith also denies any literary dependence of John on Lk in view of the radical departure in the Fourth Gospel at vv.22,23 (the gift of the Holy Spirit)<sup>61</sup>. "[John] was clearly using a tradition which had diverged from the common stream before Luke wrote his Gospel"<sup>62</sup>. Again, "There can be no doubt, however, that the third and fourth gospels are describing the same scene, and the differences are not greater than we should expect in two accounts written in different localities at different dates"<sup>63</sup>. As for the genuineness of the two readings he writes: "The two 'Western non-interpolations' are probably no part of the original text, the first having crept under the influence of the fourth Gospel (xx.19-26), and the second being a marginal note"<sup>64</sup>.

One thing clearly emerges from these scholarly judgments despite their conflicting character: the theory of a common source behind the two Gospels at this point is not a decisive criterion in evaluating the authenticity of the two disputed verses in Lk. This is precisely in keeping with our finding regarding the high degree of deviation between the two pericopes.

At the same time, the suggestion that the two pericopes go back to a popular tradition is confirmed

by the substantial agreement between Lk 24:36-49 and Mt 28:16-20<sup>65</sup>. In the list of similarities below, the differences, which are considerable, are given in parenthesis:

1. Both appearances involve the Eleven (in Lk the appearance is not exclusively to this 'inner circle'; place and time in Mt - Galilee (Mt does not specify the 'mountain'); day time - are quite different from what Luke indicates).
2. Both Evangelists mention the initial doubt of the disciples before recognizing the Lord. (Matthew: 'some doubted'; but finally they all worship Him; Luke: all disciples share the mood of fear, doubt and joy; no mention of their worshipping Him).
3. In both accounts, this is the only appearance to the disciples as a group; in fact it is the farewell appearance (Mt: departure only implied; Lk: departure = Ascension, narrated).
4. Both contain instructions from the departing Lord about the future work of the Church: (Mt: emphasis on baptism in the name of the triune God; Lk: emphasis on preaching of repentance).
5. Both narratives include a promise from the departing Lord: (Mt: 'lo, I am with you always...'; Lk: the sending of the promise of the Father -

of course, the difference lies in the theological emphasis, not in subject matter).

6. A mountain figures in both accounts: (Mt: a mountain in Galilee; Lk: Bethany = Olivet, cf. Ac 1:12).

These points of contact as well as the divergences among the three pericopes lead to the inevitable conclusion that a very early tradition regarding the Lord's appearance to the apostolic community had developed along three divergent routes and that each Evangelist is re-editing the particular version of that tradition which he received to suit his individual plan<sup>66</sup>. It is, of course, difficult to be precise about how much information each Gospel writer had received from his source and how much editorial modification he introduced. This could partly be determined by our decision as to whether this source was written or oral<sup>67</sup>. According to some scholars, the Emmaus story and the appearance to the Eleven belonged to the same written document<sup>68</sup>. But if we concede a high degree of editorial modification on the part of Luke, it would be better to treat them as separate oral traditions. From considerations of vocabulary, Easton<sup>69</sup> affirms that the present pericope goes back to a source (oral) different

from L, viz. the Jerusalem tradition, which Lk shares with Jn. This view is challenged by F.C. Grant<sup>70</sup>. After an analysis of those passages in Jn where contacts with Lk are reported, he concludes that Jn's contacts with Lk are really limited to Q and L, and not to the present section. John may have known the special source behind Luke's Passion narrative, although it is uncertain whether it reached him in oral or written form. According to Grant, the bare resemblances between Lk and Jn as at Lk 24:36b, *ἔστιν ἐν μέσῳ αὐτῶν*, are due to the definite form they had already reached due to constant oral repetition<sup>71</sup>. However, in order to dismiss all Lk-Jn contacts in the remaining part of the pericope, Grant had to invent a gloss theory in respect of 24:37, 39-43 which has no foundation in MS evidence (except at v.40). He attributes such resemblances as at vv.41 (cf. Jn 21:5), 42 (cf. 'broiled fish' Jn 21:9), 43 (cf. Jn 21:13) to the interference of a scribe<sup>72</sup>. It is clear that since Grant had made up his mind about dispensing with any suggestion of similarity, he even overlooks the glaring and basic discrepancy between the two Gospel references relating to the Holy Spirit which a scribe is least likely to introduce!; he is also not prepared to concede the very possibility of oral tradition which he has recognized at v.36b.

Thus Grant's attempt at discrediting the Lk-Jn contacts with regard to the appearance to the Eleven is founded on too weak a basis to be convincing.

With regard to the Third Evangelist's editorial work, Leaney makes the following comments<sup>73</sup>: There was a common source underlying Lk 24:12 and Jn 20:3-10 which both Luke and John adapted in favour of Peter and the 'beloved disciple' respectively. Then each Evangelist added material from his special source - Luke the Emmaus story and John the appearance to Mary Magdalene. Leaney suggests that 24:33-35 may be Luke's own transitional introduction between the Emmaus story and the appearance to the Eleven. At the close of the account of the latter appearance in the Upper Room, "each evangelist [is] writing under the influence of his own conviction, Luke revealing his belief in an intermediate period between the Messiah's exaltation and the bestowal of power on the Apostles, John his 'realized eschatology' ". The reason why there is no counterpart in Jn to Lk 24:44-47 is that this "integral part of the account of the overcoming of the Apostles' doubt, would naturally be omitted in the Fourth Gospel".

All this is well said. But Leaney's use of this methodology of the Evangelists to defend 36b and 40 is not cogent. Because of the possibility of a common source behind the two pericopes Lk 24:36-49 par. Jn 20:19-22, he assumes that vv.36 and 40 must necessarily belong to the Lukan pericope. But his argument in support of 36b is based on too flimsy an evidence, viz. the reference to 'peace' in Lk 10:5<sup>74</sup>. As a matter of fact this reference which is part of the instructions to the Seventy, is also found in Mt 10:5-41, so that there is no doubt about the common origin (from Q) of these passages<sup>75</sup>. Therefore it cannot be held as a valid argument for the greeting of peace being characteristic of Luke. The 'similarity' which Leaney finds between Lk 24:49 and Jn 20:21 is based on an isolated word ( ἀποστέλλω / ἀπέστειλεν )<sup>76</sup>. But whereas in the Lukan context the 'sending' has reference to the Holy Spirit, the Johannine context makes it applicable to the 'commissioning' of the disciples.

P. Schubert would allow for considerable editing of his material on Luke's part: "The various materials of tradition which make up the chapter were not much more than a miscellany of data. The empty tomb story , the Emmaus story and the appearance to

the Eleven as the three large units have in their "original" version little in common, and little to bind them together, to say nothing of the smaller items of information...Luke pulls the three major items of his material together by furnishing each of them with the same climax which we may briefly call "the proof from prophecy" that Jesus is the Christ"<sup>77</sup>. Elsewhere Schubert also adds: "Since this proof-from-prophecy theology is Luke's central theological idea throughout the two-volume work, he had no difficulty or hesitancy in incorporating it into any materials he liked for the purpose, and thus transforming these traditional materials more or less drastically and effectively"<sup>78</sup>.

W. Manson's comment on Luke's literary activity in this connexion is limited to vv.36-42. But he also detects considerable working over by the author on traditional material: "A representation which unites a spiritual-supernatural interpretation of the resurrection-body of Christ (vers 36a-37) with features distinctly material (vers 39-43) suggests the blending of two somewhat different types of tradition... The tendency of Luke's source and of his own mind was to materialize spiritual experience, and a comparison of the above narrative with Mt xxviii.

16-20 makes it possible that in Luke we have the overlaying of a less by a more corporeal conception of the Saviour's risen life..."<sup>79</sup>.

Regarding the relative stage at which each Evangelist got his material, there is no consensus among scholars. Gardner-Smith thinks that Matthew marks the first stage in a process of development of which Luke's and John's show different stages. The original nucleus may be a simple statement that the Lord appeared to the disciples and instructed them regarding their future work. "Matthew knowing, or at least adopting, an exclusively Galilean tradition, had no choice but to put the instruction into the mouth of Jesus in Galilee, and Luke, who followed an exclusively Judaeian tradition, was similarly constrained to represent Jesus as instructing his disciples in Jerusalem"<sup>80</sup>.

P. Winter characterises the Lukan account of the appearance to the disciples as "a haggadic story that repeats another story so as to bring out with greater clarity one particular point and drive home one particular moral... In other words, [it] belongs to that later tradition that also produced John xx.19b-20 (of which John xx.24-29 may

be a later elaboration)"<sup>81</sup>. P. Parker also ascribes the Lk-Jn correspondence in the present context to a much developed oral tradition formed within the Church in which both Evangelists were associated together at least for a time: "We must posit two Evangelists, of quite variant temperaments, working long in the same areas, hearing the same words about their Lord, perhaps participating in the same discussions; then each remembering these things in his own way and digesting them in his own way, put his record down"<sup>82</sup>.

In contrast, V. Taylor questions the suggestion of a late date for our pericope: "...the very bare form in which the Ascension...is described, together with the fact that apparently this event belongs to the end of the same day of the Resurrection itself, is surely a clear sign of the comparatively early date of the source in which it occurs"<sup>83</sup>. He goes on to add that the emphasis here on the corporeality of the Resurrection body of Jesus itself is suggestive of a very early date in view of the prevalent Jewish notions regarding the state of a recently departed soul<sup>84</sup>.

The conflicting views on the state in which Luke had received his tradition and the extent of editorial modification he brought about are not surprising because of the similarities as well as differences that this section exhibits in comparison with Jn. If our conclusion regarding the stories of the Empty Tomb and of the Emmaus disciples is correct, we may safely assume that Luke's editorial touches on the section vv.36-43 have been minimal. In the subsequent section, vv.44-49, where Luke's theology of history is so well-pronounced, we can discern a high degree of creative imagination<sup>85</sup>. This is particularly remarkable because of the way in which the author links up this section with vv.25-27. The extent of editorial alterations that the two Evangelists have worked into their common material does suggest that its roots were in oral tradition rather than in any written document. I submit that this compromise solution accommodates all the facts of the case we have explored in this connexion. All suggestions regarding a high proportion of Lukan apologetic in regard to vv.36-43 have to be relegated to the period of the oral transmission before the account reached Luke<sup>86</sup>. This conclusion is in conformity with Gardner-Smith's hypothesis that the narratives of the appearance to the Eleven as

recounted by Luke and John represent different stages of development of the same tradition as in Mt 28:16-20 (vide supra).

This rather lengthy digression into the parallel pericopes in the three Gospels is justified on the ground that it exposes the weak foundation on which some scholars have tried to build up a case for the originality of Lk 24:36b,40. It is now obvious that none of the above considerations can recommend either of our verses to be regarded as a genuine part of the Lukan pericope. The unity of this pericope is not in any way impaired by the deletion of these verses. So Leaney's claim that the authenticity of vv.12 and 40 "may be defended on the principles of textual criticism" because both share a tradition with the Fourth Gospel (i.e. 20:3-10; 20:19-23 respectively, cf. Lk 5:1-11 par. Jn 21:1ff),<sup>87</sup> cannot be sustained (vide on Lk 24:12). What is, however, pertinent for our purpose is that the verbal correspondence between Lk and Jn in regard to the present context is of a much lesser degree than in the case of Lk 24:12. In the midst of such dissimilarity as we have already discovered between the two parallel pericopes, the verbal identity of vv.36b,40 with their Johannine counterparts leads to

the conclusion that they properly belong to Jn rather than Lk. It is immaterial in this connexion whether they entered the Lukan stage for the first time as glosses - most probably they did as we had reason to suspect. In any case their spuriousness is beyond doubt, the first being non-Lukan and the second being redundant.

A further point raised by Curtis<sup>88</sup> to discredit vv.36b,40 is now to be examined. This has to do with two stylistic features which, though rare in Lk, appear in these verses. It is true that Luke uses the historic present only 12 times (including the disputed verses 24:12,36b), and 13 times in Ac<sup>89</sup> as against the 162 occurrences in Jn and the 151 of Mk. But this is not a sufficiently strong argument because the present context might happen to be one of the rare cases of such Lukan usage. Jeremias in fact uses the same evidence in support of the verse. According to him, in 92 places where Luke is copying Mk, the historic present is left out; and it is retained only once in Lk (8:49 cf. Mk 5:35). Jeremias' explanation is that "the historical present goes back to the Vorlage used by Luke, which he has stylistically refined much less basically than the text of Mark taken over by him, as the historic presents directly show".<sup>90</sup>

However, the 13 occurrences of the phenomenon in Ac where the use of a Vorlage is not suspected, casts doubts on the validity of the above explanation. Besides, it may be noticed that in Mk 5:35 the author uses the plural ἔρχονται which Luke modifies into the singular ἔρχεται τις . If he had a natural aversion for the historic present he would have altered the Markan expression accordingly. Nor can we cite Curtis' reference<sup>91</sup> to the rare occurrence of καὶ τοῦτο εἰπὼν in Lk as against Jn in favour of the deletion of v.40. He himself admits that "The evidence does not allow of a dogmatic conclusion" of either verse<sup>92</sup>. Unless an author avoids an expression absolutely, there is always the possibility of a disputed passage conforming to his rare usage, so that stylistic considerations are of no positive help in this context one way or the other.

### Conclusion

Since we have strong Intrinsic Evidence to render the two verses under discussion spurious, we reject both in the company of the vast majority of scholars<sup>93</sup>. Despite Jeremias' arguments against this position cited above, his final verdict is that "the decision must remain in suspense"<sup>94</sup>. This shows his own lack of conviction about the linguistic

evidence he adduces. To the best of my knowledge, no critic has tried to assess the problem in all its aspects, and hence the mutually conflicting views on the matter. It is my firm belief that the suspicion attached to the present readings by Westcott and Hort is more than justified by the foregoing discussion, although Hort's 'Notes' on them<sup>95</sup> are too inadequate to carry conviction.

- <sup>1</sup>St. Luke, p.568. We should supplement this with the addition of syr<sup>s</sup> and Marcion.
- <sup>2</sup>Cf. Ellis, Luke, pp.272-3; Aland, NTS 12 (1965-66), p.206.
- <sup>3</sup>Ibid.
- <sup>4</sup>JBL 56 (1937), p.301.
- <sup>5</sup>Textual Commentary, p.187.
- <sup>6</sup>TSK, 1896, p.271.
- <sup>7</sup>Gore's Commentary, ad.loc.
- <sup>8</sup>Evangelion, II, p.232.
- <sup>9</sup>Text and Canon, p.128.
- <sup>10</sup>Luke, pp.272-3.
- <sup>11</sup>NTS 12 (1965-66), p.206.
- <sup>12</sup>Marcion, TU XV (1921), Beilage IV, p.230, n.1.
- <sup>13</sup>So Bailey, Traditions Common to Luke and John, p.86, n.2; cf. Leaney, NTS 2 (1955-6), p.112.
- <sup>14</sup>So the vast majority of scholars. See n.16 below.
- <sup>15</sup>Cf. F.C. Grant, JBL 56 (1937), p.301. He says that such tampering was facilitated by the fact that these verses were in the last column of the Gospel.
- <sup>16</sup>Loc.cit.; cf. Hort, Notes<sup>2</sup>, p.71; Gore's Commentary, ad.loc.; Moffatt, Introduction to the Literature of the N.T.<sup>3</sup>, p.276; Plummer, loc.cit.; B. Weiss, Markus u. Lukas (Meyer's Kommentar)<sup>7</sup>, p.645; J. Weiss, Schriften des N.T., p.484; Holtzmann, Hand-Commentar, p.301; Godet, St. Luke, II (E.T.), p.356; Easton, St. Luke, p.364, n; Tasker, Greek N.T., ad.loc.; Kilpatrick, Essays in Honour of G.H.C. MacGregor, p.193; J. Schmitt, RevScr 25 (1951), Pt.II, p.230; V. Taylor, Passion Narrative, p.113, n.2.
- <sup>16\*</sup>So Klostermann, Handbuch, p.608; Lagrange, Luc, p.611; Bailey, loc.cit.

<sup>17</sup>Those who express doubt include Godet, loc.cit., Graefe, TSK, 1896, p.271; B. Weiss, loc.cit.; [Contrast Dodd, Historical Tradition in the Fourth Gospel, p.145] cf. Dodd, Studies in the Gospels (ed. Nineham), pp.16-17: "V.40 is not certainly part of the text". M.H. Shepherd, JBL 64 (1945) p.233: "It is generally agreed that xxiv:12 and xxiv:40 are interpolations from John"; see also W. Manson, Luke, p.269. However, K. Lake, Historical Evidence, pp.104-5 and Streeter, Four Gospels, pp.403-4, are non-committal on both verses.

<sup>18</sup>Narratives of the Resurrection, p.71.

<sup>19</sup>Loc.cit.

<sup>20</sup>Johannine Vocabulary, § 1804a, p.305, n.1.

<sup>21</sup>The Greek N.T., I<sup>5</sup>, ad.loc.

<sup>22</sup>Schriften des N.Ts., II, p.1570.

<sup>23</sup>Luc, p.612.

<sup>24</sup>Textual Commentary, p.187.

<sup>25</sup>Cf. J. Weiss, Schriften des N.T., p.484: "Dass die Seitenwunde nicht mitgenannt ist, liegt daran, dass der Lanzenstich bei Lukas nicht erwähnt ist".

<sup>26</sup>Cf. Creed, St. Luke, p.300: "Here, as at v.12, the Johannine text has itself influenced the Lucan. The non-Western insertions in vv.36 and 40 are taken from John XX:19,20. (τὴν πλευράν is replaced in Luke by τοὺς πόδας to harmonize with v.39. The piercing of Christ's side is not related in Luke)".

<sup>27</sup>Vol. II, p.71; cf. RB (n.s.) 10 (1913), pp.503f.

<sup>28</sup>Loc.cit.

<sup>29</sup>A.C. Clark, Primitive Text, does not cite these among his instances of omission at the end of a στίχος (cf. p.53).

<sup>30</sup>Loc.cit.

<sup>31</sup>RevScR 25 (1951), Pt.II, p.230.

<sup>32</sup>Cf. Aland, loc.cit. Another way of stating this is shown by E.A.Abbott (loc.cit.). He says the verse "was omitted by copyists on account of their preference for John's words". In that case insertion from Jn would have been most natural to a scribe, judging from the notorious scribal tendency! Vide Easton (loc.cit): "There was no reason for omitting this verse while its insertion from v.39, aided by Jn XX:20, was extremely easy".

<sup>33</sup>Cf. A.B.Bruce, Expositor's Greek Testament, p.649; Bailey, loc.cit.; Lampe, Peake's Commentary, ad.loc.

<sup>34</sup>Luc, p.586; Cf. Lagrange, Luc, p.613: "Nous retenons donc le verset, très naturel dans Luc...".

<sup>35</sup>Eucharistic Words (E.T.), p.151.

<sup>36</sup>Marcion, TU XV (1921), Beilage IV, pp.229f.

<sup>37</sup>Adversus Marcionam, iv:43; cf. Plummer, op.cit., p.160.

<sup>38</sup>Loc.cit.

<sup>39</sup>The Fourth Gospel in Research and Debate, p.368.

<sup>40</sup>Op.cit., p.548: "La conformité avec Jean n'a rien qui doive surprendre, puisque notre évangéliste paraît avoir connu la quatrième évangile et qu'il l'exploite dans ces récits "(!). The relation between Lk and Jn is a much debated topic. V.Taylor (Behind the Third Gospel, p.224), following Harnack. [Luke the Physician (E.T.), p.231], leaves the question open. Moffatt, op.cit., pp.534f, maintains that on the one hand the two Gospels go back independently to common oral traditions or sources; on the other hand, the two simply represent in one aspect the climax of a development which can be traced back from Mt to Lk. Bacon, loc.cit. : "In its general structure, the outline of the Fourth Gospel...reproduces that of Mark as modified by Luke. Stanton (Gospels as Historical Documents, iii, p.220) denies the dependence of Jn on Lk; Cf. J.Schmitt, op.cit., pp.230-31; Parker, NTS 9 (1962-63), p.335.

Streeter (Four Gospels, p.424f): "The Gospels of Mark, Luke and John form, it would seem, a series, Luke being dependent on Mark, and John on both the others". Those who hold Luke's dependence on Jn are certainly in a minority.

<sup>41</sup>Loc.cit.

<sup>42</sup>Introduction, II<sup>4</sup>, pp.298-99.

<sup>43</sup>Loc.cit.

<sup>44</sup>Leaney, St. Luke, pp.29f; Metzger, loc.cit. V. Taylor, Passion Narrative of St. Luke, pp.113f, completely ignores Jn here (except for the mention in a foot-note that the two problematic passages are probably assimilations to Jn) and states: "On critical and linguistic grounds we are justified in concluding that the account of the appearance to the Eleven is to be explained as Luke's embellishment of an earlier source". John's dependence on Lk or any of the Synoptics is questioned by E.R. Goodenough, JBL 64 (1945), pp.145-82, on the basis of the common source theory. For a criticism of this position, vide, R.P.C. Casey, JBL 64 (1945), pp.535-42. Casey strongly maintains John's indebtedness to Lk.

<sup>45</sup>Cf. F.C. Grant, op.cit., p.302.

<sup>46</sup>Cf. A.B. Bruce, Expositor's Greek Testament, pp.864-65. But Gardner-Smith, Narratives, p.83, ascribes the three-fold greeting in Jn ch.20 to the sources.

<sup>47</sup>St. John and the Synoptic Gospels, p.80, n.1.

<sup>48</sup>A.B. Bruce, op.cit., p.868, quotes Aristophanes, Clouds, 137, Frogs, 33, to show that *παῖδες* was the usual form of address to men at work (= 'lads'). A modern equivalent would be 'fellows' or 'pals'. The tone of intimacy is significant enough.

<sup>49</sup>No doubt, it could be argued that such inconsistency is in keeping with Luke's poor handling of his material from different sources. But the cumulative effect of the difficulties presented by the present pericope

does not lend itself to such easy solutions. Gardner-Smith, *Narratives*, p.75, regards this pericope as belonging to the same tradition as that of the Emmaus story. In that case the inconsistency is inexcusable.

<sup>50</sup>Loc.cit.

<sup>51</sup>Loc.cit.

<sup>52</sup>Cf. J. Schmitt, *RevScR* 25 (1951), II, p.230.

<sup>53</sup>Cf. Gardner-Smith, *Narratives of the Resurrection*, p.158.

<sup>54</sup>Cf. *ibid.*, pp.82ff. V. Taylor, *Behind the Third Gospel*, p.223. Also, Gardner-Smith, *St. John and the Synoptic Gospels*, pp.71ff.

<sup>55</sup>It might be argued that John relegates the reference to Jesus' partaking of food to ch.21 (So Lampe, Peake's *Commentary*; see on Lk 24:36,40). But apart from the question of authorship of this chapter, there is no hint here of Jesus' actual eating; rather He acts as the host (v.12 cf. Lk 24:30). Were there more than one occasion of such post-Resurrection meals? If *συναλιχέσθαι* in Ac 1:4 could mean 'to dine' it would confirm this suggestion. Cf. Resch, *TU X*, 2 (1901), pp.796-97.

<sup>56</sup>In fact Jn has no account of the Ascension. Is it because by the time this Gospel came to be written the Ascension was not conceived as an event apart from the Resurrection? Then what about the first two Gospels?

<sup>57</sup>Loc.cit.

<sup>58</sup>*Four Gospels*, pp.403-4.

<sup>59</sup>*Historical Tradition in the Fourth Gospel*, p.145 and n.1.

<sup>60</sup>Loc.cit., Cf. Leaney, St. Luke, pp.29-30; P. Parker, NTS 9 (1962-63), p.333: "Resemblances are not influences, and influences certainly are not sources. To suppose that John must have depended on the Synoptics is to ignore the outstanding role of preaching, and of debate in the early Church. From these came an oral tradition fully adequate to produce the features John shares with the other Gospels. The differences, on the other hand, are often inexplicable on the assumption of a direct literary relationship".

<sup>61</sup>St. John and the Synoptic Gospels, p.83.

<sup>62</sup>Ibid.

<sup>63</sup>Narratives, p.72.

<sup>64</sup>Ibid., p.71.

<sup>65</sup>Cf. *ibid.*, pp.156ff; 62ff. In the light of what follows, Aland's statement (*loc.cit.*), "Wir haben in Luc xxiv.36-43 und Joh. xx.19-23 einen der den beiden Evangelien gemeinsamen Stoffe, die Markus und Matthäus fremd sind", is questionable (*italics mine*).

<sup>66</sup>Streeter, Four Gospels, p.424, detects an evolution in the doctrinal emphasis in the various Gospels.

<sup>67</sup>Ibid., p.396: "John has so much in common with Luke that, if he did not use our Third Gospel, we must conclude that John and Luke had a common source, either in the form of a written document or of oral tradition".

<sup>68</sup>Leaney, St. Luke, p.30; Cf. F.C. Grant, *op.cit.*, pp.302f. Grant arrives at this conclusion by explaining away anything that does not conform to his view as glosses derived from Jn. He rates 24:37, 39-43 as secondary as Mk 16:9-20. Contrast M. H. Shepherd, *op.cit.*, pp.233-34: "It is not necessary...to posit a theory of interpolation from John for the other verses (*sc.* verses other than the two 'Western non-interpolations'). Rather it may be that later developments of tradition favouring a more physical interpretation of the resurrection in opposition to Doceticism, have led to expansion of

the Lukan narrative". In support of this suggestion he draws an evidence from the Aramaic Targum of Mt used by a Nazarene sect: the famous logion, "I am not a bodiless demon", is believed to have come from this Nazarene 'Gospel of Matthew'. But in effect Shepherd's suggestion also discounts the pre-Lukan oral tradition.

<sup>69</sup>St. Luke, pp.xxix, 365; cf. J. Schmitt, op.cit. p.231.

<sup>70</sup>Op.cit., pp.303-4.

<sup>71</sup>Ibid., p.302.

<sup>72</sup>Ibid., p.303. Grant speaks of the "wild and Western" accretions to the Text in the period of textual "pre-history".

<sup>73</sup>St. Luke, pp.30f.

<sup>74</sup>NTS 2 (1955-56), p.112.

<sup>75</sup>Cf. H. Balmforth, Saint Luke, p.185.

<sup>76</sup>Leaney, St. Luke, p.31.

<sup>77</sup>NT Studien für R. Bultmann (1954), p.173.

<sup>78</sup>Ibid., p.176.

<sup>79</sup>Luke, p.269.

<sup>80</sup>Narratives, p.73.

<sup>81</sup>StTh vii-viii (1954-55), pp.168-69. Winter also says that these verses "owe their origin to a situation in which the believers collected evidence not just for the resurrection as such, but in which they found themselves compelled to combat docetic views on the nature of the Risen Christ"; Cf. P. Schubert, loc.cit.; "Lk xxiv.36-43 is as it stands...a complete, fairly popular and fairly late tradition. There can be no doubt that the early Christians had a vital interest in protesting the judgment that the appearances of the risen Lord were mere phantasma...".

<sup>82</sup>Op.cit., p.336.

<sup>83</sup>Behind the Third Gospel, p.67.

<sup>84</sup>Ibid.; cf. p.227.

<sup>85</sup>Cf. J. Schniewind, Die Paralleelperikopen bei Lukas und Johannes, p.92: "...die Verse Lk.24, 44-49 bieten spezifisch lukanische Gedanken über Geist und Predigt".

<sup>86</sup>Against Dodd, et al; Cf. Studies in the Gospels, pp.16-18: "[This pericope] is certainly more remote from the original tradition, orally handed down, than the typical narratives of Class I (sc. 'concise' narratives), but the obvious work of an author has not altogether disguised the form of the tradition which underlies" (p.18). Earlier he describes how the story has been extensively worked over in the interest of controversial apologetic and dramatic vigour; Cf. Taylor, Passion Narrative of St. Luke, p.137: "The narrative of the appearance to the Eleven stands nearer to the original source, but it is marked by theological and apologetic motives in the emphasis it lays on the bodily resurrection of Jesus".

<sup>87</sup>St. Luke, p.31.

<sup>88</sup>ExpT 83 (1971-72), p.344.

<sup>89</sup>Cf. J.C. Hawkins, Horae Synopticae<sup>2</sup>, p.149.

<sup>90</sup>Eucharistic Words (ET), pp.150-51; cf. E.A. Abbott, loc.cit.

<sup>91</sup>Loc.cit.

<sup>92</sup>Ibid.

<sup>93</sup>For a confirmation of the line of argument I have followed here, see J. Schniewind: , op.cit., pp.91f, cf. Lagrange, RB (n.s.) 10 (1913), p.505.

<sup>94</sup>Op.cit., p.152.

<sup>95</sup>Notes<sup>2</sup>, p.72.

I.D. The Ascension

a. Lk 24:51: The ἀνάλημψις

Disputed words: [καὶ ἀνεφέρετο εἰς τὸν οὐρανόν]

External Evidence:

Nestle<sup>26</sup>

Greek NT<sup>1</sup> (UBS)

καὶ ἀνεφέρετο εἰς τὸν οὐρανόν. [καὶ ἀνεφέρετο εἰς τὸν οὐρανόν].  
(vorher nicht im Text)

txt P<sup>75</sup> X<sup>c</sup> ABC KLW X Δ Θ Π Ψ 063 f<sup>1</sup> f<sup>13</sup> 28 33 565 700 892  
1009 1010 1071 1079 1195 1216 1230 1241 1242 1253 1344 1365  
1546 1646 2148 2174 2174 2174 I<sup>85m</sup> aur c f q (r<sup>1</sup>) vg syr<sup>p. h. pal</sup> sa bo arm  
georg<sup>2</sup> Aug<sup>2,3</sup> Cyr Cosmos || [ἀνεφέρετο] syr<sup>s</sup> (— διέστη ἀπ' αὐτῶν)  
|| — X\* Da b d e ff<sup>2</sup> l georg<sup>1</sup> Aug<sup>1,3</sup>

b. Lk 24:52: The Worship of the Departing Lord

Disputed words: [προσκυνησαντες αὐτόν]

External Evidence:

προσκυνησαντες αὐτόν (vorher [προσκυνησαντες αὐτόν]  
nicht im Text)

txt P<sup>75</sup> X ABC KLW X Δ Θ Π Ψ 063 f<sup>1</sup> f<sup>13</sup> 28 33 565 892 1009  
1010 1071 1079 1195 1216 1230 1241 1242 1365 1546 1646 2148  
2174 2174 I<sup>85m</sup> aur f q syr<sup>p. h. pal</sup> sa bo arm georg || προσκυνησαντες  
700 1253 1344 c vg || — Da b d e ff<sup>2</sup> l syr<sup>s</sup> Aug

Observations

(i) Just as in the case of 24:36b,40, the textual problems relating to the two readings above are supposed to be inextricably linked together because of their common context as well as of the nearly identical witness for their omission. The only difference between the two groups of witnesses is the additional support of X\* for the 'Western Text at v.51. Aland<sup>1</sup> is therefore not really

justified in laying stress on the divided state of the Old Latins or the positive witness of the Alexandrian authorities and the Versions as if this is something peculiar to this part of the chapter only.

(ii) Syr<sup>C</sup> is deficient at v.51, but the Syr<sup>S</sup> reading<sup>2</sup> seems to be a pointer in the direction of the long text being original, for though it omits *σεέστη ἀπ' αὐτῶν* and *εἰς τὸν οὐρανόν*, it still retains the essence of our disputed words<sup>3</sup>.

(iii) The section 24:50-53 testifies to several other instances of scribal manipulation of the text. Since the omissions in these cases is not confined to the 'Western' family alone, it would probably mean that the two 'Western non-interpolations' under discussion have to be studied against the background of the former omissions as well.

### Internal Evidence

#### Transcriptional Probability

##### (i) Possibilities of Insertion

(a) According to H. Sahlin<sup>4</sup> and others, Lk-Ac was originally a single volume in which Lk 24:49 was immediately followed by Ac 1:6. But in the second century, the books were separated in order to realign

Lk with the rest of the Gospels. Then it was inevitable that the Third Gospel and the Acts should have a suitable conclusion and introduction respectively. Hence the insertion of Lk 24:50-53 and Ac 1:1-5 in their present positions<sup>5</sup>.

W.G. Kümmel<sup>6</sup> has cogently refuted this argument along the following lines:

1. There is hardly any evidence for the secondary nature of the insertion of the entire section Lk 24:50-53.
2. According to the ancient practice of book-making, the dual volume would have been too long for a papyrus roll of standard length, whereas the two separate books, being almost equal in bulk, would conform to that standard<sup>7</sup>.
3. It is highly illogical to suggest that an interpolator would have created the kind of 'conflicts' that exist between these two corresponding passages (vide infra).
4. There is no evidence to show that Lk-Ac became canonical as a combined unit; on the contrary, the available evidence points to the fact that the Gospel received such status much earlier than the Acts.

5. If Ac 1:6 were originally intended to follow Lk 24:49, some of the more serious conflicts in the other parts of the respective chapters would be difficult to explain; e.g. Lk 24:36; the disciples are in a house; but Ac 1:12: the Ascension occurs on Olivet without any indication in the narrative that the followers had accompanied Jesus to that place; Lk 24:33, *Εὐρον ἡθροισμένους τοὺς ἑνδεκά* is apparently in conflict with Ac 1:6, *οἱ μὲν οὖν συνελθόντες*.
6. It would require an extraordinary genius to invent Ac 1:8 which fits in so well with the theme of the entire book.
7. Ac 1:1-5 gives evidence of some of the stylistic peculiarities of Luke which an interpolator could hardly have noticed<sup>8</sup>.

Leaney feels that Sahlin's hypothesis does not satisfactorily explain the double reference to the Ascension in Ac 1:2 and 9. For him the ending of the Gospel entails no contradiction: "Indeed, the whole of Acts 1:1-14 appears to retell the story of Luke xxiv 50-53, adding details of the Ascension, but this may be held to throw suspicion on Acts 1:1-14 rather than on Luke xxiv. 50-53"<sup>9</sup>.

Leaney's approach is indeed based on a

strange misreading of the relation between the ending of Lk and the beginning of Ac. Most scholars would rather find a contradiction between Lk 24:50f and Ac 1:3 based on the forty days' interval between the Resurrection and the Ascension mentioned in the latter passage (vide: infra). Ac 1:2, whoever its original author be, is definitely meant to recall the reference in Lk 24:50-53. Ac 1:9 is only a detailed description of the manner of the Ascension to which reference was already made in v.2, and as such, it need not constitute a contradiction at all<sup>10</sup>.

Sahlin himself, however, admits that Lk 24:50-53 is Lukan in style<sup>11</sup>. For Leaney, suspicion is only with regard to Ac 1:3-5; 1:9-11 is linked up with the Gospel not only by the verb ἀναφέρειν, but also by the 'cloud' (cf. Lk 21:27) and 'the two men in white apparel' (cf. 9:30). And Lk 9:51 speaks of the ἀνάληψις which Jesus was to accomplish in Jerusalem: His death was the means to enter into His glory (Lk 9:22; 22:69 cf. Ac 1:2,11,22; also Jn 20:17; Ac 2:34). Thus Leaney agrees that Ac 1:9-11 is also definitely Lukan and that Ac 1:6 takes up the story as the author had left it at Lk 24:49<sup>12</sup>.

The self-contradiction of Sahlin's position is now all the more apparent. Neither he, nor Leaney, has any suspicion of the Lukan characteristics of the last four verses of the Third Gospel. Yet Sahlin claims that this section is a later addition which synchronized with the 'spurious' insertion, Ac 1:1-5. So acceptance of Sahlin's theory would be possible only by subscribing to the hypothesis of the double recension of Lk-Ac by the author himself, proposed by Blass. But this is not what Sahlin's theory is all about.

(b) Graefe<sup>13</sup> applies Blass's theory of the double recension to the end of the Gospel and concludes that the text of D and its allies represents the original edition sent to Theophilus: "Er war froh, diese Worte welchen D u. Genossen bieten, noch ganz unten auf seiner Rolle unterzubringen, und sandte das Buch an Theoph. ab, hoffend, die Himmelfahrt in der zweiten Schrift zu behandeln. In der zweiten Ausgabe hatte er Platz genug, um die Worte... einzuschieben. Er machte diese Zusätze in dem richtigen Gefühl, dass es keinen würdigeren Abschluss des Lebens Jesu geben könne, als eine kurze Andeutung der Himmelfahrt, die er bereits in der Acta näherbeschreiben hatte".

Scholarly opinion has, of course, not found favour with the application of this theory to the Third Gospel, as, we have pointed out on more than one occasion. In fact in his treatment of this section in Lk Blass does not seem to bank upon this theory, but he only tries to defend the long readings from considerations of Intrinsic Probability<sup>14</sup>. But insofar as Graefe is prepared to ascribe the 'insertion' at v.51 to Luke, it fits in with our finding. But his defence of the disputed words at vv.52 and 53 does not tally with our view to be stated shortly. The reading of AC<sup>2</sup> KWX<sup>0</sup>Ψ f<sup>1</sup>33 at v.53 is definitely a conflation, as Metzger<sup>15</sup> points out.

(c) According to Gardner-Smith, it was the extreme brevity of the narrative, Lk 24:50ff, that prompted the two 'additions': "Nothing could be briefer than the narrative... That a feeling very soon grew that this was an inadequate account of the ascension is indicated by the two 'Western non-interpolations', but there can be no doubt that they do not belong to the true text"<sup>16</sup>.

Metzger's comment<sup>17</sup> appeals as a fitting reply to this suggestion: The most likely place that would have provided the material for the insertion is Ac 1:1-2. But the fact that we have in Lk the

verb ἀναφέρειν<sup>18</sup> instead of some form of ἀναλαμβάνειν as in Ac makes the possibility of such a borrowing very remote. (This observation holds good even if it be conceded that the opening verses of Ac were introduced by the same scribe who made the 'insertions' at the close of the Gospel). Therefore it is tempting to dismiss Leaney's note, "The phrase (sc. ἀνεφέρετο κ.τ.λ. ) may...rightly be suspect on the ground that it dismissed too casually an event which has great importance in Luke-Acts"<sup>19</sup>, as of little weight. But such a hasty decision would not be without difficulties in view of the evidence of Mk 16:19 where we have ἀνελήμφθη εἰς τὸν οὐρανόν. There is a general consensus that this wording was inspired by Lk 24:51<sup>20</sup>. This seems plausible in view of the alleged influence of Lk ch.24 on Mk 16:12ff. So the problem arises as to why the scribe who attempted to make good the deficiency of the Conclusion of Mk preferred to use ἀναλαμβάνειν rather than ἀναφέρειν. The answer to this may well lie in suggesting that Ac. was more influential on this interpolator than Lk. For example, ἀναλαμβάνειν occurs thrice in the opening chapter of Ac at vv.2,11,22; cf. 2:34; ἐκάρθισεν ἐκ δεξιῶν τοῦ θεοῦ, cf. 2:33; 7:56; the reference to the disciples' preaching everywhere

and the Lord's confirming their preaching by means of signs, Mk 16:20, is clearly reminiscent of Ac: cf. especially 8:4; 4:33; 5:12ff. If this is a valid point, it is a strong evidence in support of Metzger's defence of Lk 24:51.

(d) Plummer comments that the disputed words at both vv.51 and 52 may have got into the text from marginal glosses<sup>21</sup>.

Despite Godet's objection<sup>22</sup>, I am inclined to view this suggestion favourably in regard to v.52. Godet argues that since there is no analogous expression in Ac 1, it is not likely to have come from an extraneous source. But he overlooks the many parallels that this Lukan section has got in Mt 28:16-20 (vide infra). The occurrence of *πρὸς ἐκὺς ἡσδρ* in Mt 28:17 (cf. v.9) would have been a natural temptation for a rash scribe to bring about this harmonistic insertion at Lk 24:52<sup>23</sup>. But this has to await confirmation from other data.

(ii) Possibilities of Omission

From the point of view of Transcriptional Probability it is generally assumed that the disputed words in the two passages under discussion will stand or fall together. It could be argued that without

some reference in v.51 to a heavenward disappearance, the mention of 'worship' in v.52 would be unjustified. Therefore it is instructive to compare this context with the reference to 'worship' in the Matthaean account of the Resurrection appearances, 28:9,17. The gesture of reverential obeisance on the part of both the women and the apostolic band on these two occasions was a token of their recognition of their Master's supernatural identity. This sense of awe was awakened in them not at the time of His parting, but on His sudden appearance. For this was the first time that they had a glimpse of His future glory in heaven. This experience of Jesus' followers is quite different from what three of their representatives were privileged to have on the mountain of the Transfiguration. All the Synoptists are agreed in informing us that the disciples were afraid on that occasion. Even Peter who expressed a desire to linger on under the spell of that heavenly experience is reported as being not really aware of what he was talking about. But in the context of the Resurrection appearances, Matthew would have his readers know that the followers of Jesus confronted their risen Lord in an attitude of worship even though the perplexity of at least some of them was not completely removed (cf. v.17, 'some doubted'). But in

the Lukan context, the alleged act of worship on the part of the disciples is not in connexion with the sudden appearance of the Lord, but on the occasion of His final departure<sup>24</sup>. Of course, the word *δέεσθαι* is adequate to convey the sense of a final departure as far as the readers are concerned. But it would have been quite unnatural for the disciples to worship their Master unless they were aware that they were standing at the very gate of heaven. Thus it could be argued that, for the author, the reference to 'worship' of the departing Lord is a corollary to the heavenward ascent; i.e. if *καὶ ἀνεφέρετο εἰς τὸν οὐρανόν* was part of the original, *προσκυνησάτες αὐτόν* would be a logical concomitant. On the other hand, it could also be argued that the extra words at v.51 alone were original and that a scribe, reasoning along the above line, felt compelled to introduce the reference to the worship of the Lord by the disciples. Now, if we compare the Infancy Narratives of Lk and Mt, we find that whereas the latter has three references to the worship of the divine child, the former has none at all (Mt 2:2,8,11 cf. Lk 2:15ff, 28,38). This and a number of other reasons yet to follow would suggest that the words in question at v.52 are an interpolation in imitation of Mt<sup>25</sup>.

With this preliminary observation let us proceed to examine the chances of deletion of the above words, had they been part of the Lukan autograph:

(a) The usual theory is that the 'contradiction' between Lk 24:51 and Ac 1:3 arising out of the 'forty days' interval between the Resurrection and the Ascension mentioned in the latter passage led to a suppression of the disputed words at Lk 24:51<sup>26</sup>.

This does not, however, conform to the general tendency of the scribes behind the 'Western' Text. The normal procedure for a copyist would be to make use of the additional information from Ac in the form of an interpolation in the text or a marginal note in the corresponding passage in Lk.

(b) A more plausible suggestion<sup>27</sup> is that the shorter reading at v.51 may have arisen from a slip of the copyist's eye, facilitated by the combination of the letters NKAI A occurring in two successive lines of an exemplar, thus:

ΑΥΤΩΝ ΝΚΑΙ ΑΝΕΦΕΡΕΤΟ  
ΟΥΡΑΝΟΝ ΚΑΙ ΑΥΤΟΙ

The only difficulty with this explanation is that if we admit this to be a slip, we are immediately up against the possibility of a similar

slip at v.52. The omission in the latter case might have happened due to homoeoarcton, the eye of the scribe passing inadvertently from the  $\alpha\upsilon\tau\omicron\iota$  at the beginning of the verse to the  $\alpha\upsilon\tau\omicron\nu$  at the end of the verse<sup>28</sup>. It needs a little stretch of imagination to visualize a scribe making two accidental omissions in such quick succession, although it is not altogether impossible.

(c) F.H. Chase<sup>29</sup> is of opinion that the compressed phraseology of the Old Syriac at v.51 led to the disappearance of the words referring to the ascent to heaven. For Syr<sup>S</sup> reads: (And-when-He-blessed them, He-was-lifted-up from them). This text, while preserving the idea, does abbreviate the actual wording. So a copyist who assimilated the Greek to an Old Syriac text would easily be led to drop the words  $\kappa\alpha\iota\ \ \alpha\ \nu\epsilon\ \phi\acute{\epsilon}\rho\epsilon\tau\omicron\ \ \epsilon\iota\varsigma\ \ \tau\omicron\nu\ \ \omicron\upsilon\ \rho\alpha\ \nu\acute{o}\nu\ .$

C.S.C. Williams' objection<sup>30</sup> against this has not been satisfactorily met. He says that the above suggestion raises the question of the priority of Old Syriac. Are we justified in saying that a rendering from the Old Syriac was made early enough so as to influence an ancestor of  $\mathcal{N}$  D a b d e ff 1? Moreover, Chase's suggestion does not adequately explain the disappearance of  $\alpha\ \nu\epsilon\ \phi\acute{\epsilon}\rho\epsilon\tau\omicron$  which essentially

implies an ascent.

(d) With regard to the absence of the *προσκυνησάμετες αὐτὸν* at v.52, it has been suggested that the absence of a reference to 'worship' in Ac 1:9-11 may have brought this about (harmonistic motive)<sup>31</sup>.

This is a very important point and will receive more attention under Intrinsic Probability. For the present it would suffice to mention that this argument can turn out to be a factor in defending the short reading: the very fact that a reference to 'worship' is wanting in the Preface to Ac would suggest that the same was originally true of the Conclusion of the Gospel as well, because of the identity of design of both sections (vide infra).

#### Intrinsic Probability

Against those who maintain that there is a conflict between Ac 1:1-14 and Lk 24:50-53<sup>32</sup>, I submit that if the comparison be made more broad-based to cover the entire section Lk 24:44-53, it would be seen that the Conclusion of the Gospel is no more or less than a summary of the Preface to Ac. Note the following correspondence in outline:

(a) Jesus' instruction of the disciples - motif of fulfilment of prophecy, Ac 1:23 par. Lk 24:44-46 (The things concerning the Kingdom, Ac 1:3 = fulfilment of prophecy, Lk 24:44ff).

(b) The message of salvation must be preached to all nations, Ac 1:8 par. Lk 24:47.

(c) The disciples are to be witnesses to their Lord, Ac 1:8 par. Lk 24:48.

(d) The promise of the Holy Spirit and the command to await this divine gift, Ac 1:4-5 par. Lk 24:49.

(e) The final departure of the Lord, Ac 1:9-11 par. Lk 24:50-51.

(f) The impact of the departure on the disciples, Ac 1:12-14 par. Lk 24:52-53.

This analysis adequately shows that the close of the Third Gospel had a well-planned but necessarily compressed design which only awaited amplification in the author's second volume. If we leave out the reference to the Ascension in Lk 24:51, this neat balance of design is disrupted. It may

be thought that the angelic announcement of the parousia, Ac 1:10-11, disrupts the parallelism between the two passages. But I think that the 'great joy' of the disciples despite the Lord's departure for good, Lk 24:52, can be fully explained only in the light of some such reassurance. The disciples were still in the pre-Pentecost days. Although their initial scepticism and fear had been replaced by a mixed feeling of bewilderment and joy at the Lord's words of instruction and exhortation, His departure would still demand reassurance. The angels are part of the conventional imagery. They are supposed to give an inspired interpretation of the incredibly strange events (cf. 24:4): they are only a "literary personification of divine inspiration", guaranteeing the truth of the message<sup>33</sup>. Therefore I submit that Luke was already aware of the tradition regarding the angelic words in Ac 1:10-11 when he wrote the lines which now form the concluding paragraph of his Gospel. It is possible that he was constrained by limitation of space to exercise the maximum economy of words<sup>34</sup>: the unusually compressed nature of the information he provides towards the end of the Gospel justifies such a conjecture. Even otherwise, the hope of a speedy publication of a second volume would naturally lend itself to such a condensed style at

the conclusion of the first. The very nature of the subject that he was going to deal with in the companion volume - the birth of the Christian Church and the expansion of the Kingdom of God (= the spread of the Gospel) under the power and guidance of the Holy Spirit - entailed a more elaborate survey of the same ground he had touched upon at the conclusion of the first volume. So the details found at the beginning of Ac 1 arise not so much from a change of theological motif as from a consolidated scheme covering the twin works of the same author<sup>35</sup>.

This is not to deny that there is a shift in emphasis on the part of Luke when he comes to the Acts. Haenchen observes: "The ascension actually possesses a somewhat different meaning at the conclusion of Luke from that at the beginning of Acts. Luke is making use of two different traditions here to express two different aspects of the ascension. The Gospel triumphantly closes the existence of the Lord on earth. In the context of Acts, on the other hand, it links the instruction of the disciples by the risen Lord - and thus the Church's instruction by him - with the further history of the Christian proclamation and the growing congregation"<sup>36</sup>. While endorsing Haenchen's view with regard to the

new emphasis in Ac, I should think that this purpose is served even without recourse to a dependence on two different traditions (italics mine). Metzger means the same thing when he finds an attempt "to introduce a subtle theological differentiation between the Gospel and Acts"<sup>37</sup>, but unlike Haenchen, he attributes this to the 'Western redactor'. Whoever be the author of the Preface to Ac, it is important to recognize the basic fact that there is a unity of design between this section and the concluding part of the Third Gospel. If this is acceptable, then one serious divergence between the two sections comes into relief, namely the absence of any reference to 'worship' in Ac 1. Since the passage in Ac does not involve a textual problem in the context of the Ascension, vv.9-11, it seems reasonable to assume that the original text of Lk also did not contain a reference to worship at 24:52. Conversely, we uphold the genuineness of the reference to the Ascension at v.51 because of the repetition of the same theme in Ac 1.

Now, Ac 1:2 is fraught with textual difficulties which have been the subject of lengthy discussions<sup>38</sup>. Metzger's Textual Commentary<sup>39</sup> gives us a neat resumé of this debate and draws a

conclusion in favour of retaining the usual text which has the support of all the Greek witnesses except

D: . . . ἄχρι ἧς ἡμέρας ἐντειλάμενος τοῖς ἀποστόλοις  
διὰ πνεύματος ἁγίου οὕς ἐξελέξατο ἀνελημφθῆ

(...until the day when he was taken up, after he had given commandment through the Holy Spirit to the apostles whom he had chosen).

The reading of Codex Bezae, while textually difficult, still retains ἀνελημφθῆ, the word having been moved forward to follow immediately after ἄχρι ἧς ἡμέρας; it has also the addition καὶ ἐκέλευσε κηρύσσειν τὸ εὐαγγέλιον after ἐξελέξατο. This reading is attested by the Peshitta, Hk1<sup>mg</sup> and the Sahidic Version, though in slightly transposed forms which do not affect the reference to the Ascension.

Several Old Latin witnesses including codex Gigas, supported by Augustine and Vigilinus, give evidence of another reading which is obviously an attempt to get rid of the difficulties of D and its allies by eliminating the ἀνελημφθῆ. According to the reconstruction of J.H. Ropes and A.C. Clark who defend it, this text runs as follows at v.2:

'...on the day when he chose the apostles through the Holy Spirit and commanded them to proclaim the gospel'. Apart from the omission of a reference to the Ascension, the spuriousness of this reading is evident from its identification of the commencement of Jesus' ministry with the time of His choosing His disciples, which is, of course, in conflict with the Gospel records. Another major defect of this text is its omission of any indication of the end-point of the first volume of the author (i.e. the absence of *ἄχρι* ). Thus we may safely conclude that the witness of the traditional text retaining *ἀνελήμφθη* at v.2 is authentic, even if we may not be absolutely successful in reconstructing the original Lukan form of the opening verses of Ac<sup>40</sup>.

This conclusion gives us an added reason to emphasize that it is illogical to presume that the two references to the Ascension at Lk 24:51 and Ac 1:2 have no intrinsic unity, as Westcott-Hort have done. For while they rejected the former reading as spurious, they found reason to retain the latter as genuine<sup>41</sup>. This is a fallacy. As Graefe observes: "Entweder muss man sich entschliessen, sowohl am

Schluss des Evangelii, wie am Anfang der Apostelgeschichte den westlichen Text ohne jede Himmelfahrt für das Original zu halten; oder aber anerkennen, dass der erste Urheber des westlichen Textes mit bewusster Absicht beide Stellen bearbeitet und die Himmelfahrt eliminiert hat"<sup>42</sup>. And Graefe himself has decided in favour of retaining the majority reading in both places, which lends support to our position.

A further confirmation of the long text at v.51 may be derived from the fact that even without the *καὶ ἀνεφέρετο εἰς τὸν οὐρανόν*, the scene describes a final parting, very much in the manner of the Conclusion of Mt. Without involving ourselves in a scientific debate about the actual process of the Ascension and the 'geography' of heaven, it could reasonably be assumed that the early Church had unquestioningly identified the Ascension as the denouement of the Resurrection appearances<sup>43</sup> because no less than three books of the N.T. bear witness to a final departure of Jesus from this earthly scene. Whatever name with which the sophisticated and materialistically conditioned modern man would want to label that final scene, it is difficult to imagine how, without some mental

imagery of levitation, the primitive Church would have been able to sustain the Ascension as a part of the regular preaching (cf. Ac 2:33; Phil 2:9; Col 3:1; 1 Tim 3:16; Heb 1:3)<sup>44</sup>.

Again, Lk 24:52f describes the disciples as returning to Jerusalem with great joy and continually meeting in the Temple for worship. This drastic change from a mood of fearful hiding would not have been possible by a mere appearance or even a series of appearances of the Lord. It could only have been the result of a farewell encounter with Him (cf. Mt 28:16-20)<sup>45</sup>. He had, by His very act of blessing, conveyed to them the unshakable assurance of His eternal presence with them (v.50 cf. Mt 28:20b). They had also witnessed His departure in some spectacular manner as would have convinced them that the appearances were no longer to be expected. Despite His departure, they had a new schedule of life to follow and a faith to abide by. Henceforth they would be morally and spiritually sustained, not by His visible presence, but by His Spirit - a fulfilment of the promise of the Father (Lk 24:49).

P. Schubert is not correct in paralleling the Lord's departure at v.51 with that at v.31<sup>46</sup> because it is clear that the author's intention was to portray the former as a permanent departure. Otherwise the description of Jesus' leading the disciples up to Bethany would be utterly irrelevant<sup>47</sup>. This again, is parallel to Matthew's mention of a mountain ( *'Εξήγαγεν . . . ἕως πρὸς Βηθανίαν* cf. *ἔπορεύθησαν εἰς τὴν Γαλιλαίαν εἰς τὸ ὄρος οὗ ἑτάξατο αὐτοῖς ὁ Ἰησοῦς* ).

Both contexts stress the initiative and the deliberateness of the Lord's action, although the venue is different in each case.

The blessing in v.50 is to be seen in the light of the permanent parting scenes in the O.T. such as the blessings of Jacob, Isaac, Moses et al, and not in the light of the Aaronic blessing (Nu 6:24ff; Ecclus 50:20)<sup>48</sup>. Therefore I think that van Stempvoort's attempt<sup>49</sup> to draw a parallel between the note of blessing at the beginning and at the end of the Gospel is too far-fetched. He suggests that at the beginning of the Gospel Luke showed how a departing priest was unable to bless his people because of his lack of faith; in sharp contrast to this, the Gospel ends with the picture of a depart-

ing priest, who, by the very act of His blessing, becomes instrumental in evoking an abiding faith. By the same token, it is also tempting to argue how the theme of worship in the Temple (v.53) links the close of the Gospel with the scene of worship with which the Gospel begins<sup>50</sup>. Again, this would be stretching an incidental parallel beyond limits because it is the joy of the disciples and the unity of their spirit which find expression in their worship<sup>51</sup>. The fact of their being attuned to God in an unprecedented spirit of fellowship is Luke's emphasis here, as may be judged from a comparison with Ac 1:14. In any case, the theme of 'worship' in the last verse of the Gospel is no argument for retaining the additional words in v.52, which, as we have seen, are most likely to be an assimilation from Mt.

Much of the textual difficulty of v.51 is due to the assumption that the author of the Third Gospel had conceived of the Ascension as taking place on the same day as the Resurrection<sup>52</sup>. Some scholars wonder how the two events could have happened in the same night because, in the light of the Emmaus narrative, the Ascension should have taken place too late in the night<sup>53</sup>. This, to my mind, is not the crux of the matter. If Jesus had to avoid embar-

rassment to His fear-stricken disciples, the only way was to keep them off from public exposure. (He could, of course, disguise Himself at will)<sup>54</sup>. Since Bethany (= Mount of Olives<sup>55</sup>) was only a Sabbath day's journey (= 5 furlongs) from Jerusalem (Ac 1:12), there is nothing implausible about a late night Ascension<sup>56</sup>.

On the other hand, there are more weighty considerations which raise doubts as to whether Luke really wanted his readers to believe that the two events did happen during the span of 24 hours. If Jesus was to reappear to the two Emmaus disciples on their return to Jerusalem on the same night (the two men were not of the Twelve), why should He appear to them at all earlier on the way to Emmaus? We have already discussed the problems raised by the reference to the appearance to Peter (v.34, vide on 24:12), which again highlights the present difficulty. Thus we are led to believe that the Emmaus incident and the appearance to the apostolic band were separated by at least some days' interval. Similarly we may argue that the latter appearance and the trip to Bethany were also marked by some interval of time. The *δε* at v.50 which marks the beginning of the Ascension account need not necessarily indicate an immediate chronological sequence<sup>57</sup>.

A.N. Wilder thinks that 24:26 is an allusion to an Ascension from the Cross<sup>58</sup>, but the *Kdl* there which is in the consecutive sense does not stress any indication of time one way or the other<sup>59</sup>. Nor should Jesus' promise to the penitent malefactor (23:43) be taken as a proof-text to establish an Ascension from the Cross<sup>60</sup> because this would conflict with the early Christian tradition that the Resurrection occurred on the third day after the Crucifixion, to which all the Evangelists testify.

As for the 40 days' interval mentioned in Ac 1:3, there have been various speculations. B.W. Bacon<sup>61</sup> is of the opinion that the Ascension took place on the very day of the Resurrection. (He regards v.3 as a later parenthetical insertion by a copyist). According to this hypothesis, the later appearances recorded in Jn (20:24ff; 21:1ff) were from His heavenly abode<sup>62</sup>. Without entering into the metaphysical aspect of the problem, and letting ourselves be guided by the evidence of the N.T., it can firmly be asserted that the number 40 was not part of an article of faith of the early centuries<sup>63</sup>; but at the same time, there were various traditions current about some interval between the Resurrection and the Ascension<sup>64</sup>.

Besides the evidence from John cited above, indirect clues as to Luke's own awareness of a time-lag were referred to in passing in the two previous paragraphs. Further, Luke's loose way of referring to this period at Ac 10:41; 13:31 goes to confirm that there was no rigid standard for specifying it.<sup>65</sup> I am not convinced by the suggestion that in between the writing of the two volumes of his work Luke would have come across fresh information which he has adopted at Ac 1:3<sup>66</sup>. For during the compilation of the Gospel Luke must have collected information from first-hand sources, possibly in Palestine<sup>67</sup>. It is least likely that he would have come across such a piece of information as the 40 days at a subsequent stage, had it been current among the close followers of Jesus.

Bacon<sup>68</sup> cites Jn 20:21-22 in support of the Fourth Evangelist's awareness of an early tradition regarding the quasi-synchronistic view of the two events (cf. 20:17). But considering the veiled theological allusions of which John is capable, it would be unwise to read too much into this passage. It is most likely that John is presenting here an acted parable, symbolic of what the disciples were to expect on the day of Pentecost. The Evangelist cannot, after all, be charged with ignorance of the

events of that momentous day which marked the birth of the Church.

The very silence of the other Evangelists regarding this particular detail is only an indirect evidence for the lack of interest of the early Church in the details of the Ascension. Even Matthew's account of the appearance to the Eleven, which is, to all intents and purposes, a farewell scene, makes no mention of either the Ascension or even disappearance of the Lord. But the fact that the venue of this final meeting was in Galilee is ample proof that, in the First Evangelist's view, at least a few days had elapsed before the risen Lord appeared to His disciples for the last time. The correspondence between this passage and Lk 24:36ff which we examined in detail on an earlier occasion, also confirms this inference despite the obvious difficulty arising out of the difference of the venue in the two accounts. From these considerations it is clear that Luke is not to be isolated for special mention with regard to the lack of precision in all his references to the date of the Ascension except at Ac 1:3: he was only reflecting the mood of his times. But at Ac 1:3 he is furnishing a detail of which he was cognizant even at the time of his completing his

Gospel. So the way Luke begins the account of the Ascension at v.50 is to be attributed not to his lack of knowledge, but to a loose habit of referring to the event which he shared with many other Christians of his generation.

C.S.C. Williams<sup>69</sup> asserts that since the Ascension formed part of the *κήρυγμα* of the early Church we should expect a mention of it at least in one of the written Gospels.

This is not a strong argument because the very absence of such a reference in at least two of the Gospels (if we exclude the spurious Mk 16:19) would only make one wonder whether even Luke had any intention to incorporate an Ascension account in his Gospel. On the other hand, Luke himself provides us with two significant clues about not only his awareness of the Ascension as a historical event, but also the fact of his incorporation of this information in his Gospel. First, his use of the word *ἀνάληψις* in the solemn context of Jesus' setting his face towards Jerusalem (9:51) can be justifiably interpreted as an anticipatory reference<sup>70</sup>. This word is in fact the regular LXX usage for an ascension and not for death:(2 Kgs 2:9ff ; Ecclus 48:9; 49:14; 1 Macc 2:58).

But in Lk 9:51 the word is used in an absolute sense to represent the suffering, death and Ascension of Jesus. Secondly, Ac 1:2 has a retrospective reference to the fact that the author had definitely terminated his preceding volume with an account of the Ascension<sup>71</sup>.

Finally, considerations of style may also help tilt the balance of probabilities more towards the retention of the disputed words at v.51. Jeremias points out that since vv.50 and 52-53 exhibit two co-ordinate clauses joined with the conjunction *καί*, the rhythm of the whole section would require a similar construction in v.53 as well<sup>72</sup>. Others have called attention to the imperfect tenses, *ἦεν*<sup>73</sup> and *ἀναφέρετο*, which convey the impression that the final departure of Jesus was a gradual process. With regard to the second verb, van Stempvoort says: "It is a picturesque imperfect, describing his being carried up to heaven as a movement that takes some time. As for the semasiology of the verb *ἀναφέρειν*, it has a concrete and realistic meaning, quite in agreement with the realistic theology of Luke in ch. xxiv"<sup>74</sup>. As a matter of fact, this picturesque word fits in well with the imagery of the cloud in Ac 1:9. Conzelmann<sup>75</sup> points out that it was con-

ventional in both pagan and Jewish apocalyptic literature for a cloud to intervene in the departure of heroes from this world. The word ὑπολαμβάνειν really means 'to take up by getting under',<sup>76</sup>. So the cloud is picturesquely presented as a royal chariot taking Jesus up by getting under him and thus taking him out of the disciples'<sup>77</sup> sight. Thus Lk 24:50ff and Ac 1:9ff exhibit a stylistic solidarity.

#### Conclusion

The present section is one of the most widely debated passages in the Gospels and our survey has highlighted the complexities of the textual problem involved here. I trust that enough has been said to indicate the arbitrariness of Hort's conclusion regarding v.51: "A Western non-interpolation. Text was evidently inserted from an assumption that a separation from the disciples at the close of the Gospel must be the Ascension. The Ascension apparently did not lie within the proper scope of the Gospels, as seen in their genuine texts: its true place was at the head of the Acts of the Apostles, as the preparation for the Day of Pentecost, and thus the beginning of the history of the Church"<sup>78</sup>. We have shown that a final separation from the disciples was

tantamount to the Ascension in the conception of the early Church and especially of Luke; we have also shown that the Ascension did lie within the proper scope of the Third Gospel because of the solidarity of its Conclusion with the Preface of the Acts. The reason why modern scholarship is decidedly against Hort's one-sided judgment at this point is now clear<sup>79</sup>. On the other hand, we have also exposed the inherent weakness of the widely accepted view that the resolution of the textual problem of v.52 is dependent upon one's approach to v.51. Our adherence to the principle of a unity of design behind the close of the Gospel and the beginning of Ac. has served to disprove this and to show that the reference to 'worship' in v.52 is not original.

<sup>1</sup>NTS 12 (1965-66) p.208. Similarly Metzger's defence of the additional words at v.51 on the basis of their occurrence "in so many and such diversified witnesses, beginning with P<sup>75</sup>" does not carry much weight (cf. Textual Commentary, p.189).

<sup>2</sup>Souter, Text and Canon, p.127, justifies the omission of Syr<sup>S</sup> at v.52, but finds its evidence doubtful at v.51; cf. Burkitt, Evangelion, II, p.229.

<sup>3</sup>Cf. Graefe, TSK, 1896, p.272: "Im allegemeinen... bestätigt der neue Syrer den kürzeren Text; doch hat er entweder das *ἀνεφ.* schon vorgefunden...oder er hat *διεστῆναι ἀπ' αὐτῶν* so verstanden, wie es der Zusatz der Alexandriner erklärt; Cf. F.H. Chase, Syro-Latin Text, pp.130f; Aland, op.cit., pp.208f; Metzger, loc.cit.

<sup>4</sup>Messias u. Gottesvolk, pp.13ff; cf. A.N. Wilder, JBL (1943), pp.311-12; Creed, St. Luke, p.301.

<sup>5</sup>Contrast C.S.C. Williams, Alterations, pp.51f, who makes use of the same argument for the originality of the disputed words in the concluding section of Lk!: "It may be that the editor of Luke or of the Gospels and Acts, while attempting to put them into a single codex, found it necessary to cut off the conflicting parts from Luke. It is very possible, though incapable of proof, that the origin of the 'Western' text and the origin of the Christian use of the codex-form were simultaneous". The very fact that the same argument has been made use of to defend two conflicting views clearly shows that the premise on which it is based, namely that the two Lukan works entered the canon as a single unit, is wrong.

<sup>6</sup>Introduction (E.T.), p.110.

<sup>7</sup>Cf. Cadbury, Book of Acts, pp.138f.

<sup>8</sup>Cf. Haenchen, Acts of the Apostles (E.T.), ad.loc.

<sup>9</sup>St. Luke, p.295.

<sup>10</sup>Even Wilder, loc.cit., who regards the section Lk 24:50-53 "as a summary of Ac 1:1-11, added to Luke when the two works were separated from each other", thinks that there is no conflict between Ac 1:2 and 1:9.

<sup>11</sup>Loc.cit.; cf. K. Lake, *Historical Evidence for the Resurrection*, p.115. He thinks that the author is here following either an Aramaic source, but giving a very free translation, or more probably, an oral tradition. He also links Ac ch.1 with the 'Jerusalem tradition' which forms part of the Resurrection narratives of the Third Gospel (pp.109,115f).

<sup>12</sup>Op.cit., pp.295-96.

<sup>13</sup>TSK, 1898, p.137.

<sup>14</sup>Cf. *Philology*, p.139; E. Nestle, *Einführung*<sup>2</sup> (1899) pp.193f, however, defends Blass here.

<sup>15</sup>Op.cit., pp.190f.

<sup>16</sup>Narratives, pp.76f.

<sup>17</sup>Op.cit., p.189.

<sup>18</sup>Ibid.; Metzger notes that the word is commonly used in the technical sense of 'to offer up' (a sacrifice); the general sense of 'to lead up' is extremely rare (Mk 9:2; Mt 17:1). No doubt the Ascension is nowhere represented as the offering of a sacrifice. However, while Metzger makes use of this information in defence of the long text, Leaney (loc.cit.) regards the rare occurrence of the verb in the N.T. as of sufficient weight to discredit them.

<sup>19</sup>Loc.cit.

<sup>20</sup>Cf. Lagrange, *Critique Textuelle*, II, p.69 (vide on Mk 16:9-20).

<sup>21</sup>St.Luke, ICC<sup>2</sup>, p.565.

<sup>22</sup>St.Luke (E.T.), II,, ad.loc.

<sup>23</sup>So Plummer, loc.cit; cf. Lagrange, *Luc*, p.595; Loisy, *Luc*, p.565.

<sup>24</sup>Cf. Lagrange, *Critique Textuelle*, II, p.69: "...le prosternement paraissait peu à propos quand une personne est déjà partie".

<sup>25</sup>Against Aland, *op.cit.*, p.209, who suggests that since the mention of 'worship' here is peculiar to this passage in Lk (except for the demand of Satan in the Temptation story), a scribe may have felt compelled to delete it. But judging from our understanding of scribal habits, the probability for interpolation is much stronger.

<sup>26</sup>Cf. Moffatt, Introduction to the Literature of the N.T.<sup>3</sup>, p.276; Loisy, *Luc*, p.591; P.A. van Stempvoort, *NTS* 5 (1958-59), p.36; A.N. Wilder, *op.cit.*, Lampe (Peake's Commentary), *ad.loc.*; Metzger, *op.cit.*, p.190; C.S.C. Williams, *Alterations*, p.52; Streeter, *Four Gospels*, p.143; Lagrange, *Critique Textuelle*, II, p.69.

<sup>27</sup>Cf. Alford, *Greek N.T.*, I<sup>5</sup>, *ad.loc.*; also attributed to G.D. Kilpatrick by Jeremias, *Eucharistic Words* (E.T.), p.151. A.C. Clark, *Primitive Text*, p.153, must be referring to the same possibility when he cites this passage as an example of omission in the middle of a stichos in an ancestor of D.

<sup>28</sup>So Alford, *loc.cit.*; Metzger, *loc.cit.*

<sup>29</sup>*Syro-Latin Text*, pp.130f.

<sup>30</sup>*Alterations*, p.52.

<sup>31</sup>*Ibid.* The reasons for a possible omission proposed by Lagrange and Aland, and mentioned in foot-notes a short while ago, are worth recollecting here.

<sup>32</sup>Cf. Gardner-Smith, *Narratives*, pp.77ff. Contrast P. Benoit, *RB* 56 (1949), pp.191-93: "D'une part, en effet, l'Ascension de Lc XXIV.51 se donne certainement comme une manifestation visible et comme un départ définitif, à la façon de celle de Act 1:9-11, nullement comme une Exaltation invisible, suivie de retours. D'autre part, les entretiens de Act 1:3-8 ne peuvent se distinguer de ceux de Lc XXIV.36-49..."; Cf. A. Loisy, *Les Livres du N.T.*, pp.410ff; D.J. Dupont, *NTS* 8 (1961-62), pp.154-57; Aland, *op.cit.*, p.208.

<sup>33</sup>*Vide* Harrington (*New Catholic Commentary*), p.1080; cf. J.P. Charlier, *L'Evangile de l'Enfance de l'Eglise* (1966), p.23.

<sup>34</sup>A. Ruegg, TSK, 1896, pp.94-101; he compares the Conclusions of the Third Gospel and of the Acts and observes: "Der Schluss der beiden Lukasschriften scheint gleicherweise auf eine Fortsetzung angelegt zu sein". (p.95); cf. Graefe, TSK, 1898, p.137; contrast Enslin, JBL 47 (1928), pp.60-73.

<sup>35</sup>Ruegg, loc.cit., cf. A.B. Bruce, Expositor's Greek Testament, p.651.

<sup>36</sup>Acts (E.T.), p.57.

<sup>37</sup>Op.cit., p.190; Cf. Creed, St. Luke, p.301: "...it is fitting at the end of the Gospel to portray the departure of Christ on the close of the personal intercourse... In Acts it is equally appropriate to treat the departure in a different aspect, viz. the exaltation of Christ, on which the gift of the Spirit and the victorious progress of the Church depend". Also, see Kummel, Introduction (E.T.), p.111; Barrett, Luke the Historian, p.56; Benoit, op.cit., p.191.

<sup>38</sup>Vide K. Lake, Beginnings of Christianity, Part 1, Vol. IV, pp.2-4; vol. V, pp.1-4; J.H. Ropes, The Text of Acts (Beginnings of Christianity, Vol. III), p.2; pp.256-261; A.C. Clark, Acts of the Apostles, pp.2; 336-37; F. Graefe, TSK, 1888, pp.522-41; TSK, 1898, pp.136-137; J.M. Creed, JTS, 35 (1934) p.180.

<sup>39</sup>Pp.273-77.

<sup>40</sup>Cf. D.J. Dupont, op.cit., p.155; J.M. Creed, loc.cit; P. Benoit, RB 56 (1949), p.190.

<sup>41</sup>Notes, ad.loc.

<sup>42</sup>TSK, 1888, p.539; cf. O. Bauernfeind, Die Apostelgeschichte, Leipzig, 1939, p.19; O. Michaelis, Die Erscheinungen des Auferstehenden, Basel, 1944, p.82.

<sup>43</sup>Moule, ExpT 68 (1957), p.208, aptly describes the Ascension as "an acted declaration of finality" to the Resurrection appearances; cf. G. Salmon, Some Thoughts on Textual Criticism, pp.94f; Barrett, loc.cit. ; Aland loc.cit.

<sup>44</sup>It is significant to note that all the Synoptists record 'the coming' of the Son of Man 'in the clouds' (Mk 13:26 par). This would imply a knowledge on the part of the authors about His 'going up' 'in the clouds': Cf. Jn 3:13, 6:62, 20:17.

<sup>45</sup>Cf. Blass, *Philology*, p.139; Taylor, *Behind the Third Gospel*, p.228.

<sup>46</sup>N.T. *Studien für R. Bultmann*, p.168, n.; cf. Ellis, *Luke*, ad.loc.

<sup>47</sup>So Haenchen, *op.cit.*, p.57.

<sup>48</sup>Several scholars miss this point because they overlook the fact that the blessing 'priest' in the present context is not inside the Temple, but on Olivet and that the occasion is not essentially a dismissal of the 'congregation', but a permanent farewell. Vide D. Daube, *N.T. and Rabbinic Judaism*, p.234; A. Schlatter, *Lukas*, p.269; Strack-Billerbeck, *Kommentar zum N.T.*, pp.2,55f,76. It is very tempting at first sight to be carried away by a few verbal resemblances at Ecclus 50:20ff. But these have to be balanced against the total picture of Lk-Ac.

<sup>49</sup>NTS 5 (1958-59), pp.34f.

<sup>50</sup>*Ibid.*

<sup>51</sup>A.B. Bruce, *Expositor's Greek Testament*, p.651, suggests that their regular worship in the Temple was in harmony with the programme 'beginning at Jerusalem' (Ac 1:8) outlined for them by the Lord. But I think it is far-fetched to ascribe this motif to Luke at this stage as he makes it clear that the evangelistic awakening of the disciples was primarily the after effect of their experiences on the Day of Pentecost; cf. van Stempvoort, *op.cit.*, p.34.

<sup>52</sup>Cf. Alford, *Greek N.T.*, I<sup>5</sup>, ad.loc.: "The Ascension appears to be related as taking place after the foregoing words were spoken - but there is an uncertainty and want of specification about the narrative, which forbids us to conclude that it is intended as following immediately upon them".

<sup>53</sup>Cf. Gardner-Smith, Narratives, pp.65f.

<sup>54</sup>Holtzmann, Hand-Commentar, ad.loc; assumes that Jesus walked undisguisedly through the streets of Jerusalem leading the disciples towards Bethany!; Cf. A.B. Bruce, op.cit., p.651.

<sup>55</sup>So Moule, ExpT 68 (1956-57), p.206, n.6; IDB vol K-Q, p.599; Ropes, Beginnings of Christianity, Vol.V, pp.475f.

<sup>56</sup>Vide Enslin, op.cit., p.61.

<sup>57</sup>Barnabas 15:9 is often cited in support of the theory that has been refuted here (cf. Schmiedel, Encyclopaedia Biblica, IV, col. 4060, §16, Ascension, (c). But the quotation runs thus: ἄγομεν τὴν ἡμέραν τὴν ὀγδόην . . . ἐν ἣ καὶ ὁ Ἰησοῦς ἀνέστη ἐκ νεκρῶν καὶ φανερωθεὶς ἀνέβη εἰς οὐρανοὺς  
(We keep holy the eighth day (sc. Sunday) . . . in which also Jesus rose from the dead and, after appearing, went up to heaven). It seems to me that this reference, while being based on Lk ch.24, gives as hazy a picture as that of Lk with regard to the chronology of the events (italics mine).

<sup>58</sup>Op.cit., p.313.

<sup>59</sup>Cf. Zerwick, Biblical Greek (E.T.), §455(b), p.153.

<sup>60</sup>Cf. W. Baird, Interpreter's One Volume Commentary, ad.loc.

<sup>61</sup>Exp 1909, pp.254-61; contrast, M.S. Enslin, op.cit., p.64.

<sup>62</sup>The appearance to Paul or the vision of Stephen at the time of his martyrdom should not be confused with the Resurrection appearances as many a commentator has done. These are later events, the precise nature of the manifestations being not clear from Luke's accounts in Ac chs. 9, 22, 26 on the one hand and in Ac ch.7 on the other. The only concrete basis for the appearance to Paul is his claim in 1 Cor. 15:8 (The ἕσχατος here does

not necessarily mean that this appearance was the last in the series of earlier appearances); cf. 1 Cor 9:1; 2 Cor 12:1ff; Ac 23:11. In modern times more than one Christian saint in India have testified in no uncertain terms to having had manifestations of Christ similar to Paul's or Stephen's. (Cf. R.H.S. Boyd, *India and the Latin Captivity of the Church*, Cambridge, 1974, p.77).

<sup>63</sup>Cf. Ph.H. Menoud, *Freundesgabe O. Cullmann* (1962), p.152: "Le 40 jours après la résurrection n'est une date chrétienne ni pour Luc ni pour l'Eglise des trois premiers siècles"; Cf. Enslin, *op.cit.*, p.72. Some scholars regard the number 40 as a round figure denoting the period covered by the appearances listed by Paul in 1 Cor 15:3-8 (e.g. Bacon, *op.cit.*, p.260; M.H. Shepherd, *JBL* 64 (1945), p.236; Menoud, *op.cit.*). This is opposed by Kümmel, *op.cit.*, p.111. The text of Blass (*Acta Apostolorum, sive Lucae ad Theophilum liber alter*, Göttingen, 1895), eliminates the reference to the 40 days, but it cannot be treated as authoritative because of its conjectural basis.

<sup>64</sup>Cf. Enslin, *op.cit.*, pp.72f.

<sup>65</sup>Cf. Ramsay, *St. Paul the Traveller*; pp.17f: "Luke's style is compressed to the highest degree; and he expects a great deal from the reader. He does not attempt to sketch the surroundings and set the whole scene like a picture before the reader; he states the bare facts that seem to him important and leaves the reader to imagine the situation". W. Manson, *Luke*, p.270, is inclined to attribute this indifference to detail to Luke's source. But it is rather an inherent weakness of our Evangelist.

<sup>66</sup>Cf. Streeter, *Four Gospels*, pp.142f; K. Lake, *Historical Evidence for the Resurrection*, p.114; V. Taylor, *Behind the Third Gospel*, p.228; G. Salmon, *Some Thoughts on Textual Criticism*, p.97; Enslin, *op.cit.*, p.61 and n.2; p.62; H.E.W. Turner, *ExpT* 68 (1956-57), pp.370-71.

<sup>67</sup>Cf. F.W. Farrar, *St. Luke*, p.20.

<sup>68</sup>*Op.cit.*, pp.257-58.

<sup>69</sup>Alterations, p.51.

<sup>70</sup>Cf. Enslin, op.cit., p.61; Leaney, St. Luke, p.295; Blass, Philology, p.139; Moule, op.cit., p.205, n.3; Creed, St. Luke, p.301: "The event here described is not to be distinguished from the ἀναλήμψις of ix.51". Although Creed ascribes the disputed words at 24:51 to the work of a later hand, he treats the διέστη ἐπ' αὐτῶν as a definite allusion to the Ascension. Cf. A.B. Bruce, loc.cit.

<sup>71</sup>So J.H. Ropes, Beginnings of Christianity, III, pp.256-61; Lampe (Peake's Commentary), ad.loc.; Metzger, op.cit., p.189. Zahn, Introduction (E.T.) III, p.87, n.14: "Quite independently of the question concerning the correctness of the text in Luke xxiv.51, and of the harmonistic difficulties suggested by a comparison of Luke xxiv.44-53 and Acts 1:1-14, it is shown by Acts 1.2 that the author is conscious of having already given an account of the ἀναλήμψις in Luke xxiv.51".

<sup>72</sup>Op.cit., p.151; cf. Metzger, op.cit., p.189; Loisy, Luc, p.591: "Il semble que le rythme de cette finale ist plus satisfaisant dans le texte le plus complet."; see also, van Stempvoort, op.cit., p.36.

<sup>73</sup>Plummer, St. Luke, ICC<sup>2</sup>, p.565; Alford, Greek N.T., I<sup>5</sup>, ad.loc.

<sup>74</sup>Loc.cit.

<sup>75</sup>Apostelgeschichte, p.23; cf. Harrington (New Catholic Commentary), p.1080.

<sup>76</sup>Cf. Liddell and Scott, Greek-English Lexicon, ad.loc.

<sup>77</sup>Cf. van Stempvoort, op.cit., p.37.

<sup>78</sup>Notes, p.73.

<sup>79</sup>Aland, loc.cit., H.W. Montefiore, citing van Stempvoort (op.cit., p.42) approvingly, remarks as follows: "It seems that all the events recorded in Lk xxiv take place on Easter Day, although Luke's language is

vague. The two stories apart from this, do not contradict each other: the account in Acts is an extended version of the Gospel record with a different theological meaning" [NovT 4 (1960), p.308, n.3].

I.E. Mt 27:49b: The Piercing of the Lord's Side

Disputed words: [ ἄλλος δὲ λαβὼν λόγχην . . . . .  
ὕδωρ καὶ αἷμα ]

External Evidence:

αὐτόν. A D K W Δ Θ Π 090 f<sup>1</sup> f<sup>13</sup> 28 33 565 700 892 1009 1071  
1079 1195 1216 1230 1241 1242 1253 1344 1365 1546 1646 2148 2174 *Byz Lect*  
it<sup>a</sup>.aur.b.c.d.f.g<sup>1</sup>.h.i.q.r<sup>1</sup> vg syr<sup>s</sup>.p.h.pal<sup>ms</sup> cop<sup>sa</sup>.bo goth arm eth<sup>pp</sup>.ms<sup>c</sup> geo  
Diatessaron Origen<sup>1st</sup> Apostolic Canons Eusebius Hilary Jerome Au-  
gustine // αὐτόν. ἄλλος δὲ λαβὼν λόγχην ἐνυξεν αὐτοῦ τὴν πλευράν, καὶ  
ἐξῆλθεν ὕδωρ καὶ αἷμα. (see Jn 19.34) & B C L 1010 syr<sup>pal</sup><sup>ms</sup> eth<sup>ro</sup>.ms<sup>\*</sup>  
Chrysostom<sup>4<sup>th</sup></sup>. to Severus

### Observations

(i) The words enclosed within double square brackets by Westcott-Hort and listed along with the 8 Lukan passages as being most probably a 'Western non-interpolation' find support from  $\aleph$ BCL Chrysostom. In the words of Scrivener, "We are here brought face to face with the greatest interpolation yet laid to the charge of B, whose tendency is usually in the opposite direction"<sup>1</sup>.

(ii) However, the unusual unanimity of the Old Latins and the Vulgate, supported by most of the other Versions in reading the short text is impressive indeed. The majority of the Greek MSS are on the side of Codex Bezae in this respect.

This is, of course, one of the instances where MS evidence does not help arrive at a decisive

conclusion<sup>2</sup>. Nestle, however, considers the evidence as sufficiently strong to discredit the authenticity of the passage<sup>3</sup>. Burkitt follows the same line: "Here clearly the line of transmission represented by  $\mathcal{N}B$  contains an error, which originally could have come by a mere scribal blunder"<sup>4</sup>. Westcott-Hort, though reluctant to drop the passage from the text altogether on other grounds, are impressed by "the absence of the words from all the earlier versions, though the defectiveness of African Latin, Old Syriac... and Thebaic evidence somewhat weakens the force of this consideration"<sup>5</sup>.

### Internal Evidence

#### Transcriptional Probability

##### (i) Possibility of Insertion

(a) There is an obvious possibility of insertion if the short reading represents the original work of Matthew, namely, an attempt to harmonize the account of the death scene of Jesus with the Johannine account. Zahn's remark, "Eine ungeschickte Interpolation aus Jo XIX.34"<sup>6</sup>, has been shared by many scholars.

But the glaring differences between the two corresponding passages cannot be overlooked. McNeile explains this by suggesting that the present

reading in Mt originally entered an exemplar in the form of a gloss, being an adaptation of Jn 19:34. He thinks that the order 'water and blood' in the Matthaean insertion is due to 1 Jn 5:6<sup>7</sup>.

Since the death scene has nothing else in common between Mt and Jn, the presence of this verse should certainly be suspect. But if the scribe had a copy of Jn before him, it was more natural for him to copy the Johannine passage instead of rewriting it in such a radical way: in fact Mt 27:49b contains as many deviations from Jn 19:34 as there are similarities, so that one might say that it is only the idea of the latter that is retained in Mt.; the few similarities are, of course, dictated by the context. Although the theory of a gloss is acceptable, it seems more reasonable to trace it back to some floating tradition than to the Fourth Gospel. Metzger's guess is that the differences in wording of this passage may have been due to a gloss produced from memory<sup>8</sup>.

(b) Scrivener postulates the origin of the passage in Mt as due to "the unintentional blunders of Harmonists like Tatian"<sup>9</sup>. He cites a scholion found in Codex 72 (11th century) in his support, which reads: εἰς τὸ καθ' ἑστορίαν εὐαγγέλιον Διοδώρου καὶ

Τατιάνου καὶ ἄλλων διαφόρων ἁγίων πατέρων, on the authority of Chrysostom<sup>10</sup>.

V08bus identifies this as Min.52 MS Br. Mus. Harley 5647 and supports the theory of Tatianic origin by claiming that this finds confirmation in Codex Fuldensis in which this passage is omitted in Jn<sup>11</sup>.

But Hort had discredited this evidence long ago on the following grounds<sup>12</sup>:

1. The authority of Chrysostom for the above scholion is not reliable because another cursive, 238, attributes what is apparently the same scholion to Severus. "The authorship is however rendered doubtful by a more authentic fragment of Severus".
2. "The opposite view as to the reading is implied in a sarcastic statement of the Chronicle of Victor Tunuensis...that 'at Constantinople the holy Gospels were by command of the emperor Anastasius censured and corrected, as having been composed by unlettered (idiotis) evangelists' ".
3. Nothing is known of the work of Diodorus mentioned in the scholium, although the well-known Diodorus

of Tarsus wrote a commentary on the Gospels which is not extant.

4. Ephrem's Commentary on the Diatessaron does not show the words in Mt, while it contains an exposition of them or of the corresponding words in Jn.
5. "Even if the words ἄλλος δὲ κ.τ.λ. had a place here in Tatian's Diatessaron<sup>13</sup>, the hypothesis that they originated in its harmonistic arrangement is practically excluded by their remarkable documentary attestation, pointing to the highest antiquity. There is moreover no evidence that this obscure work was known out of Syria, where Tatian founded his sect; and the evil repute attached to his name renders the adoption of a startling reading from such a source highly improbable"<sup>14</sup>.

One also wonders whether much weight can be attached to the evidence of Codex Fuldensis on account of the conflicting attestation from Ephrem<sup>15</sup>. With regard to the evidence in the Arabic and Armenian Versions of the Diatessaron, J.R. Harris provides the following evidence: While the Arabic version gives the passage in Jn without any sign of dis-

placement, the Armenian text of Ephrem's commentary certainly places the piercing after Jesus' death. A comparison of Ephrem's text with the Arabic displays several displacements of the order by the latter. But the quotation in Ephrem is accompanied by the remark: 'This was done that they might know that he was alive after death' which shows that Ephrem was aware that the piercing was done after Jesus' death<sup>16</sup>.

In view of these considerations the opinion of Abbott becomes very apt: "Regarded as an intervention of John, the phenomena would be unique; regarded as a careless and misplaced interpolation from the Johannine tradition . . . the insertion in Matthew is fairly explicable"<sup>17</sup>.

(ii) Possibility of Omission

If the present reading was original, there is obviously only one possibility of accounting for its absence in Mt from D etc: it is placed here in a very awkward position, namely before Jesus' death, and immediately following the cry of dereliction. Since this is in conflict with the Johannine record, a scribe may have considered it expedient to delete it<sup>18</sup>.

Metzger's comment is a fitting answer to this conjecture: the most sensible thing that a scribe would do in that case is to transfer this reference to the piercing of Jesus' side to a position at the close of v.50 or 54 or even 56<sup>19</sup>. Indeed a scribe is least likely to have left out such an important detail pertaining to the account of the Lord's death which has had deep spiritual meaning for the believers. A scribe who was intellectually capable of detecting the conflict with Jn could be naturally expected to resort to the former expedient rather than the latter. The awkward positioning of the interpolation is readily explicable as due to a careless scribe's hasty decision<sup>20</sup>. Plummer observes that he must have been tempted to draw a comparison between the εἰς ἔξ αὐτῶν (Mt 27:48) and εἰς τῶν στρατιωτῶν (Jn 19:34)<sup>21</sup>. But I would prefer to ascribe the actual insertion into the text as the second stage in the origin of the passage in Mt, the first stage being the appearance in the form of a marginal note (vide supra).

#### Intrinsic Probability

If we can trust the avowed eye-witness account of the Johannine record (19:34), the position of the verse in Mt cannot be justified. Snodgrass

asks whether an interpolator would have done this kind of "sloppy work"<sup>22</sup>. We should, however, be justified in making the same question applicable to the author who manifests a remarkable editorial skill in his work. To attribute a patent sign of carelessness to a scribe is more logical than to attribute it to such an intelligent person as Matthew.

The First Gospel bears ample evidence of the author's predilection towards the use of O.T. quotations in connection with solemn or memorable occasions in the life of Jesus. It seems unnatural to assume that the Scriptural allusions from Ps 34:20 and Zech 12:10 found at Jn 19:36,37 would have escaped the recollection of Matthew if he had access to the Johannine tradition with regard to the piercing<sup>23</sup>. In my judgment this is the most valid argument against the authenticity of this reference in Mt. Therefore Streeter is quite justified in comparing this passage with the other spurious additions in certain MSS such as the ones at 3:16 ( X B etc: Light at the Baptism) and at 20:28 (D f<sup>13</sup> Syr<sup>c</sup> etc: 'seek ye to rise...', par. Lk 16:28)<sup>24</sup>.

### Conclusion

Although we had noted at the beginning that the presence of this verse in the Alexandrian MSS could possibly lead to another conclusion, our considerations of the Transcriptional and Intrinsic Probabilities would now produce the inescapable verdict that it is no part of the original work of Matthew. Lagrange is confident about the short reading here and lists this insertion here under "Les fautes du type B"<sup>25</sup>. Alford, in rejecting the evidence of B  $\lambda$  etc., points to the fact that Syr<sup>pal</sup> has this portion containing the account of Jesus' death "twice among the lections of which it consists, one time omitting, and the other inserting, the doubtful words"<sup>26</sup>. This is yet another evidence for doubting the authenticity of this passage here.

<sup>1</sup>Introduction, II<sup>4</sup>, p.302.

<sup>2</sup>Cf. J. Weiss, *Schriften des NT*, p.369: "Es ist dies einer der zahlreichen Fälle in denen die handschriftliche Überlieferung die Texte der Evangelien an einander angeglichen hat".

<sup>3</sup>Einführung<sup>2</sup>, pp.190f.

<sup>4</sup>JTS 33 (1922), p.364.

<sup>5</sup>Hort, *Notes*<sup>2</sup>, p.22; cf. Streeter, *Four Gospels*, p.122, n.2. He infers from Lucian's omission of this passage as well as the insertion at Mt 20:28f (found in D) that they were not known to Lucian.

<sup>6</sup>Quoted by Plummer, *St. Matthew*, p.400; cf. B. Weiss, *Meyer's Kommentar*<sup>8</sup> p.484, n. ; Klostermann, *Matthäusevangelium*, ad.loc.,

<sup>7</sup>*St. Matthew*, ad.loc.; cf. B.T.D. Smith, *St. Matthew*, ad.loc.; Allen, *St. Matthew*, ICC, ad.loc.

<sup>8</sup>Textual Commentary, p.71.

<sup>9</sup>Loc.cit.

<sup>10</sup>Cf. Salmon, *Historical Introduction*, p.86, n: "It seems to me that this is not a sufficient reason for discrediting the text" (cf. *ibid.*, p.119, n). But Plummer, *loc.cit.*, quotes Salmon (*Human Element*, p.524) in rejecting the authenticity of the verse: "If the passage had ever been in the genuine text of the First Gospel, it could never have been eliminated, so as to leave so little trace of its existence". This change of position only goes to show how subjective one's judgment in such matters could be!

<sup>11</sup>*Early Versions*, p.16 and n.1; cf. Snodgrass, *JBL* 91 (1972), p.377.

<sup>12</sup>*Notes*<sup>2</sup>, pp.21f, Cf. J.R. Harris, *Diatessaron*, p.16.

- <sup>13</sup>It is true that the Syriac Version of the Diatessaron contains the passage in Mt.
- <sup>14</sup>Plummer, loc.cit., rejects Tatianic influence behind the insertion on account of intrinsic considerations (vide infra).
- <sup>15</sup>It must be noted that even von Soden who has a propensity for alleging Tatianic influence in most cases of insertion, is prepared to ascribe the present Alexandrian reading to the Harmony of Ammonius! (Schriften des neuen Testaments, I.2, p.1570.)
- <sup>16</sup>Loc.cit., Harris, however, infers from this quotation that the question as to whether Jesus was dead or alive at the time of the piercing occupied Ephrem's mind. This seems to me rather far-fetched.
- <sup>17</sup>Johannine Vocabulary, pp.272-3, §1756; cf. Kilpatrick, Essays in Honour of G.H.C. MacGregor, p.193.
- <sup>18</sup>Snodgrass, loc.cit.
- <sup>19</sup>Textual Commentary, p.71.
- <sup>20</sup>Cf. Abbott, loc.cit.
- <sup>21</sup>Loc.cit; Cf. McNeile, loc.cit. I should even think that he must have confused the &lambda;λ(α) in Jn with &lambda;λος.
- <sup>22</sup>Loc.cit.
- <sup>23</sup>So Plummer, loc.cit.
- <sup>24</sup>Four Gospels, p.122.
- <sup>25</sup>Critique Textuelle, II, pp.71,100-1; cf. RB (n.s.) 10 (1913), p.504.
- <sup>26</sup>Loc.cit.

Category II : THE 'OTHER' WESTERN NON-INTERPOLATIONS'  
OF WESTCOTT-HORT

A. The Matthaean 'Non-interpolations'

a. Mt 6:15: 'their trespasses'

Disputed words: [τὰ παραπτώματα αὐτῶν]

External Evidence:

ἀνθρώποις & D f<sup>1</sup> 892<sup>xt</sup> it<sup>a</sup>.aur.c.θ<sup>1</sup>.k<sup>1</sup>.h.k<sup>1</sup> vg syr<sup>p</sup> cop<sup>b,om</sup>.syr  
Diatessaron Eusebius Augustine // ἀνθρώποις τὰ παραπτώματα αὐτῶν  
B K L W Δ Θ Π f<sup>13</sup> 28 33 565 700 892<sup>ms</sup> 1009 1010 1071 1079 1195 1216 1230  
1241 1242 1253 1365 1546 1646 2148 2174 Byz Lect (it<sup>b</sup> omit αὐτῶν) it<sup>f,a</sup>  
syr<sup>c,b,pal</sup> cop<sup>sa,bo</sup>ms<sup>ss</sup> goth arm eth geo Ps-Chrysostom

Observations

(i) This omission is not confined to the 'Western' family alone, and as such it does not deserve to be labelled 'Western non-interpolation' in the strict sense.

(ii) The vast majority of witnesses headed by B, and including three Old Latins, part of the Old Syriac and all the other ancient Versions, retain the above words.

(iii) The Old Latins are divided; the majority of them belonging to both the African and the European traditions exhibit the short text in the company of such authorities as Eusebius and Augustine. The attestation of the Versions, however, need not be a reliable guide in this instance because it always

lies within the freedom of a translator to omit phrases which could be dispensed with without sacrificing the meaning of the original.

(iv) What may be considered as scribal interference is already evident in certain Old Latin witnesses which omit *τοῖς ἀνθρώποις* <sup>1</sup>.

(v) This is an instance where *N* deserts B to join D and its allies, a situation where the testimony of the 'Western' witnesses is viewed with suspicion by experts<sup>2</sup>. But our judgment need not be prejudiced by this, as one of the premises of this thesis is that each passage must be considered on its merits (cf. on 24:51).

### Internal Evidence

#### Transcriptional Probability

##### (i) Possibilities of Insertion

(a) Is this a case of intrusion from Mk brought about by a copyist "to produce a balanced liturgical style", as Metzger suggests?<sup>3</sup> In support of his theory he points out that at Mk 11:25, on the same theme of prayer as here, some witnesses have a parallel statement apparently copied from Mt, but not inclusive of the words bracketed by Westcott-Hort.

This point is worth considering. But we notice that the addition at Mk 11:25 is not identical with Mt 6:15 even in other respects. So it is difficult to decide whether that insertion goes back to Mt or to a piece of floating tradition. It is universally acknowledged that what is known as the Sermon on the Mount is only a collection of the teachings of Jesus given on various occasions. Matthew and Luke have, of course, assembled them in two different ways. It is also an axiom of Textual Criticism that many of the sayings of Jesus were transmitted orally as isolated pericopes and that the Gospel records of these sayings often represent the verbal formulations which they had assumed according to the Sitz im Leben of the various local congregations. So the scribal addition at Mk 11:25 may well go back to a dominical teaching that was widely known in the circle in which that particular scribe flourished.

(b) Since the textual evidence for Mk 11:26 is so weak as to justify its elimination by the UBS Editorial Committee, we need not debate the authenticity of that reading, despite its appearance in the TR. So we may safely rule out any possibility that the Matthaean reading was derived from Mark<sup>4</sup>.

This is all the more true when we remember that the problematic Mk 11:26 is found in the TR and allies in a context which has nothing to do with forgiveness! (Mt 21:20-22 par. Mk 11:20ff).

(c) Could the present reading be an assimilation from Mt 6:14? Cf. Lagrange: "L'addition a voulu répéter servilement la tournure du v.14"<sup>5</sup>.

Although this view has been rejected by Allen<sup>6</sup> who regards the present verse as well as 18:35 to have been derived from the "Logia", it must be pointed out that the origin of these verses need not be held as an argument against deleting the disputed words because they have only to do with refinement of style. But this point will have to be deferred for consideration under Intrinsic Probability.

#### Possibility of Omission

The only plausible way in which a deletion of these words could be conceived is in terms of their redundancy. For, although both v.14 and v.15 are made up of the very elements that could cause an error due to homoeoteuton, the nature of the lacuna in this instance apparently suggests that somehow these verses have been spared of that kind of error. Redundancy, of course, has to do with style, and an

omission due to this cause could be conceded only in so far as we can permit this in an author like Matthew. That style is often a matter of opinion can be seen from Lagrange's vacillation here. In his Critique Textuelle, II, p.70, he had listed this passage among readings where the omission is certainly or almost certainly authentic. But in his Commentary on Matthew (ad.loc.) he says: "Il est sûrement plus élégant que le mot ne paraisse que deux fois, une fois envers les hommes et une fois envers Dieu; cela suffit pour le parallélisme". But he goes on to state, "Peut-être cependant est-ce par élégance qu'on a supprimé après  $\alpha\nu\theta\rho\nu\pi\omicron\iota\varsigma$  :

$\tau\alpha\ \pi\alpha\rho\alpha\pi\tau\omega\rho\mu\alpha\tau\alpha\ \alpha\upsilon\tau\omega\nu$  ".

These statements clearly show that Lagrange does not have a fixed opinion about either the authenticity of the short reading or the question relating to style.

#### Intrinsic Probability

So the crux of the matter is whether such repetitions conform to the author's style or not<sup>7</sup>.

That the Sermon on the Mount follows a rhythmic pattern in many places is evident to all (cf. ch.13).

Metzger is right in pointing to the chiastic configuration of vv.14 and 15, their structure being of the order a/b.....b/a:<sup>8</sup>

- ἀφῆτε . . . . τὰ παραπτώματα αὐτῶν (a)  
 ἀφήσει . . . . (b)  
 μή ἀφῆτε . . . . [           ?           ] (b)  
 ἀφήσει . . . . τὰ παραπτώματα ὑμῶν (a)

The retention of the disputed words would really disturb this chiasmus<sup>9</sup>. It may well be that as an ancient oriental teacher Jesus did employ memorable aphorisms which have a structural identity and compactness of their own especially in Aramaic<sup>10</sup>. Thus stylistic considerations certainly justify the deletion rather than the addition in respect of this text.

If this argument is valid, then the most natural place to look for as a possible source of the extra words is v.14 itself.

### Conclusion

For the majority of scholars this passage does not constitute any textual difficulty, and hence their silence on this point in their Commentaries. On the whole, they support the short text. The line of argument followed here will receive further confirmation as we consider 6:25 and 23:26 which also present stylistic problems.

<sup>1</sup>Bengel, Gnomon (E.T.), I, p.197.

<sup>2</sup>Cf. Warfield, Textual Criticism, p.133: "...ND is everywhere, and in every combination (if B be absent), very suspicious".

<sup>3</sup>Textual Commentary, p.17; cf. B.T.D. Smith, St. Matthew, ad.loc.; Allen, St. Matthew, ad.loc.

<sup>4</sup>Burkitt, Gospel History and Its Transmission, p.162: "It is hardly likely that these verses from the Sermon on the Mount were derived from Mk".

<sup>5</sup>Critique Textuelle, II, p.70; cf. p.95.

<sup>6</sup>Loc.cit.

<sup>7</sup>J.C. Fenton, StEv 1(=TU 73,1959), pp.174-79, has called attention to two types of stylistic devices consciously adopted by Matthew: (1) Inclusio: the repetition of more or less the same words from the beginning of a paragraph at the end of the paragraph. E.g. Mt 6:19/20; 7:16/20; 18:1/4; 19:30/20:16; cf. 1:1/17. (See also B.C. Butler, Originality of Matthew, pp.150f; Lagrange, S. Matthieu, gives a long list of such examples, many of which are far-fetched.) (2) Chiasmus: two sets of ideas, words, lines or even paragraphs are arranged first in one order and then in the reverse order: (a) single words: e.g. 10:28; 11:4,5; 12:22; 12:49f; 17:25; (b) phrases or lines: e.g. 3:8-10; 5:43-47; 6:9-13; 7:6; 9:13; 9:15; 11:17-19; 11:29f; 13:13,14; 13:15; 23:25f; 25:31-46; (c) verses or paragraphs or groups of paragraphs: e.g. 23:16-22 (v.16, 'if anyone swears by the temple...', v.18, 'if anyone swears by the altar...', / v.20 'he who swears by the altar...', v.21, 'he who swears by the temple...'); 1:1,2 (chiastic arrangement of Jesus Christ, David, Abraham); 13:37-50 (chiasmus around the parables on : tares, treasure/pearl, net); 23:16,17/23:18,19 (besides the chiasmus mentioned above, there two such arrangements here based on the themes: temple, gold / gold, temple, and altar, gift / gift, altar). Fenton detects such arrangements in many other parts of the Gospel; but admittedly, "these are natural and necessitated by context" [cf. T.W. Manson's Review of 'Chiasmus in the New Testament' by N.W. Lund in JTS 45 (1944), pp.81ff.]. But the point stressed by all the above scholars about Matthew's propensity for repetition is to be taken note of. Fenton (op.cit., p.179) has rightly observed that

"The recognition of Chiasmus may help in the restoration of the original text of Matthew" in certain disputed readings like 15:30,31, although the present passage has not received his attention.

A.M. Farrer, in St. Matthew and St. Mark, ch.x, tries to show that the bulk of the Sermon on the Mount is of a chiastic follow-up of the Beatitudes. But I am not convinced by his argument.

<sup>8</sup>Loc.cit.

<sup>9</sup>Snodgrass, JBL 91 (1972), p.376, in defence of the disputed words, says that "if we leave out the words chiasmus is the result" (!) In fact it is the chiasmus that justifies their deletion.

<sup>10</sup>Cf. Clem. Rom. xiii,2: ἀφίετε ἵνα ἀφεθῆ ὑμῖν ;  
Ep. Polyc. ii,3: ἀφίετε καὶ ἀφεθήσεται ὑμῖν .

II.A.

b. Mt 6:25: 'or what you shall drink...'

Disputed words:  $\left[ \overset{\eta}{\eta} \tau\acute{\iota} \pi\acute{\iota}\eta\tau\epsilon \right]$

External Evidence:

$\overset{\eta}{\eta} \tau\acute{\iota} \pi\acute{\iota}\eta\tau\epsilon$  B W f<sup>13</sup> 33 1230 it<sup>aur.e.f.g.l.h.q</sup> cop<sup>sa</sup> mss. bo arm eth<sup>ms?</sup>  
geo<sup>1</sup> (geo<sup>B</sup> omit  $\tau\acute{\iota}$ ) Origen (Eusebius) Athanasius mss<sup>acc. to Jerome</sup> Marcus  
Maximus-Confessor //  $\kappa\alpha\iota \tau\acute{\iota} \pi\acute{\iota}\eta\tau\epsilon$  K L  $\Delta$   $\Theta$   $\Pi$  (28 1071  $\pi\acute{\iota}\eta\tau\epsilon$ ) 565 700  
1009 1010 1079 1195 1216 1241 1242 1253 1365 1546 1616 (2148  $\pi\acute{\iota}\eta\tau\alpha\iota$  [=  $\pi\acute{\iota}\eta\tau\epsilon$ ])  
2174 Byz Lect  $\Gamma$ <sup>6ms</sup> syr<sup>p.h</sup> goth eth<sup>ms?</sup> geo<sup>A</sup> Basil // omit  $\kappa$  f<sup>1</sup> 892 it<sup>a.b.n<sup>1</sup>.k.l</sup>  
vg syr<sup>6.pal</sup> cop<sup>sa</sup> mss eth<sup>ro.pp</sup> Justin Clement Origen Adamantius Methodius  
Hilary Athanasius Basil Epiphanius Chrysostom Augustine (Cyril)

Observations

(i) Here also the category of 'Western non-interpolations' is not strictly applicable.

(ii) If we ignore the minor stylistic variations between  $\overset{\eta}{\eta}$  and  $\kappa\alpha\iota$ , the vast majority of authorities including Bwf<sup>13</sup> 33 Origen et al support the long reading.

(iii) Codex Bezae is not extant here, but significantly enough, b k do testify to the omission.

(iv) Neither the divided testimony of the Old Latins, nor the support of the other Versions for the omission need be held as a weighty evidence because, as was mentioned in connexion with the previous passage, it should not be surprising if a sensible translator felt free to omit a phrase

which in his judgment was otiose.

(v) Once again, the support for the 'Western' omission from  $\aleph$  would be regarded by experts as a clue to its spuriousness (cf. on 6:15), but our decision will have to await until the Internal Evidence is also examined because the External Evidence shows a cleavage among the individual members of each family.

#### Internal Evidence

##### Transcriptional Probability

###### (i) Possibilities of Insertion

(a) I doubt if the variation between  $\eta'$  and  $\kappa\delta\acute{\iota}$  read at this point by diverse authorities could be adduced as a valid argument to point to the extraneous nature of the bracketed words as a whole, as Metzger does<sup>1</sup>, because the exchange of these conjunctions would well fall within the freedom that many a copyist or translator would claim for himself. He would especially be tempted by the occurrence of the  $\eta'$  in a similar context in v.31 if that conjunction was not already there in v.24. So this does not conclusively prove that the rest of the phrase was originally missing in v.25.

(b) That brings us to a second possibility for the appearance of these disputed words in this context. As we have observed in the case of the previous example, the presence of a particular phrase in the immediate vicinity is a temptation for scribes to assimilate them in a similar context. Since v.31 below provides the combination of the forms of  $\phi\alpha\gamma\epsilon\acute{\iota}\nu$  and  $\pi\acute{\iota}\epsilon\acute{\iota}\nu$  it was natural for a scribe to adopt that combination in v.25 also. We shall see more of this in our discussion of the Intrinsic Probability.

(ii) Possibilities of Omission

(a) The proximity of  $\phi\alpha\gamma\eta\tau\epsilon$  to  $\pi\acute{\iota}\eta\tau\epsilon$  might have occasioned an oversight on the part of the scribe, thus leaving out the three words now bracketed, the termination  $-\eta\tau\epsilon$  being common to both<sup>2</sup>.

This possibility is indeed very real, judging from what we gather from other instances of omission of a similar nature. But considerations of Intrinsic Probability would suggest that this is not likely to be the cause of the omission.

(b) Is this a case of omission in the interest of assimilation to the corresponding Lukan passage, 12:22; cf. B. Weiss: "...es ist nach  $\phi\alpha\gamma\eta\tau\epsilon$  zufällig oder nach Luk ausgefallen"<sup>3</sup>.

This possibility has to be ruled out on two counts: First, these two Synoptic parallels are not identical in every respect. So it is least likely that such an attempt at harmonization would be limited to a part of a pericope; nor do we find a satisfactory explanation as to why such harmonization should be a one-way traffic!<sup>4</sup> Secondly, as Lagrange rightly observes, the harmonizing tendency of scribes was usually in the direction of augmentation rather than of deletion<sup>5</sup>.

(c) According to Snodgrass, the superfluous nature of the words might have led to their deletion<sup>6</sup>.

This would well be the case if the long reading was original. So like the first suggestion, this possibility should remain open until we are satisfied whether it is genuine or not.

#### Intrinsic Probability

(i) There is a real parallelism involved in the present context as in the case of so many other parts of the Sermon on the Mount: thus τῇ ψυχῇ corresponds to τί φάγητε, while τῷ σώματι corresponds to τί ἐνδύσασθε. In aphoristic teachings of this kind such parallelisms facilitate memorization; and so a wise teacher like Jesus is

not likely to clutter his sayings with anything more than what is absolutely essential. Brevity is indeed the essence of sound teaching method. So Klostermann's observation, " ἡ τί πιήτε nach 31 = Lc.XII:29, aber es ist hier überflüssig und stört den Parallelismus"<sup>7</sup>, is quite valid (cf. on 6:15).

(ii) Further, the passage corresponding to Mt 6:25, namely Lk 12:22, does not have a reference to drinking, whereas Lk 12:29 (which corresponds to Mt 6:31) has such a reference. Therefore it seems to me that *πιήτε* is really superfluous at Mt 6:25: *φάγητε* (in the generic sense) covers the aspect of drinking as well, especially in view of 25b, where the word *τροφή* is used generically.

(iii) Bengel rightly observes that whereas in v.31 "the careful are introduced as themselves speaking", in v.25 "our Lord speaks in His own person". So in v.31 the express mention of 'drinking' is a necessity; but here in v.25 it is not essential<sup>8</sup>.

### Conclusion

Thus we find that it is easier to explain the addition to a short text than the deletion from

a genuine text. So Metzger is right in suggesting that v.31 (which is genuine) is the real source of the addition (rather than Lk 12:22 or 29) where  $\tau\acute{\iota} \pi\acute{\iota}\omega\mu\epsilon\gamma$  is not at all superfluous<sup>9</sup>.

Lagrange notes that in Jerome's Commentary on Matthew, the comment on this passage is such as to impute spuriousness to the words in question<sup>10</sup>.

The silent manner in which most commentators of the present day eliminate them may be considered as confirmation of our conclusion. Westcott-Hort's suspicion of these words is thus justified, and both the UBS edition of the NT and the NEB have done well in deleting them.

<sup>1</sup>Textual Commentary, p.17.

<sup>2</sup>Metzger, Textual Commentary, loc.cit., cf. Alford Greek N.T. I<sup>5</sup>, ad.loc.

<sup>3</sup>Meyer's Kommentar<sup>8</sup>, Matthäusevangelium, p.144.

<sup>4</sup>F.C. Grant JBL 59 (1940), p.15, points out that in view of the extreme popularity of Mt vis-à-vis the other Gospels in the early centuries, scribal tendency used to be more in the direction of assimilation to Mt.

<sup>5</sup>Critique Textuelle, II, p.71.

<sup>6</sup>JBL 91 (1972), p.376.

<sup>7</sup>Matthäusevangelium, ad.loc., cf. MacNiele, St. Matthew, ad.loc.

<sup>8</sup>Gnomon, I (E.T.), pp.197-8.

<sup>9</sup>Loc.cit.

<sup>10</sup>Critique Textuelle, II, p.289: "Dans quatre cas, Jerome constate l'accord, dont il se felicite, ou bien ouvertement ou bien en ne citant que peu d'autorites en sens contraire". As example he cites the omission at 6:25 of neque quid bibatis; cf. Bengel, op.cit., p.198, n.1.

II.A.

c. Mt. 9:34: '...by the prince of demons'

Disputed words: [οἱ δὲ φαρισαῖοι ἔλεγον, Ἐν τῷ  
ἄρχοντι τῶν δαιμονίων ἐκβάλλει τὰ δαιμόνια]

External Evidence:

include verse 34 N B C K L W X Δ Θ Π f<sup>1</sup> f<sup>13</sup> 28 33 565 700 892  
1009 1010 1071 1079 1195 1216 1230 1242 1253 1344 1365 1546 1646 2148 2174  
Byz Lect it<sup>aur.</sup> b. c. f. g<sup>1</sup>. h. l. a vg syr<sup>p.</sup> h. pal cop<sup>sa.</sup> bo goth arm eth geo // omit  
verse 34 D it<sup>a.</sup> d. k syr<sup>a</sup> Diatessaron Juvencus Hilary

Observations

- (i) Here ζBCLW @ f<sup>1</sup>f<sup>13</sup> 28 33 565 700 etc.  
are followed by the majority of the Old Latins and  
the Vulgate as well as the other Versions in defend-  
ing the verse.
- (ii) The Latin and Greek sides of Codex Bezae  
as well as a k and Syr<sup>S</sup> give evidence of the short  
text. Although the Diatessaron omits the verse in  
this context, that need not be taken as a helpful  
guide because of the occurrence of the same in  
Mt 12:24<sup>1</sup>. However, the support given by Syr<sup>S</sup> for  
the deletion of the verse is significant enough.
- (iii) What little patristic evidence we have is  
in favour of the short text.

(iv) Plummer points to the variations in the wording of this verse in different MSS which throw some doubt on its authenticity<sup>2</sup>.

External Evidence alone is thus quite indecisive. Streeter's statement that "it is a 'Western non-interpolation' with more than ordinary good MS support"<sup>3</sup> is therefore rather exaggerated.

### Internal Evidence

#### Transcriptional Probability

##### (i) Possibility of Insertion

Many scholars think that if we maintain the extraneous origin of this verse its insertion can be explained in terms of assimilation to Mt 12:24 (so J. Weiss<sup>4</sup>, Klostermann, Plummer, Kilpatrick<sup>5</sup>, Allen, McNeile) or to Lk 11:15 (so Wellhausen, Filson) or to Mk 3:22 (so Zahn<sup>6</sup>, Streeter<sup>7</sup>, Levertroffe & Goudge<sup>8</sup>, Tasker<sup>9</sup>, Smith).

Despite this impressive list, it is difficult to accept this view at its face value because none of these scholars has tried to explain why the term *Βεελζεβούλ* which is present in all the parallel texts mentioned above is missing here. We will soon discover that at least the third possibility has to be completely ruled out.

(ii) Possibilities of Omission

(a) A.C. Clark has expressed his surprise at the separate ways in which Westcott-Hort have treated 27:49b and 9:34 by attaching different degrees of suspicion to each<sup>10</sup>. According to him both had possibly 6 lines of 60 letters in an exemplar. Thus he seems to attribute the negligence of scribes as the contributory factor in the omission of both passages.

But this is a far-fetched attempt to tie down every possible omission to his stichometric calculation. He does not in fact explain in detail how an accidental omission due to oversight of this magnitude could conceivably happen. Even if one may not agree with Westcott-Hort's conclusions, one has to concede that their judgment was based on the insight that if the above-cited texts were original, each of these omissions was due to a different cause.

(b) Since an omission due to the usual causes such as oversight or offence is unthinkable here, the only plausible explanation that can be given is that a scribe must have felt the verse to be unnecessary here in view of the occurrence of essentially the same statement in 12:24<sup>11</sup>.

However, one has to remember that doublets are not strange to the First Gospel: cf. 19:30 and 20:16; 8:12 and 13:42,50, 22:13, 24:51, 25:30; 13:12 and 25:29. So the argument of superfluity is not a valid one in explaining the omission. In fact Alford regards 9:34 as authentic as 12:24 in view of the habit of the Pharisees to ascribe Jesus' miracles of exorcism to the power of Beelzebul<sup>12</sup>. Similarly Lagrange attributes the omission to a desire to distinguish this incident from the one in Mt ch.12<sup>13</sup>.

(c) Commenting on Tasker's view of the spuriousness of the passage, Snodgrass writes: "Not only is the external evidence...decisively against this, but the internal evidence as well makes it unlikely. There is nothing gained by the addition of this verse, but it is easy to find a plausible motive for its omission. The statement plays no importance in this context, and the issue is dropped thus leaving the charge unanswered. In Mt xii 24-25; Mk iii 22-23; Lk xi 14-15, the logic of the charge is refuted"<sup>14</sup>.

It is a pity that Snodgrass cannot see the logical conclusion of his own premise. It is precisely by virtue of this kind of reasoning that we become suspicious of the authenticity of the verse. Why should Matthew introduce such a statement at 9:34

if the issue was to be dropped so abruptly and if the charge was not going to be refuted? We shall discuss more of this shortly.

From our discussion so far, it is clear that the addition is more easily explained than the omission, the difficulty with regard to the absence of the term Beelzebul notwithstanding.

#### Intrinsic Probability

(i) Those who defend the genuineness of the verse regard Mt 9:32ff (healing of a dumb demoniac) as a doublet of 12:22-24 (healing of a blind and dumb demoniac). Since Matthew is fond of doublets involving not only the utterances of our Lord (vide supra), but also such miracles as in ch.9 (9:27-31, cf. 20:29-34, healing of two blind men), they argue that the healing miracle at 19:32ff allows for the present verse (cf. Mackenzie, Jerome Commentary, ad loc.).

The difficulties arising from this comparison, however, cannot be ignored<sup>15</sup>. In ch.12 the beneficiary of the healing miracle is a blind and possessed man; here it is a mute and possessed man; there Jesus is reported as healing the man; here He 'casts out' the demon. Further, the reaction of the crowds is different in both places. In ch.12 they wonder if Jesus could perhaps be the Son of David; in ch.9 they simply 'marvel'. Thus

the real points of similarity between the two episodes are the blindness of the man and the accusation of the Pharisees (with the significant lacuna concerning the name of the 'prince of demons' at 9:34).

Judging from these differences in detail, we are to infer that 9:34 is a hurriedly assimilated statement in this context.

(ii) The charge that Jesus was in league with Beelzebul is the occasion of a long controversy in 12:25-27. But in the present context there is no such controversy. The close of neither of the other two miracles at 9:18ff and 9:27ff leads on to a confrontation with the Pharisees. It is therefore doubtful whether Matthew had any intention of presenting the third miracle at 9:32f in the setting of such a conflict. The motive of the author in the present section is quite different. Here we have three miracles involving death, blindness and dumbness. The author is presenting us with a comprehensive summary of the saving power of Jesus as the Messiah (cf. Jesus' reply to the messengers of John the Baptist 11:4,5)<sup>16</sup>. The fact that Matthew adds the kind of detail in this section that he has omitted elsewhere while copying Mk is adequate proof of this<sup>17</sup>:

e.g. the compassion of Jesus, 9:36; Mk 6:34 cf. Mt 14:14. The predominant note of this chapter is the rise of the messianic expectations among the populace (9:8,26, 31,33) as against the blind criticism of the religious leaders (9:3,11). But as far as the closing part of the chapter is concerned, Matthew's intention is to present Jesus against the background of His popularity. The section vv.35-38 depicting Him as an itinerant preacher of the Messianic Kingdom and as a miracle worker full of compassion for the physical as well as spiritual needs of the masses is not only a fitting conclusion to the chapter, but also a commentary on His public ministry as a whole. So v.34 appears to be quite out of place in this chapter.

It is true that, following Mark, Matthew also traces the series of events dealing with Jesus' confrontation with the Jewish leaders which culminated in the Cross. (Cf. on Mk 10:2). But unlike Mark, he does not give preponderance to this motif so early in Jesus' ministry. Thus the narratives relating to the healing of the paralytic and the call of Matthew the tax-collector where the Jewish leaders figure as the opponents of Jesus are so presented as to draw the contrast between His popularity (vv.8,10) and their blind fanaticism. The question about fasting, according to our author,

is raised by the followers of the Baptist; the Pharisees are referred to only incidentally. The fact that Matthew ends this pericope on the note: '...and so both are preserved', shows that Matthew did not subscribe to the view that the new way of life introduced by Jesus would be a threat to the old (vide on Mk 2:22; cf. Mt 5:17). Therefore in ch.9 there is no question of a full-fledged confrontation between Jesus and His opponents in relation to the scheme of the First Evangelist.

Thus it follows that conceding Matthew's fondness for doublets would in no way justify the retention of v.34 in its present context. Bonnard's words help to sum up the foregoing conclusion. Comparing the contexts of the healing of the possessed man in ch.9 and ch.12 he succinctly asserts: "Les contextes didactiques des deux chapitres sont d'ailleurs très différents: au chap. XII, il s'agit d'une série de conflits des Jésus avec les Pharisiens introduisant la troisième grande instruction sur le mystère du Règne (chap. XIII); ici, il s'agit d'un group de guérisons rapides illustrant l'autorité de Jésus et introduisant l'instruction du chap.X aux hérauts du Règne"<sup>18</sup>.

(iii) Holtzmann thinks that the allusion in 10:25b to a past accusation by the Pharisees makes it absolutely essential that v.34 should be retained in ch.9<sup>19</sup>.

This view depends upon the assumption that ch.9 is chronologically anterior to ch.10. But if we accept Zahn's position, this assumption will be proved to be baseless. According to him, there is no chronological connection either between chaps.9 and 10 or chaps. 11 and 12; but without substantiating his statement he goes on to make certain conclusions based on that premise. We shall consider his approach to the present passage shortly. But in this section we shall confine our attention to defending Zahn on the question of the chronological link between chaps. 9 and 10<sup>20</sup>.

An examination of a good Synopsis of the Gospels would provide the following tabulation in respect of Mt chs. 8-12.

MATTHEW	MARK	LUKE
8:18-22	-	9:57-62
8:23-27	4:35-41	8:22-25
8:28-34	5:1-20	8:26-39
9:1-8	2:1-12	5:17-26
9:9-13	2:13-17	5:27-32
9:14-17	2:18-22	5:33-39
9:18-26	5:21-43	8:40-56
9:27-31	-	-
9:32f	-	-
9:35-38	-	-
10:1-4	3:13-19	6:12-16
10:5-15	6:7-13	9:1-6

<u>MATTHEW</u>	<u>MARK</u>	<u>LUKE</u>
10:16-25	13:9-13	12:12-17
10:26-31	-	12:2-7
10:32-33	-	12:8-9
10:34-39	-	12:51-53; 14:26-27
10:40-42	9:41	-
11:1-19	-	7:18-35
11:20-24	-	10:13-15
11:25-30	-	10:21-22
12:1-8	2:23-28	6:1-5
12:9-14	3:1-6	6:6-11
12:22-32	3:20-30	11:14-23

Taking into consideration the Synoptic relationship of Mt ch.10, we notice that it is a hotch-potch chapter consisting of various strata of tradition which bear no chronological sequence to each other. Therefore the context in which the statement of 10:25 is placed by the author does not necessarily imply that it has reference to a chronologically anterior event. In other words, the relative positions of 10:25 and 9:34 in Matthew's Gospel is no guarantee that the alleged events to which they are attached are sequentially irreversible. The pericope 10:24,25 has in fact every appearance of having originally belonged to another context.

We also notice from the above tabulation the correspondence Mt 12:22-32 par. Mk 3:20-30 par. Lk 11:14-23; 12:10. There is, however, no real correspondence between Mt 9:32f and Mk 3:22f. Besides, the wordings of Mt 9:34 and Mk 3:22 are

different in important details, e.g.

Mt: Pharisees: 'chief of the demons'

Mk: Teachers of the Law from Jerusalem:

'Beelzebul'...'chief of the demons'.

So even if Matthew were the author of 9:34, it is obvious that he was dependent on a source other than Mk here. Kilpatrick's statement that the form of 9:34 is much closer to Mk 3:22 than Mt 12:24 is therefore not tenable<sup>21</sup>.

(iv) Zahn's arguments for the elimination of v.34 as spurious may be summarised as follows: Since there is no chronological connection between chaps. 9-10 and 11-12, "it would not be inconceivable that Matthew should, in passing, touch upon a single event as a conclusion of the sketches, viii.18-ix.34, which are in time very closely connected, and then moreover, should once again narrate the same event with more precision and detail (xii.22f) in a connection where it was of importance for describing the conflict with the Pharisees". In Zahn's view the reaction of the people, 9:33, has reference to the series of miracles narrated between 8:18 to 9:32. But the charge of the Pharisees is confined only to the last miracle. "The fact that in ix.32f the author tells about a dumb man, and in xii.22 about

a blind man who is also dumb, gives no warrant for distinguishing the narratives; but just as little also for identifying them. If ix.34 is to be omitted...every reason for this disappears; for the words of the people, ix.33, have nothing in common with xii.23, and do not refer to the one deed of healing last mentioned, but to the entire chain of varied deeds and words in viii.18 - ix.32"<sup>22</sup>.

While I agree with his suggestion for the elimination of v.34, I cannot subscribe to the view that the reaction of the crowd in 9:33 relates to the series of preceding events because the author is careful to record the same kind of popular reaction in connection with every miracle in the above section; cf. 8:27 (8:34); 9:8; 9:26; 9:31. Thus 9:33 appears to have reference only to the miracle at 9:32f from the point of view of the crowd. But from the point of view of the author, it is not the people's reaction at 9:33, but the incident of the healing of the dumb man taken as a unit, which marks the culmination of the foregoing miracles. Thus it is Matthew's mode of recording the miracles (event + people's reaction, without reference to the opponents of Jesus) that justifies our conclusion that v.34 is out of place. Therefore while I accept Zahn's final verdict on this verse, I would very much question the premise on which it is based!

(v) Streeter rightly highlights this verse as "an example of the importance of textual criticism for the synoptic problem". He is also right in concluding that the verse in question does not belong here. But his arguments are not at all convincing. According to him, without v.34, the present story "looks like an abbreviated version of Mk vii.32ff (with the "offending" details excised), transferred after Matthew's manner to another context". He regards this as a "telescoping" of the Markan story in ch.7 coupled with the one in Mk 8:22-26. But actually the basis for this is very shaky. He thinks that 'touched their eyes' Mt 9:29 ( ἤψατο τῶν ὀφθαλμῶν ) may have come from Mk 8:23, although the actual wording there is ἐπιλαβόμενος τῆς χειρὸς τοῦ τυφλοῦ . Again, his statement, "When, however, in copying Mark he actually reached the story of Bartimaeus, Mk x.46ff, [Matthew] preferred to retell it in its original context, but forgot to delete it in the earlier part of the Gospel", seems too far-fetched<sup>23</sup>. Thus in the end Streeter does not prove anything, but only affirms his preference for the omission.

## Conclusion

Apart from a few dissenting voices here and there, the majority of scholars are in favour of eliminating this verse from its present setting. As to its origin, we can only say that it could be a careless and hasty insertion from Mt 12:24 or Lk 11:15 (but not Mk 3:22). As J. Weiss says, "V.34 ist nach wichtigen Zeugen als ein hier völlig unangebrachter Eintrag aus xii.24 zu streichen"<sup>24</sup>. Some scholars do not ascribe historical authenticity to the miracle at 9:32f on the ground that it is an artificial doublet. B.T.D. Smith says: "The cure of the dumb demoniac may be an abbreviated version of Mk.vii.32f; it would appear to have been assimilated to the Q miracle of Lk.xi.14f = Mt.xii.22f. Scribes carried this a step further by the addition of v.34..."<sup>25</sup>. This may well be true in view of the existence of so many doublets in Mt. Its possibility is particularly striking in view of the general scholarly scepticism about the two versions of the Feeding of the Multitude in Mk and Mt. But this point need not be debated here because we are concerned only as to whether the present miracle goes back to our author himself. Kilpatrick's statement that "The verse is not required by the context"<sup>26</sup> well sums up the result of our discussion. He cites this as one of the examples

of harmonisation in the Alexandrian and most  
other witnesses, but absent in D and its allies.<sup>27</sup>

<sup>1</sup>Against von Soden, *Schriften des N.Ts. I*, p.1570.

<sup>2</sup>St. Matthew, *ad.loc.*

<sup>3</sup>Four Gospels, p.170; cf. Metzger, *Textual Commentary*, p.26.

<sup>4</sup>*Schriften des N.T.* p.284. Unless otherwise stated, all the following scholars are cited after their Commentaries on Mt.

<sup>5</sup>Essays in honour of G.H.C. MacGregor, p.193; *JTS* 50 (1949), p.148.

<sup>6</sup>Introduction, II (E.T.), p.598.

<sup>7</sup>*Loc.cit.*

<sup>8</sup>Gore's Commentary, *ad.loc.*

<sup>9</sup>Greek N.T., *ad.loc.*

<sup>10</sup>Primitive Text, p.56.

<sup>11</sup>Cf. Lagrange, *Critique Textuelle*, II, pp.69-70.

<sup>12</sup>Greek N.T. I<sup>5</sup>, *ad.loc.*

<sup>13</sup>*RB* (n.s.) 10 (1913), p.506.

<sup>14</sup>*JBL* 91 (1972), p.376.

<sup>15</sup>Cf. P. Bonnard, *St. Matthieu*, p.138; cf. Wellhausen, *Matthaei*, pp.43f; Contrast Mackenzie, *Jerome Commentary*, *ad.loc.*

<sup>16</sup>Cf. *Jeremias, Parables* (E.T.), p.93, n.17.

<sup>17</sup>Mackenzie, *Jerome Commentary*, *ad.loc.*

<sup>18</sup>*Op.cit.*, p.139.

<sup>19</sup>*Hand-Commentar*, *ad.loc.* cf. Metzger, *Textual Commentary*, p.26.

<sup>20</sup>Loc.cit.

<sup>21</sup>JTS 44 (1943), p.28, Cf. D. Wenham, NTS 21 (1975), p.299.

<sup>22</sup>Introduction, II, (E.T.), pp.598, 600, n.14.

<sup>23</sup>Loc.cit.

<sup>24</sup>Loc.cit.; cf. J.C. Hawkins, Oxford Studies, p.46.  
Contrast Lohmeyer, Meyer's Kommentar<sup>4</sup>, p.181.

<sup>25</sup>B.T.D. Smith, St. Matthew, ad.loc.

<sup>26</sup>JTS 50 (1949), p.148.

<sup>27</sup>Essays in Honour of G.H. C. MacGregor, p.193.

## II.A

d. Mt 13:33: 'Another parable...'

Disputed words: [ἐλάλησεν αὐτοῖς]

External Evidence:

Tischendorf<sup>8</sup>, I

*ελάλησεν*: c 243. *παρεθρηνεν* | *αυτοις* c. BEFGKSVI<sup>8</sup> al pler itP<sup>1</sup>vg  
(exc am) cop syr<sup>utr</sup> ... SCLM<sup>1</sup>UX 13. 124. 126. 243. al<sup>10</sup> fere g<sup>2</sup> h l am  
arm add *λεγων* (al pauc *λεγ.* o *ἰς* vel o *ἰς* *λεγ.*, syr<sup>p</sup> c.\* o *ἰς*) ...  
D (*ἀλλην παραβολην*· nil amplius) 76. k (*alia similitudo*) syr<sup>cu</sup> om<sup>1</sup>  
*ελαλ.* αυτ. | *γωνη*: syr<sup>cu</sup> *femina sapiens* | *ειρηνη*, c. sncd unc<sup>11</sup> et min  
certe pm (Valent ap Ir<sup>28</sup> *την της ζουης παραβολην, ην η γωνη*  
*εγγεγραφηται λεγεται εως αλευρου σατα τρια*) Clem<sup>694</sup> Chr<sup>506</sup> ... GL  
al plus<sup>110</sup> Cyr<sup>4,365</sup> Chr<sup>montf</sup> al *εζουνην* (t<sup>b</sup>) :: cf Le

### Observations

(i) The MS evidence is overwhelmingly in favour of the retention of these words.

(ii) The authorities for the omission are significantly too few - Codex Bezae, Old Syriac and Codex Bobiensis.

(iii) The various attempts to modify the texts, as seen from the MSS, are all apparently based on the long reading. This adds to the authenticity of the latter.

## Internal Evidence

### Transcriptional Probability

According to Snodgrass, the words could have been either omitted as unnecessary, or added to provide a smoother reading<sup>1</sup>.

Both these suggestions are indeed utterly beside the point, for the 'Western' reading presents a non-grammatical half-sentence whose subject is in the accusative case! That could hardly have originated with the author.

The MS evidence suggests that the omitting authorities owe their origin to an ancestor of D. The two words that stand within brackets in the Westcott-Hort text consist of just 14 letters, which would suggest that they constituted a single *στίχος* in the exemplar, and as such they were liable to a slip of the eye. As Lagrange says, "L'omission paraît être une simple erreur du copiste dans le grec...". He draws attention to two important points which lend support to this hypothesis:

(a) In k, the words corresponding to "Ἀλλήν παραβολήν" are not in the accusative (aliam parabolam) as in d, but in the nominative (alia similitudo), which could

be a correction intended to make sense out of the strange reading of the exemplar.

(b) The Syriac, of course, does not distinguish between the 'cases'<sup>2</sup>. Thus one is left with a short reading containing the accusative in  $\bar{D}$  alone whose ancestor must be the originator of the error.

### Intrinsic Probability

In this chapter of parables, it is particularly noteworthy that Matthew follows two fixed formulae to introduce most of the parables:

"Ἀλλήν παραβολὴν παρέθηκεν αὐτοῖς λέγων ...

(vv. 24, 31 cf. 33) and (Ἄλλιν ) ὁμοίᾳ ἐστὶν ...

(vv. 44, 45, 47). One cannot but be intrigued by the

careful balancing of the three parables before and the three parables after the explanation about the

Parable of the Weeds. [Note the refrain, Ἠκούσατε

ὅτι ἔρρέθη ... at 5:21, 27 (cf. 31), 33, 38, 43].

So the disputed words at 13:33 are quite in keeping

with our author's style of maintaining a certain

rhythm; if we leave them out, the result would be a

disruption in the well-balanced introductory formula.

Cf. Snodgrass: "The Western reading is not characteristic of Matthew's style"<sup>3</sup>.

It is instructive to note in this connection that Matthew introduces none of the parables without a verb in the introductory formula. So ἐλάλησεν in v.33 was necessarily designed to correspond to παρέθηκεν in vv.24 and 31. Cf. 24:32: μάθετε τὴν παραβολήν . . . .

Klostermann suggests that the Lukan formula καὶ πάλιν εἶπεν (cf. Lk 13:20) may have been the original formula in Q<sup>4</sup>. This may well be true because Luke often uses an introductory formula similar to it in the parabolic contexts: e.g. 13:8, Ἔλεγεν οὖν . . . ; 15:3,11, εἶπεν δὲ . . . ; 16:1, Ἔλεγεν δὲ . . . etc. We also notice that Luke too, like Matthew, has either παραβολή or a form of ὁμοιώω in many places, though not consistently; e.g. 18:1, ἔλεγεν δὲ παραβολήν αὐτοῖς . . . ; 21:29, καὶ εἶπεν παραβολήν αὐτοῖς . . . ; 13:20, καὶ πάλιν εἶπεν, τίνι ὁμοιώσω . . . . . ; 13:18, ἔλεγεν οὖν, τίνι ὁμοία ἐστὶν . . . . . ; (but 13:6: ἔλεγεν δὲ ταύτην τὴν παραβολήν . . . ; cf. 15:3,11; 16:1).

Therefore it is reasonable to believe that both Mt and Lk owe the introductory formulae of the parables to their common source Q, and that while they have maintained their characteristic styles, they always retain a verb in the introductory part. This insight adds to the strength of a conclusion in favour of the long reading at Mt 13:33.

### Conclusion

Thus both External and Internal Evidences point only in one direction, viz. the retention of the disputed words. The majority of scholars silently confirm our view without bothering to comment on this text. Both the UBS text and the NEB are right in retaining them. This time the suspicion of Westcott-Hort is proved to be baseless.

<sup>1</sup>JBL 91 (1972), p.376.

<sup>2</sup>Critique Textuelle, II, p.70.

<sup>3</sup>Loc.cit.

<sup>4</sup>Matthäusevangelium, ad.loc. On the relative styles of Matthew and Luke in introducing the parables, see J. Jeremias, Parables (E.T.), pp.77ff.

## II.A

e. Mt 21:44: 'And he who falls on this stone...'

Disputed words: [Καὶ ὁ πέσων ἐπὶ τὸν λίθον τοῦτον  
συνθλασθήσεται· ἐφ' ὃν δ' ἄν πέσῃ λικμήσει αὐτόν.]

External Evidence:

*omit verse 44* D 33 it<sup>b,d,e,f,l,2,r</sup> syr<sup>s</sup> Diatessaron<sup>v</sup> Irenaeus<sup>sr,lat</sup>  
Origen Eusebius // *include verse 44* (see Lk 20.18) 8 B C K L W X Δ Π 0138  
(Θ 1079 1546 *omit* καὶ) f<sup>1</sup> f<sup>13</sup> 28 565 700 892 1009 1010 1071 1195 1216 1230 1241  
1242 1253 1344 1365 1646 2148 2174 *Byz Lect* it<sup>aur,c,t,g,l,h,t,q</sup> vg syr<sup>c,p,h</sup> cops<sup>a,b</sup>  
arm eth geo Aphraates Ephraem Chrysostom Augustine Ps-Chrysostom

### Observations

(i) While the majority of the Greek witnesses including BCLW Θ f<sup>1</sup> f<sup>13</sup> 28 565 700 etc. are supported in their attestation of the verse by a good many of the Versions along with the Vulgate, the omission is shown not only by D and the best of the Old Latins, but also by Syr<sup>s</sup>.

(ii) Patristic evidence for the omission is earlier than that for the retention. According to Burkitt, "the Diatessaron retains the verse,...but there is nothing to show that it is not the equivalent of Lk xx.18"<sup>1</sup>. But he is actually referring to the evidence from Moesinger's edition<sup>2</sup> which does not, of course, represent the exact text of the original. In fact elsewhere he cites this passage as an example of the agreement of Syr<sup>s</sup> with the Diatessaron in

omitting a text<sup>3</sup>, which is in keeping with our data above. With regard to the authenticity of the patristic evidence, R.H. Gundry notes: "In view of the omission in the Western text of Mt. in Origen's copious commentary on this passage, and in Irenaeus and Eusebius, who copy verses 33-43 and would hardly have failed to include so striking a concluding sentence, the verse in Mt. may be an interpolation from Lk."<sup>4</sup>

In the light of such rather evenly balanced attestation, a decision is difficult on external grounds alone. Salmon would, however, consider the balance of probabilities to be slightly in favour of the omission: "...though the words are found in the text of St. Matthew, as given by an overwhelming majority of Greek MSS, including all the oldest, yet there is a minority, insignificant in numbers, no doubt, but sufficient to establish the fact that a text from which these words were wanting, early obtained some circulation. And then we must admit it to be possible that the shorter reading represents the original text of St. Matthew, and the longer, one which a very early transcriber had filled up by an addition from St. Luke"<sup>5</sup>.

## Internal Evidence

### Transcriptional Probability

#### (i) Possibility of Insertion

If the reading, v.44, is not original in Mt, there is only one possibility for the words, as Salmon's statement has already indicated: they could have been inserted from Lk 20:18 for the sake of harmonization<sup>6</sup>. The usual objection against this suggestion, as voiced by B. Weiss<sup>7</sup>, Metzger<sup>8</sup> et al, is that both the wording and the position of this verse are different from those in Lk. In Mt its logical position should be at the end of v.42.

With regard to the wording of v.44, Salmon, with justification, has defended his case thus: "We have the very same words in St. Luke, xx.18, with only the exception of *ἐκεῖνον λίθον* for *λίθον τοῦτον* (one may add the *πᾶς* in Lk which Mt lacks). It is certainly not likely that two independent translators from the Aramaic should hit on identical expressions"<sup>9</sup>. The second part of the objection, however, is more serious though not insurmountable, as the Intrinsic Probability will reveal.

The similarity of these parallel pericopes is disrupted only by the additional sentences in Mt 21:43,44. This is so striking that Resse rightly asserts: "Die sachliche Nähe zu sonstigen Ausführungen des Mt. ist kein Argument für die Ursprünglichkeit des Verses, sondern des Grund für seine Einfügung in einem Teil der Überlieferung"<sup>10</sup>. This is not the place to enter into a discussion as to the origin of the present verse. But the above statement has been cited to highlight the alien nature of the verse in its present position.

In contrast to this we may note the observation of B. Weiss: "unzweifelhaft, dass Lk die Parabel in der ihm mit Mt gemeinsamen Quelle (Q) las, und dass dieselbe dort mit diesem Spruch schloss"<sup>11</sup>. These conflicting views with regard to the source of the two parallel readings will be discussed shortly.

(ii) Possibilities of Omission

On the other hand, if the verse was original in Mt, two possibilities exist for its absence from some MSS:

(a) It could be accounted for on the assumption of an accidental by-passing of the  $\alpha\upsilon\tau\eta\acute{s}$  at the end of v.43 and the replacement of it by the  $\alpha\upsilon\tau\acute{o}\nu$  at the end of v.44 (homoeoteleuton)<sup>12</sup>.

(b) It could also be argued that deliberate suppression was resorted to by some scribes on account of the awkward position of the passage<sup>13</sup>. And it is more logical to conceive such an anomalous situation being created by an early scribe than by the author (vide infra).

(c) Von Soden<sup>14</sup> suspects the possibility of omission due to Origen's influence. But according to Tischendorf<sup>15</sup>, Origen does attest both for and against the text.

#### Intrinsic Probability

No doubt the passage Mt 21:43-44 refers back to Dan 2:34-35 and 2:44-45. We note that up to 21:42 Matthew, like Luke, has been following Mk rather faithfully. But at v.43 he introduces extra material and exercises his characteristic freedom in identifying the vineyard with the 'Kingdom of God' using the connecting link 'And so I tell you'. It is intriguing to observe that the author departs here

from his stock phrase 'Kingdom of heaven', thus betraying his indebtedness to Dan. 'He could hardly have used the term "the Kingdom of the heavens", because he everywhere employs this term to signify the Kingdom announced by Christ as coming in the near future. Here the parable necessitates the use of a term to describe some privilege corresponding to the vineyard, already in the possession of the Jewish nation'<sup>16</sup>. But by the introduction of v.43 in between vv.42 and 44 Matthew has disrupted the continuity of thought that binds them together.

Swael, however, argues that there is no awkwardness or discontinuity in respect of the present position of v.44. He thinks that since the general thought of v.43 has reference to the whole of Dan 2:44, it leads naturally into the allusive quotation in v.44. And the use of the words ἀποθήσεται and δοθήσεται (v.43) with reference to the Kingdom was likely to remind one of the Kingdom received by the Saints of the Most High (Dan 7:22) which will not be given away to another nation, and which will last for ever (Dan 2:44)<sup>17</sup>.

There is, of course, no difficulty in conceding this point. But this does not solve our

problem satisfactorily. For v.44, which takes up the theme of the head-stone in v.42, is not following up the point of v.43; and v.45 has reference to v.43 and not to v.44. Thus there is no denying the fact of a break in the logical sequence<sup>18</sup>.

Bonnard tries to circumvent this difficulty by suggesting that the passage may be a commentary by the early Church added to the saying of Jesus. But he does not say whether this was incorporated into Mt by the author himself or by a later scribe. If it is by the author himself, according to Bonnard's own admission, there is a discontinuity of thought with what precedes v.44: "Cette citation déplace l'accent de la parabole, de la vigne ou des vigneronns sur la personne du fils dont rien, jusqu'à la fin du verset précédent, n'annonçait la réapparition en force"<sup>19</sup>.

It is noteworthy that while both Allen and Swaels trace back the earliest roots of the disputed passage to the O.T. sources, their conclusions are mutually conflicting. Allen is right in pointing to the fact that the interpretation of the parable (in terms of the privilege being taken away) is already closed at v.43 and that v.44 appears to be

extraneous matter. He thinks that "it may have been interpolated at a very early date, and may have been read as part of the first Gospel into the third at so early a date that no hint of spuriousness there is given by the extant witnesses to the text of that Gospel"<sup>20</sup>. Swael's conclusion, however, is not at all convincing: "Nous croyons dès lors que l'arrière-fond scriptuaire de Matt. XXI 44 témoigne du lien qui unit dans la pensée du rédacteur Matt. XXI 43 et XXI 44. La critique interne confirme ainsi les données de la critique externe: il faut maintenir l'appartenance de Matt. XXI 44 à la parabole"<sup>21</sup>.

Stendahl's approach to the problem is one of vacillation. In the Peake's Commentary he maintains that the passage is an interpolation<sup>22</sup>. But in 'The School of St. Matthew', he makes a radical departure<sup>23</sup>: "It is true that v.43 breaks in upon what is almost a composite quotation in Luke; but it cannot wholly be ruled out that Matthew knew the tradition of testimonies which we see in more elaborate form in 1 Peter and in Romans". He detects a knowledge of the Hebrew text of the O.T. quotations in question on the part of the author and believes that this Hebrew text had been dove-tailed into an original LXX quotation from Ps 118:22 with the

resultant loss of the second part of that verse. Stendahl, however, offers no solution to the infelicitous nature of the present Matthaean reading as a whole when he says: 'It seems equally possible that we have here a verbum Christi which is enlarged into a compound testimony by being supplemented by other sayings about "the stone" '. Indeed the crux of the textual problem here is whether Matthew would have 'appended' an extract from such a testimony (conceding his knowledge of these testimonies) in a context where it has no connexion with his central theme. This becomes particularly important when we remember that Matthew is one of the N.T. writers who uses O.T. quotations profusely and with good effect. The fact that the proof-text was already associated in Christian circles with the Isaianic and the Daniel passages<sup>24</sup> does not necessarily mean that an author would not use it with discretion. (Cf. Rom 9:33 = Is 28:16; but Rom 10:11 = Is 28:16 (part only); 1 Pet 2:4 = an editorial version of Ps 118:22 + Is 28:6; 2:6 = Is 28:6; 2:7 = Ps 118:22; 2:8 = Is 8:14-15)<sup>25</sup>. These instances are adequate to invalidate the proof-text theory. The divergent treatment meted out to the quotation from Ps 118:22-23 by Matthew, coupled with his introduction of v.43 as a deviation from Lk (and Mk), clearly precludes from

his text the awkward verse 44 which is nothing short of a careless insertion<sup>26</sup> that cannot be attributed to our author<sup>27</sup>.

### Conclusion

Thus v.44 bears all the marks of a gloss which was unwittingly inserted into an inappropriate place by a later scribe. Both B. Weiss's theory of a common source (vide supra) and Ellis' suggestion that this verse may probably go back to a double-tradition ("Q") of the sayings of Jesus<sup>28</sup> lose their validity in the light of the foregoing discussion. Among those who claim v.44 to be spurious may be mentioned such names as J. Weiss<sup>29</sup>, Streeter<sup>30</sup>, Hawkins<sup>31</sup>, Bornkamm<sup>32</sup>, Grundmann<sup>33</sup>, Klostermann<sup>34</sup>, Lohmeyer<sup>35</sup>, Holtz<sup>36</sup>, Kilpatrick<sup>37</sup> and Levertoff<sup>38</sup>. On the other side of the scale may be mentioned B. Weiss<sup>39</sup>, Jülicher<sup>40</sup>, Jeremias<sup>41</sup>, and Brandon<sup>42</sup>. Considering all the probabilities and the weight of scholarly opinion, we conclude that the verse has no claim to originality. Lagrange decides that "Le texte court est certainement authentique"<sup>43</sup>. The UBS Editorial Committee's minute in this regard is worth quoting: "While considering the verse to be an accretion to the text, yet because of the antiquity of the reading and its importance in the textual

tradition, the Committee decided to retain it in the text, enclosed within double square brackets"<sup>44</sup>. While the first part of this scholarly decision is to be welcomed, one doubts the wisdom of the second part. We have indeed nothing to lose by rejecting this passage because the idea is already well embedded in Scripture in contexts where it is not suspect.

<sup>1</sup>Evangelion II, p.238; cf. Lagrange, RB (n.s.) 10 (1913), p.504.

<sup>2</sup>Moesinger, Ephraim, p.193; cf. Lagrange, loc.cit.

<sup>3</sup>Op.cit., p.235; cf. p.232: "That some of these widely spread interpolations were foisted into the text of the Evangelion da-Mepharreshe is evident from Mt. XXI.44 which is read by C, but omitted by S, and from Lk X.41f".

<sup>4</sup>Use of the OT in Matthew, p.84. Later in the book, however, Gundry assumes the originality of the verse in Mt.

<sup>5</sup>Historical Introduction, p.144.

<sup>6</sup>See Lagrange, Critique Textuelle, p.71; St.Matthieu, ad.loc; cf. Holtz, TU 104 (1968), p.162; Gore's Commentary, ad.loc.; Tasker, the Greek N.T. ad.loc.: "a clumsy insertion from Lk xx.18". Allen, St. Matthew ad.loc; McNeile, St. Matthew, ad.loc.

<sup>7</sup>Meyer's Kommentar<sup>8</sup>, Matthäusevangelium, p.363 and note: "Er is aber unzweifelhaft echt; denn wäre er aus Luk eingekommen, so würde er hinter V.42 stehen"; Cf. B.T.D. Smith, St. Matthew, ad.loc., Filson, St. Matthew, ad.loc.

<sup>8</sup>Textual Commentary, p.58.

<sup>9</sup>Loc.cit.

<sup>10</sup>Alttestamentliche Motive, p.171, n.13.

<sup>11</sup>Meyer's Kommentar<sup>9</sup>, Markus u. Lukas, p.602; cf. The Life of Christ(E.T.), iii, pp.244f; 245, n.1. Here Weiss claims that Mt 21:44 originated in Mk 11:10f, from which Matthew copied it along with v.42 "because it corresponded exactly with his didactic point of view to repeat the ruin of the people as being brought about by the hierarchy" (cf. Vol. I, pp.66f, but vide infra).

<sup>12</sup>So Metzger, loc.cit.; cf. Kilpatrick, JTS 50 (1949), p.150.

<sup>13</sup>Gundry, *op.cit.*, p.85: "As it is, v.43 disrupts the natural connection between vv.42 and 44, and this may be the very reason for the omission in the Western Text".

<sup>14</sup>Schriften des N.Ts. I, p.1449.

<sup>15</sup>Novum Testamentum Graece, I, p.139.

<sup>16</sup>Allen, St. Matthew, *ad.loc.*

<sup>17</sup>NTS 6 (1959-60), pp.310-33. Contrast Allen, *loc.cit.*, who suggests that a glossator may have been reminded of Dan 2:44 by the word *ἐβραεῖ* (Mt 21:43).

<sup>18</sup>Cf. Kilpatrick, JTS 50 (1949), p.150; D. Buzy, RB 40 (1931), p.337: "Le v.42 n'est pas la suite littéraire de l'histoire des vigneron. Cette dernière développe en allégorie la métaphore centrale de la vigne affirmée à une première catégorie de fermiers, puis à une deuxième catégorie; tandis que le v.42 développe en allégorie la métaphore de la pierre d'abord rejetée, devenue ensuite tête d'angle". Despite this observation, Buzy is in favour of defending the authenticity of v.44 as a kind of appendix.

<sup>19</sup>Saint Matthieu, p.317.

<sup>20</sup>*Loc.cit.*

<sup>21</sup>*Op cit.*, p.313.

<sup>22</sup>*Ad.loc.*

<sup>23</sup>Pp.68-69.

<sup>24</sup>Lampe, Peake's Commentary, on Lk 20:18.

<sup>25</sup>Cf. Selwyn, First Epistle of St. Peter, pp.270-71.

<sup>26</sup>Plummer, St. Matthew, *ad.loc.*: 21:43: not found in either Mk or Lk; 21:44: not found in Mk, but present in Lk.

<sup>27</sup>See also Bornkamm, TDNT, IV, 1967, p.281, n.7.

<sup>28</sup>Mélanges...B. Rigaux, p.310; cf. Resse, op.cit., p.171: "Denn wenn Mt.xxi.44 ursprünglich zu Mt. gehört, so besteht ein Recht zu der Frage, ob nicht Mt.und Lk.gegenüber Mk.auf eine gemeinsame Tradition zurückgreifen. Es bliebe dann freilich immer noch zu klären, warum Mt.das Ps.Zitat in derselben Länge wie Mk, also unter Einschluss von Ps.cxviii.23 bringt, und warum er zwischen das Ps.Zitat und v.44 noch v.43 eingeschoben hat... Ist aber Mt.xxi.44 sekundär, so entfällt jede Diskussion über das Vorhandensein einer Q-Tradition an dieser Stelle".

<sup>29</sup>Schriften des N.T., p.338.

<sup>30</sup>Four Gospels, p.319.

<sup>31</sup>Oxford Studies, pp.46, 110.

<sup>32</sup>TDNT, IV, 281, n.7.

<sup>33</sup>Lukasevangelium, p.372, n.4.

<sup>34</sup>Matthäusevangelium, p.193.

<sup>35</sup>Matthäusevangelium, pp. 313-5.

<sup>36</sup>Loc.cit.

<sup>37</sup>Loc.cit.

<sup>38</sup>Gore's Commentary, ad.loc.

<sup>39</sup>Meyer's Kommentar<sup>8</sup>, Matthäusevangelium, loc.cit.

<sup>40</sup>Die Gleichnissreden Jesu, II, 1963 (=1910), p.401.

<sup>41</sup>Parables (E.T.), p.60, n.54; p.83, n.91.

<sup>42</sup>Fall of Jerusalem, p.247.

<sup>43</sup>Critique Textuelle, II, p.71.

<sup>44</sup>Textual Commentary, p.58.

## II.A.

f. Mt 23:26; 'and of the plate'

Disputed words: [καὶ τῆς παραψίδος]

External Evidence:

τοῦ ποτηρίου...τὸ ἐκτὸς αὐτοῦ Θ f<sup>1</sup> 700 it<sup>a</sup>.<sup>o</sup> syr<sup>a</sup> geo<sup>1</sup>.A //  
τοῦ ποτηρίου...τὸ ἐκτὸς it<sup>π2</sup>.r<sup>1</sup> geo<sup>B</sup> Irenaeus<sup>1a</sup> // τοῦ ποτηρίου...τὸ ἐξωθεν  
αὐτοῦ D (it<sup>d</sup> Clement omit αὐτοῦ) // τοῦ ποτηρίου καὶ τῆς παραψίδος...  
τὸ ἐκτὸς αὐτοῦ B\* f<sup>13</sup> 28 1009 1253 1344 2148 [12, 69, 70, 59, 303, 333, 374, 517, 559, 950<sup>d</sup>, 1127, 1579  
eth Basil<sup>1/2</sup> John-Damascus // τοῦ ποτηρίου καὶ τῆς παραψίδος...τὸ ἐκτὸς  
αὐτῶν (N\* ἐντος) N<sup>c</sup> B<sup>2</sup> C K L W (Δ omit τό) Π 0138 33 565 892 1010 1071 1079  
1195 1216 1230 1241 1242 1365 1546 1646 2174 Byz Lect<sup>1/2</sup> syr<sup>p</sup>.(b).pal cop<sup>ss</sup>.bo  
arm Basil<sup>1/2</sup> // τοῦ ποτηρίου καὶ τῆς παραψίδος...τὸ ἐκτὸς X it<sup>aur</sup>.a.f.π<sup>1</sup>.  
a<sup>1</sup>.b.1 vg

### Observations

(i) This is not strictly a case of 'Western non-interpolation'.

(ii) It is strange that such an inconsequential bit of text should have given rise to a long and variegated history of textual emendation. This only goes to prove the secondary nature of most of the variants (vide infra).

(iii) The above attestations are divisible into two broad categories viz., those that retain the phrase καὶ τῆς παραψίδος and those that omit it.

(iv) The divided nature of the members of different groups among themselves does not encourage us to form a conclusive opinion about the authenticity of the text.

(v) According to Burkitt, this is an instance where in view of the conflict between S<sub>1</sub> and B, as opposed to Syr<sup>S</sup>, the reading of the latter is to be honoured<sup>1</sup>.

### Internal Evidence

#### Transcriptional Probability

The following points deserve to be taken note of in respect of the Transcriptional Probability:

(a) The word ἔντος read in S<sub>1</sub>\* instead of ἐκτός is obviously due to scribal negligence, made possible by the earlier τὸ ἔντος which may have stood in the line immediately above in the exemplar.

(b) The variations between those readings omitting αὐτοῦ and those that retain it are well within the limits of the editorial freedom that some translators or copyists would tend to assume. Those who left out this pronoun must have mentally identified the cup with the Pharisees: i.e., they must have personified the cup rather than conforming to the letter of the metaphor so as to imply: 'You blind, Pharisee, first cleanse the inside (of your heart) so that the outside also may become clean' (cf. the comparison of the Pharisees to tombs in vv. 27f). If this inference is correct, αὐτοῦ

was part of the original text.

(c) τὸ ἐξῆς ἠθελον read by Codex Bezae is obviously an attempt to assimilate v.25 by replacing an original ἐκ τὸς .

(d) Those authorities which retain the words καὶ τῆς παροψίδος outnumber those that leave them out; the former have ℵ\* as well as the Coptic and Armenian Versions on their side.

(e) The reading of B\* f<sup>13</sup> etc. involves two nouns which are subsequently represented by the singular αὐτοῦ ; this is a grammatical howler! The author of the First Gospel could be expected to know better, and so the presence of καὶ τῆς παροψίδος in these witnesses is evidently the thoughtless insertion of a later scribe from v.25. Cf. Klostermann: " καὶ τῆς παροψίδος fügen SB nach 25 hinzu, weshalb dann auch αὐτοῦ in αὐτῶν geändert werden musste (so ℵB<sup>2</sup> u.s.w.)" <sup>2</sup>. Thus the very fact that the corrector of B felt compelled to improve upon the grammar of his exemplar by changing the αὐτοῦ to αὐτῶν proves that there was no scope for a second noun in the original text. It is indeed difficult to imagine that an original αὐτῶν would be replaced by an αὐτοῦ .

### Intrinsic Probability

(i) The grammatical point mentioned above is a pointer in the direction of the Intrinsic Probability.

(ii) V.25 is actually a general remark about the behaviour of the Pharisees. V.26 contains an admonition based on it. So it is not likely that Jesus would have repeated the reference to the plate. In the second context *ποτήριον* would in essence stand for the generic sense of 'vessel', 'utensil' ( *σκεῦος* ) (cf. on 6:25).

(iii) It is important in this connexion to recognise how Matthew understood this utterance of Jesus. Allen says: "It seems most natural to suppose that Matthew rightly understood the saying to be aimed at the casuistical distinction between clean and unclean utensils: 'use the vessels only for food honestly procured, and it will be unnecessary to ask if the outside is ceremoniously clean' ". He thinks that in the parallel passage, Lk 11:40,41, Luke might have understood the saying as a metaphor for men as in Mt 23:27 (cf. the scribal deletion of *αὐτοῦ* mentioned above)<sup>3</sup>. If this distinction is correct, the general remark in v.26 need not include 'plate', but just 'cup', as a common term for utensils.

In the light of the foregoing discussion, the following statement by Wellhausen becomes irrelevant: "Wenn die Gefässe Menschen bedeuten, so hätte xxiii 25 nicht neben dem Becher auch noch die Schüssel genannt werden dürfen. Sie wird deshalb in xxiii 26 auch ausgelassen"<sup>4</sup>.

(iv) Just as we saw in the case of the relation between 6:25 and 6:31, so here, the presence of *καὶ τῆς παροψίδος* in v.25 must have easily prompted a scribe to adopt the same phraseology in v.26 also.

#### Conclusion

The majority of commentators are silent on this issue; there are only very few who treat the present words as authentic. Those who uphold that they are spurious are also generally agreed about the fact that they are derived from v.25: e.g., Lagrange<sup>5</sup>, Plummer<sup>6</sup>. For Lagrange, the short text is "plus probablement la leçon authentique". Once again we can be confident of being on safe ground with regard to our conclusion here.

<sup>1</sup>Evangelion, II, p.233.

<sup>2</sup>Matthäusevangelium, ad.loc.

<sup>3</sup>St. Matthew, ad.loc.; cf. B.T.D. Smith, St. Matthew, ad.loc. He thinks that Matthew and Luke are not using a common source here. Filson, St. Matthew, ad.loc., attributes the divergences in these Gospels to independent renderings of the same Aramaic source.

<sup>4</sup>Matthaei, p.118.

<sup>5</sup>Critique Textuelle, II, pp.70-71; cf. RB (n.s.) 10 (1913), p.504.

<sup>6</sup>St. Matthew, ad.loc.

## II.B THE MARKAN 'NON-INTERPOLATIONS'

a. Mk 2:22c: 'But new skins for new wine'

Disputed words: [ἀλλὰ οἶνον νέον . . . καινούς]

External Evidence:

ἀπόλλυται καὶ οἱ ἄσκοι B 892 cop<sup>bo</sup> // καὶ οἱ ἄσκοι ἀπολούνται (see footnote 9) D it<sup>a,b,d,e,ff<sup>2</sup>,i,r,t</sup> // ἐκχέεται καὶ οἱ ἄσκοι L (syr<sup>pal</sup>) // ἐκχέεται καὶ οἱ ἄσκοι ἀπολούνται (see Mt 9.17; Lk 5.37) N A C K (W ἀπόλλυνται) Δ (Θ ἀπόλλυνται) Π 074 f<sup>1</sup> f<sup>13</sup> 28 33 565 700 1009 1010 1071 1079 1195 (1216 ἀπόλλυνται) 1230 1241 1242 (1253 ἀπόλλυνται) 1344 1365 1546 1646 2148 2174 Byz Lect it<sup>aur,c,t,l,q</sup> vg<sup>(cl),w</sup> syr<sup>a,(p),b</sup> cop<sup>sa</sup> goth arm eth geo // ἐκχέεται <sup>190</sup>

ἀλλὰ οἶνον νέον εἰς ἄσκοὺς καινοὺς N\* B // ἀλλὰ (στ ἀλλ') οἶνον νέον εἰς ἄσκοὺς καινοὺς βλητέον (see Lk 5.38) N<sup>a</sup> A C K L Δ<sup>st</sup> Θ Π 074 f<sup>1</sup> f<sup>13</sup> 28 33 565 700 892 1010 1071 10<sup>79</sup> 1195 1216 1230 1241 1242 1253 1344 1365 1546 1646 2148 2174 Byz Lect it<sup>aur,c,t,q</sup> vg syr<sup>b,pa</sup> arm // ἀλλὰ οἶνον νέον εἰς ἄσκοὺς καινοὺς βάλλουσι (see Mt 9.17) W it<sup>e,t</sup> syr<sup>a,p</sup> cop<sup>sa,bo</sup> goth eth geo Diatessaron<sup>a</sup> // omit D it<sup>a,b,d,ff<sup>2</sup>,i,r,t</sup>

### Observations

(i) The four sets of readings above can be conveniently classified into the 'longer', 'shorter' and 'short' readings, the difference between the first and the second being the presence or absence of a form of *βάλλω*.

(ii) The Old Latin authorities present either *βλητέον* or *βάλλουσι* in addition to the 'short' text.

(iii) The attestation of Syr<sup>s</sup> in favour of the reading ending in *βάλλουσι* is significant, although one cannot be precise whether this is decisive or not in the present context<sup>1</sup>.

(iv) The 'best' MSS,  $\mathcal{N}^*B$ , support the 'shorter' text which carries no verb. This would suggest that the 'longer' readings were attempts at improving upon the 'shorter' text rather than vice versa<sup>2</sup>.

(v) The overall impression that we gather from the External Evidence is that all the three forms of text can be traced back to as early as the 4th century.

(vi) Patristic evidence is unfortunately not forthcoming to suggest an earlier date for any of the three readings.

Thus the MS evidence does not lead to any conclusive solution to the textual problem involved.

### Internal Evidence

#### Transcriptional Probability

##### (i) Possibilities of Insertion

(a) The first part of v.22 bears ample MS evidence suggesting attempts at partial assimilation to the corresponding Matthaean or Lukan texts<sup>3</sup>:

e.g.  $\epsilon\kappa\chi\epsilon\iota\tau\alpha\iota$  is read in place of  $\rho\eta\tau\epsilon\iota$

(cf. Mt 9:17) by  $\mathcal{N} ACLKW\Delta\Theta TT f^1 f^{13} 28 a1$   
 $it^{aur}, c, e, f, ff^1, g^1, l, q, v_g (cl), ww, syr^s (p), h$

$cop^{sa}$  goth eth arm;

Several of the above authorities such as W, it<sup>e,f</sup> Syr<sup>s</sup> cop<sup>sa</sup> goth eth arm also carry another Matthaean word, βάλλουσιν at the end of v.22; καὶ ἀμφοτέρωθεν συνηροῦνται from Mt is attested by 118<sup>mg</sup>, it<sup>e,f</sup>; ὁ νέος is inserted after οἶνος (cf. Lk 5:37b) by AC<sup>2</sup> it<sup>e,f</sup> syrr goth eth; ἀπολοῦνται (Lukan) is read instead of ἀπόλλυται by D it<sup>a,b,d,e,ff2,i,r1,t</sup> in addition to the witnesses which have interpolated ἐκχεῖται from Mt. All this suggests that the Markan text did not initially correspond to either Mt or Lk at this point.

(b) J.P. Brown has attributed the many textual variants in the ancient authorities for Mark's Gospel including 2:22 to a revised version or versions which existed in the first century and is best represented by the Caesarean Text<sup>4</sup>.

However, since Brown himself admits that this is only a conjecture, it cannot be taken as authoritative. Moreover, his assertion that the S\*B reading is the intermediate stage in the process of interpolation into the 'short' text does not square with the actual facts considered above.

(c) The view that the present text is possibly either a gloss from, or assimilation to Lk 5:38<sup>5</sup> is also not tenable not only in view of the textual evidence, but from the discussion that will follow in regard to the Intrinsic Probability. Suffice for the time being to point out that such a gloss or assimilation would most likely have included an additional element from the next verse which is uniquely Lukan.

(ii) Possibilities of Omission

Assuming any one of the three forms other than the 'short' reading to be original, what are the possible causes that led to its deletion in D and its allies?

(a) Snodgrass' suggestion<sup>6</sup> that the apparent superfluity of the statement would have been held to be a valid reason for deletion is a very likely explanation, although, as we shall see below, these words are not so redundant, after all. However, the possibility remains that a scribe may have misunderstood its real import. At the same time, it is legitimate to ask whether scribal predilection was more in the direction of omission than of addition.

(b) C.H. Turner conjectures that if the preceding clause, εἰ δὲ μή . . . . ἄσκοί be treated as a parenthesis, the disputed words could be taken as belonging to the original text of Mark<sup>7</sup>. Without the "parenthesis" the passage would read smoothly as follows: καὶ οὐδεὶς βάλλει οἶνον γέον εἰς ἄσκου̅ς παλαιούς . . . . ἀλλὰ οἶνον γέον εἰς ἄσκου̅ς καινούς <sup>8</sup>: the verb βάλλει would now be applicable to 22c as well. So scribal assimilation or deletion, according to this hypothesis, arose out of a misjudgment of the syntax of the complete verse.

The difficulty with regard to this theory has confronted us more than once in Mt: it has to do with the symmetric pattern of the aphoristic sayings of our Lord which Turner has ignored. A comparison of v.22 with the preceding verse would at once show that they were meant to counterbalance each other in a well-designed parallelism<sup>9</sup>:

21a: οὐδεὶς . . . ἐπιβάπτει

21b: εἰ δὲ μή, ἄρει . . . καὶ . . . γίνεται

22a: οὐδεὶς . . . βάλλει

22b: εἰ δὲ μή, ῥήξει . . . καὶ . . . ἀπόλλυται

Thus it would seem that, in the author's scheme, 22c forms the last part of the verse, deliberately added for the sake of emphasis. Acceptance of Turner's

proposal would disrupt the harmony of the closing section of this pericope. It is not therefore surprising that this theory has met with very little approval.

(c) Streeter<sup>10</sup>, taking a cue from R. Harris<sup>11</sup> in respect of the great antiquity of the sense-lines of D, has pointed out that if 22c originally belonged to an ancestral MS, "the words *οἶνος ἀσκούς* , separated by only a few letters, would have occurred in each of three successive lines". This situation, says Streeter, is easily prone to an omission involving a slip of the eye from the second line to the line immediately following the third.

Streeter's theory is nothing new because A.C. Clark had stated the same thing and had demonstrated it in respect of other contexts<sup>12</sup> (cf. on 12:19; 12:21). The plausibility of this may be seen from the following configuration which could be conceived in an ancestor of D<sup>13</sup>:

ΕΙΔΕ ΜΗΡΗΞΕΙ Ο ΟΙΝΟΣ ΤΟΥΣ ΑΣΚΟΥΣ  
ΚΑΙ Ο ΟΙΝΟΣ ΚΑΙ ΟΙ ΑΣΚΟΙ ΑΠΟΛΟΥΝΤΑΙ  
ΑΛΛΑ ΟΙΝΟΝ ΝΕΘΝΕΙ Ο ΑΣΚΟΥΣ ΚΑΙ ΝΟΥΣ

(d) According to Kilpatrick, as the equivalents of the disputed words are to be found at Mt 9:17 and Lk 5:38, their omission in Mk "is an attempt to

create an agreement between Matthew and Luke against Mark"<sup>14</sup>.

This is indeed a baseless argument because it assumes a critical awareness on the part of ancient scribes comparable to that of modern scholarship! In fact elsewhere Kilpatrick charges some of the modern scholars with the same obsession (vide infra).

It must be mentioned in closing this section that other scholars like Lohmeyer<sup>15</sup> who attribute the omission to a scribe have not cared to substantiate their statement. All that we can say as a result of our discussion so far is that the omission was either accidental (oversight) or deliberate (redundancy).

#### Intrinsic Probability

(i) It could be argued that in view of Mark's characteristic usage of the impersonal plural (vide on 10:2), the reading of W it<sup>e</sup> Syr<sup>s</sup> employing βάλλουσιν must be original and that while Matthew has copied this, Luke has substituted it with βλητέον.

But against this must be weighed the following considerations:

(a) The Transcriptional Probability, already considered.

(b) It is not likely that a verb would be dropped by a scribe from a statement, because its retention only makes for greater clarity of meaning.

(c) Mark's penchant for one particular style does not necessarily mean that he was not likely to deviate from it when occasion so demanded (cf. on 10:2), especially if he was quoting a popular saying, as seems the case in 22c.

(ii) The real clue to the solution of the present textual problem lies in the understanding of the Synoptic relationship involved. Kilpatrick's criticism of scholars like Turner, Burkitt and Streeter on this issue seems to be unjustified. He says that they "have not followed D etc. here because they do not subscribe to the Synoptic theory which does not allow Matthew and Luke to agree in passages in which it is believed that they derive their material from Mark alone"<sup>16</sup>. But it is most likely that unlike Matthew, Luke is not following Mark at this point, but an independent tradition (cf. on Lk 5:39). Moreover, Kilpatrick compares

the above Synoptic relationship with that at Mt 21:44 par. and charges these scholars with the same accusation<sup>17</sup>. But both Burkitt<sup>18</sup> and Streeter<sup>19</sup> have expressed themselves clearly in favour of the omission of this passage in Mt . thereby allowing Matthew to disagree with Luke. Turner follows suit when he writes that "the agreement of Matthew and Luke in giving exactly these six words (sc. Mk 22c) in common, while they provide different forms of  $\beta\alpha\lambda\lambda\omega$  to complete the construction, is a strong argument for their genuineness in Mark"<sup>20</sup>. In any case, Kilpatrick is contradicting himself when he states that "it is not the presence of these words (in Mk) but their absence which creates an agreement between Matthew and Luke against Mark, as their equivalents are found at Mt.ix.17 and Lk.v.38"<sup>21</sup>. For since Turner et al are in favour of the retention of these words, it is obvious that they have subscribed to an agreement of Matthew and Luke against Mark.

(iii) Rawlinson seems to have caught the real import of the passage when he suggests that the saying under discussion is perhaps a popular proverb<sup>22</sup>. The twin parables in this context involving the mending of a garment and the storage of wine might have been uttered by Jesus on different occasions as

in the case of so many other dominical sayings preserved by the Evangelists (cf. on Lk 5:39). It was perhaps Mark himself who appended the present "proverb" to these parables in order to drive home their message forcefully. "It is, however, perhaps also possible that Jesus himself purposely went beyond the limit of the poetic form he was using in order to sum up positively and emphatically the burden of the pair of parables"<sup>23</sup>.

It is worth pointing out in this connexion against Allan Menzies<sup>24</sup>, Wellhausen<sup>25</sup> et al, that the two parables in Mk 2:21-22 have real relevance to the question about fasting raised by certain people (v.18). The interrogators, brought up in the *παραδοσις* of the Elders (7:3), thought of piety in terms of conformity to certain external norms. But the whole tenor of Jesus' teaching was that while He did not come to "abolish" but to "fulfil" the old, He demanded a new orientation in one's attitude to God and fellow-men. He did not establish a new set of regulations for His followers, but enunciated certain basic principles that should become the hallmark of every member of the Kingdom of God which He came to inaugurate. The quality of life of the citizens of this Kingdom could only be described in terms of a new birth (10:15 cf. Jn 3:3). The twin parables

here were probably meant to emphasize the need for spiritual regeneration<sup>26</sup> or the incompatibility of the new and the old<sup>27</sup>. Their stress is not so much on the danger to the old religion<sup>28</sup> as on the intrinsic vitality of the new way of life<sup>29</sup> which is bound to "burst" the wineskin of the old so that men will no longer think of religious obligation in terms of "merit" before God and "respectability" in society, but in terms of love for God and fellow-men<sup>30</sup>.

### Conclusion

It is immaterial for our purpose whether the words at the close of these parables go back to a genuine dominical utterance or to the Sitz im Leben of the early Church. One thing, however, is clear from a comparative study of the Synoptics at this point, viz. that the three Evangelists understood the emphasis of the parables in rather different ways. Matthew, with his insight of Christianity as the crown of Judaism, felt justified in introducing some extra words to pinpoint the idea that the old is not to be completely wiped away by the new:

καὶ ἀμφοτέρωθεν συντηροῦνται . Luke's stress

was on the need for tolerance and patience on the part of the followers of the new way in their dealings with those who would still regard the old as

good (cf. on Lk 5:39). Mark's insight, as we have seen, emerges as pre-eminently radical in its attitude towards the old. This is brought into sharp relief by the retention of the disputed words in 22c. Viewed in this light, we cannot but conclude that these words are neither a redundant appendix nor an interpolated matter, but a constituent part of Mark's original Gospel. The proverbial nature of the statement adds to its relevance in its present context, and this is heightened by the rejection of either form of *βάλλω*. The MS evidence as well as the Internal Evidence only confirms this view<sup>31</sup>.

Once again we have a clear demonstration of the oddness of the term 'Western non-interpolation'. "If then, the omission is wrong, we have clear warning both that the Western authorities are not on every occasion right and that it is not the shorter (sc. our 'short') text which is always right"<sup>32</sup>.

<sup>1</sup>Syr<sup>s</sup> is generally reputed to be free from the 'Greater Interpolations' in the 'Western' witnesses. While Souter (Text and Canon, p.128) thinks that the Syr<sup>s</sup> reading is probably right, Burkitt (Evangelion, II, p.230) supports the B<sub>W</sub> reading; Burkitt regards the former as an assimilation to Mt 9:17.

<sup>2</sup>So Abbott, Diatessarica, II, p.78, § 360.

<sup>3</sup>Cf. Klostermann, Markusevangelium, ad.loc.; F.C. Grant, Interpreter's Bible, ad.loc.

<sup>4</sup>JBL 78 (1959), pp.215-227. According to him, the "many divergences from Mk in Mt and Lk and...the bulk of the agreements of Mt and Lk against Mk" are accounted for by this recension; Cf. Sanday, Oxford Studies, p.21; Hawkins, Horae Synopticae, p.212. Contrast C.H. Turner, Inaugural Lecture<sup>1</sup>, Oxford, (1920), p.43: "A post-Marcian 'recension' is open to the same sort of objections as a pre-Marcian 'Ur-Marcus' ".

<sup>5</sup>F.C. Grant, Interpreter's Bible, ad.loc.; E.P. Gould, St. Mark, ad.loc.; S.E. Johnson, St. Mark, ad.loc.

<sup>6</sup>JBL 91 (1972), p.378.

<sup>7</sup>JTS 26 (1925), p.147.

<sup>8</sup>Cf. H.B. Swete, St. Mark, ad.loc.

<sup>9</sup>V. Taylor, St. Mark, ad.loc.

<sup>10</sup>Four Gospels, p.311; Cranfield, V. Taylor and Lohmeyer, in their Commentaries, support Streeter here.

<sup>11</sup>Texts and Studies, II (1891), pp.241ff.

<sup>12</sup>Primitive Text, pp.50ff. It seems significant that the idea has not struck Clark in respect of this verse.

<sup>13</sup>Cf. Scrivener, Codex Bezae Cantab.

<sup>14</sup>JTS 44 (1943), p.29.

<sup>15</sup>Meyer's Kommentar, Markus (1967), p.61; cf. Streeter, Four Gospels, p.311.

<sup>16</sup>JTS 50 (1949), p.152.

<sup>17</sup>JTS 44 (1943), p.29.

<sup>18</sup>Evangelion, II, p.232.

<sup>19</sup>Four Gospels, p.319.

<sup>20</sup>JTS 26 (1925), p.147.

<sup>21</sup>JTS 44, loc.cit.

<sup>22</sup>St. Mark, ad.loc.

<sup>23</sup>Cranfield, St. Mark, p.112.

<sup>24</sup>The Earliest Gospel, pp.87-88.

<sup>25</sup>Evangelium Marci, p.21; cf. A. Kee, NovT 11 (1969), p.172: "The reply... (sc. 19ff.) does not come from Jesus, but from the early Church. It answers not the Question About Fasting, put by the disciples of John, but the question(s) of major interest to the early Church on the subject of fasting". (!)

<sup>26</sup>So Cranfield, loc.cit.

<sup>27</sup>Loisy, Marc, p.99; cf. Jeremias, Parables (E.T.), pp.117-19; Lagrange, S. Marc, p.50.

<sup>28</sup>F.C. Grant, Interpreter's Bible, ad.loc.; S.E. Johnson, St. Mark, ad.loc.

<sup>29</sup>Cf. Klostermann, Markusevangelium, p.28: "Man kann das alte Kleid des Judentums nicht mit neuen Flickern konservieren, und man kann den neuen Wein des Christentums nicht in alten Schläuchen konservieren".

<sup>30</sup>Allan Menzies' statement that v.22 "appears to postulate a new set of religious forms for the Christian principles" is a misreading of the whole tenor of the Gospels (loc.cit.). For the opposite view, see Wellhausen, loc.cit. S.G.F. Brandon, Jesus and the Zealots, p.266, and n.3, on the other hand, reads into the present words an allusion to the fall of Jerusalem, A.D. 70: "The ruin

of the Temple had ended the compromise which the Jerusalem Christians had sought to maintain, and now Christianity was free of its Judaistic swaddling bands". This is to do violence to the commonly held date for Mk. Even without resorting to such an extreme hypothesis, the meaning of the parables is quite clear.

<sup>31</sup>Cf. Lagrange, *Critique Textuelle*, II, p.68, who affirms that this form (i.e. our 'shorter' reading) is certainly or most certainly authentic; Cf. Resch, *TU X,2* (1895), p.56, for a similar note of confidence. See also Seesemann, *TDNT*, V, pp.162ff.

<sup>32</sup>Kilpatrick, *JTS* 44 (1943), p.29.

## II.B

b. Mk 10:2: The Interrogators at Peraea

Disputed words: [προσελθόντες Φαρισαῖοι]

External Evidence:

καὶ προσελθόντες Φαρισαῖοι A B K L Δ Π Ψ J<sup>3</sup> 23 700 892  
1010 1079 1546 1646 Byz<sup>pc</sup> cop<sup>bo</sup> goth // καὶ προσελθόντες οἱ Φαρισαῖοι  
K C X (J<sup>1</sup> οἱ Φαρισαῖοι after αὐτόν) 1009 1071 1195 1216 1230 1241 1242  
1253 1344 1365 2148 2174 Byz<sup>pc</sup> (Lect omit καὶ at beginning of lection) Diates-  
saron<sup>a</sup> // προσελθόντι τῷ Ἰησοῦ οἱ Φαρισαῖοι I<sup>155</sup> // καὶ προσελθόντες οἱ (or  
omit οἱ) Φαρισαῖοι it<sup>aur.,c.,l.,a</sup> vg syr<sup>p.,b</sup> geo // οἱ δὲ Φαρισαῖοι προσελθόντες  
W Θ 565 cop<sup>sa.,(1,2,3)</sup> arm // et accedentes quidam it<sup>12</sup> // καὶ D it<sup>a.,b.,d.,k.,r.</sup> syr<sup>r</sup>  
Origen

### Observations

(i) All these readings except the last two are virtually the same; the difference between the long and short readings consists in the presence or absence of the subject *Φαρισαῖοι* with or without the definite article.

(ii) The Old Latin authorities are divided in their attestation.

(iii) The combined testimony of the principal representatives of Italy (D), of Gaul (a,b) and of Carthage (k) along with that of Syr<sup>s</sup> and of Origen speaks of the fairly widespread existence of the verse without the specific subject from the 4th century<sup>1</sup>:

(iv) However, the evidence provided by the 'best' MS tradition headed by  $\delta$ BCLX f<sup>1</sup> f<sup>13</sup> 28 700 etc. supports the retention of the subject.

(v) Further patristic evidence is lacking either to corroborate or to contradict Origen's authority.

### Internal Evidence

#### Transcriptional Probability

The External Evidence only brings into relief how knotty the problem is. On the one hand, there is a wide range of witnesses to the long reading; on the other, if the term  $\phi\alpha\pi\iota\sigma\delta\iota\omicron\iota\upsilon$  had originally stood in the text, there is no apparent reason why it came to be omitted in Codex Bezae etc. It would seem that the addition could be more readily explained than the omission.

(a) Metzger and Wikgren, in a dissenting note to the UBS Committee's majority decision in this respect suggest that the term  $\phi\alpha\pi\iota\sigma\delta\iota\omicron\iota\upsilon$  could be an intrusion from Mt 19:3<sup>2</sup>.

The majority decision, however, is worth quoting against this: "Despite the plausibility of such a possibility, the fact that the Matthean passage

is not absolutely parallel (προσῆλθον αὐτῷ Φαρισαῖοι)  
and the widespread and impressive support for the  
longer reading led a majority of the Committee to  
retain the words in the text".

The same type of dilemma is observable  
in Lagrange's approach to the problem. In his  
commentary on Mark he decides in favour of the  
Pharisees because he thinks that the question raised  
here is in their very spirit<sup>3</sup>. But in an earlier  
work he had judged that a decision was most difficult  
and suggested that this was a case of insertion from  
Mt<sup>4</sup>.

(b) V. Taylor's argument that "it would not be  
strange for an allusion to the well-known opponents  
of Jesus to be supplied if the verb lacked a subject"<sup>5</sup>,  
can be countered by saying that the probability lies  
exactly in the opposite direction; for Mark is least  
likely to have left such a group of interrogators  
unspecified (vide infra, p.297 ).

(c) E.A. Abbott tries to show that it was due to a  
misunderstanding of the word Πειράζοντες (to mean  
"tempt") that scribes introduced the subject  
on the analogy of the several occasions when Jesus  
was "questioned" by the Pharisees. He conjectures

that in Mark's original Aramaic source there was only a reference to the "questioning" (ܘܨܘܘܩܐ = ἑπερωτᾶν = to question, to discuss), the verb being in the impersonal plural. Mark inserted ΠΕΡΠΑΛΟΥΝΤΕΣ at the end of the sentence as a supplementary explanation of the questioning without any pejorative significance attached. Matthew, while copying Mark, placed this word earlier in the sentence by substituting it for ἑπερωτᾶν. Luke simply rejected it as an error. In support of his theory Abbott calls attention to the divergences of the wording of the synoptic parallels at this point and infers that this is due to a conflation of tradition in Mt and Lk<sup>6</sup>.

In spite of his elaborate discussion, I do not think that Abbott has succeeded in proving his case: First, (even if we concede the origin of ΠΕΡΠΑΛΟΥΝΤΕΣ to Mark), the very fact that Mark felt the need for a "supplementary explanation" shows that he had intended to convey something deeper than a mere "questioning" in the sense of eliciting information. Secondly, Abbott is contradicting himself when he suggests that ἑπερωτᾶν implies "legal questioning" in this context<sup>7</sup>. The questioners were not simply interested in asking questions as an intellectual exercise, but were

hoping to open what they regarded as a legal debate. Thirdly, it will be shown below that the pejorative sense of *πειράζειν* is more predominant in the Gospels than its milder sense and that this meaning suits the Pharisees much more than the representatives of the people. Finally, Abbott's inference from the relative positions of *πειράζοντες* in Mt and Mk is too far-fetched in that such stylistic deviations can be detected in many other Synoptic parallels.

#### Intrinsic Probability

(a) The point raised by Abbott in regard to the use of *πειράζειν* in the Synoptic Gospels offers a clue precisely in the opposite direction. The pejorative use of the verb is, of course, obvious in the account of the Temptation in all the Synoptics: Mk 1:13 par. Mt 4:1 par. Lk 4:2, cf. Mk 1:13, *σατανᾶς* = Mt 4:2, *διάβολος* = Lk 4:2, *ὁ πειράζων* (cf. *ὁ διάβολος*, v.5). It is clear that *ὁ πειράζων* is, for Mk and Mt, *σατανᾶς* (= adversary, Job 1:6ff, 2:1ff), and for Mt *διάβολος*, one who sets himself in opposition to everything that is godly. Special attention must be paid to the use of *πειράζειν* in connexion with the series of vexing questions put

to Jesus by the Jewish leaders (Mk 12:15; Mt 22:18,35; Lk 10:25). Unlike in Jas 1:12 (contrast vv.13-15; 1 Cor 10:13), the sense of 'to prove one's worth by testing' (cf. Gen 22:1; Ps 139:23) is absent in any of these contexts. It is particularly noteworthy that in Mk 12:13ff, *Πεπράζειν* is identified with *ἀγρεύειν* (= to trap), and that in Mt 22:15ff, the same verb is identified with *παγιδεύειν* (= to trap, to entangle). (Cf. also the use of *Πεπράζειν* in association with *συζητεῖν* (= to argue), Mk 8:11 par. Mt 16:1 (*Πεπράζοντες ἐπηρώτησαν*)). So there is no denying the fact that in the Synoptic Gospels, especially in Mk, *Πεπράζειν* has a more significant role than is suggested by Abbott<sup>8</sup>. Thus it is abundantly clear that the subject of *Πεπράζοντες* in Mk 10:2 is most likely to be a specific group of people who had habitually set themselves in opposition to Jesus and frequently attempted to entangle him in the religio-political controversies of the day. We will see below that this delineation suits the Pharisees in the Second Gospel more than any other group.

(b) Many a commentator is indebted to C.H. Turner's study of the use of the impersonal plural as a predominant stylistic feature of Mark<sup>9</sup>. He provides an impressive array of 14 (+3?) instances to prove his case and tries to show that this is particularly true of the present context, where the verb ἐπιγνώτω implies the impersonal subject 'they' (= people). His conclusion is stated as follows: "The evidence shows conclusively that the idiom is a regular and common one in Mark's narrative, and that on two occasions on an average out of three it is in some way got rid of by the other synoptists"<sup>10</sup>.

No doubt, one has no difficulty in accepting this verdict on Mark's style in relation to the other Evangelists. This fact, however, should not blind us to the other traits of his treatise. One of the contributions of Mark as an Evangelist is the way in which he develops the motif of Jesus' confrontation with the Jewish leaders from the very beginning of His public ministry. This is presented in direct contrast to His growing popularity as one who was introducing "a new teaching" and was casting out demons "with authority" (1:27). As a prologue to the development of this antithetical motif Mark remarks, 1:22 : 'And they were astonished at his teaching, for he taught them as one who had authority.

and not as the scribes' (cf. 1:32ff,37,45).

Mark then goes on to show that a teacher who deviates from the conventional role of a religious leader and is bold enough to defy the acknowledged leadership is bound to come into conflict with the establishment. Thus the first confrontation with the Teachers of the Law on the question of the authority to forgive sins is presented as early in the Gospel as 2:6ff.

Again, the contrast between the reaction of the leaders and that of the people at large to Jesus' public ministry is brought into sharp relief at 2:12 and 2:16f. It is instructive to note that this antithetical note is consistently maintained by Mark right through to the end of the public ministry<sup>11</sup>: 3:7,20 cf. 3:22; 4:1; 5:21 cf. 6:14f (cf. 8:28); 6:31, 33f, 54ff cf. 7:1ff; 7:14,24,32,37; 8:1 cf. 8:11; 8:31; cf. 8:34; 9:14; 10:1; cf. (10:2); 12:13; 10:46; 11:8f; cf. 11:18,27f; 12:13,18, 35ff; 12:37b cf. 12:38ff; 14:1,10 cf. 14:2. The chapter-wise distribution of this contrast is most intriguing, although the present chapter divisions are not, of course, Mark's creation. The only gap in the chain is ch.13, which is devoted to the long discourse on the eschatological events. J. Reumann<sup>12</sup> is therefore quite justified in suggesting that "it would make abundant sense to move directly" from 3:6 to 14:1.

Even though all the different categories of Jewish leadership come in for this kind of anti-thetical presentation, it is the Pharisees who are most frequently marked out in the Second Gospel for special condemnation as the distinct representatives of the old system<sup>13</sup> (cf. on 2:22; the disciples of the Pharisees are particularly mentioned in v.18). At 3:6, it is the Pharisees who plot with the Herodians (cf. 12:13) to kill Jesus because of His healing work on the Sabbath. This event is narrated even before the account of the choosing of the Twelve Disciples, and not surprisingly, commentators have wondered if Mark is not guilty of an anachronism in bringing the antagonism of the Pharisees to a head at so early a date in Jesus' public career<sup>14</sup>. However, Mark's intention and his method are quite evident. No wonder, then, that he reports Jesus as warning the disciples against the "leaven" of the Pharisees and of Herod (8:15 cf. 3:6, 6:16ff). By this juxtaposition Mark tries to show that the lay leaders of the "pious" among the Jews felt no compunction in debasing themselves to the extent of collaborating with an unpopular political system in their bid to destroy Jesus. The Evangelist thus tells us in no uncertain terms that the Pharisees had as much a hand in delivering Jesus up for

execution as the Sadducees, the priestly class.

(Cf. also 1:16, where some Teachers of the Law are mentioned as belonging to the Pharisaic sect).

### Conclusion

One basic principle that we cannot ignore in Textual Criticism is to do justice to an author's theological motifs and his treatment of them. So if we are to be fair to Mark's development of his theme, viz. the conflict between Jesus and the Jewish leaders culminating in the Crucifixion, we are obliged to take *οἱ φαρισαῖοι* as the subject of the sentence at 10:2. Depriving ch.10 of the role of the Pharisees in approaching Jesus with the tricky question of divorce would result in a long gap in Mark's development of the above motif.

It might be argued that since the incident is reported as taking place in Peraea, the interrogators were a group other than the Pharisees. If this view is conceded, they could probably be a group that was opposed to the Pharisaic adherence to the Tradition of the Elders, and yet genuinely interested in the new teacher's solution to the long-standing controversy that had divided the Rabbis and their followers into the schools of

Hillel and Shammai. Could they, in that case, be the followers of the Essenes?

This view, however, is not acceptable on the following counts:

(a) If the question proceeded from a group of earnest seekers after truth, why should Mark qualify their query by using the word *ΠΕΡΙΛΑΪΤΕΣ* ?

(b) If they were a specific group recognizable by their religious scruples or affiliations, why should Mark suppress their label, seeing that he has not done so in regard to the other well-known parties of the day such as the Pharisees, the Sadducees, the Teachers of the Law, the Herodians and the Zealots (cf. 3:18)?

(c) Besides, there is no evidence that there was no Pharisaic group in the region of Trans-Jordan during Jesus' time. Since they certainly flourished in Galilee, as Mark makes it abundantly clear, there is no reason why they should not have been found in Peraea<sup>15</sup>.

Thus we conclude that it was none other than the Pharisees who raised the question on divorce before Jesus at Mk 10:2. Despite the difficulties raised above, it is gratifying to note that many commentators leave this problem undiscussed and take for granted that the subject of the sentence is  $\phi\alpha\rho\iota\sigma\alpha\iota\omicron\iota$ .<sup>16</sup>

<sup>1</sup>Cf. V. Taylor, Text of the N.T., p.86.

<sup>2</sup>Cf. Metzger, Textual Commentary, p.104.

<sup>3</sup>S. Marc (1942), p.257.

<sup>4</sup>Critique Textuelle, II, (1935), p.70. However, see RB (n.s.) 10 (1913), p.508, where he rules out any possibility of interference of the Matthaean text via Tatian because of the fact that while the order of the text in the Diatessaron follows Mt, that of the longer Markan text in respect of the position of ΠΕΙΡΑΖΟΝΤΕΣ is different.

<sup>5</sup>Text of the N.T., p.86.

<sup>6</sup>Diatessarica, II, pp.199ff; § 466; cf. Mk 3:6 par. Mt 12:14 par.Lk 6:11; Mk 8:1 par.Mt 16:1 par.Lk 11:16; Mk 12:13 par.Mt 22:16 par.Lk 20:20.

<sup>7</sup>Ibid., § 446 (d)a, n.1. Cf. Mt 16:1: ΠΕΙΡΑΖΟΝΤΕΣ ἘΠΗΡΩΤΗΣΑΝ, par. Mk 8:11 (ΠΕΙΡΑΖΕΙΝ in association with ΣΥΛΗΤΕΙΝ).

<sup>8</sup>See Seesemann, TDNT, VI, pp.34f.

<sup>9</sup>JTS 25 (1924), pp.377-86; cf. V. Taylor, St. Mark, ad.loc.; Tasker, Greek N.T., ad.loc.; Burkitt, Gospel History and Its Transmission, p.98.

<sup>10</sup>Turner, op.cit. p.383.

<sup>11</sup>The only exception to this generalization is found at 5:17 (the terrified Gerasenes), 5:40 (the incredulous derision of the crowd at Jairus' house), and 6:2ff (the unbelief of the people of Nazareth). But on all these three occasions the people concerned were acting in a negative way (out of their incredulity or fear) rather than launching a calculated attempt to oppose Jesus.

We have in fact no means of knowing whether these groups of people were later converted to Jesus' side. But the opposition of the Jewish leaders bears no comparison to these isolated instances; cf. Snodgrass, JBL 91 (1972), p.378.

<sup>12</sup>Jesus in the Church's Gospels, p.258; cf. V. Taylor, St. Mark, p.91, who opines that these data of the conflict with the Jewish leaders were "compiled by Mark or an early collector in order to show how Jesus came to his death through conflict with the Rabbis".

<sup>13</sup>Reumann, op.cit., p.256 "...it would seem that [in Mark] while the Pharisees are virtually never mentioned in the Passion narrative itself, they are the chief enemies of Jesus during the Jerusalem ministry, but that the chief priests and their helpers who play the leading roles in the arrest and trial are almost never mentioned among Jesus' enemies prior to that time". Brandon, Jesus and the Zealots, p.249, explains this prominence given to the Pharisees among the enemies of Jesus as follows: "...to the Gentile Christians of Rome, the Pharisees represented the typical Jewish religious leaders" (cf. 7:13); "for them *οἱ ἀρχιερεῖς* would not have denoted persons distinctively Jewish"; cf. V. Taylor, St. Mark, p.335.

<sup>14</sup>Cf. T.A. Burkill, RevHR 154 (1958), pp.24-6; he interprets Mk 3:1-5 as a Christian defence for the practice of healing on the Sabbath and describes v.6 as "la note de l'éditeur", which "pointe réellement vers le résultat final du conflit qui sera décrit en détail dans le récit de la passion"; cf. P. Winter, Trial of Jesus, pp.111ff. This view is also held by S.G.F. Brandon, op.cit., pp.248-49: "The statement reveals more of the mind of Mark than it does of the event described. For, in the first place, it seems incredible that, after only their fourth encounter with Jesus over fairly trivial issues, the Pharisees become so hostile that they plan to destroy him. The statement is undoubtedly proleptic, being designed to prepare the way for the ultimate presentation of the Jewish leaders as responsible for the Crucifixion".

Another related issue is the mention of the Herodians in 3:6. B.W. Bacon JBL 39 (1920), pp.102-112, following Cheyne (Encyclopaedia Biblica, under 'Herodians') considers this an absurdity. But the editors of 'Beginnings of Christianity', I, pp.119-20, identify them as the political supporters of Herod Antipas, Tetrach of Galilee. Brandon (loc.cit., also p.200, n.4), however, describes them as the "supporters of the native pre-Roman dynasty, whose head, Agrippa II, and his sister Berenice (sic) well-known in Rome after A.D. 70 in fact, they would have formed a little foreign coterie there who were in ill-repute with the Romans, owing to the infatuation of Titus for Berenice. Thus, the author of Mark associates with the Pharisees, in

their intent to kill Jesus, this Jewish group, whom the Roman Christians would have been predisposed to dislike. Accordingly, in setting the stage for his apologia concerning the Roman execution of Jesus, the Markan writer gives his readers an early clue to the problem of making known to them the malignant design of these two detestable Jewish parties". Their political colour is, at any rate, indisputable: cf. H.H. Rowley, JTS 41 (1940), pp.14-27. B. Weiss, The Life of Christ, (E.T.) ii, p.242, also ratifies the early appearance in Mk of the 'Herodians'. He accounts for the absence of the term "from the evangelical history and for the fact that our Gospels make no mention of success attending their machinations" in terms of the troubled conscience of Herod Antipas over the murder of John the Baptist.

<sup>15</sup>T. Keim, The History of Jesus of Nazara, V, pp.27f, commenting on this pericope says: "Either these Pharisees were themselves pilgrims to the feast on the Peraea, or they were dwellers in the district, perhaps of Jericho, since there was no part of the country where Pharisees were not".

<sup>16</sup>Cf. J. Reumann, op.cit., p.227; p.339, n.7; B. Weiss, The Life of Christ (E.T.), ii, p.293 and p.294, n.1.

II.B.

c. Mk 14:39 : 'saying the same words'

Disputed words: [τὸν αὐτὸν λόγον εἰπῶν]

External Evidence:

τὸν αὐτὸν λόγον εἰπῶν 8 A B C K L W X Δ Θ Π Ψ 0112  
0116<sup>riid</sup> f<sup>1</sup> f<sup>13</sup> 28 565 700 892 1009 1010 1071 1079 1195 1216 1230 1241 1242 1253  
1344 1365 1546 1646 2148 2174 Byz Lect it<sup>aur.</sup> f. l. a vg syr<sup>a, p, h</sup> cop<sup>sa, bo</sup> arm geo //  
omit D it<sup>a, b, c, d, f, g, k</sup>

Observations

- (i) The majority of witnesses including  
8 B A C L W Θ f<sup>1</sup> f<sup>13</sup> 28 565 700 1241 are for retaining  
the four words bracketed by Westcott-Hort.
- (ii) They are also supported by most of the  
early Versions including the Old Syriac and some of  
the Old Latins.
- (iii) On the other side, we have the leading Old  
Latin authorities attesting the deletion in the  
company of D.
- (iv) Patristic witnesses are not forthcoming.

Judging from the numerical strength,  
geographical diffusion and the quality of the attest-

ing authorities, it is not unfair to say that the balance of probabilities lies on the side of the long text.

### Internal Evidence

#### Transcriptional Probability

##### (i) Possibilities of Insertion

It is indeed difficult to justify any theory of addition to an originally short text. V. Taylor, without stating any valid reason, notes that "the phrase has every appearance of a gloss<sup>1</sup>". Kilpatrick regards the identity of these four words with their Matthaean counterpart sufficient justification to treat them as an interpolation from Mt 26:44<sup>2</sup>.

Probably Kilpatrick has overlooked the fact that the relative positions of these words in Mt and Mk are not the same<sup>3</sup>. Besides, in Mt they are found in conjunction with Jesus' third act of prayer, while in Mk they occur at the second<sup>4</sup>. Even a mediocre interpolator could be expected to do a better job than that if his interest was in harmonizing the two texts! Besides, the pericopes concerning the scene in Gethsemane are not identical in every respect in the two Gospels. Therefore the defenders of the view of harmonization have an

obligation to explain why the most poignant part of the scene, Mt 26:39 par. Mk 14:36; Mt 26:42b cf. Mt 14:39, concerning Jesus' surrender to the Father's will, has been spared from this harmonistic attempt. A glance at a Synopsis of the Gospels is enough to convince us that while Matthew has followed Mark's outline in describing the Agony in Gethsemane and borrowed his phrases in many places including the present, he has maintained his characteristically elaborate style. The reason why he has dropped Mark's τὸν αὐτὸν λόγον εἰπὼν (14:39) at 26:42 is obviously due to stylistic reasons. He describes the first act of prayer in v.39 giving the details of the words of the petition. The second act of prayer is mentioned in v.42 with the introductory formula πάλιν ἐκ δευτέρου. Not being satisfied with Mark's brief reference to this occasion, Matthew records the words of the second petition which is quite different in tone from the previous one. Finally in v.44, he introduces the third occasion of prayer with the formula: πάλιν ... ἐκ τρίτου, τὸν αὐτὸν λόγον εἰπὼν. It is obvious that he had deliberately reserved the Markan phrase τὸν αὐτὸν λόγον εἰπὼν until this occasion and that he had clearly grasped the import of the phrase, viz. 'praying in the same spirit' as before (not:

'repeating the same words', vide infra). Since he did not hesitate to use the redundant phrase *πάλιν ἐκ δευτέρου* at v.42, it is clear that he spared the Markan phrase in question not in the interest of economy: on the contrary, that was the only way he could introduce the poignant words of Jesus' self-surrender on the second occasion.

(ii) Possibilities of Omission

What are the possible causes of omission from a supposedly long reading?

(a) Perhaps these four words originally constituted a stichos in an ancestor of D, thereby leading to an oversight in copying<sup>5</sup>.

Considering that the words are made up of just 18 letters, this possibility cannot be ruled out (cf. on 2:22; Lk 12:19).

(b) An equally valid surmise would be a deliberate deletion due to the apparently superfluous nature of the phrase<sup>6</sup>.

The fact that the same MSS which omit this phrase also omit *πάλιν* in the adjacent v.40 strengthens this view<sup>7</sup>. There is no question of omission due to homoeoarcton involving the *καὶ πάλιν*

of successive lines, because in that case the intervening words, ἀπελθὼν προσήύξατο would also have been dropped.

(c) The least likely reason for the deletion is that offered by Lagrange<sup>8</sup>: "Peut-être a-t-on omis parce que cette phrase n'était pas en harmonie avec les termes exprès de la seconde prière dans Mt".

It must be emphatically pointed out that this kind of argument is quite baseless because deletions in the interest of such minute harmonistic details are unknown in the history of textual transmission. The edited Synopsis of this pericope reveals numerous textual emendations other than the present one; in none of them can we point to an instance of omission for the sake of harmonization. As is well-known, scribal harmonizations, as a rule, had to do with borrowing from parallel passages, and not in the dropping of words and phrases.

#### Intrinsic Probability

The textual problem relating to the present context boils down to one basic issue: whose account of the Agony in Gethsemane is more original - Mark's or Matthew's? At the outset it must be pointed out that there is nothing non-Markan

or non-Matthaeian about this phrase. But we have already examined in detail the reasons why it should belong originally to Mark rather than Matthew (pp.301f).

Commentators do not agree among themselves as to the exact meaning of the phrase. Our examination of the origin of this phrase has also shown that its very application by Matthew in connexion with the third act of prayer suggests that the First Evangelist had rightly understood its sense: 'praying in the spirit of the first petition'. This view is shared by Swete and Lagrange, while Rawlinson and Taylor take it in the literal sense: 'repeating the same words'<sup>9</sup>. That the wording of the two petitions is quite different in Mt is our strongest argument to suggest that Mark himself was most likely to have meant the phrase in the non-literal sense. Rawlinson, however, lays stress on the theological motive behind Matthew's wording at v.42 par. Mk v.39 when he says that it is "so calculated as to suggest that Jesus has by now attained to a calmer acceptance of the Divine will, together with a growing realization that the 'Cup' cannot pass away except He drink it". If one can thus detect a definite design behind v.42 (the second occasion of prayer), it can equally be true of the first and the third; and so it was by

choice that Matthew shifted the Markan phrase τὸν αὐτὸν λόγον εἰπὼν to v.44.

The following consideration also confirms the Markan origin of the phrase. Wansbrough has called attention to a three-fold repetition of many events concerning the life of Jesus in the Synoptics as well as in John's Gospel<sup>10</sup>: e.g. the three Temptations, the three prophecies of the Passion, the three denials of Peter, the three declarations of Jesus' innocence by Pilate (Jn 18:38; 19:4; 19:6), the three-fold commissioning of Peter (Jn 21), the last two cases being unique to the Fourth Gospel. He suggests that such repetition may be due to 'stylization' - "a recognized means of emphasis". "Many of Jesus' phrases are found also in various places of the gospels, which...suggests that details of the scene, especially of Jesus' words (sc. in Gethsemane), may derive from the evangelists..."<sup>11</sup>. If this view is acceptable, one may go a step further and point out that a two-fold repetition is also a distinct feature of the Gospels. Cf. the two-fold crowing of the cock (Mk 14:30,72, Cf. Mt 26:34,75); Peter's two-fold assertion of his loyalty to Jesus (14:29,31); the High Priest questions Jesus twice (14:60,61, Mt 26:62,63, cf. Lk 22:67,70); so also

does Pilate (15:2,4, par. Mt 27:11,13). (The only parallel that one can think of in Jn is the Ἀμήν ἄμήν put in the mouth of Jesus before solemn statements). This goes to strengthen our view that the two-fold repetition was also a conventional form of emphasis which Mark used with advantage in relation to the events of Passion Week.

Applying these insights to the scene in Gethsemane, we cannot fail to notice an interplay of the three-fold and the two-fold styles of repetition in Mk. In addition to the reference to the thrice-repeated prayer (actually Mark does not specifically mention the third act of prayer, but it is certainly implied in the words, καὶ ἔρχεται τὸ τρίτον, v.41), we may legitimately assume that Mark had a reference to the effect that Jesus repeated the words of the first petition<sup>12</sup>: in other words, the phrase τὸν αὐτὸν λόγον εἰπὼν is very much in keeping with Mark's treatment of several other occasions relating to the Passion.

Of course, none of the Gospels mentions any eye-witness to the actual prayer scene in Gethsemane. The three trusted disciples were asleep most of the time and were rather physically removed

from Jesus. Unlike . . . the case of the story of the Temptation, there was no chance of Jesus' conveying His experience of agony to the disciples. The only possible eye-witness to the scene is the young man with the linen cloth (14:51) who was 'following' ( συνηκολούθει ) Jesus and His companions. I do not see any reason why we should reject the well-known theory of identifying this young man with our author himself<sup>13</sup>. His house would have been the venue of the Last Supper as well as of the meeting together of the believers in the post-Resurrection milieu (Ac chs. 1;2;12)<sup>14</sup>. The considerations which encourage me to accept this hypothesis are two-fold:

(a) Why should this otherwise trivial incident be mentioned by an Evangelist, the author of the shortest Gospel, when he is dealing with the events concerning the supreme sacrifice of the Son of God?<sup>15</sup>

(b) The young man could not have suddenly appeared on the scene of the arrest had he not been secretly watching Jesus and His disciples from a vantage position. The fact that he was clad only in a linen cloth suggests that he had suddenly got out of his bed impelled by an intuitive compulsion or curiosity. This would, of course, have happened a short while

after Jesus and party had left the scene of the Last Supper. Although all this is in the realm of conjecture, its logic is very persuasive indeed.

If this theory has any semblance of reality, then Mark's account of the Agony in Gethsemane is most trustworthy, and the variations in the First Gospel account are in keeping with the usual freedom that Matthew exercises in his handling of Mark's treatise.

### Conclusion

Our acceptance of the disputed words at Mk 14:39 is in line with the opinion of many outstanding scholars. In addition to the names we have already enlisted in our support those of Souter<sup>16</sup> and Lagrange<sup>17</sup> may also be mentioned. Souter cites this as one of the four examples where he is confident that the Syr<sup>S</sup> reading (as against the omission in D and its Old Latin allies) is correct. Lagrange judges this as one of the texts where the long reading is "certain" or "almost certain". To this list of scholars may also be added those who have not bothered to treat this textual critical problem in their commentaries, presumably because of their confidence in the accuracy of the long reading<sup>18</sup>. The UBS Editorial Committee's

decision to free these words from brackets is a clear signal with regard to their authenticity<sup>19</sup>.

<sup>1</sup>St. Mark, p.555; cf. Cranfield, St. Mark, ad.loc.

<sup>2</sup>Essays in Memory of G.H.C. MacGregor, 1965, p.193; cf. Tasker, Greek N.T., ad.loc.

<sup>3</sup>Cf. Snodgrass, JBL 91 (1972), p.378.

<sup>4</sup>Ibid.; cf. Lagrange, Critique Textuelle, II, p.68.

<sup>5</sup>So Metzger, Textual Commentary, p.114.

<sup>6</sup>Cf. Snodgrass, loc.cit.

<sup>7</sup>Cf. V. Taylor, loc.cit.; notes this phenomenon, but draws a different conclusion.

<sup>8</sup>Critique Textuelle, II, p.68.

<sup>9</sup>Cf. their respective Commentaries on Mk, ad.loc.

<sup>10</sup>New Catholic Commentary, ad.loc.

<sup>11</sup>How widespread this tendency was among the Evangelists is amply demonstrated by Luke: he not only repeats the same theme, but also groups incidents in triads. Examples of the former trait may be found in the following places: (a) Lk 1:80, 2:40, 2:52; (b) Lk 4:14b, 4:37, 7:17; (c) Lk 22:16,18; (d) Lk 22:40,46; (e) Lk 22:67,70; (f) Lk 23:16,22c; (g) Lk 23:19,25; (h) Lk 24:26,46; (i) Ac 2:41-47; 4:32-35; 5:11-16; (j) Ac 6:7; 9:31; 12:24; 16:5; 19:20; [cf. also, (a) Lk 24:36-49; Ac 1:2-12 (instruction of the disciples by the risen Lord); (b) Lk 6:13-16; Ac 1:12f (list of the disciples); (c) Ac 9:1-22; 22:6-21; 26:12-23 (Paul's conversion); (d) Ac 10; 11:1ff; 15:7-9 (the Cornelius episode); (e) Ac 15:19; 15:28-29; 21:25 (the decree of the Jerusalem Council); (f) Ac 23:12-15; 23:20-21 (the plot against Paul)]. Instances of the latter Lukan characteristic can be found at Lk 22:25-60; 19:41-44; 21:20-21; 23:27-31; 23:14-16,20,22; 23:35,36,39; 23:47,48,49; 24:1-10, 13-35, 36-49; 24:15,34,36. (See A.M. Perry, Sources of Luke's Passion Narrative, pp.63f; H.J. Cadbury, "Some Features of Lukan Style"; in Studies in Luke-Acts, ed. Keck and Martyn; also, Cadbury, Beginnings of Christianity, Pt.I,V, ed. Lake and Cadbury, pp.392-402.)

<sup>12</sup>D.F. Strauss, *The Life of Jesus* (E.T.), 1898, p.640, has a slightly modified form of our approach; but still his evidence too confirms our position: "(Matthew and Mark) count the retirements of Jesus, marking them by the expressions ἐκ δευτέρου and ἐκ τρίτου ". This "at once shows that the number three was a point of importance to them, and when Matthew, though he certainly gives in the second prayer an expression somewhat different from that of the first, in the third makes Jesus only repeat the same words, τὸν αὐτὸν λόγον, and when Mark does this even the second time, this is a significant proof that they were embarrassed how to fill up the favourite number three with appropriate matter".

<sup>13</sup>Cf. J.W. Doeve, *StEv 1* (= TU 73) (1959), pp.476-7.

<sup>14</sup>Cf. O. Cullmann, *Early Christian Worship*, p.9.

<sup>15</sup>The cutting off of the ear of the High Priest's servant by one of the disciples (14:47) is not so trivial because it was a misguided attempt at self-defence.

<sup>16</sup>*Text and Canon*, p.128.

<sup>17</sup>*Critique Textuelle*, II, p.68.

<sup>18</sup>Cf. J. Weiss, *Schriften des N.T.*, ad.loc.; B. Weiss, (*Meyer's Kommentar*), *Markus u. Lukas*, ad.loc.; A. Loisy, *Marc*, ad.loc.

<sup>19</sup>Cf. Metzger, loc.cit.; T. Keim, *Jesus of Nazara* (E.T.), vi, p.14, n.1. Despite his theory about the priority of Mt, he confirms our conclusion with regard to this text.

II.C. THE LUKAN 'NON-INTERPOLATIONS'

a. Lk 5:39: The Parable of the Connoisseurs  
of the Old Wine

Disputed words: [καὶ οὐδεὶς πινὼν παλαιὸν . . . . . χρηστός ἐστιν.]

External Evidence:

Nestle<sup>26</sup>

Greek NT<sup>1</sup> (UBS)

καὶ οὐδεὶς πινὼν παλαιὸν θέλει	[καὶ] οὐδεὶς πινὼν παλαιὸν θέλει
νέον· λέγει γὰρ ὁ παλαιὸς χρηστός	νέον· λέγει γὰρ, Ὁ παλαιὸς χρη-
ἐστιν. (unverändert)	στός ἐστιν.
Vers P <sup>1</sup> . 75 x̄ ABC KLW X Δ Θ Π Ψ f <sup>1</sup> f <sup>13</sup> 28 33 565 700 892 1009	
1010 1071 1079 1195 1216 1230 1241 1242 1253 1344 1365 1546	
1646 2148 2174 Ɔ Ɔ Lect aur f q vg syr <sup>p</sup> . h. p <sup>31</sup> sa bo goth arm georg II	
— (Mt. 9, 17; Mk. 2, 22) D a b c d e f f <sup>2</sup> r <sup>1</sup> Mcion Ir Eus	

Observations

(i) The fact that the majority of witnesses including the 'best' read the long text here is a pointer to the direction in which the balance of probabilities lies<sup>1</sup>. The Greek witnesses have also the support of the Vulgate and three Old Latins as well as the other ancient Versions.

(ii) The best of the Old Latin authorities, however, support the short text in the company of D. The evidence of Marcion, Irenaeus and Eusebius is also not to be ignored.

We shall see how the Internal Evidence supports the impression produced by the MS data.

## Internal Evidence

### Transcriptional Probability

#### (i) Possibility of Insertion

It is very difficult to believe that some one other than the author himself would undertake to insert this saying at the end of the two parables. For although there are some striking resemblances in regard to these parables in the other two Synoptics, the saying in v.39 is unique in Lk<sup>2</sup>. The statement of Snodgrass, "There is no apparent motive for the addition of these words"<sup>3</sup>, well sums up the general view of scholars on this. As a matter of fact the omission can be better explained than the addition.

#### (ii) Possibilities of Omission

(a) Marcionite influence is a plausible factor as the textual evidence would suggest<sup>4</sup>. But Marcion's real objection would not have been against the enjoyment of wine, as made out by Aland<sup>5</sup>, because in any case we find the mention of wine in association with Jesus retained in vv.37,38 and in 22:18. On the other hand Marcion's well-known abhorrence of anything that smacked of tolerance of Judaism may have operated behind the omission. Cf. Lagrange: "Le v. a pu paraître beacoup trop favorable à l'ancienne loi, si on prenait pour acquis le proverbe

que 'le vieux est meilleur'. Dans ce cas l'omission serait marcionite"<sup>6</sup>.

(b) Assimilation to the parallel passages Mk 2:22, Mt 9:17 has been suggested by Burkitt<sup>7</sup>.

This is, however, not convincing.

One is at a loss to understand why harmonization did not proceed in the reverse direction by borrowing from the Lukan verse which would have been more in keeping with the tendency of the scribe behind Codex Bezae. Burkitt himself is not convinced about this since he proposes two other possibilities including that of interpolation.

(c) Burkitt<sup>8</sup>, Alford<sup>9</sup> and Aland<sup>10</sup> suggest an alternative possibility of deletion due to the difficulty of understanding the passage. "Auch jemand, der nicht aus theologischen oder ethischen Gründen gegen den Weingenuss war, konnte an diesem Exkurs in die Weinkunde - wie es schien wenn man den Skopus des verses nicht verstand - Anstoss nehmen"<sup>11</sup>.

In answer to this one must state that there is no evidence to substantiate this argument even if we concede such a possibility (contrast on 10:41-42, where we have at least some MS evidence to justify our suspicion). One must also bear in mind that

scribes were not so intolerant of all difficult passages, as is evidenced by their retention of passages such as Mk 7:19b; 9:49,50; Lk 7:35; 11:49a; 16:16 etc.

### Intrinsic Probability

The juxtaposition of the third parable of v.39 with the foregoing two has caused considerable difficulties to commentators and has also given rise to a lot of speculation. Montefiore's evaluation of the meaning as "obscure" and "puzzling"<sup>12</sup> seems to be unjustified. Harrington regards the verse as alien to the context and conjectures that the author himself has interpolated it here, having been prompted by the catchwords *οἶνος* and *νέος* (vv.37,38)<sup>13</sup>. He is supported by Creed<sup>14</sup> as well. In view of such confusion it is important to put the role of v.39 in perspective.

It is true that like Mark, Luke is at times inclined to link isolated sayings of Jesus by means of catchwords which have no inherent connection with each other (Mk 9:33-50 cf. Lk 16:1-13, 14-18). But that is not the case in the present context. Like Matthew, Luke also betrays his dependence on Mk in the pericope 5:35-39. At the same time he

also reveals his independence of Mk at v.36.

In Mk 2:21 = Mt 9:16 we have the first parable involving the piece of cloth used to patch up an old garment; the result of such mending would be a worse tear. Luke has turned this figure into a piece cut out from a new garment in order to patch up an old cloth; the result in this case also would be unsuccessful; but Luke describes it more elaborately: the new garment is already destroyed; then the new piece will not match the old cloth. The wording of the second parable which employs the metaphor of the wine and wineskins is virtually the same in all the three Synoptics. But the additional parable in v.39 is peculiar to Luke. It has, of course, the catchwords *δι' υ'σ* and *νε'ος* which link it with the foregoing parables. But that is not what constitutes the rationale for its present position; it has an originally designed role in the total context of the entire pericope. Alford observes: "It contains the conclusion of the discourse and the final answer to the question in v.38, which is not given in Mt. and Mk."<sup>15</sup>. I fully endorse his view that v.39 provides Luke's final answer to the question on fasting. But I do not agree with him that Matthew and Mark leave the question unanswered. On the contrary, in our discussion of

Mk 2:22 we have tried to show how the concluding statement of each Evangelist serves his purpose well and how the differences in these conclusions arose from a difference in emphasis of each author.

Leaney also has recognized the significance of v.39 in the author's intention: "In the original the saying (sc. he means the pericope as a whole) seems to have hinted that the new substance of the life and teaching which Jesus has brought must be cast into new forms. Luke emphasizes a different point, viz. that the new teaching will not match the old and that it will not be accepted by those who have grown fond of the old"<sup>16</sup> (italics mine). But Leaney puts the wrong emphasis on the italicised words because it is not the inevitability of the non-acceptance of the new by the adherents of the old that Luke wants to highlight; rather he is emphasizing the 'why' of this indifference to the new and also the need for forbearance on the part of the followers of the new way.

Montefiore also misses the mark when he comments on this verse: "It may suggest that the disciples of John may justifiably continue in their ancient practices, while the disciples of Jesus must not imitate them"<sup>17</sup>.

By means of his peculiar wording in regard to the first parable, Luke implies that Jesus had emphasised the need for a new frame of mind both to comprehend and to practise His teaching. If the adherents of the old religious establishment were to understand His teaching they had to renounce their legalistic and ritualistic approach to God and to imbibe a fresh apprehension about religious obligation. Half-hearted obedience to His teaching would only mean that in the end men will be left with neither the old nor the new. Now the additional parable at v.39 underlines Jesus' advocacy of a balanced and realistic attitude towards those who are slow to catch up with the implications of the new because of their indulgence in the old. Just as connoisseurs of old wine are not even prepared to taste new wine, so scrupulous followers of the old system are so self-content that they do not even want to try the new. They will say: 'the old is good enough for us' ( *χρηστός* represents an Aramaism which expresses the comparative by means of the positive<sup>18</sup>. This suggests an early Aramaic source behind the passage and is therefore likely to be authentic: vide evidence from the Gospel of Thomas, infra).

Luke, however, is neither a pessimist nor a cynic. So he takes particular care to present this valuable insight from the dominical utterances in order to emphasize its relevance for mission to the Church of his day. By means of this additional parable he may have meant to convey the message of patience and optimism to his fellow-Christians in the face of set-backs during their evangelistic outreach to the Jews in particular. Grundmann recognizes here an allusion to the problems between the Jewish and Gentile Christians in the early Church<sup>19</sup>. But it is not so much the tension within the Church as the reluctance of the Jews to appreciate the worth of Christianity that is the main burden of this last parable. Plummer puts in a nutshell the gist of the foregoing exegesis: The first two parables "show how fatal it would be to couple the new spirit of the Gospel with the worn-out forms of Judaism", while "the third shows how natural it is that those who have been brought up under these forms should be unwilling to abandon them for something untried. The conversion of an outcast *τελώτης* who has no such prejudices, may be easier than one whose life is bound up in the formalism of the past"<sup>20</sup>. The same idea is sustained by Stuhlmüller: "Luke insists that anyone rigidly and proudly bound up with the

past can scarcely sustain the vitality of the new"<sup>21</sup>. This aspect of human nature has perhaps been nowhere better demonstrated than in the mission fields of the orient, where, while the down-trodden and outcast people have found a new freedom in Christianity, the staunch followers of the ancient religions have generally shown a remarkable resistance against it.

I think that in the light of the above exegesis, the word *εὐθεύς* inserted before *θελεῖ* in TR A<sup>0</sup> f<sup>13</sup> Vg, some Old Latins and a few other Versions should be retained. Otherwise the note of optimism in Luke's message loses much of its force. The omission of the word can be explained in terms of a failure to grasp its implication. I do not find common ground here with Creed: "The addition of *εὐθεύς* suggests that in time he may change his mind; this introduces another thought and weakens the saying"<sup>22</sup> (cf. the evidence of the Gospel of Thomas, *infra*).

Several commentators think that the parable of v.39 originally did not belong to this context. They derive this conclusion from its absence in Mt and Mk. But even they deny neither its dominical character nor its Lukan insertion at

this place<sup>23</sup>. Harrington notes: "While its original sense is not clear, Luke would appear to understand it as the sad reflection of Jesus that the new wine he offers is not to the taste of those who have drunk the old wine of the Law"<sup>24</sup>. It may be difficult to concur in his view that this was a "sad reflection" on the part of Jesus because it would be wrong to ignore the Gospel presentation of Him as one with great compassion. He could be expected to have shown great sympathy for those who had genuine intellectual difficulties in following Him. We know from the Gospels that while He condemned the wilful and fanatical opposition of the Jewish leaders in blindly ignoring God's manifest power at work through Himself, He did evoke the appreciation of at least a section of the leadership (Mk 12:28) (cf. Joseph of Arimathaea, Nicodemus). Jesus is reported as commending a Pharisee for his spiritual insight (Mk 12:34). Therefore we have every reason to believe that the statement of v.39 was not spoken in a cynical vein. Even if it had been uttered in a different context, the theological insight with which Luke inserted it in this setting deserves appreciation. As Godet observes: "The independence of Luke's text, and the originality of its sources, come out clearly from this last passage,

which forms such an excellent close to this portion"<sup>25</sup>. Leaney's remark that "Luke has missed the point of the original in Mk ii.21"<sup>26</sup> is to do injustice to Luke's genius.

The Evidence from the Gospel of Thomas brings out the primitive nature of this passage. In Logion 47 we read the three parables under discussion in words which are reminiscent of the Lukan text, though in a transposed order:

"No man drinks old wine and immediately desires to drink new wine; and they do not put new wine in old wineskins, lest they burst, and they do not put old wine in a new wineskin, lest it spoil it. They do not sew an old patch on a new garment, because there would be a rent"<sup>27</sup>.

This shows that the grouping together of the three parables was most probably not the work of Luke, but that they belonged together to a very early tradition. The essential differences of the Lukan pericope from that of Mark suggest that Luke had another source besides Mk to consult. Mark's

information may have come from oral tradition. Whether Mark's source contained the reference to those who taste wine is difficult to say. It is possible that Mark decided to end his section on the question of fasting on a different note, using a popular proverb (cf. on Mk 2:22). If the transposition of the third parable to its present position is due to Luke, it is a tribute to the originality of Luke the theologian. Resch confidently asserts that v.39 should be retained: "als ein Zusatz von der Hand des dritten Evangelisten zu erachten". He also confirms that the words of this pericope ultimately go back to the "Logia"<sup>28</sup>.

### Conclusion

It is surprising that Westcott-Hort should have expressed doubt about the authenticity of this passage. Scholarly opinion, as we saw, is overwhelmingly in favour of its retention. In fact I have hardly come across any dissenting voice on this issue. This is one of the readings of which Lagrange has almost absolute certainty<sup>29</sup>.

<sup>1</sup>Metzger, Textual Commentary, p.138; "The external attestation for the inclusion of the verse is almost overwhelming".

<sup>2</sup>Aland, NTS 12 (1965-66) p.201: "Er steht weder bei Markus noch bei Matthäus, wer sollte ihn eingefügt haben, wenn nicht der Verfasser?"

<sup>3</sup>JBL 91 (1972), p.374.

<sup>4</sup>Zahn, GK, I, p.681; Harnack, Marcion, TU XV (1921), Beilage IV, pp.224f.

<sup>5</sup>Loc.cit.; cf. Snodgrass, loc.cit.

<sup>6</sup>Critique Textuelle, II, p.68; cf. Godet, St. Luke (ET), I, p.283; Lampe, Peake's Commentary, ad.loc.

<sup>7</sup>Evangelion, II, p.230; cf. Snodgrass, loc.cit.

<sup>8</sup>Loc.cit.

<sup>9</sup>Greek NT I<sup>5</sup>, ad.loc.

<sup>10</sup>NTS 12, (1965-66), p.200.

<sup>11</sup>Ibid.

<sup>12</sup>Synoptic Gospels, II, ad.loc.

<sup>13</sup>New Catholic Commentary, ad.loc.

<sup>14</sup>St. Luke, ad.loc.

<sup>15</sup>Greek NT I<sup>5</sup>, ad.loc.

<sup>16</sup>St. Luke, p.128.

<sup>17</sup>Loc.cit.

<sup>18</sup>Cf. M. Black, Aramaic Approach<sup>3</sup>, p.117. Most commentators are agreed on this rendering of *Χριστός*. Plummer, St. Luke, ICC, ad.loc., rightly says that

"the comparative merits of the old and the new wine are not touched by the parable, but the taste for them"; cf. Gore's Commentary, ad.loc; Alford, Greek NT I<sup>5</sup>, ad.loc.: good enough. Contrast J. Jeremias, Parables (ET), p.80 and n.84.

<sup>19</sup>Lukas<sup>2</sup> (1961), ad.loc.

<sup>20</sup>St. Luke, loc.cit.

<sup>21</sup>Jerome Commentary, ad.loc.

<sup>22</sup>St. Luke, p.84; cf. B. Weiss, Meyer's Kommentar, p.362, n; Contrast Loisy, St. Luke, I, pp.282-83.

<sup>23</sup>Cf. J. Jeremias, Parables of Jesus (ET), p.94.

<sup>24</sup>Loc.cit.

<sup>25</sup>Op.cit., p.283.

<sup>26</sup>Loc.cit.; cf. A. Kee, NovT 12 (1970), p.13. "There are many instances in the Gospels of words of Jesus being misapplied"; cf. Mt 20:16; "In fact the ending which Luke adds to his account of the double parable destroys the meaning of the parable on any interpretation...Jesus may well have spoken these words to illustrate his teaching on some matter, but the sentence is out of place here. We cannot assume that even if the double parable is authentic it is appropriately applied..." Jeremias, Parables (ET), p.80, qualifies the Lukan addition as "unfortunate"!

<sup>27</sup>Cf. G. Quispel, NTS 5 (1958-59), p.281.

<sup>28</sup>TU X,2 (1895), p.56.

<sup>29</sup>Critique Textuelle, II, p.68. See also Seesemann, TDNT, V, pp.162ff.

## II.C

b. Lk 10:41-42: Jesus' Reply to Martha:

The Most Excellent Choice

Disputed words: [μεριμνᾶς καὶ . . . ὀλίγων δὲ ἐστὶν  
χρεῖα ἢ ἑνός]

External Evidence:

Nestle<sup>26</sup>

Greek NT<sup>1</sup> (UBS)

μεριμνᾶς καὶ θορυβᾶζει περὶ μεριμνᾶς καὶ θορυβᾶζει περὶ  
πολλά, ἑνός δὲ ἐστὶν χρεῖα· (vor- πολλά, ἑνός δὲ ἐστὶν χρεῖα·  
her: . . . ὀλίγων δὲ ἐστὶν χρεῖα ἢ  
ἑνός)

txt P<sup>45, 75</sup> C<sup>\*, 3vid</sup> W Θ Bas || μεριμνᾶς καὶ τυοβάζει περὶ πολλά, ἑνός  
δέ ἐστὶν χρεῖα A K P Δ Π Ψ f<sup>13</sup> 28 565 700 892 1009 1010 1071 1079  
1195 1216 1230 1241 1242 1253 1344 1365 1546 1646 2148 2174  
ΣΠ 180<sup>m</sup>, 89<sup>m</sup>, 70<sup>m</sup>, 185<sup>m</sup>, 547<sup>m</sup> Cl Bas Mac Chr JohDamasc || μεριμνᾶς  
καὶ θορυβᾶζει (oder τυοβάζει) περὶ πολλά, ἑνός δὲ ἐστὶν χρεῖα aur f (q) vg  
syr<sup>c</sup>·p·h·sa || μεριμνᾶς καὶ θορυβᾶζει περὶ πολλά, ὀλίγων δὲ ἐστὶν χρεῖα  
ἢ ἑνός P<sup>3</sup> (N<sup>\*</sup> — χρεῖα) N<sup>c</sup> (B χρεῖα ἐστὶν) C<sup>2</sup> L f<sup>1</sup> 33 syr<sup>hms</sup> bo aeth  
Or<sup>1/2</sup> Bas Hier Cyr || μεριμνᾶς καὶ θορυβᾶζει περὶ πολλά, ὀλίγων δὲ  
ἐστὶν χρεῖα 38 (syr<sup>pal</sup>) bo<sup>ms</sup> arm georg Or<sup>1/2</sup> || μεριμνᾶς καὶ θορυβᾶζει  
περὶ πολλά c || θορυβᾶζει περὶ πολλά (Cl) Aug || θορυβᾶζει D d || —  
a b e ff<sup>2</sup> i l r<sup>1</sup> Amb Possidius

### Observations

The following features are readily  
observable among these divergent readings:

- (i) There is an introductory statement common to the first four sets, namely μεριμνᾶς καὶ θορυβᾶζει περὶ πολλά.
- (ii) All the short readings exhibit what appears to be a progressive shedding of a bit of the longest reading in the process of transmission through the different routes of tradition. An

alternative possibility suggested by the nature of the readings is that the longest reading could be a conflate of the first and third readings.

Plummer, however, rules out the latter possibility because the boh-eth-Orig combination is older than the witnesses for the third reading<sup>1</sup>.

(iii) Since even Codex Bezae testifies against a complete omission on both its Greek and Latin sides, the last reading can be ruled out straightaway.

(iv) In Plummer's judgment, the D reading itself is not authentic; but since that too makes good sense we cannot make such a conclusion based on External Evidence alone, although Codex Bezae has no other supporting authority.

(v) The poor attestation for the third, fourth and fifth readings makes their authenticity extremely suspect.

(vi) If we leave out the D reading the real contestants are the 'longest' reading headed by P<sup>3</sup> B S<sup>3</sup>\* etc. and the 'shorter' reading led by P<sup>45,75</sup> etc. Burkitt cites this passage as well as Lk 9:54ff to illustrate the eclectic nature of P<sup>45</sup>

in joining hands with the Caesareans except Cod.1<sup>2</sup>. But this judgment was made before the discovery of P<sup>75</sup> which now supports P<sup>45</sup>. The numerical strength of the witnesses on the side of P<sup>45,75</sup> is massive indeed. On the other hand, both the excellence and the antiquity of virtually the entire group that supports the longest text is so important that Scrivener<sup>3</sup>, Plummer<sup>4</sup>, Gore<sup>5</sup> et al are all impressed with this evidence. W. Manson<sup>6</sup> and Tasker<sup>7</sup> favour the shorter reading on the assumption that the former is a conflation. But Plummer has rightly questioned the wisdom of charging the B $\mathcal{Y}$  group with such a flaw. Probably all that we can say at the moment is that the MS evidence is rather evenly divided especially in view of the conflict between P<sup>75</sup> and B $\mathcal{Y}$ . In my judgment the diverse shorter readings indicate successive emendations made during various stages of textual transmission. Aland concurs in this<sup>8</sup>, and this will be confirmed by the Intrinsic Probability as well.

### Internal Evidence

#### Transcriptional Probability

##### (i) Possibilities of Insertion

(a) Snodgrass's statement that there is no apparent motive for addition<sup>9</sup> may not be shared by all; it is

indeed an over-simplification in his attempt to defend the P<sup>75</sup> text.

(b) Montefiore<sup>10</sup> suggests that the original reading may have ended with *ἑνὸς δὲ ἐστὶν χρεῖα* (AKF<sup>13</sup> et al), in the sense that Jesus meant to say, 'but I need few things', i.e. 'my wants are few', and that *πολλά* would then mean 'much and various food and elaborate entertainment'. The first scribal correction was in terms of a contrast between the physical need of Jesus and the really significant thing which he prized in life: 'but there is need of (i.e. I need) few things or one thing' (spiritual sense). Finally, to sustain the spiritual interpretation, the reference to the 'few things' was left out.

(c) A. Baker<sup>11</sup> holds Origen indirectly responsible for the evolution of a 'conflate' reading as represented by P<sup>3</sup> B N \* et al. In his commentary on Lk, Origen interprets the 'one thing' to mean 'loving one's neighbour' and the 'few things' to mean 'laws'. These, Baker says, were initially two distinct variants according to the Alexandrian and Caesarean traditions known to Origen. The latter's treatment of them led scribes to believe that both were integral

to the original text. Baker's chief basis for this conjecture is that in fragments of Origen's commentary on Jn now extant, he has the short form *ὀλίγων δὲ ἔστιν χρεῖα*, which is preferred by von Soden. According to this theory, the original text, as represented by P<sup>45,75</sup> et al, read only up to *ἑνὸς δὲ ἔστιν χρεῖα*: the *ἑνός* was understood to mean 'one dish'; in the next stage of textual transmission, scribes tried to make this sense more credible (who will entertain an honourable guest with just one dish!); so they replaced the *ἑνός* with *ὀλίγων δὲ ἔστιν χρεῖα*. At a later stage, under Origen's influence, the two readings got conflated.

This hypothesis calls for several comments: First, Origen's influence was, according to Wallace-Hadrill<sup>12</sup>, felt exactly in the reverse direction. For, although since his arrival in Caesarea in A.D.231, Origen had abandoned the Alexandrian Text in preference to the Caesarean, his reputation as a scholar resulted in the wide transmission of the Text he had brought with him. However, the divided testimony of the Alexandrian MSS in this context makes it difficult to decide whether Origen's earlier Text is to be identified with P<sup>75</sup> or the B $\mathcal{L}$  group. Wallace-Hadrill says that by the close of

the 3rd century, MSS such as the ancestors of  $\Theta$  and  $\Gamma$  were being influenced by the Alexandrian Text<sup>13</sup>. But as a matter of fact these MSS also reflect the division among the Alexandrian witnesses<sup>14</sup>, so that no clear picture emerges as to the original state of the Alexandrian Text. Further, one cannot see why Origen, with his deep interest in Textual Criticism, would have failed to mention any textual deviation in his commentary even if he preferred one Text to the other. It is also difficult to believe that scribes of the 3rd century were well-versed in Origen's Commentaries and that his commentary on Lk would be more influential with them than that on Jn. In any case, as was pointed out earlier, Baker joins the ranks of those who would not hesitate to assign a conflate reading to the B $\Delta$  group. If the longest text were not original, then Montefiore's theory would be more convincing than Baker's.

(ii) Possibilities of Omission

On the assumption that a longer reading was original, what are the chances of omission?

(a) The possibility of omission due to oversight has been put forward by Kilpatrick<sup>15</sup> and Metzger<sup>16</sup> along different lines. According to the former,

the reading of P<sup>45</sup> etc. ending in  $\acute{\epsilon}\nu\omicron\varsigma \delta\acute{\epsilon} \acute{\epsilon}\sigma\tau\iota\nu \chi\rho\epsilon\acute{\iota}\alpha$   
may be visualized in an exemplar as follows:

MAPΘAMAPΘA  
MEPIMNAICKAIΘO  
PYBAZHITEPITOLA  
ENOCΔEECTINXPEIA  
MARIAMΓAP . . . . .

There is a "slight resemblance between  $\acute{\mu}\acute{\alpha}\rho\theta\alpha$   
and  $\chi\rho\epsilon\acute{\iota}\alpha$  (sc. similar ending) and a closer one  
between  $\mu\epsilon\rho\iota\mu\upsilon\delta\acute{\alpha}\varsigma$  and  $\mu\alpha\rho\iota\acute{\alpha}\mu$  . Together  
they might account for the omission".

Although Kilpatrick dismisses this hypo-  
thesis with the above observation it is easily seen  
that none of the shorter texts conforms to his  
proposal. This does not satisfactorily explain  
precisely the actual differences among them. Metzger  
suggests homoeoarcton (  $\acute{\mu}\acute{\alpha}\rho\theta\alpha . . . . . \mu\alpha\rho\iota\acute{\alpha}\mu$  )  
as a possible cause of omission<sup>17</sup>. Even this fails  
to account either for the different patterns of  
omissions or for the retention of  $\acute{\mu}\acute{\alpha}\rho\theta\alpha$  in all  
the shorter texts.

(b) This passage indeed presents an illustration  
where none of the usual explanations would suit the  
actual state of affairs of the shorter texts. The  
MSS provide ample evidence that scribes had con-  
siderable difficulty with the rare verb  $\theta\omicron\rho\nu\beta\acute{\alpha}\lambda\epsilon\sigma\theta\alpha\iota$   
which has been often replaced by the more common  
 $\tau\upsilon\rho\rho\acute{\alpha}\chi\epsilon\iota\nu$  . So it should not be surprising if

a scribe, copying from a MS which had the form *θορυβάζη*, deliberately chose to omit that part of the sentence in order to get over a difficult passage. Thus Metzger's alternative suggestion as a possible cause of omission is more convincing: "The omission of both clauses (as well as *γάρ* after *Μαριάμ*) from it <sup>a,b,e,ff<sup>2</sup>,i,l,r<sup>1</sup></sup>, Syr<sup>S</sup> ...probably represents a deliberate excision of an incomprehensible passage..."<sup>18</sup>. Burkitt's statement that Syr<sup>S</sup> represents the original text is based on the fact that while the non-Syriac word *turb'* has been retained in Syr<sup>C</sup> as a transliteration, it is missing in Syr<sup>S</sup>. But he himself admits that "the *δέ* or *γάρ* after *Μαριάμ* at the beginning of v.42, which is an integral part of both the longer readings, is represented neither in C nor S"<sup>19</sup>. It would appear that the possibility lies in the opposite direction, namely that both the *θορυβάζη* and the *γάρ* got omitted in Syr<sup>S</sup> as well as in the *Itala*, as Metzger suggests, because the scribe could not make sense out of them.

Of course, this explanation gives a solution only in respect of the omission from Syr<sup>S</sup> and its *Itala* allies. As was hinted at the beginning, since there are several variant readings which represent a gradation in the length of the reading, the only

possible way in which this phenomenon could be explained is in terms of successive attempts to make sense out of an otherwise difficult text. The variations between these sets of readings betray a difficulty in comprehending the full purport of Jesus' reply involving the *πολλά* and the *ὀλίγων* on the one hand, and the *πολλά* and the *ἑνός* on the other (vide infra). While some scribes tried to get out of the situation by dropping the reference to either *ἑνός* or *ὀλίγων*, others tried to solve the problem by deleting the whole thing beyond either *θορυβάζη* or *πολλά*.

(\*) Easton postulates that the impulse of some scribes who refused "to admit that Christ could speak of any food as necessary" might have led to a deletion resulting in the shorter texts<sup>20</sup>.

This is not at all tenable in view of the sundry other genuine texts where Jesus is reported as taking food and receiving hospitality even from the Pharisees. (cf. especially 7:34)!

#### Intrinsic Probability

The context in which Jesus' reply to Martha is reported is very important in arriving at a sensible solution to the textual problem of this

passage<sup>21</sup>: Jesus is chief guest in a home where He is most respected and loved. True to the traditions of oriental hospitality, Martha is running about the house, anxious to prepare a grand meal for the Master and His disciples. Jesus' reply is in response to Martha's complaint about her sister's apparently callous indifference to her concern to entertain the family guests. While Jesus did welcome the hospitality of those who could afford it, He encouraged it only in so far as it enabled them to have the privilege of participating in His mission of extending the Kingdom of God (cf. 8:1-3). He Who "came not to be ministered unto, but to minister" (Mk 10:45 cf. Lk 22:27) did not want to impose Himself as a liability on others. So it would be only natural if in His reply to Martha Jesus emphasized that she need not be unduly troubled about feeding Him because, as far as He was concerned He needed only the barest minimum number of dishes. Then he goes on to qualify this statement<sup>22</sup> by introducing a play on words<sup>23</sup> as He was accustomed to do (cf. Jn 3:3; 4:10). The *ἔγὼς δὲ ἔστιν χρεία* anticipates *τὴν ἀγαθὴν μερίδα*. It has both a material sense (surface meaning) and a spiritual sense (deeper meaning). Godet rightly stresses this dual characteristic of Jesus' words when he asserts: "It

is simpler to hold that by the expression one thing, Jesus meant to designate spiritual nourishment, the divine word, but not without an allusion to the simplicity in physical life which naturally results from preponderance given to a higher interest"<sup>24</sup>.

The material sense of Jesus' statement refers to the choicest portion ( *μερίς* ) reserved for the guest (Gen 43:34; 1 Sam 1:4; 9:23; Neh 8:12)<sup>25</sup>. In the spiritual sense it signifies the imperishable joy and satisfaction of listening to the eternal Word of God. Jesus' reply contains a gentle invitation to Martha to follow her sister's example of making this excellent choice.

Thus the longest of all the series of texts reads quite naturally in its setting and is most likely to be original. Without the progression from *ὀλίγων* to *ἑνός* Jesus' reply would sound so unnatural and harsh. It would be unnatural because no meal is worth the name, whether in the orient or in the occident, if it consists only of one item. It would be harsh because it brushes aside Martha's most dedicated (though misguided) service<sup>26</sup>, and shows a rashness on Jesus' part in driving home a spiritual message which is not warranted by the context. (Contrast Jesus' reply to Nicodemus, Jn 3:3,

where the context is different. Nicodemus is an earnest seeker after truth, but is also an intellectual, ὁ διδάσκαλος τοῦ Ἰσραὴλ . But in the case of the Samaritan woman, Jn 4, we notice a gradual progression in leading her to a knowledge of herself as well as of His own divinity. That Jesus met each person on his own emotional, intellectual and spiritual level is only too evident from the Gospel accounts. Hence a brushing aside of the sincerity of a devoted hostess in an impersonal manner would not be characteristic of Him). Since Martha was anxious and troubled about many things, the ὀλίγων and the ἑνός contrast well with the πολλά 27.

Alford's exegesis at this point is in keeping with our discussion so far. He explains that μεριμνάς refers to Martha's inner anxiety', while θορυβάτη signifies 'the outer bustle and confusion' which she manifested as a result of this. He also sees an indirect rebuke couched in the double vocative Μάρθα, Μάρθα . He comments: "I can hardly doubt that our Lord, in the first and most obvious meaning, indicated that simpler preparation would have been all that was needful, but the πολλά leads to the ἑνός , and that to the ἀγαθὴ μερίς , the ἑνός being the middle term of

comparison between the natural *πολλά* and the spiritual *ἀγαθὴ μερίς* "28. Ellis, on the contrary, ignores the physical aspect of the interpretation altogether: "Martha's concern was to be a proper hostess, Mary's to be a proper disciple. Jesus' concern, as ever, was to serve and fulfill His mission, not to have His rights as a guest. To Martha - and Luke implies, to all her daughters, - the Lord offers a gentle rebuke: don't let ordinary dinners spoil your appetite for the real dinner"29. The Gospel accounts do not, however, encourage us to take this one-sided view with regard to Jesus' attitude to hospitality. One wonders why he frequented this friendly home in Bethany in the midst of his strenuous ministry if it were not for a time of quiet and rest. (Mk 11:11,12,19,27 par. Mt 21:17; Jn 12:1ff; cf. Lk 21:37; Mk 11:1). So we reject all attempts to defend the readings which end only in *ἐνὸς δὲ ἑστίν ἡ χρεία*, based on the theory that the textual confusion had arisen due to a misunderstanding of the 'one thing' in the material sense. We take our stand with those who defend the longest form of the readings on the assumption that the variants have arisen out of an attempt to take only the spiritual sense of 'the one thing'30.

## Conclusion

Scholarly opinion is rather evenly divided on this vexed textual problem. Apart from the scholars already cited, those who are in favour of our position include Lagrange<sup>31</sup>, Resch<sup>32</sup>, B. Weiss, Holtzmann, Aland<sup>33</sup> and Dummelow. According to Lagrange<sup>34</sup>, Jerome had knowledge of the longest form of the text, but he himself preferred the shorter one after some Greek authority. Among those who approve of the reading of P<sup>45</sup> etc. may be mentioned Creed, Harrington<sup>35</sup>, J. Weiss<sup>36</sup>, Loisy and Zahn<sup>37</sup>. Easton's defence of the longest text is based on an assumption which is different from ours. He says: "It is unnatural to apply the words in different senses, "a few dishes would have been enough or, indeed only devout attention to my words"! In other words, he is restricting himself only to the material sense<sup>38</sup>. But this is certainly a position which does violence to a "solemn speech of our Divine Master" which "has shaken many a pulpit, and sanctified many a life"<sup>39</sup>. This is indeed one of the several texts listed by Lagrange in his category of "Leçons longues, certaines, ou presque certaines".<sup>40</sup>

<sup>1</sup>St. Luke, ICC, ad.loc.

<sup>2</sup>JTS 33 (1932), p.366.

<sup>3</sup>Introduction, II<sup>4</sup>, p.349.

<sup>4</sup>Loc.cit.

<sup>5</sup>Gore's Commentary, ad.loc.

<sup>6</sup>St. Luke, ad.loc.

<sup>7</sup>Greek NT, p.420.

<sup>8</sup>NTS 12 (1965-66), p.201.

<sup>9</sup>JBL 91 (1972), p.374.

<sup>10</sup>Synoptic Gospels, II, ad.loc.

<sup>11</sup>CBQ 27 (1965), p.136. W. Alford, Greek NT I<sup>5</sup>, ad.loc., says that "the variations have arisen from understanding ἐνός to refer merely to the provisions then being prepared, then softening it by ὀλίγων, and finally combining both readings".

<sup>12</sup>HTR 49 (1956), p.111.

<sup>13</sup>Ibid.

<sup>14</sup>See Tischendorf, Novum Testamentum Graece<sup>8</sup>, 1, p.560.

<sup>15</sup>JTS 44 (1943), p.26; also JTS 50 (1949), p.151; cf. Easton, St. Luke, p.174, n.1.

<sup>16</sup>Textual Commentary, p.154; cf. Easton, loc.cit.; Snodgrass, loc.cit.

<sup>17</sup>Cf. A.C. Clark, Primitive Text, p.53.

<sup>18</sup>Ibid.

<sup>19</sup>Evangelion, II, p.294.

<sup>20</sup>St. Luke, p. 174.

<sup>21</sup>T. Keim, Jesus of Nazara (ET), iii, p.151, refers to the present passage as one of the examples of Jesus' "house-and-table-talk". Although he thinks that "Luke's copious collection of table-talk leaves much to be desired with reference to occasion and genuineness", he goes on to confirm the longest text in this case. B. Reicke, TU 73 (=St Ev.18), 1959, pp.206-216, places this story as well as the story of the Samaritan village in the setting of "Instruction and Discussion in the Travel Narrative". Cf. B.W. Bacon, JBL 37 (1918), pp.20-53.

<sup>22</sup>Leaney, St. Luke, p.183, rightly calls it "an after-thought" on the part of Jesus.

<sup>23</sup>J. Weiss, Schriften des NT, p. 430, confirms that "Das ist ein Wortspiel", but wrongly attributes this to a later scribe! B. Weiss, however, has weighed the evidence correctly when he refers to "the ingenious play upon words lying in the best certified reading, which, on account of its very difficulties, is indubitably original". [The Life of Christ (ET), iii, p.203, n.1; Cf. Meyer's Kommentar<sup>8</sup>, p.446].

<sup>24</sup>St. Luke, II (ET), p.45.

<sup>25</sup>Cf. Field, Notes on Translation, pp.63-4.

<sup>26</sup>So Aland, loc.cit.

<sup>27</sup>Holtzmann, Hand-Commentar, p.214.

<sup>28</sup>Greek NT I<sup>5</sup>, ad.loc.

<sup>29</sup>St. Luke, p.160.

<sup>30</sup>Lagrange, Luc, ad.loc.

<sup>31</sup>Critique Textuelle, II, p.68, Unless otherwise stated, the rest of the authors are named after their Commentaries on Lk.

<sup>32</sup>Resch, TU X, 2 (1895), pp.223f.

<sup>33</sup>Loc.cit.

<sup>34</sup>Op.cit., pp.286, 288.

<sup>35</sup>New Catholic Commentary, ad.loc.

<sup>36</sup>Schriften des NT, p.430.

<sup>37</sup>Lucas<sup>4</sup>, ad.loc.

<sup>38</sup>St. Luke, pp.173-4.

<sup>39</sup>Scrivener, Introduction, II<sup>4</sup>, loc.cit.

<sup>40</sup>Lagrange, loc.cit.; cf. RB (n.s.) 10 (1913), p.508. An interesting evidence to confirm our decision in this connexion is provided by G.G. Willis, StEv 1 (= TU 73, 1959), pp.811-813. He points out that in the so-called Book of Mulling, a typical Vulgate MS of the Gospels written in Ireland in the latter half of the 7th century, the present context has paucis uero opus est uel etiam uno which is the equivalent of the longest reading of  $\mathcal{N}^*B$  etc. The only other Latin authority for this reading is Cassian (5th century). Willis conjectures that this text came to Ireland from Lerins, the monastery of Cassian. Cassian, in turn, had brought it from Egypt during his long travels in the East. "During the whole of the fifth and sixth centuries there was constant contact between Lerins and Ireland. So it is reasonable to assume that Cassian brought to Lerins, and eventually sent to Ireland in the fifth century by St. Patrick and others, a biblical text derived from Egypt, and in Greek. Thus there is no difficulty in believing that the Irish of St. Mulling's time may have possessed a Greek Gospel text derived from Egypt, and may have corrected their Vulgate exemplaria

by reference to it". The important point to note is that "in Mulling the Greek element is of a very interesting character, and is distinctively non-Western. Hardly ever does Mulling agree with the Western Greek text against other families. Generally speaking, when Mulling diverges from the Latin tradition [as here] to follow the Greek more closely, he is found in agreement with the Alexandrian or the Caesarean families, and not with the Byzantine or Western". Thus we have evidence to the fact that the longest reading in the present case, preserved in BY\* f<sup>1</sup> 33 etc. has a better claim to authenticity than the readings of D on the one hand, and P<sup>45</sup> W<sup>6</sup> on the other. The fact that P<sup>75</sup> (and C\*) supports the latter group here does not add to the reliability of its attestation.

II.C

c. Lk 12:19: The Happy-go-lucky Way of Life

Disputed words: [κείμενα εἰς ἔτη πολλά ἀναπαύου

External Evidence: φάγε, πίε]

Nestle<sup>26</sup>

Greek NT (UBS)

κείμενα εἰς ἔτη πολλά ἀναπαύου, φάγε, πίε (unverändert) κείμενα εἰς ἔτη πολλά ἀναπαύου, φάγε, πίε  
txt P<sup>45, 75</sup> X A B K L N W Γ Δ Θ Π Ψ 070 f<sup>1</sup> f<sup>13</sup> 28 33 565 700 892  
1010 1241 1424 381 aur f q vg sa bo sy<sup>c, s</sup> || κείμενα εἰς ἔτη πολλά  
ilr' || — D a b c d f f<sup>2</sup> e

Observations

(i) The text goes back to a very primitive tradition as is attested by P<sup>75</sup>. According to Burkitt<sup>1</sup>, the present parable including the disputed words is attested in the Diatessaron by Aphraates, 381. Since this evidence has not been questioned, we can assume it to be genuine. Thus we have a long line of Christian tradition dating back to the middle of the second century which supports these words.

(ii) Three of the Old Latins favour the long reading while three others leave out only ἀναπαύου, φάγε, πίε — this is an indication that both of the shorter readings are of secondary origin.

(iii) The attestation of Syr<sup>s</sup> along with the best of the MS tradition in support of the long reading is a positive sign<sup>2</sup>.

Thus the antiquity of the text as well as its presence in all branches of tradition encourages us to be confident of its genuineness even without considering the Internal Evidence<sup>3</sup>.

### Internal Evidence

#### Transcriptional Probability

Although many scholars have identified a possible background for the disputed words in Jewish Wisdom Literature, none has suggested that they could be attributed to a scribe. This is not surprising because of the very position of the words spread across two sentences - the lacuna in the short reading covers the last quarter of v.19a and the major length of the succeeding sentence except for a single word *εὐφραΐνου*. So the only plausible explanation is in terms of omission from a long text rather than addition to a short text. Therefore Aland is right in rejecting the possibility of an intrusion into the text arising out of the Sitz im Leben of the early Church<sup>4</sup>.

### Possibilities of Omission

The possible reasons that have been offered for the deletion are the following:

(a) The words might have constituted an offence to the Christian conscience because of the Epicurean philosophy of life that they adumbrate<sup>5</sup>.

(b) Some scribes might have debated in their mind as to how a 'soul' could 'eat' or 'drink'<sup>6</sup>.

Deliberate omission of the words due to either of these reasons is indefensible on one major consideration: If the words ἀναπαύου, φάγε, πίνε were deemed objectionable, why should the non-objectionable words κείμενα εἰς ἔτη πολλά which belong to the previous sentence be deleted? The first suggestion is indeed irrelevant when we remember that similar utterances in the mouth of Jesus and St. Paul (12:45; 15:30; 1 Cor 15:32, vide infra) have never come in for such objection. Moreover, the fact that three Old Latins (i, l, r<sup>1</sup>) retain the last-mentioned words shows that they were at any rate non-objectionable in some circles of Christendom and that the omission of the remaining words was due to some other cause. Further, the above theory does not explain why the words in question

were not rejected by such a staunch ascetic and encratite as Tatian.

With regard to Burkitt's suggestion, one has only to point to the fact that if scribes were bent on eliminating Scripture portions on account of their figurative usages they would have had to delete many passages in the NT. The simple truth that such objections would never occur to an ancient copyist as they do to a modern scholar has to be accepted.

(c) Lagrange's surmise: "Peut-être a-t-on choqué de cette redondance apparente..."<sup>7</sup> is not at all convincing. As a matter of fact there is no redundancy about these words; they only add to the graphic narration of the parable as a whole. One can easily point out many other instances in the NT where more glaring redundancies have been left intact without scribal interference (cf. Mk 1:32; Mt 26:42). Thus we are led to look for the solution to the mystery of the missing words in some unconscious process rather than in a wilful act of suppression.

(d) Easton's explanation in terms of homoeoteleuton occurring between  $\pi\theta\lambda\lambda\acute{\alpha} \dots \pi\theta\lambda\lambda\acute{\alpha}$  is also not satisfactory<sup>8</sup>. This solution is of course applicable only to the first part of the missing words. The remain-

ing words, ἀναπαύου, φάγε, πίε, according to him, were deliberately left out as being inappropriate for the "soul" (cf. Burkitt's view, supra.)

In view of the resemblance of the lines

ΨΥΧΗ ΕΧΕΙ ΤΟ ΛΛΑ ΑΓΑΘΑ  
ΚΕΙΜΕΝΑ ΕΙ ΣΤΗ ΤΟ ΛΛΑ,

this hypothesis is apparently convincing. But the first objection to this theory is that a scribe who has paused over this verse, reflecting over the inappropriateness of the latter part in conjunction with the ψυχῆ of the first part, is least likely to have overlooked the intervening part! Accidental omissions involving scribal oversight necessarily imply hurried and mechanical copying devoid of reflexion on what was being copied. The second objection has already been dealt with in connexion with Burkitt's suggestion. This theory, however, could be upheld in respect of i, l, r<sup>1</sup>.

(e) The most plausible explanation relating to the omission has been offered by A.C. Clark. He says that there is reason to believe that D was ultimately derived from an ancestor consisting of 10-12 letters per line<sup>9</sup>. In this exemplar, the disputed words constituting 33 letters may have occupied 3 lines.

But he also states: "The omissions of D must be treated with caution when they occur at the end of a  $\sigma\tau\acute{\iota}\chi\omicron\varsigma$  , since here a line may have been lost both in the Greek and the Latin, though a double omission is not very likely"<sup>10</sup>. Then he cites Lk 12:19 as an example of this type of omission. This would mean that in the immediate exemplar of D all the 33 letters must have constituted a sense-line. There is no other way in which they could all escape the eye of a copyist.

If this suggestion is not acceptable, then the only alternative is to consider the possibility of accidental obliteration of the words due to damage of the scroll. But this has to be viewed in the light of the partial retention of the disputed words in  $i, l, r^1$ , and also of the fact that 12:21 is missing in D a b d. Since  $i, l, r^1$  completely retain v.21, it is difficult to judge whether the deletion in both places was due to the same cause. On the other hand,  $c, e, ff^2$  which omit the disputed words at v.19, retain v.21. Since Easton's theory of accidental omission at v.19 holds good for  $i, l, r^1$ , it is reasonable to uphold Clark's theory in respect of the omissions in Codex Bezae and its allies including  $c, e, ff^2$ . In other words, the group  $i, l, r^1$  which is an ally of the  $c, e, ff^2$  group in the retention of

v.21 could be judged to be an ally of the same group in the accidental omission at v.19. This would encourage us to believe that the other allies of this combined group in the omission at v.19, namely D a b d, could be judged to have brought about this omission through scribal oversight. Thus the evidence of the i,l,r<sup>1</sup> group in both v.19 and v.21 provides the clue to the cause of the omission in the present case.

#### Intrinsic Probability

The words in question, no doubt, add to the graphic narration of the story and are thus characteristic of the parables of Jesus. There is no reason why they should not form part of a dominical utterance recorded by Luke. The saying involved has parallels in Prov 27:1; Ecclus 8:15, 11:9, 29:12; Job 7:10; Wisd 15:18 cf. Is 22:13; 1 Cor 15:32; Jas 4:13-17. In fact Loisy expressly states that the fuller text here may have been inspired by Ecclus 8:15; but he does not use this as an argument to question the authenticity of the disputed words<sup>11</sup>. Loisy's suggestion is not at all surprising when we remember how steeped Jesus was in the Jewish lore. Jeremias points out that the word ἔτος is certainly Lukan<sup>12</sup> (cf. Lk 2:36; 4:25,31;

13:8; Ac 7:6,36; 24:17), as also the juxtaposition of  $\phi\upsilon\gamma\epsilon\acute{\iota}\nu$  and  $\pi\lambda\epsilon\acute{\iota}\nu$ <sup>13</sup> (Lk 7:33ff; 10:7; 17:8,27, cf. 12:45; 15:23; 21:34; 22:30). His statement, "Linguistic usage speaks for the originality of the Long Text", is to be viewed with great favour in this connection<sup>14</sup>.

### Conclusion

The Internal Evidence only lends support to the unusually strong External Evidence on the side of the long text at Lk 12:19. This is further strengthened by the presence of the same parable in the "Letter to Publius", a very ancient document, which employs almost identical words in respect of the rich man: "My soul, eat and drink and rest and be merry, because lo, much produce is stored up for thee for many years"<sup>15</sup>.

This parable is also quoted in Aphraates, 381 in very close verbal agreement with the above extract. "It is difficult to assign any sufficient cause for this marked agreement between the two and also the Old Syriac except a common use of the Diatessaron"<sup>16</sup>.

The Commentaries bear evidence of the puzzle or over-confidence of scholars with regard to the authenticity of this passage. Lagrange includes this among the list of passages where he is certain or almost certain of their genuineness<sup>17</sup>. Creed is non-committal, as also Plummer<sup>18</sup>, Wellhausen<sup>19</sup> and Blass<sup>20</sup> follow the shorter text. B. Weiss assumes its genuineness and passes over its textual problem<sup>21</sup>. J. Weiss<sup>22</sup> and Loisy<sup>23</sup> retain the words within simple brackets, but do not feel justified to drop them. The UBS edition of the NT is so certain of the genuineness of these words that they do not figure in the critical apparatus. Thus on the whole our decision in respect of this passage is safely on the side of the majority of modern scholars.

<sup>1</sup>Evangelion, II, p.133.

<sup>2</sup>Ibid, p.230; Cf. Souter, Text and Canon, p.128.

<sup>3</sup>Cf. Aland, NTS 12 (1965-66), p.201.

<sup>4</sup>Ibid.

<sup>5</sup>Ibid, p.202; Snodgrass, JBL 91 (1972), p.374.

<sup>6</sup>So Burkitt, loc.cit.

<sup>7</sup>Critique Textuelle, II, p.68.

<sup>8</sup>St. Luke, p.201, n.

<sup>9</sup>Primitive Text, p.25.

<sup>10</sup>Ibid, p.53.

<sup>11</sup>Luc, p.346.

<sup>12</sup>Cf. Eucharistic Words (ET), p.149, n.2.  
The relative statistics for the use of the word are self-explanatory: Mt: once; Mk: twice; Jn: thrice; but Lk-Ac: 26 times. Similarly Jeremias points to *κεῖσθαί* as "a favourite word of the Vorlage of Luke".

<sup>13</sup>Kilpatrick, JTS 50 (1949), p.151. He considers style as a major factor in favour of the authenticity of these words.

<sup>14</sup>See also Bauernfeind, TDNT, I, 1964, p.350.

<sup>15</sup>Cf. Burkitt, Evangelion, II, p.133 The said letter is catalogued in the British Museum under: B.M. Add 7190, fol. 19lv, sic ('Quotations' 72).

<sup>16</sup>Ibid. A similar monologue is also recorded in the Gospel of Thomas §72, although it is not exactly identical as this one.

<sup>17</sup>Loc.cit.

<sup>18</sup>Vide their respective Commentaries on Lk.

<sup>19</sup>Evangelium Lucae, ad.loc.

<sup>20</sup>Euangelium secundum Lucam, ad.loc.

<sup>21</sup>Meyer's Kommentar, ad.loc.

<sup>22</sup>Schriften des NT, ad.loc.

<sup>23</sup>Les Livres du NT, ad.loc.

II.C.

d. Lk 12:21 : The Rich Fool's Destiny

Disputed words: [ οὕτως ὁ θησαυρίζων ἑαυτῷ.... πλουτῶν ]

External Evidence:

Nestle<sup>26</sup>

Greek NT<sup>1</sup> (UBS)

οὕτως ὁ θησαυρίζων ἑαυτῷ καὶ μὴ οὕτως ὁ θησαυρίζων ἑαυτῷ καὶ μὴ  
εἰς θεὸν πλουτῶν (vorher αὐτῷ, εἰς θεὸν πλουτῶν  
sonst unverändert)  
txt (z. T. αὐτῷ) P<sup>45, 75</sup> A B K L W X Δ Θ Π Ψ 070 f<sup>1</sup> 28 33 565  
700 1009 1010 1079 1195 1230 1241 1242\* 1253 1344 1365 1546  
317/76. 185. 956 aur c e f f<sup>2</sup> i l q r<sup>1</sup> vg syr<sup>c</sup> s. p. h. palms sa bo arm georg II —  
D a b d II txt + ταῦτα λέγων ἐφώνει· ὁ ἔχων ὧτα ἀκούειν ἀκούειτω  
(Lk 8, 8; Mt. 11, 15) f<sup>13</sup> 892\* (892<sup>ms</sup> πρὸς ταῦτα) (1071 — ταῦτα...  
ἐφώνει) 1216 1242<sup>ms</sup> 1646 2148 Lect syr<sup>palms</sup>

Observations

(i) The External Evidence is overwhelmingly in support of the long reading: whereas D is followed only by three of the Old Latin authorities in its omission, the majority of the latter (including e) and the Old Syriac as well as P<sup>75</sup> B<sup>75</sup> retain the bracketed words<sup>1</sup>.

(ii) The divided witness of the Old Latins should not be brought in as an argument in the present case against the long reading as Aland does, not only because of the numerical strength of the witness against D, but also because of the support that they have from the Vulgate and the other Versions.

The support given by Syr<sup>S</sup> to the B $\mathcal{N}$  group is particularly significant.

(iii) The additional sentence,  $\tau\alpha\upsilon\tau\alpha\ \lambda\acute{\epsilon}\gamma\omega\ \acute{\epsilon}\phi\omega\gamma\epsilon\iota\ \dots\ \acute{\alpha}\kappa\omicron\upsilon\acute{\epsilon}\tau\omega$ , inserted at the end of v.21 in several MSS, need not engage our attention seriously as neither Westcott-Hort, nor any modern editors have considered it as genuine. Its spuriousness is at once evident not only from the poor attestation, but also from the various indications of scribal attempts at tampering with it. As Tasker says, these words are "a conventional sequel to sayings of a somewhat cryptic nature", and are evidently an insertion from Lk 8:8 or Mt 11:15<sup>2</sup>.

### Internal Evidence

#### Transcriptional Probability

##### (i) Possibilities of Insertion

If the verse did not belong to the original, why was it added?

(a) According to Creed, it was probably meant as "a transition to the discourse that follows"<sup>3</sup>.

(b) Harrington remarks that it "has all the appearances of a generalizing conclusion" and that "it serves to underline the truth that the man is indeed a 'fool' and to emphasize the worthlessness of worldly possessions"<sup>4</sup>.

Both these arguments, though attractive, will be proved to be wrong on consideration of the Intrinsic Probability.

(ii) Possibilities of Omission

What are the chances of omission from a supposedly long text? As a matter of fact it defies any satisfactory explanation. Since the parable of the Rich Fool is unique to Lk there is no question of the operation of the harmonizing tendency of scribes. The explanations suggested have to do with the following causes:

(a) The superfluity of the sentence<sup>5</sup>: On the contrary, far from being superfluous, the statement in v.21 is quite relevant to the context, as will be seen when we discuss the Intrinsic Probability.

(b) The apparent offence attached to the concept of riches - how can one be said to be 'rich' in the sight of God? Cf. Lagrange: "L'omission ne s'explique

guère aurait-on jugé la richesse mauvaise en soi de sorte qu'on ne pouvait pas être riche en vue de Dieu?"<sup>6</sup>: This suggestion is too subjective to deserve comment. Suffice to point out that those who raise such points fail in their duty if they do not undertake to explain why scores of statements in the Bible which are apparently of an objectionable nature especially when torn off from their context, still do not constitute any textual problem! Obviously, scribes were less discreet in their attitude towards Scriptural texts.

(c) Accidental omission: (Metzger, Textual Commentary p.160).

Metzger does not elaborate on the nature of the accident. Omission due to homoeoarcton or homoeoteleuton is out of the question. But it must be noticed that the length of this entire sentence is almost the same as that of the disputed words at v.19 except for the excess word πλουτῶν at the end. At v.19 we noted that there was a strong case for accidental omission of a sense-line caused by oversight. Since the authorities D a b d are common to both texts in their attestation of the omission, there is every possibility of believing that such an accidental error had occurred in an ancestor of D. Even if an omission due to negligence is not acceptable, there

is greater probability of obliteration of v.21 by natural causes than in the case of v.19 because of the very position of the sentence at the end of a pericope.

#### Intrinsic Probability

(i) To my mind, in the absence of this statement in v.21, the episode concerning the man who approached Jesus with his real estate problem (v.13) would be without a proper conclusion. The parable of the Rich Fool (vv.16ff) was spoken in answer to the man who, in the judgment of Jesus, had centred his life on worldly riches<sup>7</sup>. While Jesus never condemned material wealth as such so long as it was considered as a trust to be handled with a great sense of responsibility (cf. 16:9), He gave strict warning against its excessive indulgence. (Cf. Mk 10:17-31 par. Mt 19:16-30 par. Lk 18:18-30; Mt 6:19-21 par. Lk 12:32-34; Mt 6:24-34 par. Lk 16:13; 12:22-31). It is true that v.20 is quite adequate to expose the rich man's folly in being utterly oblivious of his ultimate destiny (cf. 16:19ff), and as such it constitutes an apt conclusion to the parable. But the episode in whose context it is placed by Luke would not be complete without a concluding remark proceeding from Jesus Himself (cf. v.15)<sup>8</sup>. In other

words, v.21 has, from the point of view of logic, an authentic role within the present pericope. As Lagrange observes, "La conclusion ne dépasse pas la parabole... L'essentiel est de ne pas imiter l'insensé qui y mettait tout son espoir comme si Dieu n'existait pas, et si l'on est riche, qu'on soit riche en regardant Dieu comme sa fin"<sup>9</sup>.

It is, however, difficult to decide whether the statement constitutes the ipsissima verba of our Lord or whether it is part of the editorial work of the author himself. Loisy writes: "Ce n'est pas la morale qui résulte directement de parabole, mais l'idée est dans l'esprit de notre évangile, et fait transition de la parabole à ce qui suit"<sup>10</sup>. Thus Loisy makes Luke responsible for the creation of v.21 as a transition from the present episode to the succeeding pericope on Care and Anxiety (vv.22-31).

Perhaps a comparison of this parable with those in 14:7-11; 18:1-8; 9-14 (cf. 15:3-7; 8-10) might serve as a clue to the understanding of Luke's handling of Jesus' parabolic teaching. Each of these parables ends on a solemn teaching or warning attributed to Jesus. Such parables are comparable to the 'pronouncement stories' as in Mk 2:15-17, 18-22, 23-28; Lk 19:1-10 etc. where each event leads to a memorable saying of our Lord. It is particularly instructive

that Luke places the theme at the very beginning of each of the above parables. Thus the parable of the Importunate Widow is introduced by the preface: "And he told them a parable, to the effect that they ought always to pray and not lose heart". Cf. the preface to the parable of the Pharisee and the Tax Collector, v.9. Similarly we find the theme of the parable under discussion stated at v.15: 'And he said to them, "Take heed, and beware of all covetousness; for a man's life does not consist in the abundance of his possessions," v.16. 'And he told them a parable saying, ...' (cf. 14:7).

It is true that Luke does not introduce or end all the parables in this manner. In some places the beginning is abrupt, but the moral, ascribed to Jesus Himself, appears at the end: e.g. 16:9. In others there is no moral specifically stated at the end, but there is enough indication at the beginning of the parable or the chapter to call attention to the theme: e.g. the parable of the Prodigal Son, 15:11ff; cf. 13:1-9. There are yet others in which the readers are left to draw the moral on their own without any direct statement either at the beginning or at the end: e.g., the Parable of Dives and Lazarus, 16:19ff. So it is abundantly clear that the few exceptions only confirm

our general rule regarding Luke's treatment of Jesus' parables: we have sufficient evidence to affirm that the context of the present parable demands that it should end in a concluding statement as in v.21.

If this theory is valid, the concluding remark here goes back to Jesus Himself because in none of the above cases did we find a statement at the end of a parable that could be conceivably ascribed to Luke<sup>11</sup>. However, it is immaterial for our purpose of establishing the authenticity of the text whether the sentence comes from Jesus or from the author's pen. Loisy's statement is quite pertinent here: "L'omission de ce verset dans quelques témoins ne semble pas un argument décisif contre son authenticité. C'est grâce à lui que la parable prend un sens chrétien; par elle-même ce n'est qu'un thème de vieille sagesse orientale"<sup>12</sup>.

(ii) Another serious consideration that confirms our inference above has also to do with Luke's style. Metzger rightly points out that "a careful author such as Luke would not be likely to pass directly from  $\epsilon\iota\pi\epsilon\iota$  of ver.20 to  $\epsilon\iota\pi\epsilon\iota$  of ver.22" especially because the speakers are different in the two contexts<sup>13</sup>.

## Conclusion

The strong MS support, coupled with the Intrinsic Probability, helps us to conclude that Lk 12:21 is genuine. Most scholars leave the textual problem of this verse undiscussed presumably because of their confidence in its originality. Those who do speak against it are either undecided in their final verdict (e.g. Plummer<sup>14</sup>), or have not made any serious attempt to establish their case (e.g. Creed<sup>15</sup>). It must be pointed out in this connexion that Lagrange<sup>16</sup> lists this passage among the texts on which he is quite confident or almost confident about authenticity. C.F.D. Moule<sup>17</sup> observes how the present parable, with its concluding moral in v.21, "forms an apt illustration for the passage in the Pastoral epistles, not to mention the even more familiar saying from the Sermon on the Mount about treasure in heaven". F. Hauck<sup>18</sup> draws attention to the close affinity of this verse with Jas 5:3. Thus we have the assurance of being in the company of the vast majority of modern scholars who would retain this verse in the text of Luke.

<sup>1</sup>Aland, NTS 12 (1965-66), p.202, not being impressed with this evidence, cites this as an illustration that the expression 'Western non-interpolation' either exaggerates or distorts the actual situation in textual matters; Cf. the views of Burkitt and Souter on Syr<sup>s</sup> readings cited under Mk 2:22c,

<sup>2</sup>The Greek NT, p.421.

<sup>3</sup>St. Luke, ad.loc.

<sup>4</sup>New Catholic Commentary, ad.loc; cf. Aland, loc.cit.

<sup>5</sup>So Snodgrass, JBL 91 (1972), p.374.

<sup>6</sup>Critique Textuelle, II, pp.68-9.

<sup>7</sup>Cf. F. Hauck, TDNT, III, 1965, p.138: "The piety of Jesus is distinguished by His definite repudiation of laying up of earthly goods. High estimation of the acts of love as such, and obedience with a view to the last hour, will determine the judgment. Insofar as the heaping up of earthly goods expresses a this-worldly and egoistic attitude, it is a contradiction of God".

<sup>8</sup>Since v.21 is a necessary concomitant of v.20, I do not find any conflict between the natural sense of that verse and the natural sense of the parable as a whole. Contrast D. Buzy, RB 31 (1940), pp.334-35: "Le sens naturel de la finale évoque l'idée d'un homme qui thésaurise égoïstement pour lui, sans rien donner à Dieu ni à son culte, quelles que soient d'ailleurs ses idées philosophiques ou théologiques sur la richesse. Le sens naturel de la parable au contraire évoque l'idée d'un homme qui s'imagine que sa vie est solidaire de sa fortune, suivant le mot de l'introduction: Un homme a beau se trouver dans l'abondance, sa vie ne dépend pas de ses richesses (15)". Buzy, however, admits the genuineness of v.21.

<sup>9</sup>S.Luc, p.360.

<sup>10</sup>Luc, p.347.

<sup>11</sup>Against J. Jeremias, *Parables of Jesus* (ET), p.82, and n.88. I am not convinced by his argument that "The eschatological note of warning...has been transformed into a warning against the wrong use of wealth". There is no reason why the one should not be intimately related to the other in the original context. His claim that v.21 is a Lukan addition that "gives a moralizing meaning to the parable, which blunts the sharp edge of its warning", seems unjustified.

<sup>12</sup>Loc.cit.

<sup>13</sup>Loc.cit.

<sup>14</sup>St. Luke, ICC, ad.loc.

<sup>15</sup>St. Luke, ad.loc.

<sup>16</sup>Loc.cit.

<sup>17</sup>JTS 3 (n.s.) (1952), p.76.

<sup>18</sup>Loc.cit.

II.C.

e. Lk 12:39 : 'he would have been awake...'

Disputed words: [ ἔγρηγόρησεν ἄν καὶ οὐκ ]

External Evidence:

Nestle<sup>26</sup>

Greek NT (UBS)

οὐκ (ἄν ἀφῆζεν... αὐτοῦ)  
(unverändert)

οὐκ (ἄν ἀφῆζεν... αὐτοῦ)

txt P<sup>75</sup> R\* (D — ἀφῆζεν ... αὐτοῦ) (d) e i syr<sup>c,s</sup> sa ach arm Mcion  
Tert || ἐγρηγόρησεν ἄν καὶ οὐκ (Mt. 24, 43) (R<sup>a</sup> — ἄν) R<sup>c</sup> A B K L  
P W X Λ Θ Π Ψ 070 f<sup>1</sup> f<sup>13</sup> 28 33 565 700 892 1009 1010 1071 1079  
1195 1216 1230 1241 1242 (1253 οὐκ ἄν) 1344 1365 1546 1646  
2148 2174 D<sup>l</sup> Lect (aur) b c (f) ff<sup>2</sup> (l q) r<sup>1</sup> (vg) syr<sup>p,h</sup> sa<sup>ms</sup> bo (bo<sup>mss</sup> —  
καὶ) aeth georg Bas JohDamasc

Observations

(i) Obviously, this instance is not strictly a 'Western non-interpolation' because of the support accorded to the omitting 'Western' witnesses by P<sup>75</sup> R\*.

(ii) The D reading is, of course, an accidental omission as it presents a nonsensical text which calls for something more to fill the lacuna.

(iii) So the real choice has to be made between the weighty evidence of P<sup>75</sup> R\* it (d), e, i | Syr<sup>c,s</sup> and other Versions in the company of Marcion and Tertullian for the omission of ἔγρηγόρησεν ἄν καὶ on the one hand, and the equally impressive array of the vast majority of authorities belonging to

all the branches of ancient tradition headed by  
BALW @ f<sup>1</sup>f<sup>13</sup> 28 33 565 700 etc. in support of the  
long text on the other.

(iv) The Old Latin witnesses are divided here,  
although the majority including b favour the long  
form.

(v) The testimony of a Harmony like the  
Diatessaron is not reliable in contexts such as this  
where we have a parallel passage containing an  
identical reading in Mt 24:43<sup>1</sup> (vide infra).

The MS evidence thus does not provide a  
decisive solution to the textual problem. Aland  
remarks that the evidence of P<sup>75</sup> would not be  
decisive here<sup>2</sup>; but we have to await a final  
decision until all the probabilities have been con-  
sidered.

### Internal Evidence

#### Transcriptional Probability

##### (i) Possibilities of Insertion

(a) According to Metzger, it is more likely that  
scribes would have assimilated the longer reading  
found in the parallel Mt 24:43, whereas there is  
no good reason for a possible deletion from a

supposedly genuine text<sup>3</sup>. B. Weiss calls attention to the form *διορυγήσαι* in place of *διορυχθήσαι* read in Mt by B W @ f<sup>1</sup>f<sup>13</sup> etc. Orig. The *ς* in Lk 12:39 also reads *διορυγήσαι* <sup>4</sup>. This would suggest that the source of the T.R. reading in Lk in respect of both *διορυγήσαι* and the double *ἀν* is Mt. But since B and its other allies read *διορυχθήσαι* in Lk, we cannot conclusively prove that their double *ἀν* is derived from Mt.

(b) Kilpatrick cites Harnack's study of Marcion's Gospel and says that Marcion had used a text of Lk that had already been influenced by Mt: e.g. *κοδράντην* for *λεπτόν* Lk 12:59 (cf. Mt 5:26). So he thinks that the disputed words in the present case is due to Marcion's text<sup>5</sup>.

In answer to this it must be stated that a convincing case cannot be made for such a theory from Mt, a Gospel which is so conspicuously pro-Semitic.

In any case, assimilation to Mt is not tenable because Mt 24:43 is not, after all, identical in every respect with the Lukan passage:

Mt	Lk
ἐκεῖνο	τοῦτο
τῆν οἰκίαν	τὸν οἶκον
φυλακῆ	ᾠρᾶ

Why should a scribe be content with such half-hearted harmonization? Again, we notice that there is near-perfect harmony between Mt 24:44 and Lk 12:40 except for the opening phrase *διὰ τοῦτο* in Mt. There is, however, no trace of evidence that an attempt was ever made by a scribe to assimilate that phrase in Lk. In fact such close similarity of parallel passages is a clear warning that we should not be rash in our conclusions about harmonistic attempts without weighing all the other probabilities. The occurrence of such harmony of readings can as well be explained in terms of the common source (Q) (vide infra).

(ii) Possibility of Omission

As we have seen at the very beginning, the D reading suggests that its ancestor had a longer text. A.C. Clark cites this as one of the examples of the way in which the D reading manifests a possible omission from genuine texts<sup>6</sup>. We might reconstruct v.39 in the exemplar of D as follows:

ΤΟΥΤΟ ΔΕ ΓΕΙΝΩΣΚΕΤΕ ΟΤΙ  
 ΕΙ Η ΔΕΙΘΙΚΟ ΔΕΣΤΟΤΗΣ  
 ΠΙΔΑ ΩΡΑΟΚΛΕΠΤΗΣ ΕΡΧΕΤΑΙ  
 ΕΓΡΗΓΟΡΗΣΕΝ ΑΝΚΑΙΟΥΚΑΝ  
 ΑΦΗΚΕΝ ΔΙΟΡΥΧΘΗΝΑΙ ΤΟΝ ΟΙΚΟΝ ΑΥΤΟΥ

This would neatly explain the omission of the last line due to the oversight of the scribe behind D, giving rise to the present awkward reading. If this explanation is reasonable, it follows that from the very length of the average στίχος, the last but one line should necessarily have contained ἔχρηγόρησεν ἄν , as otherwise the remaining καὶ οὐκ ἄν alone would not make up a sense-line.

#### Intrinsic Probability

(a) First we have to inquire whether the repetition of ἄν in conditional sentences is in keeping with Lukan style or whether it is confined to the first Gospel alone. And from Lk 17:6, a non-disputed passage, we find that the Third Evangelist himself was not averse to such a usage (cf. 7:39). So every occurrence of a double ἄν in Luke need not necessarily involve an assimilation to Mt.

(b) Secondly, the motif of 'to be awake' as the hallmark of the authentic Christian life-style is not confined to Matthew alone. It was in fact a favourite theme of Luke's spiritual mentor and colleague, St. Paul (Rom 13:11); Eph 5:14; 1 Thess 5:6-8; cf. 1 Pet 4:7; 5:8; Rev 3:3<sup>7</sup>.

So there is no reason why Luke should not have retained this popular Christian theme which he had most probably found in his source Q (cf. Lk 12:35ff; 17:26ff; 21:34-ff; Mt 24:13,43; Mk 13:13, 32-37)<sup>8</sup>.

### Conclusion

Thus weighing all the pieces of evidence at our disposal, the justification for upholding the reading *ἐργηγορήσει ἄνδρα καὶ οὐκ ἄνδρα* at Lk 12:39 becomes clear. Alford aptly denies the theory of assimilation: "Nothing can be more exact and rigid than the connection as it now stands"<sup>9</sup>. The difficulty of many a scholar in arriving at a decisive conclusion is understandable. Probably the silence of most commentators at this point may be a reflection of this difficulty. Both J. Weiss<sup>10</sup> and Loisy<sup>11</sup> retain the words in brackets, without comment, suggesting doubt about authenticity, yet reluctance to drop them. Lagrange's list of difficult passages where a decision is most uncertain includes this one<sup>12</sup>. Yet our decision to defend the long text will remain until convincing arguments are forthcoming to discredit the points raised here in its favour.

<sup>1</sup>Cf. Lagrange, RB (n.s.) 10 (1913), p.504.

<sup>2</sup>NTS 12 (1965-66), p.202.

<sup>3</sup>Textual Commentary, p.161.

<sup>4</sup>Meyer's Kommentar<sup>7</sup>, p.474.

<sup>5</sup>JTS 44 (1943), p.27; Cf. Harnack, Marcion  
(= TU 45, 1921), Beilage IV, p.198, n; pp.223-4; 224, n.1

<sup>6</sup>Primitive Text, p.39.

<sup>7</sup>See further, Selwyn, First Epistle of St. Peter,  
pp.375ff, especially p.380.

<sup>8</sup>Selwyn assigns all these passages to the source Q.  
He also links these with other eschatological  
sayings of our Lord at Mk 10:30f; Mt 10:32f  
[= Lk 12:8f] (ibid, pp.376ff; 442ff).

<sup>9</sup>The Greek NT I<sup>5</sup>, ad.loc.

<sup>10</sup>Schriften des NT, ad.loc.

<sup>11</sup>Les Livres du NT, ad.loc.

<sup>12</sup>Critique Textuelle, II, p.70.

II.C.

f. Lk 22:62 : The Penitence of Peter

Disputed words: [καὶ ἐξελθὼν . . . πικρῶς]

External Evidence:

Nestle<sup>26</sup>

Greek NT<sup>1</sup>(UBS)

καὶ ἐξελθὼν ἔξω ἔκλαυσεν πικρῶς καὶ ἐξελθὼν ἔξω ἔκλαυσεν πικρῶς  
(unverändert)

txt P<sup>75</sup> א AB DK L T W X Δ Θ Π Ψ 063 0124 0250 f<sup>1</sup> f<sup>13</sup> 28 565  
700 892 1009 1010 1071 1079 1195 1216 1230 1241 1242 1253  
1344 1365 1546 1646 2148 30 Lect aur c d f l<sup>c</sup> q vg. syr<sup>c</sup> s. p. h sa bo  
arm georg || — 0171<sup>vid</sup> a b e f f i l\* r<sup>1</sup>

Observations

(i) This is an instance where the Greek and Latin sides of Codex Bezae join the company of the great uncials and a number of the Old Latins and the Vulgate as well as of several of the other Versions including the Old Syriac in supporting the text<sup>1</sup>.

(ii) But the omission of the verse in both the African and European Latins is an evidence pointing in the opposite direction.

(iii) The fact that witnesses such as ΑΓΔΛ  
vg it<sup>f</sup> g<sup>1.2</sup> q<sup>1</sup> syr<sup>r</sup> eth insert ὁ Πέτρος  
after ἐξῶ both here and in the corresponding

passage, Mt 26:75b, shows that the present reading existed in both the Gospels at least from the 5th century on. The insertion is, of course, stylistically unjustifiable (cf. the presence of the same proper noun in vv.60,61 cf. Mt 26:75a) and is poorly attested; but it shows that at the time of the insertion, both Mt and Lk read the passage in question<sup>2</sup>.

(iv) This evidence is only a confirmation of the much earlier attestation for Lk provided by P<sup>75</sup>.

(v) The lack of patristic evidence rather weakens the case for or against the text.

### Internal Evidence

#### Transcriptional Probability

##### (i) Possibilities of Insertion

If the passage be not genuine, its appearance in Lk could be explained only in terms of the harmonizing tendency of scribes because it is exactly identical with Mt 26:75b. Scholars such as Griesbach<sup>3</sup>, B. Weiss, Loisy, Klostermann, Wellhausen, Kilpatrick<sup>4</sup> and Tasker<sup>5</sup> definitely hold this view along with Burkitt and Souter, while

Plummer, Creed, Easton, Leaney and Lampe<sup>6</sup> are hesitant to make up their minds. Loisy's comment that "il paraît faire aussi surcharge dans la strophe" need not be taken seriously as we are here dealing neither with poetry nor with any of the aphoristic or solemn teachings of Jesus as, say, in the case of the Sermon on the Mount. If his remark is valid at all, it should be equally applicable to Mt 26:75b and Mk 14:72b; but since those passages have not posed any such textual difficulty, the best thing is to allow due credit to the author himself for the creation of this verse. Burkitt's inference is based on his exaggerated statement regarding the textual evidence, viz. that "they are omitted by all the MSS of the Old Latin Version". Therefore his conclusion, "It is impossible to supply any cogent reason for this on the supposition that the words are genuine; it is therefore probable that the verse in Luke is an early harmonistic addition derived from Mt xxvi:75 itself"<sup>7</sup>, loses its weight. There is no minimizing of the problem posed by the identical nature of the two synoptic passages at this point. But we have to defer a decision until all the probabilities are weighed against each other.

(ii) Possibilities of Omission

Assuming the genuineness of the Lukan verse, various suggestions have been made in regard to the disappearance of it from the ancient authorities:

(a) According to Aland, the offence caused by the acknowledgment and confession of his guilt by Peter might have constituted an offence to a scribe<sup>8</sup>.

The difficulty with this solution is that one has also to explain why such an attempt to protect Peter's honour was not made in connexion with the several Gospel accounts depicting Peter's many human frailties including his conduct at the time of Jesus' agony in the garden and at the arrest, and above all, during the trial of Jesus at the High Priest's residence. Further, how could the confession of his guilt by Peter be deemed a more disgraceful act than the very denial of the Master, even in the evaluation of a scrupulous scribe?

(b) It could be thought that the omission was effected in the interest of harmonization with the Johannine version of Peter's denial. Lagrange is right in answering this point, raised by himself, in the negative<sup>9</sup>. Although he

does not make the answer explicit, the reason for this refutation is obvious: The Lukan account of Peter's denial has more in common with the other two Synoptics than with Jn. (Cf. Jn 18:27).

(c) A more plausible reason has been offered by Snodgrass, namely, that the *καί* of v.62 and the *καί* of v.63 must have contributed to homoeoarcton<sup>10</sup>.

Remembering that this short sentence consisting only of 27 letters could have constituted a *στίχος* in an ancestor of D, this possibility is very real indeed. (Cf. on 12:19).

#### Intrinsic Probability

(i) That Matthew has followed Mk rather closely in the present narrative is quite obvious. In spite of the several divergences in Luke's account, he has followed Mk at least in the barest outline. The obscure Markan phrase *ἐπιβαλὼν ἐκλαίει* (14:72b) would no doubt have caused serious difficulty to both Matthew and Luke<sup>11</sup>. The several textual emendations attempted by scribes provide proof for this difficulty: How they reacted to it is a problem over which scholars have expressed divergent views. Burkitt thinks that Luke would simply have avoided it, and hence

the short reading in Lk 22:62<sup>12</sup>. On the other hand, Lagrange opines that Luke would have rewritten it in a clearer style<sup>13</sup>. A third alternative presented by Winter is more convincing in view of the total situation which we face in Lk. In his opinion, "the Third Evangelist could have had at his disposal a record different from the Marcan"; and "the Lucan version was the more 'primitive' "<sup>14</sup>. There is ample justification for this view when we compare the Lukan account of the Denial with the Marcan. The Lukan account differs from Mark's in four respects:<sup>15</sup>

<u>Luke</u>	<u>Mark</u>
(a) 3 different persons accuse Peter	The maid twice; a bystander once
(b) Omits Peter's cursing and swearing and replying with an oath	Ascribes these 3 acts to Peter
(c) The statement, 'The Lord turned and looked at Peter' is peculiar to Lk (v.61)	No such statement (cf. 10:21)
(d) No change of scene during the denials	Peter's movements in between the denials reported

On the other hand, except for the mention of 'another maid' (v.71), the Matthaean version is based on Mark's. The minor differences are in keeping with the kind of freedom that Matthew

consistently maintains while copying Mk. Thus we notice that there is a great divergence between Mt and Lk with regard to the present narrative. Of course, it is in such contexts that we have a strong case for assimilation in respect of identical wording in two Gospels. However, it will be shown below that certain other aspects of the Intrinsic Probability force us to conclude differently.

(ii) Lampe says: "If 62 is not authentic, 61a is Luke's dramatic climax to the story which he infers from Mark"<sup>16</sup>. But a comparison of the episode concerning Peter's Fall in the Four Gospels shows that it is difficult to regard 61a as the climax. Unlike John, Peter's remembrance of Jesus' prior warning to him is recorded by all the Synoptists. Obviously, John had intended to make the cock-crow the climax to the story. In the Synoptics, however, the mention of Peter's recollection necessarily implies an immediate reaction on his part. Luke in fact states that 'the Lord turned and looked at Peter' just before mentioning Peter's remembrance. It is difficult to believe that Luke had meant this look of Jesus as a mere reminder without a reference to a corresponding change of heart on the part of Peter.

The deletion of v.62 would not only tone down the poignancy of the whole scene, but would also render 22:32 ('And when you have turned again, strengthen your brethren') meaningless. Leaney's remark on v.62, "if it is genuine, the repentant tears mark the beginning of Peter's 'turning back' ", is to the point.

### Conclusion

Having weighed all the probabilities, the difficulty of explaining the identity of our reading with the Matthaean passage comes into relief. The strong MS evidence as well as the Intrinsic Probability would recommend a decision in favour of retaining the verse in the Lukan text. Granted that Luke had to resort to a source other than Mk in the present context, as in the case of his concluding chapter, the only explanation possible for the identical readings in Mt and Lk is that this source contained a parallel tradition in which the three-fold accusation of Peter by the servants of the High Priest's household, the three-fold denial and the cock-crow which signalled Peter's 'turning back' were the basic ingredients. And a reference to the penitence of Peter must have formed the finale to this narrative. The wording of this

reference must have been so ingrained in Christian circles that the identity of the closing sentence in Mt and Lk was inevitable. The difficulty caused by the Markan wording had led Matthew to draw from this well-known oral tradition to conclude his narrative regarding Peter's denial. B. Weiss's statement endorses the above view: "V.62 ganz übereinstimmend mit Mt xxvi.75. So pflegte man wohl in der mündlichen Ueberlieferung nachdrücklicher als es Mark xiv.72 geschah, die Busse des Petrus zu schildern"<sup>17</sup>.

Certain additional facts relating to Lk 22:62 which we have considered in connexion with the textual evidence for 24:12 also lend support to our present conclusion. There it has been pointed out that if 24:12 is not genuine (as there is every reason to believe), 22:62 can have a rightful claim to originality (see n.65 thereof).

<sup>1</sup>Burkitt, *Evangelion II*, pp.231-32 lists this passage along with 24:6,12, 36 as exceptional cases of interpolation in Syr<sup>S</sup> as well as in the B<sub>1</sub> group; cf. Souter, *Text and Canon*, p.128. (Vide on Mk 2:22<sup>C</sup>)

<sup>2</sup>Cf. B. Weiss, *Meyer's Kommentar*<sup>7</sup>, p.616, n.

<sup>3</sup>So Meyer, *Meyer Kommentar*, I p.471.

<sup>4</sup>JTS 44 (1943), p.29.

<sup>5</sup>The Greek NT, ad.loc.

<sup>6</sup>Peake's Commentary, ad.loc. Unless otherwise stated, all the scholars listed here are referred to after their Commentaries on Lk.

<sup>7</sup>*Gospel History and Transmission*, p.51.

<sup>8</sup>NTS 12 (1965-66), p.203.

<sup>9</sup>*Critique Textuelle*, II, p.70.

<sup>10</sup>JBL 91 (1972), p.374. This idea does not seem to have occurred to A.C. Clark (*Primitive Text*).

<sup>11</sup>So Creed, *Luke*, ad.loc.

<sup>12</sup>*Gospel History and Transmission*, p.51: "The resemblance between Mt and Lk is too close here to be the result of independent interpretation of Mark's obscure phrase...".

<sup>13</sup>Loc.cit.

<sup>14</sup>*StTh* vii-viii (1954-55), p.162.

<sup>15</sup>*Ibid.*; cf. Stuhlmueller, *Jerome Commentary*, ad.loc.

<sup>16</sup>Peake's Commentary, ad.loc.

<sup>17</sup>*Meyer's Kommentar*<sup>7</sup>, *Markus u. Lukas*, p.618.

II.C.

g. Luke 24:9 : The Return from the Tomb

Disputed words: [ ἀπὸ τοῦ μνημείου ]

External Evidence:

ἀπὸ τοῦ μνημείου P<sup>75</sup> 8 A B K L W X Δ Θ Π Ψ 063 0124 f<sup>1</sup> f<sup>13</sup>  
28 33 565 700 892 1009 1010 1071 1079 1195 1216 1230 1241 1242 1253 1344 1365  
1546 1646 2148 2174 Byz Lect it<sup>aur, f, g</sup> vg syr<sup>c, s, p, h, pal</sup> cop<sup>ss, bo</sup> eth Tertullian  
Eusebius Cyril // omit D it<sup>s, b, c, d, e, ff<sup>2</sup>, l, d<sup>1</sup></sup> arm geo

Observations

(i) To begin with, one cannot but be impressed by the attestation of the majority of the ancient witnesses from the various branches of tradition including P<sup>75</sup> B X retaining these words.

(ii) On the other hand, one cannot ignore the shorter reading merely on the ground that it is 'Western' in origin because of what we have discovered in respect of the various 'Western' readings, especially in the other parts of Lk 24. Scrivener is so impressed by the proneness to omit exhibited by this chapter that he remarks that it "almost suggests the idea of representing an earlier edition than that is now in ordinary use, yet proceeding from the Evangelist's own hand". In support of this he points to the pattern of the textual evidence for the various omissions in

this chapter which is almost uniform in all the eight cases<sup>1</sup>. Although we have already found valid reasons to reject the idea of a dual edition of the Third Gospel, we have enough warrant to treat the absence of the above three words with caution.

(iii) Besides, this particular point of the chapter involving vv.9,10 is full of evidence of both scribal negligence and meddling with style so that we have additional reason to be cautious about the present omission.

(iv) The majority of Old Latins, followed by the Old Syriac and other Versions, in the company of three early Church Fathers support the long reading, while the best authorities of the Old Latin (both African and European) favour the short reading. However, Metzger's description of the External Evidence as "overwhelming" has to be viewed with caution because of the notorious state of affairs of ch.24 (cf. Textual Commentary, ad.loc.).

Judging from previous experience one can only say at this stage that the textual evidence is slightly in favour of the long reading.

## Internal Evidence

### Transcriptional Probability

#### (i) Possibility of Insertion

The only possibility of addition to the text has been mentioned by Lagrange, viz. that "l'addition peut provenir de Mt xxviii.8"<sup>2</sup>.

In view of the dissimilarity between the accounts of the Empty Tomb in Mt and Lk, this identical reading in both Gospels might suggest the possibility of assimilation; however, the very nature of the context would suggest that the identity of reading is accidental, especially because only three words are involved. Therefore the question will remain open until all probabilities have been considered.

#### (ii) Possibilities of Omission

(a) Assuming the originality of the long reading here, Snodgrass attributes its curtailment to the superfluous nature of the words<sup>3</sup>.

This seems not justified when we compare this verse with v.2, where ἀπὸ τοῦ μνημείου has been spared by scribes despite the presence of ἐπὶ τὸ μνημόνιον closely behind in v.1. So stylistically, the occurrence of the words at v.9 is not so redundant as to warrant deletion.

(b) Since Codex Fuldensis omits the words in question, and since all the evidence points to their omission in the Diatessaron, von Soden's theory of Tatianic influence may seem at first attractive as an explanation for their disappearance from the Latin Texts. But Lagrange rightly asks why the omission did not in that case extend to the Old Syriacs<sup>4</sup>.

(c) A more convincing explanation is that of accidental omission which Lagrange also favours<sup>5</sup>. The words consisting of just 14 letters would conceivably have formed a *στίχος* in an ancestor of the 'Western' family from which they would have got eliminated by scribal oversight. (Cf. on Lk 12:19, 21, 39; Mt 13:33).

#### Intrinsic Probability

This is a phrase whose intrinsic merits would not suggest the possibility of pin-pointing it to any particular author. There is nothing unLukan about it, and if we leave out the words, although the sense would not be affected, the impression that one gets is that of a stylistic lacuna.

## Conclusion

This last-mentioned factor, coupled with all the other merits of the case we have discussed, tilts the balance of probabilities in favour of the long text rather than the short. As this is not a matter involving any controversial issue, it is reasonable to ascribe authenticity to the disputed words in the absence of any convincing evidence to disprove it. Although Lagrange considers this as one of the readings where a decision is very difficult,<sup>6</sup> the silence of the vast majority of commentators about this textual problem is adequate proof of their unquestioning support for our position.

<sup>1</sup>Introduction II<sup>4</sup>, pp.298-9.

<sup>2</sup>Critique Textuelle, II, p.70; cf. RB (n.s.) 10 (1913), p.507: "...entre Lc.XXIV,8 et Lc.XXIV,9, il y a une série de passages se terminant par Mt.XXVIII,8<sup>b</sup>-10, de sorte qu'on pouvait plus dire une seconde fois que les saintes femmes venaient du sépulcre".

<sup>3</sup>JBL 91 (1972), p.375.

<sup>4</sup>See Lagrange, RB (n.s.) 10 (1913), p.507; cf. von Soden, Schriften des N.Ts., I.2, p.1572.

<sup>5</sup>Cf. Metzger, Textual Commentary, ad.loc.

<sup>6</sup>Loc.cit. Critique Textuelle, II, p.70.

## II.D THE JOHANNINE 'NON-INTERPOLATIONS'

a. Jn 3:31-32: 'He who comes from above is above all...'

Disputed words: [Ἐπάνω πάντων ἐστίν] & [τοῦτο]

External Evidence:

ἐρχόμενος [ἐπάνω πάντων ἐστίν] ἐρχόμενος [ἐπάνω πάντων ἐστίν]  
32 ὁ ἐώραξεν καὶ ἤκουσεν τοῦτο 32 ὁ ἐώραξεν καὶ ἤκουσεν τοῦτο  
μαρτυρεῖ (vorher ohne eckige μαρτυρεῖ  
Klammer)  
txt P<sup>36</sup>vid (P<sup>66</sup>\* — ἐρχόμενος) P<sup>66c</sup> (N<sup>c</sup> — τοῦτο) BL W<sup>supp</sup> Ψ 083 086  
33 1010 1071 syr<sup>pal</sup> bo fay Cyr II ἐρχόμενος ἐπάνω πάντων ἐστίν  
καὶ ὁ ἐώραξεν καὶ ἤκουσεν τοῦτο μαρτυρεῖ A K Δ Θ Π 063 f<sup>13</sup> (28 1365  
— τοῦτο) 700 892 1009 1079 1195 1216 1230 1241 1242<sup>vid</sup> 1253  
1344 1546 1646 2148 2174 22 Lect aur c f q vg syr<sup>(p)</sup> h goth (aeth)  
Or<sup>gr.</sup> lat Chr Aug II ἐρχόμενος ὁ ἐώραξεν καὶ ἤκουσεν τοῦτο μαρτυρεῖ  
P<sup>75</sup> sa Or II ἐρχόμενος, ὁ ἐώραξεν καὶ ἤκουσεν μαρτυρεῖ (N<sup>\*</sup> ὄν für ὁ)  
D f<sup>1</sup> 565 a b d e ff<sup>2</sup> j l r<sup>1</sup> syr<sup>c</sup> (6) arm georg Tert Hipp Hil Ambst

### Observations

(i) This is not strictly a case of 'Western non-interpolation' despite the label attached to it by Westcott-Hort.

(ii) The MSS give evidence of an apparent difficulty with the text and consequent attempts to 'improve' upon it from very early times: Cf. the treatment accorded to *Kdī* and *TOYTO*. The main issue before us is whether to retain or reject the bracketed words.

(iii) The witnesses are almost evenly divided, not in regard to their numbers, but in regard to their authority. The overwhelming majority of them including P<sup>66</sup> B 700 Vg support the long text, while the vast majority of the Old Latins in the company of P<sup>75</sup> as well as of many 'Western' witnesses such as D and the Versions and the early Fathers like Tertullian and Eusebius attest the short text. The attestation of Origen is not constant. Aland rightly evaluates this textual evidence when he says that absolute clarity cannot be achieved from this, although the long text may probably be the original<sup>1</sup>.

### Internal Evidence

#### Transcriptional Probability

#### Possibility of Insertion

On the assumption that the short reading is original, how could the long reading come about? Could it be due to accidental repetition after the second ἐρχόμενος on the pattern of the words which follow the first, as suggested by several scholars<sup>2</sup>.

This seems to be least likely because:

- (a) the wide diffusion of the double reading in the different branches of tradition stands against it<sup>3</sup>;
- (b) if an insertion occurred due to this reason, we should naturally expect all the intervening words between the first *ἐπὶ πάντων πάντων ἐστίν* and the second *ἐρχόμενος* to be repeated after the second;
- (c) stylistically speaking, a copyist is not likely to make such an uncouth construction on his own.
- (d) the Fourth Gospel is full of constructions of the type of v.31; yet evidence of such accidental repetition has not been forthcoming elsewhere.

#### Possibility of Omission

On the contrary, it could be argued that the redundant nature of the words after their first occurrence in v.31 might have led to their deletion from a long text.

Barrett<sup>4</sup> is strongly in favour of this suggestion. There is every possibility that since nothing is substantially lost by deleting these words a copyist or a translator might have considered it within his right to exercise this freedom.

Other possibilities of omission do not seem plausible. Homoeoteleuton cannot be brought in as an argument because in that case the intervening words between the identical phrases would have been lost. No moral offence is involved here: in fact the very occurrence of the words in v.31 in all MSS suggests that deliberate suppression due to moral scruples is out of the question. So omission due to redundancy is the only solution we can think of<sup>5</sup>.

#### Intrinsic Probability

(i) First we have to decide whether a repetitive style is characteristic of the Fourth Gospel. Barrett affirms that this is so<sup>6</sup>. Bultmann correctly assesses the problem as follows: "The antithetical style and the terminology of the verses show that they come from the source. Yet clearly the rhythm has been disrupted (sc. Bultmann is referring to the dissimilarity between

the second line and the fourth clause ending in *μαρτυρεῖ* (except for the initial *ο* and the verbal termination). It is not really possible to reconstruct the original form of the source, since the text of the Gospel is itself uncertain"<sup>7</sup>. However, he himself provides some clues to help arrive at a solution:<sup>8</sup>

There is no gainsaying the fact that apart from theological content, it is the Johannine style which makes the Fourth Gospel so distinctive from the Synoptic Gospels: (i) Juxtaposition of antitheses: e.g. 1.7,8; 1.17,18 etc. (ii) The use of a pronoun to repeat the subject or object which is already implied in the verb, and which, according to strict grammar, is redundant: e.g. *ἐκεῖνος*, 1:33; 5:11,43; 9:37; 19:26; 15:13,26 cf. 1 Jn 3:16; *οὗτος*, 3:26; 5:38; 6:46,49,50; cf. 1 Jn 5:5,6; *τοῦτο* is sometimes used to pick up the object clause that immediately precedes it: e.g. 8:26: *Κἀγὼ ἃ ἤκουσα παρ' αὐτοῦ ταῦτα λαλῶ . . . .*; 14:13: *καὶ ὅτι ἂν αἰτήσητε . . . τοῦτο ποιήσω . . . .*<sup>9</sup>.

Thus Bultmann's analysis of the Johannine style gives every justification both for the redundancy and for the retention of *ΤΟΥΤΟ* in the present passage. In other words, Bultmann's observations help confirm Barrett's remarks about the Johannine style.

Westcott, on the contrary, maintains that such repetition is artificial: "The motive for the repetition of *ἐπὶ πάντων ἐστίν*, and then for the addition of *καὶ*, is sufficiently clear. The words therefore cannot but be regarded with great suspicion, and the sense does not clearly lose by their absence"<sup>10</sup>.

The latter part of this statement has already been touched upon: it is not a question of whether the sense would suffer by the loss of this phrase; that it does not lose even a bit of its meaning must have been the reason for its omission by ancient scribes. Westcott does not make the 'motive for the repetition' clear. What is 'clear' to him could only be guessed: In v.31 we have a contrast between the statements in parts (a) and (b), i.e. between *ὁ ἄνωθεν ἐρχόμενος* and *ὁ ὢν ἐκ τῆς γῆς*. In 31<sup>c</sup> the subject is

again ὁ ἐρχόμενος . So possibly, in Westcott's view, the scribe might have decided to repeat the ἐπὶ πάντων πάντων ἐστίν to strike a balance between 31<sup>a</sup> and 31<sup>c</sup>. The addition of καὶ at the beginning of v.32 might have been felt to be necessary because this verse continues to speak of the same subject as in 31<sup>c</sup>. If this guess is correct, it is more fitting to ascribe this repetitive motive to the author himself than to a scribe in view of such tendency that is abundantly manifest in passages like 1:2,3; 1:7,8; 6:39,40; 6:50,51; 10:7,9; 10:11,14; 14:12,13; 17:6a,6b; 17:14,16.

It must be borne in mind that it is not the concern of Textual Criticism to rewrite a particular author's style in order to make it more elegant or more intelligible to us. That may well be within the role of an exegete. If that is our main interest in the present passage, Barrett has given expression to how an exegete will approach the text<sup>11</sup>: "The effect of these omissions (which, for practical purposes, may be taken as one variant), is to combine the two verses so that we must translate: "He who comes from heaven testifies to that which he has seen and heard". Westcott

suggests that "the opposition of ὁ ὢν ἐκ τῆς γῆς λαλεῖ to ὁ ἐκ τοῦ οὐρανοῦ ἐρχόμενος ὁ ἑώρακεν καὶ ἤκουσεν τούτο μαρτυρεῖ becomes far more impressive if the words in question are omitted"<sup>12</sup>. This is precisely the point of our debate. It need not be surprising that Westcott has not suggested such elegant adjustments in respect of other redundant passages. Obviously it is due to the absence of variant readings in such passages. This indirect evidence should have enabled him to form a conclusion in favour of the redundant construction in the present context as well.

### Conclusion

Such a conclusion is indeed in conformity with the views of many modern scholars. E.g., Schnackenburg notes: "Though the shorter reading gives a more forcible context, the longer is probably original, since the repetition, which here plays the part of an inclusio, is Johannine in style"<sup>13</sup>. Bauer writes in the same vein that the words are certainly superfluous, "aber vielleicht gerade deshalb echt"<sup>14</sup>. Even Leon Morris who is impressed by the MS evidence for the omission concludes: "A decision is difficult, but perhaps

retention of the words is slightly more probable as in ARV"<sup>15</sup>. Likewise, Barrett, impressed by the strong MS evidence for the omission from very early times, goes a step further and suggests that it may well be correct. But his final verdict is in favour of the long text: "It is rightly remarked that v.31c merely repeats v.31a; but this is in fact perhaps the strongest argument in favour of the longer reading"<sup>16</sup>.

Even Sanders and Mastin<sup>17</sup> and Tasker<sup>18</sup> who are influenced by both internal and external factors to support the short text admit that the long reading is in the style of the Fourth Gospel. While Lagrange claims that "le texte court est certainement authentique"<sup>19</sup>, Aland is rather non-committal, as was noted at the beginning. Barrett's position may be the best solution<sup>20</sup> to this textual problem especially because those who favour the short reading have nothing more convincing to offer than the arguments presented above in defence of the long text<sup>21</sup>.

<sup>1</sup>NTS 12 (1965-66), p.204.

<sup>2</sup>Lagrange, Critique Textuelle, II, p.71; Tasker, Greek NT; ad.loc.; Sanders and Mastin, St. John, ad.loc.

<sup>3</sup>Aland, loc.cit.

<sup>4</sup>St. John, p.188.

<sup>5</sup>Aland, loc.cit.

<sup>6</sup>Loc.cit.

<sup>7</sup>John (E.T.), p.161.

<sup>8</sup>Ibid. , p.79, n.3

<sup>9</sup>The use of *TOU TO* by John is really a case of 'casus pendens'. Burney, Aramaic Origin of the Fourth Gospel, pp.6,34,63-65,151, counts 27 examples in Jn including this one; Cf. Moulton-Howard Grammar, II, p.424; Black, Aramaic Approach<sup>3</sup>, pp.51f, cites an example from the Elephantine Papyri, 28:15: "My sons, they shall pay thee money". See Barrett, loc.cit.

<sup>10</sup>St. John, pp.65-66.

<sup>11</sup>Loc.cit.

<sup>12</sup>Loc.cit.

<sup>13</sup>John (E.T.), I, p.383. On 'inclusio', see J.C. Fenton, TU 73 (=StEv 1) (1959), p.174 (Vide on Mt 6:15 n.7).

<sup>14</sup>Handbuch, ad.loc.

<sup>15</sup>John, p.245.

<sup>16</sup>Loc.cit.

<sup>17</sup>St. John, p.134.

<sup>18</sup>Text of the NT, ad.loc.

<sup>19</sup>Critique Textuelle, II, p.71.

<sup>20</sup>Loc.cit.

<sup>21</sup>The debate concerning the origin of vv. 31-38 is really not relevant for our purpose. Scholars are all agreed that this section is definitely a comment by the Evangelist: Cf. Schnackenburg, loc.cit.; Lock (Gore's Commentary), v.30 cf. v.11 (3:13). In any case, what is important for us is whether the matter is traceable back to John's pen or not.

II.D.

b. Jn 4:9b: The Comment on the Relation between the Jews and the Samaritans

Disputed words: [οὐ γὰρ συγχρῶνται . . . Σαμαρίταις]

External Evidence:

Nestle<sup>26</sup>

Greek NT<sup>1</sup> (UBS)

οὐ γὰρ συγχρῶνται Ἰουδαῖοι Σαμαρίταις (vorher in eckigen Klammern)

txt ̦<sup>63, 66, 75, 76</sup> ̧<sup>a</sup> ABC<sup>1</sup> KLW<sup>supp</sup> X<sup>comm</sup> Δ Θ Π Ψ 050 083 086 ̦<sup>1</sup> ̦<sup>13</sup> 28 33 565 700 892 (1009 1646 οἱ Ἰουδαῖοι) 1010 1071 1079 1195 1216 1230 1241 1242 1253 1344 1365 1546 2148 2174 ̦<sup>̄</sup>

Lect (̦<sup>1127</sup> — Ἰουδαῖοι) aur c fff<sup>2</sup> l q r<sup>1</sup> vg syr<sup>c. s. p. h. pal</sup> sa bo ach<sup>2</sup> arm georg Or Chr Nonn Cyr II — ̧<sup>a</sup> D a b d e j fay

Observations

(i) The support for the text is overwhelming indeed, not only by virtue of the ancestry of the witnesses, but also in regard to their geographical distribution.

(ii) The unusual combination ̧<sup>a</sup> \*D is generally considered as a poor attestation, although we have found exceptions to this rule. (Cf. on Mt 6:15; Lk 24:51). The best of the Old Latins are on the side of the omission, while the majority of them as well as the Vulgate and almost all the other Versions are in favour of the long text.

Thus there is no doubt about the authenticity of the long reading from the point of view of the MS evidence.

## Internal Evidence

### Transcriptional Probability

#### (i) Possibility of Insertion

Only very few scholars hold that the words were an explanatory addition. E.g., Burkitt says that it is a "gloss which arose as an explanatory paraphrase of *γυναικὸς Σαμαρίτιδος* and that it was intended to round off the woman's speech<sup>1</sup>.

To this we may reply with Tasker that "such informative notes are characteristic of the author"<sup>2</sup>; and hence the present verse fits the context well. Zahn is most probably right in observing that the retention of the words in this place makes sense to the statement of the Jews in 8:48 without any explanation there: *Οὐ καλῶς λέγομεν ἡμεῖς ὅτι Σαμαρίτης εἶ σὺ καὶ δαιμόνιον ἔχεις;*<sup>3</sup>. At any rate the defenders of the view of a later insertion have not cared to explain how this addition came about not only in almost the whole of the Greek tradition but also in the majority of the Versions<sup>4</sup>.

#### (ii) Possibilities of Omission

(a) Aland attributes the omission to the conscious reflection of scribes who might have felt the words to be superfluous<sup>5</sup>

But as we have noted above, such explanatory additions are characteristic of the author and are scattered all over the Fourth Gospel. Since none of these other notes constitute a textual problem, the argument in terms of superfluity cannot be held valid<sup>6</sup>.

(b) Kilpatrick points out that the possibility of omission due to homoeoteleuton exists in the itala Codices because there the previous clause ends in Samaritana, while the present statement ends in Samaritis (in q the former too ends in Samaritis). He also points out that except for P<sup>66</sup>  $\aleph$  BAC\* LW 083 33 Cyr et al, all Greek witnesses (including D) read  $\Sigma\alpha\mu\alpha\rho\acute{\iota}\tau\iota\delta\omicron\varsigma$  in the preceding clause instead of  $\theta\upsilon\sigma\eta\varsigma$  ; they all (except D) read  $\Sigma\alpha\mu\alpha\rho\acute{\iota}\tau\iota\delta\iota\varsigma$  in the present text. According to Kilpatrick, a similar possibility exists also in the Coptic<sup>7</sup>. So in the original exemplar of all these Greek MSS and Versions there was the real possibility of an omission of this type, especially because the whole sentence would constitute just a single line in a MS<sup>8</sup>.

(c) A third possibility suggested by Daube<sup>9</sup> has been a matter of controversy: "The reason for the absence of the clause from the above manuscripts is probably that it was no longer understood when they were written". His explanation for the non-intelligibility of the clause is that the traditional interpretation of the word *σὺνχρᾶσθαι* as meaning "to associate on friendly terms" was not known to the scribes. Daube also contends that neither the Vulgate nor the Syriac translations conveyed the traditional meaning of the word, although modern translations of the Syriac NT based on "inaccurate information" provided by the dictionaries do render the word in this way. The meaning that he ascribes to the word is "to use (vessels) together with". Since this derivation has been widely quoted by modern commentators and translators of the NT, it is necessary to examine whether Daube is right or not.

D.R. Hall has with justification questioned the whole raison d'être of Daube's argument<sup>10</sup>. He points out that the main issue here is whether the dative *ἐπιπέτραις* is governed by the compound verb *σὺνχρᾶσθαι* as a whole, or by its prefix *σύν* only. One of the weaknesses of Daube's theory is that despite his admission of lack of evidence for the latter case he maintains that

"the possibility of this construction must have existed". Hall cogently argues that the simple form  $\chi\rho\acute{\alpha}\omicron\mu\alpha\iota$  as well as the compound  $\sigma\upsilon\chi\rho\acute{\alpha}\omicron\mu\alpha\iota$  is used in two senses: (a) When the object is impersonal, it means either 'to use' or 'to experience'; but (b) when the object is personal, it means 'to treat some one in a certain way', or specifically, 'to treat as a friend' (as in the present instance). So the traditional rendering, 'to have dealings with', or 'to associate on friendly terms with some one' is to be sustained.

In the light of this, Daube's hypothesis as to the cause of the omission of v.9 is not tenable. Westcott's interpretation further strengthens this position; " $\sigma\upsilon\chi\rho\acute{\omega}\nu\tau\alpha\iota$  suggests the relations of 'familiar intercourse' not of business; offices of kindness were not expected between the Jews and Samaritans"<sup>11</sup>. If we accept Westcott's view, then Leon Morris' support for Daube's position based on 4:8 (i.e. since the disciples had gone to the Samaritan town to buy food, the meaning 'to have dealings with' is not applicable) is not valid<sup>12</sup>. Further, by Morris' own admission, certain dry foods were regarded as not conveying defilement. But even if we reject this meaning, the other meaning 'to

associate on friendly terms' is still tenable. After all, to buy food from a shop need not involve friendly association. But in the present context, to receive water to drink from some one entails more personal relationship. Schnackenburg, opposing Daube's view, holds that "underlying the general statement may be the idea that the Jews feared to defile themselves by taking food and drink from the Samaritans"<sup>13</sup>. But Leon Morris quotes the Mishnah to show that there were certain exceptional occasions when the Jews could eat together with the Samaritans. At the same time, unlike Schnackenburg, he defends Daube's position saying that the statement in 4:9 refers to "widespread Jewish scruples in Jesus' time" about eating in the company of Samaritans<sup>14</sup>. Barrett even suggests that it is a reflection on the author's knowledge of a regulation passed in A.D. 65 or 66 laying down that "the daughters of the Samaritans are menstruants from their cradle"<sup>15</sup>.

All this discussion arose from ascribing the wrong meaning to *συγχρόματι* which we have rejected above. It would be much more reasonable to assume that the reference in question is not about legal aspects but to the general attitude of prejudice among the Jews in associating on

friendly terms with the Samaritans, something which is not uncommon in many multi-racial societies even in modern times. Bultmann's observation that this statement is still proverbial among "members of different confessions when, for example, they fall out over business transactions, or when one of them disapproves of the dishonest actions of another"<sup>16</sup> helps to clarify its original meaning.

#### Intrinsic Probability

(i) Did the words originally belong to the woman's speech, or were they an explanatory note provided by the author himself? Barrett rightly asserts that they should not be treated as the woman's words<sup>17</sup>. In fact there was no need for her to provide such information to Jesus. Her very question (9a) reflects her bewilderment at Jesus' strange behaviour as a Jew; it is implicit in this question that she had assumed a knowledge on Jesus' part about the general Jewish attitude against her race. Her words do not reflect any prejudice on the part of the Samaritans against the Jews (although they would generally have hated them cf. Lk 9:52f), but rather a feeling of inferiority. In any case, the words of 9b were not originally meant to complete her speech.

(ii) How Johannine are the words in question?

The chief consideration that would help to ascribe them to John is the fact that such parenthetical notes by way of explaining a previous statement forms a distinct characteristic of the Fourth Gospel. Such explanations could be (a) in respect of a Hebrew proper name or title, or (b) by way of introducing some character(s), or (c) aimed at clarification of a foregoing statement, e.g.:

(a) 1:38, Ῥαββί ; 1:42, Κηφᾶς ; 1:41; 4:25,  
Μεσσίας ; 19:3, Γαββαθαῖ ; 20:16 :  
Ῥαββουρί ;

(b) 1:44, Philip; 11:2, Mary, Lazarus;  
18:14, Caiaphas (cf. 19:38, Joseph of  
Arimathaea; 19:39, Nicodemus);

(c) 4:2, καίτοι γε, Ἰησοῦς αὐτὸς οὐκ ἐβάπτισεν...  
11:13 ΠΕΡΙ ΤΟΥ ΚΟΙΜΗΣΕΩΣ ΤΟΥ ὙΠΝΟΥ...  
cf. 11:18, 30, 51, 52; 12:41; 14:22; 12:6.

### Conclusion

As in the case of Jn 3:31-32, it is the Johannine style that is most decisive here.

This factor, coupled with the very impressive MS evidence, enables us to conclude along with many scholars that Jn 4:9b is authentic.

<sup>1</sup>Evangelion, II, p.254.

<sup>2</sup>The Greek NT, ad.loc.

<sup>3</sup>Introduction, III, p.325.

<sup>4</sup>Aland, NTS 12 (1965-6), p.204.

<sup>5</sup>Ibid.

<sup>6</sup>The same comment is called for by E.C. Colwell's explanation. He is right, of course, in saying that if the Gospel was meant for an extra-Palestinian audience the author could have written the parenthesis in question. But one doubts his additional statement that a later scribe could have eliminated it on the suspicion of it being a gloss. [Studies and Documents, XXIX, (1967), ed. by J. Geerlings, p.4.]

<sup>7</sup>JBL 87 (1968), p.327.

<sup>8</sup>But see Lagrange, RB (n.s.) 10 (1913), p.503:  
"L'expliquer par une erreur de la traditions de l'itala,  
c'est dire simplement qu'elle est inexplicable".

<sup>9</sup>JBL 69 (1950), pp.144ff.

<sup>10</sup>ExpT 83 (1971-72), pp.56-7; cf. Blass-Debrunner-  
Funk §193.5.

<sup>11</sup>St. John, pp.68-69.

<sup>12</sup>Cf. St. John, pp.258-59.

<sup>13</sup>John, I, (E.T.), p.425.

<sup>14</sup>St. John, loc.cit.

<sup>15</sup>St. John, pp.194-95.

<sup>16</sup>John (E.T.), p.178.

<sup>17</sup>Loc.cit.; cf. J.P. Lange, The Life of Christ (E.T.),  
vi, p.288